WASHINGTON SEA GRANT PROGRAM

POPULATION DYNAMICS OF WORLD FISHERIES

May 1972

By John A. Gulland

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Foreword

In 1971, the University of Washington was designated as a Sea Grant College by the U. S. Department of Commerce with grants administered by the Division of Marine Resources at the University. One of these grants, "Aquatic Stock Management," under No. 1-35320, was established to promote innovative teaching related to management of renewable resources, and to supplement and broaden instructions being given at the College of Fisheries and in the Quantitative Science Center. To help satisfy these objectives, Mr. John Gulland, Chief of Fishery Statistics and Economic Data Branch, Fisheries Department of the Food and Agriculture Organization of the United Nations, was invited to present a series of lectures on the application of population dynamics to world fisheries.

The fifteen lectures delivered by Mr. Gulland in the fall of 1971 are presented in this book. Editing was purposely restricted in an attempt to maintain the narrative oral style of the original presentation and to convey the dry wit of Mr. Gulland. Thus, some duplication exists. The reader conscientious of details will detect some inconsistencies and deviations from the commonly accepted international nomenclature. All this tends to emphasize one of the main theses of the author: To apply population dynamics to meaningful problems, some simplification and approximations are in order. If we wait until all details have been clarified, we may have lost our ability to change the course of events.

Undoubtedly, the lectures will be modified and revised in time. Until then, they serve as a comprehensive and authoritative documentary on the state of global fisheries today.

Thanks are due to Mrs. Connie Jennings and Mrs. Ruth Jackson for their typing of the manuscripts.

Ole A. Mathisen
Professor, Fisheries Research Institute
University of Washington

Chapter 1

INTRODUCTION: GENERAL DESCRIPTION OF WORLD FISHERIES; THE RELEVANCE OF POPULATION DYNAMICS TO PRESENT-DAY PROBLEMS *

The purpose of this series is threefold: first, to describe some of the major world fisheries especially those outside the Northeast Pacific; second, to review some of the techniques used in fish population dynamics; and third (and most important), to see how the theory of fish population dynamics can be applied to real fisheries. In particular, we wish to explore how this application can help the people dealing with these fisheries--the fishermen, the fishing industries, the administrators, and the governments--to solve their problems (or if not to solve their problems completely, at least to get some way towards their solution).

Tables 1A and 1B put down in summary fashion some of the statistics relating to these fisheries during the last thirty years. The first table gives the catches by countries and outlines for the dozen or so biggest countries, that is the countries landing the most fish in 1969, the trends of their catches since 1938. All these tabulations are set down in thousands of metric tons. It is very noticeable that there is considerable geographical spread among the top six or seven countries. For example, there is a country from South America at the top, then Asia, USSR, Asia again, then Europe, North America, and Africa. Thus all of the continents except Australia have a representative in the top half dozen fishing countries.

*Contribution No. 363, College of Fisheries, University of Washington

Perhaps as striking as the geographical spread of major fishing countries is the fact that this spread covers both already developed and newly developing countries. Right at the top we find Peru, which since 1965 has been the biggest fishing country in terms of weight. Then comes Japan and then a number of other richly developed countries, USSR, Norway, USA, South Africa. Then we have a number of developing countries, India, Indonesia, Thailand. All of these have very substantial fisheries.

Another thing we notice is the rate at which some of these fisheries have changed. The outstanding one, of course, is Peru. In 1938 it was a very small fishing country with a mere 23,000 tons of fish. This situation didn't change much until 1955, but about then a real takeoff began. Peru developed the fish meal fishery for anchoveta, and between 1955 and about 1963 the catches doubled each year. By 1965 Peru was up to 7 million tons compared to Japan, with just under 7 million tons, and the U.S., with well under 3 million tons. Since then Peru's expansion has slowed down. They were taking just about all the fish that the stocks could stand. This slowing down was not set merely by the productivity of the stock, and the need to take conservation measures, but also by the capacity of the market to absorb all of this fish meal.

Peru, however, isn't the only country to have had a very rapid increase; since 1948, several other countries have increased their catches severalfold. For example the catches of the USSR have gone up 4 times, South Africa's have gone up roughly 10 times. This was mainly for the same reason as Peru, namely the building up of a large fish meal fishery. But perhaps more significant in terms of the real contribution to the food needs and the hope we have of developing countries building up their fisheries and their food supply is the rate at which some of the other countries have developed. The most significant one listed here is Thailand, which went up nearly 6 times in the 9 years between 1960 and 1969.

Table 1A

World Fish Catches (thousand tons)

	1938	1948	1955	1960	1965	1966	1967	1968	1969
Peru	23	84	235	3569	7462	8789	10134	10520	9224
Japan	3678	2518	4908	6913	6908	7103	7852	8670	8623
USSR	1523	1485	2495	3051	5100	5349	5777	6082	6498
China				5600					
Norway	1128	1422	1813	1543	2307	2865	3269	2804	2481
USA	2260	2417	2790	2813	2724	2543	2430	2442	2495
South Africa	67	184	600	864	1368	1184	1696	2200	2130
India			480	593	1331	1368	1400	1526	1605
Spain	408	547	770	970	1341	1363	1438	1503	1486
Canada	837	1053	965	934	1262	1346	1296	1499	1408
Denmark	97	226	425	581	841	851	1070	1467	1275
Chile	32	65	214	340	708	1384	1053	1376	1077
Indonesia	476		673	761	1067	1202	1180	1159	1209
Thailand	161	161	213	221	615	708	847	1089	1270
UK	1198	1206	1100	924	1047	1068	1026	1040	1083
World Total	21100	19600	23900	40000	53700	57400	61100	64300	63100

Of course we have to treat all these statistics with caution, particularly for developing countries but also to a lesser extent for the others. Sometimes an increase in the recorded catches may only reflect an improvement in the collection of statistics, rather than a real increase in catch. However, the data here are the best presently available, and certainly the large increases like those of Thailand are more than can be accounted for by improved statistical systems.

In addition to those appearing in this table, many other developing countries are building up their fisheries very rapidly. Obvious examples are Korea and Taiwan in Asia and Ghana in Africa, among others. There seems, in fact, to be a rather critical period in a country's fisheries development where the fishery tends to take off and change over from a small-scale semisubsistence fishery into a major industrial scale fishery, and this seems to occur when the country as a whole has a degree of industrial capacity and ability. The financial and economic base is there, but at the same time the standard of living is relatively low. People are prepared to go to sea and work long hours under bad conditions for relatively low pay. Thus we find that in western Europe, in England and Germany, the big fishery developments took place in the last part of the last century up to about 1900. During that period, the catch expanded very rapidly. Similarly in Japan in the 1950's the catches, as the table indicates, increased rapidly and then tended to flatten out. The developing countries mentioned above like Thailand, Korea, Taiwan, Ghana, have all recently built up a fairly substantial economic and industrial capacity and ability, yet still have a basically low standard of living.

Table 1B gives the catches in terms of the major species. The first eight in this list give, in order of the 1969 catches, the species contributing the biggest quantity in terms of weight to the world catch. A couple of other

Table 1B

World Catch of Species and Groups of Species (thousand tons)

Selected Species	1938	1948	1958	1961	1964	1965	1966	1967	1968	1969
Anchoveta	1	1	777	5270	9797	9681	9621	10530	11272	9709
Atlantic cod	2085	2085	2560	2955	2677	2766	2874	3181	3956	3654
Alaska pollack	191	192	345	465	918	1042	1221	1735	2201	2552
Mackerel	370	322	540	657	923	1159	1442	2058	2331	2407
Herring	1752	2233	2551	2473	3515	4002	4088	3877	3285	2358
S. African pilchard	1	69	511	804	1162	1119	941	1198	1728	1613
Haddock	375	406	442	462	589	747	729	484	488	963
Capelin	17	16	104	224	38	281	521	513	623	855
Yellowfin tuna	50	99	199	278	255	247	271	239	332	345
Pacific halibut	27	33	39	42	41	42	43			
Species Groups										
Freshwater fish	2200	1800	4400	5720	6160	6310	6370	6570	6650	6830
Salmons, etc. (a)	923	504	636	586	552	599	649	567	557	525
Flounders	350	500	790	1330	990	960	1090	1210	1160	1270
Cods	3370	3610	4490	5050	6050	6730	7240	8210	9460	9820
Redfishes, basses, etc.	1070	1190	2230	2400	2960	3180	3220	3170	3210	3160
Jacks, mullets	360	550	1740	1970	2010	2160	2130	2080	1990	1970
Herring, anchovies	4800	4720	7360	12600	18500	16980	18730	19750	20590	18290
Tunas	390	380	990	1200	1390	1390	1520	1580	1620	1670
Mackerels	640	570	990	990	1190	1510	1840	2510	2860	3030
Sharks, rays	290	330	340	370	400	400	430	430	460	510
Various fishes	3940	2920	4940	6120	7560	7660	7850	8360	8650	8880
Crustaceans	600	620	850	980	1170	1190	1280	1360	1450	1490
Molluscs	1200	1340	2070	2470	2670	2880	2960	3090	3350	3170

(a) excludes capelin

species have been added for local Pacific interest--the yellowfin tuna and also the Pacific halibut. Most of these species, however, are from outside the Pacific, and the only major Pacific species here is the Alaska pollack. An interesting and important feature of most of the major species is that nearly all are exploited by a number of countries. Thus some 30 countries catch appreciable quantities (over 1,000 tons) of mackerel, and the Atlantic cod supports very large fisheries (over 100,000 tons) in ten countries in Europe and North America. In the second part of the table, the total world catch has been divided into species groups. These are the groups used by FAO in publishing their world fishery statistics, (all figures given here have been taken from the FAO Yearbook of Fishery Statistics). The species groupings have been to a large extent a matter of convenience. Some of them are very clear biological groupings--for instance, the flounders, the cods, tunas, and the sharks and rays. Others tend to be rather a grouping of species left over. The main one here is the red-fishes and basses, which is a whole group of miscellaneous fish including many of the important tropical demersal species.

In neither of these tables, has there been an attempt to change over from weight to value. It would be very difficult, and furthermore the value isn't always recorded. We don't get value, for instance, from the USSR. If we did change from weight to value, it would, of course, make some differences in this table. The main one would be for the anchoveta to appear lower down the list. To bring the figures into a comparative scale, for instance equivalent in value per pound to Atlantic cod, we would divide the figure for the Alaska pollack by approximately 2. On the other hand, we would increase some of the other species to bring them into compatible value with cods. Salmon we would roughly double and tuna we would double. If we make these adjustments, the biggest change is to bring down anchoveta from being the biggest single species in terms of weight in the world, to be still one of the major species in terms of value, but coming after cod, pollack, mackerel, and herring.

Now let's examine briefly where these fish are caught. The anchoveta is caught off Peru and northern Chile; the Atlantic cod goes right across the shallow waters of the North Atlantic in a succession of more or less discrete stocks in discrete fisheries from New England, to the North Sea and the Baltic. Big stocks occur off Newfoundland and Labrador, off west Greenland, around Iceland, and in what is commonly called the Northeast Arctic; it spawns off the Lofoten Islands and feeds in the Barents Sea, as far north as Spitsbergen. The Alaska pollack is a North Pacific species occurring from northern Japan eastwards to the Bering Sea and the Gulf of Alaska. Mackerels are very wide-ranging species. Big catches are taken in the northern North Sea and in Japan. Herring, another North Atlantic species, are caught in the North Sea, and off Norway and Iceland, and on the west side from the Gulf of Maine and George's Bank northwards to Southern Newfoundland. Most of the main individual species come from these northern areas, North Atlantic and North Pacific. This gives a slightly misleading impression of the importance of the northern areas because these area tend to have a small number of species but large quantities of them; whereas the tropical areas contain a large number of species, but not very many of each.

Groups such as the redfishes, the jacks, and mullets don't appear among the major individual species but do make up an important part of the total world catch. The important trawl fisheries in the Gulf of Thailand, and in other tropical and subtropical areas, are of mixed species. The very important fisheries developing off Northwest Africa are for a number of pelagic species--including mackerels, but also sardinella, horsemackerel and others. In summary, the major fisheries occur in two main areas, first, the area of wide continental shelf, such as the North Sea, the Grand Banks of Newfoundland, the Bering Sea, and the wide area of tropical continental shelf from Indonesia around to the Yellow Sea and Japan. The other major ones are the upwelling areas with Peru, of course, the classical example. There are also upwelling areas in which important fish stocks occur off Southwest Africa and Angola, off Northwest Africa and also off California. This means that the fisheries tend to be very localized and very often the fish stocks are not where the people are or where the demand is. This had led to the development of long-range fishing fleets; the ones best known of course are from Russia and are based at Murmansk, Kaliningrad, and on the Pacific. Japan also has very important long-range fishing fleets both for tuna and for other species. There are, in fact, some 20 or more countries with long-range fleets including many from the countries of the western world such as Germany, England, and Spain; most of the eastern European countries, including Poland, East Germany, Rumania and Bulgaria, as well as several newly developing countries. Korea and Taiwan have large long-range fleets, particularly of tuna vessels. In Africa, Ghana has found that the richest fisheries off Africa are in the northwest and southwest, a long way from Ghana. It has therefore bought a fleet of long-range trawlers, which now fish as far away as Morocco. Nigeria and other African countries are considering doing the same, that is, to expand fishing activities away from the tropical Gulf of Guinea where there is a very high human population but not much fish, to these areas off the desert coasts where there is a high fish population but not many people.

Turning back now to the table of individual species, we see that it has been arranged in terms of weight caught. There is another way of looking at this table, which is in terms of the maturity of the fishery, that is the extent to which the fishery has settled down into a more or less stable situation. The oldest fishery in this sense, the most mature and the most firmly established fishery, described in the table is that for the Pacific halibut, which is the real reason why it is included. This shows from 1948 onwards a very stable catch. At the other extreme, we find the capelin with very rapid expansion and no sign yet of flattening out. This would be clearer if the 1970 figures had been included. The 1970 total world catch of capelin, most of which comes from Norway, was well over one million tons. Between capelin and halibut, we have the other fisheries. Some like capelin are still expanding; yellowfin tuna, for instance, appears to be expanding but not very rapidly, though as yet the total world catch of yellowfin shows no sign of flattening out. South African pilchard is another one that looks as though it is still expanding.

On the other hand, as the middle of the table shows, there are fisheries such as the anchoveta, which had a clear period of rapid expansion but which for the last five or so years has tended to fluctuate about a fairly stable level. And finally we have the most worrying sorts of fishery of all, where it seems that the fishery has come up to a peak and then gone down again. The clearest example in the table is the herring, which expanded fairly steadily up to 1966, reached 4 million tons in 1966, dropped down a little bit in 1967, a little bit more in 1968, and finally had a very substantial drop-off in 1969. The 1970 figures will probably show an even bigger drop.

These figures all tend to be rather misleading, however, because, except for the anchoveta, they refer to the combined catches from a number of individual stocks. If we really want to know what is happening, we must look at the catches from each individual stock. For instance the crisis in some herring stocks is very much more serious than the figures of the total catch of the species would indicate. The biggest herring fishery has been the Atlanto-Scandian herring, which spawns off Norway and migrates across the Norwegian Sea to Iceland and back. Two or three years ago, this was producing catches of between 1 and 2 million tons. The 1970 catch was down from this 1 or 2 million tons to about 20 thousand tons.

These trends in catches of various species illustrate the big questions directed toward the fishery scientist and particularly the fishery scientist specializing in population dynamics. The first of these is: Given a fishery that is just starting--a young fishery that is expanding--just how long will this expansion keep up and at what level will the expansion cease and at what level will the catches tend to flatten out? For instance, it would have been very useful in Peru about 1961 to be fairly clear that the catches were going to flatten out at around the 10-million-ton mark rather than around the 5-million-ton mark, or around the 20-million-ton mark. This knowledge, if available, would have enabled the government and industry to adjust themselves to the future capacity of the stock. As it was, the capacity of the fishing industry in Peru continued to expand very rapidly beyond the 1961 level until the present capacity of the fleet and of the processing plant in Peru is such that they could catch, handle, and turn into fish meal the entire world catch with no great trouble. However, since they have a mere 10 million tons of fish to handle, the Peruvian government has had to introduce many efficiency-restricting devices familiar in the fisheries of the North Pacific--a limitation on the number of days of fishing per week, closed seasons, etc. Hence to determine how long an expansion will last, and at what level it will flatten out is particularly important to developing countries because this enables them to establish some rational schemes for investment and for fisheries development. To know whether to put in a fleet and processing plants for a 10-thousand-ton fishery or a 20-thousand-ton fishery can save a great deal of money and other resources. The loss of capital (which in these countries is usually very scarce) in putting the wrong capacity of fleets or the wrong capacity of plants, can be very substantial.

The second problem occurs in a mature fishery, one where the flattening out of total catch has already taken place. The questions then are: Can measures be taken to insure that there is no drop-off in catch? What is necessary to make quite sure that the level of production reached will not in fact fall off still further? Will the situation be like that of the herring, where there is a serious drop in total catch? If a drop is possible, then the biologist must advise on the steps necessary to prevent it, for example, on any necessary restrictions on the total catch. In a stable fishery, advice is also needed about whether any measures can be taken to increase the average level of the catch.

It is this second group of problems on how the fishery can be maintained at a high level of sustained physical yield that have received most attention from the fishery biologist. As already stressed, the provision for early advice on the ultimate potential of the stock is also very important, as is the third type of question--that is, given the situation that the fish stock is being harvested at a rate providing close to the maximum potential harvest, how can this catch be taken at the least cost and with the greatest efficiency? Though this economic aspect has been emphasized mostly in relation to the fisheries of already developed countries, it is probably even more important for developing countries, which cannot easily afford any economic waste. The Pacific halibut regulations have been criticized because, although the stocks have been rebuilt and a high physical yield has been maintained, the regulations have reduced the efficiency of the fishery. However it is doubtful whether the loss of the possible net economic yield is very serious for the economy of western North America and it may not be socially undesirable to continue what is being described as a form of pension for old halibut fishermen. On the other hand, Thailand can ill afford to have more resources than necessary tied up in harvesting the large but limited catch of demand fish from the Gulf of Thailand. These economic considerations will not be discussed in much detail since we are more concerned with the purely biological aspects of various individual fisheries. It is, however, most important that the economic factors be included when the results of the biological analyses are used in preparing advice for fishermen and fisheries administrators.

The various problems--how long can expansion be continued, at what level will the fishing stabilize, and can this level be increased?--are best illustrated by a simple curve relating the yield to the amount of fishing. At the outset, this will increase fairly steadily, then flatten out, and then continue at about the same level (Figure 1A, curve a) or decrease slowly (curve b), or drop off rather sharply (curve c). The task of the fish population dynamics scientist is to determine first the shape of the curve for the particular fish stock he is concerned with, and then

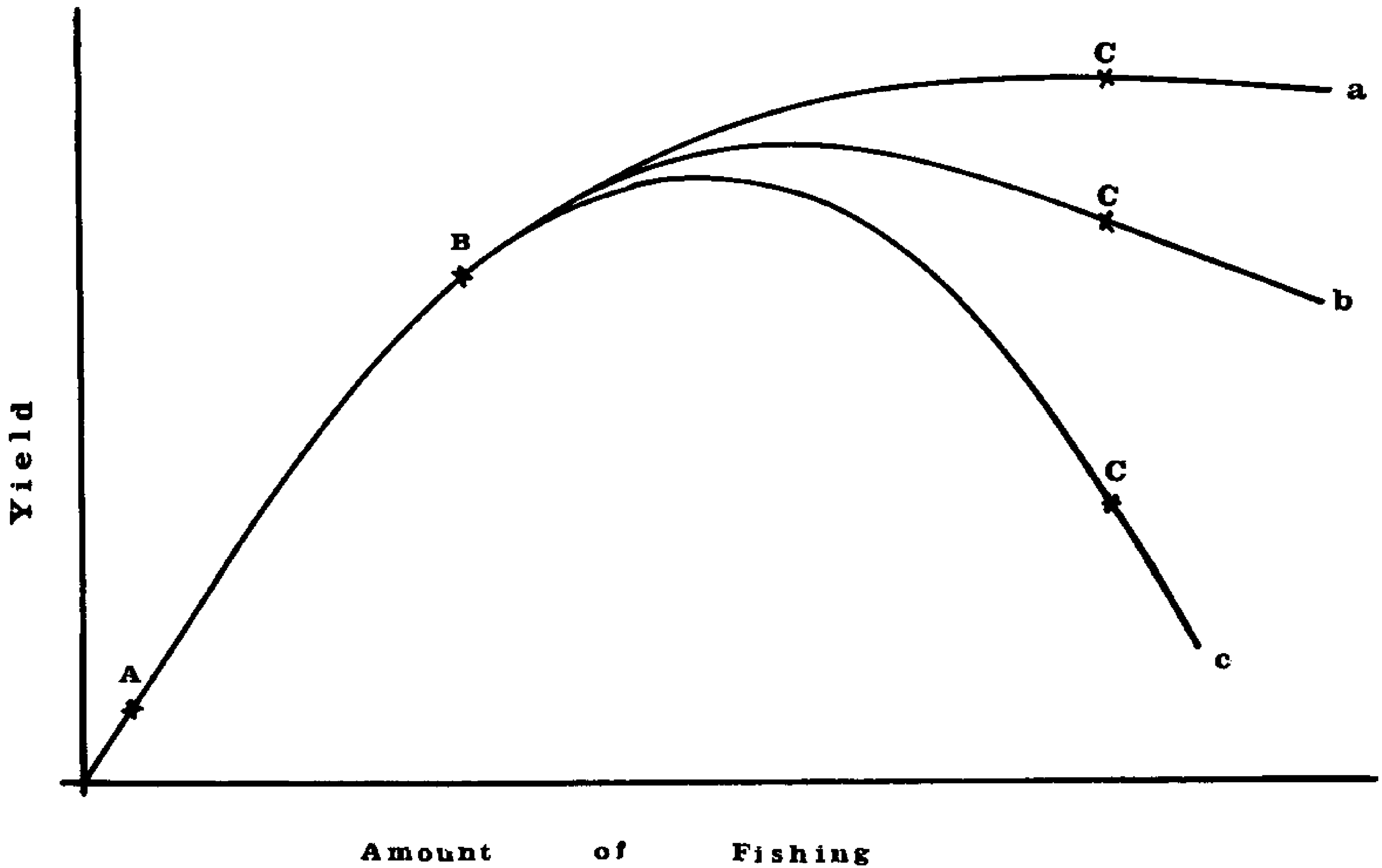

Fig. 1A : Relation of sustained yield to the amount of fishing the present position of the fishery on this curve. If we could draw up this curve precisely and say where we are on it precisely, we would then have very nearly all the information required to advise the fishermen, the fishing industry and the government what to do. For example, if the fishery was known to be well down on the left hand side, say at point A, then it would be clearly possible to expand and there is little need for further advice until, say, catches had increased, say seven times. If the fishery is on the left-hand shoulder, point B, only a very slight expansion is possible; if the fishery is over on the right-hand side, at C, it is desirable to reduce the amount of fishing particularly if we are on curve (c).

There are, of course, other aspects of this curve in which the yield has been expressed simply as a function of the amount of fishing in its broad terms. In many fisheries, the opportunity exists to change not only the amount of fishing but also the composition of the catch. It is possible to change over from catching all sizes of fish to catching just big fish or catching just small fish or catching fish before they spawn, or catching fish after they spawn. Possibilities like this change the two-dimensional diagram of catch against the amount of fishing into a multidimensional diagram. In particular, it is possible to draw up a three-dimensional isopleth diagram where yield is a function of the amount of fishing and the age at first capture, that is the age of the youngest fish that appear in the catches. A typical isopleth diagram of this type is shown in Figure 3D. This has a high point with a large amount of fishing at a high age of first capture--that is, we get the biggest catches in many fisheries if we wait until the fish are well grown and then catch pretty well all of them. We tend to get less if we either fish too hard with too small an age at first capture so they they are caught before they have time to grow, or if we do not fish hard enough, or with too high an age at first capture, so that many fish die before they are caught.

Chapter 2

YELLOWFIN TUNA IN THE EASTERN PACIFIC: SIMPLE ANALYSIS OF CATCH AND EFFORT DATA

We now begin the real meat of our subject--discussion of specific fisheries, the application of particular methods in population dynamics to these fisheries, and the provision of advice to fishing industries about what is happening to the fisheries and what may be done to improve matters.

Let us begin with the yellowfin tuna and particularly the yellowfin tuna in the Eastern Tropical Pacific, the area from California down to Peru. The yellowfin tuna has a worldwide distribution in all the tropical oceans, and it is fished in all these areas. It is a fast-growing fish, reaching up to 80 cm at the end of its second year or about 25 pounds and growing by the fourth or fifth year to a maximum size of 150 cm in length and 100-150 pounds in weight. Not too much is known with any certainty about its movements within each ocean. It appears not to range so widely as some of the other tunas, particularly the albacore and the bluefin tuna, so that within each ocean there seem to be more or less discrete stocks, and the fisheries in each part of the ocean can be treated to some extent independently. For the present it will be assumed that this is true of the yellowfin tuna in the Eastern Tropical Pacific and that the fish in this area can be treated as a single stock, independent of other areas.

Figure 2A gives a rough outline of the fishing area, and shows the regulatory area of the Inter-American Tropical Tuna Commission.

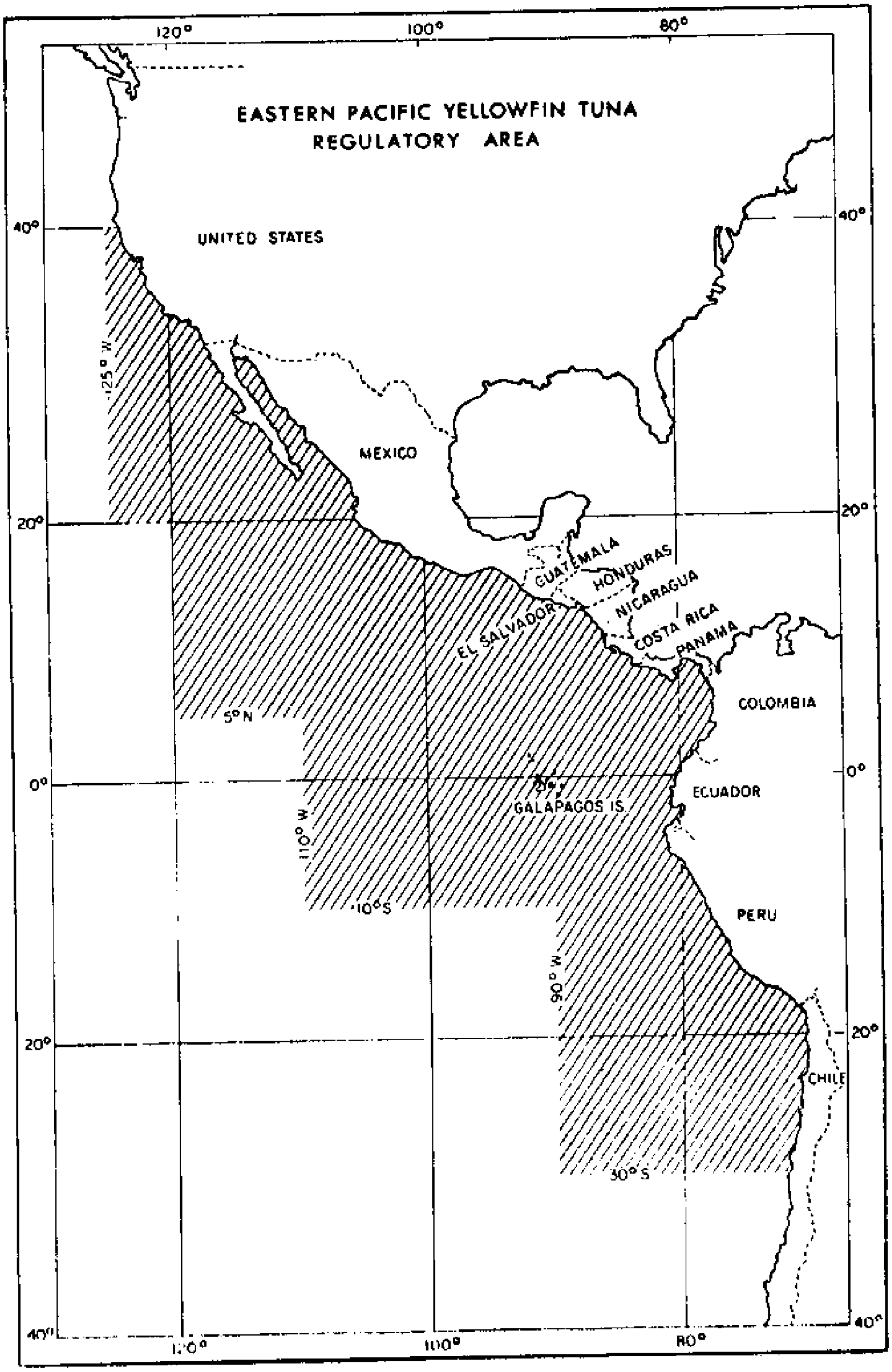

Figure 2A. The Eastern Tropical Pacific, showing the regulatory area used by the Inter-American Tropical Tuna Commission.

Initially the fishery for yellowfin in this area was based almost entirely on live bait fishing. This is a matter of throwing live bait into the sea to attract the fish and to get them into a feeding frenzy and then catching them with a pole and line. Because this method depends on a good supply of live bait--small fish such as sardines or anchovy--it has been confined mainly to an area fairly close to the coast all the way down from southern California to Peru and Ecuador, but particularly in the area of Mexico and Central America.

Since about 1959 another fishing technique for this stock, using purse seines was developed, and between 1959 and 1962 a very large proportion of the existing bait boat fishery changed over to purse seines. The percentage of the total catch taken by bait boats changed from around 90% or more until 1959 down to 10-15% after 1962. Another more recent development of importance has been the improvement of purse seining techniques and the building of bigger, better, and more modern seiners and the replacement of the small boats that formed the bait boat fishery and the initial purse seine fishery--small boats catching perhaps a couple of hundred tons--by the increasingly large super seiners carrying up to a thousand tons or more or tuna. With these big boats the tendency has been to expand out farther from the coast well into the western side of the regulatory area and indeed beyond it. In the last few years since regulations closed yellowfin fishing in the Commission's area in the later part of each year these big super-seiners have been moving out into the Atlantic, particularly into the Eastern Tropical Atlantic and the Gulf of Guinea, and also making exploratory trips westward across the Pacific to the longitude of Honolulu and farther west.

So much for the fishery. Fishery research and general studies of this fishery have been carried out principally by the Inter-American Tropical Tuna Commission, which was set up in 1950. This type of commission is very familiar in the Eastern Pacific but less familiar to people working in the Atlantic. The Atlantic type of commission usually has very small permanent staffs, and is purely an organization for coordination and cooperation and for holding meetings. The basic research needed to study various fish stocks has always been carried on and financed directly by the national governments involved. However there has been always in these Atlantic commissions very close cooperation between the scientists with little emphasis on individual national viewpoints. In the Pacific, the tradition has been for the commission itself to carry on research with its own permanent staff, though of course ultimately this is financed by the member governments.

The Tuna Commission, I-ATTC, is a good example of the work that has been carried on. Studies have included the collection and immediate analysis of catch and other statistics about fisheries. In addition there has been important theoretical work in developing models to study population dynamics of the fisheries, and also, particularly in the early years of the commission when it was relatively better financed than it is now, valuable basic studies about the biology of the area and the productivity of the waters.

Let us turn now to the models that have been developed for fisheries in general, but specifically for the yellowfin fishery. The name one always associates with this work is that of Dr. M.B. Schaefer, who was Director of Investigations of the Commission for all of its early years. These models, (Schaefer, 1954; 1957) treat the fish population as a simple mass of fish, and subject to simple laws of population growth. The basic assumption is that if the population is less than the capacity of the ecosystem in which it lives, it will tend to increase, and that the rate of increase will be some function of the population biomass.

$$\frac{dP}{dt} = f(P) \qquad (2.1)$$

This takes no account of fishing, but if in addition a catch C is taken from the stock, the actual change of population during a unit interval of time, say a year, will be given by

$$\Delta P = f(P) - C \qquad (2.2)$$

If the catch from the stock is equal to this natural rate of increase i.e. $C = f(P)$, then the population will remain unchanged. To apply this model in practice, it is necessary to have some expression for $f(P)$. The two limiting conditions are that if there is no population it can't increase, and at its maximum it also will stop increasing. The simplest expression that will do this is

$$f(P) = a\,P\left(P_{max} - P\right) \qquad (2.3)$$

where P_{max} is the maximum population. This expression will have its greatest value when the population is half its maximum value. Alternatively, considering the yield that can be taken on a sustained basis, this will be a parabolic function of population abundance, and the maximum substained yield on this model, will occur at half the maximum, unfished abundance

$$C_s = a\,P\left(P_{max} - P\right) \qquad (2.4)$$

To enable this theory to be used in any particular situation, it is necessary to have some way of locating the present position of the fishery on this curve and particularly some way of knowing what the population is relative to the maximum population, or the population giving the maximum sustainable yield. The usual assumption in fisheries literature is that the population is proportional to the catch per effort. The usual constant of proportionality used is q, that is $P = \frac{1}{q} \cdot$ (the catch per unit of effort). A major difficulty in many fisheries is determining a satisfactory measure of catch per unit of effort or of effort; tuna is no exception. The three quantities--the catch, the effort, and the catch per unit of effort--are interrelated, and only two need be determined independently. It is not necessary that the two independent measures be catch and effort, from which the catch per unit effort is estimated. It is possible to have a reasonable measure of abundance from the catch per unit of effort by a section of fishery or from research surveys and to estimate total effort from the catch per unit of effort; however obtained, the expression of sustained catch in terms of population can be rewritten in terms of catch per unit effort, that is

$$C_s = a \frac{1}{q} U \left(\frac{1}{q} U_{max} - \frac{1}{q} U \right) \qquad (2.5)$$

or changing constants,

$$C_s = b\, U \left(U_{max} - U \right)$$

also

$$\frac{C_s}{U} = b \left(U_{max} - U \right)$$

and since catch divided by catch per unit effort is equal to effort, this shows that in a steady state, catch per unit effort and effort are linearly related, that is

$$f = b \left(U_{max} - U \right)$$

or

$$\frac{C_s}{f} = U = U_{max} - \frac{1}{b} f \qquad (2.6)$$

or

$$C_s = U_{max} \cdot f - \frac{1}{b} f^2 ;$$

The plot of catch, in a steady state, as a function of fishing effort will be parabolic. The precise relation between catch and effort, or catch and population abundance depends on the assumption made concerning the nature of the function $f(P)$, but whatever the assumption made, the general shape of the curve of catch as a function of effort will be the same. It will pass through the origin, and a very large value of effort will reduce the stock to a very low level or zero, and give little or no catch. At some intermediate point, there will be some value of effort that gives the maximum catch. This maximum sustained yield (M.S.Y.) is a common term in fisheries literature, particularly in theoretical studies of the objectives of managing and regulating fisheries.

A number of methods can be used to fit these concepts to the actual observations of a particular fishery, but the simplest is to plot catch per unit effort against effort, which, from equation (2.6), should be a straight line. The same result can be obtained from a slightly different theoretical argument. This is, that other things being equal, the abundance of a fish stock will be determined by the amount of fishing--the more fishing, the fewer fish will be about--that is, in a steady-state situation, the catch per unit effort will be a function of the effort and the simplest function to assume is a straight line. Virtually no fishery is in a steady state, but the best initial assumption is usually that the fishery being analyzed is nearly in a steady-state situation. The first approximation to the steady-state situation is therefore to use values of catch, catch per unit effort, and effort applying to particular years, and to plot, for example, catch per unit effort in 1960 against the effort in 1960 and so on for 1961, 1962, 1963, and fit a line through them. This gives the estimated steady-state relation between catch per unit effort and effort. From it the corresponding relations between catch and effort, or catch and population abundance (as measured by c.p.u.e.) can easily be derived.

Although fisheries are in a steady-state situation, there are various ways of improving the situation. The one used by Schaefer is to go back to equation (2.2) which states that the change in the population during the year is equal to the net rate of increase of the population, which is a function of the mean population during the year, less the catch. This can be rewritten as a function of the mean catch per unit effort during the year, i.e.

$$\Delta P = f\left(\bar{U}\right) - C$$

or in terms of the change in catch per unit effort

$$\frac{1}{q}\left(\Delta U\right) = f\left(\bar{U}\right) - C \qquad (2.7)$$

To apply this, it is necessary to have some estimate of the coefficient q, and also of the change in c.p.u.e. during the year. This is not often directly available. The available estimate of catch per unit of effort normally refers to a period, very often to the mean catch per unit effort over the year, whereas to estimate the change during the year, point estimates of the population on January 1 and December 31 are required.

The best estimate immediately available for the population at the beginning of 1962 is the average of the average population in 1961 and 1962. This will be a reasonable first estimate of the population on January 1962; similarly a reasonable estimate of the population at the end of 1962 is average of the average population in 1962 and 1963. Then the estimate of the change of population during 1962 is half of the difference between the mean population in 1961 and 1963; i.e. in terms of c.p.u.e.

$$(\Delta U)_{1962} = \frac{\bar{U}_{1963} - \bar{U}_{1961}}{2}$$

This gives an expression to put into equation (2.7) for the change in population in 1962 as a function of the mean catch per unit effort in 1962 and the 1962 catch. This will enable a better estimate to be obtained of the relation between catch per unit effort and effort, or between effort and total catch in the steady state, even when the fishery itself is not in a steady state. Another way of doing this is to consider that one of the major reasons why a fishery may not be in the steady state is that the effort is changing or has been changing. For example, most of the fish alive in 1962 were exposed to fishing not only in 1962 but also in earlier years, so their abundance is a function of the fishing effort in 1962 and also in earlier years. One way of dealing with this is to plot the catch in 1962, not as a function of the effort in 1962 but as a function of the average effort in 1962 and earlier years, and to go back the same number of years as the average life expectancy of fish in the exploited stocks. This means that if there are fish up to 8 years old, the average life expectancy in the fishable stock may be about 3 years, so that the catch per unit effort in 1962 may be plotted as the function of the average effort in 1962, 1961, and 1960. This will therefore give a relation that is rather closer to the steady-state situation. Even with these adjustments, the relation obtained may be some way from the true steady-state situation. One reason is that the natural rate of increase of the population in say 1962 will not depend simply on the population in 1962. An important element of the increase of the stock in 1962 is the recruitment of young fish entering the stock in 1962 which may come from a parent stock several years back. For this and other reasons, the reaction of the stock will have more lag effects than these simple expressions used here. However, these expressions do prove useful.

Turning to the application of the model to the yellowfin fishery, the basic data are shown in Table 2A (derived from publications of the I-ATCC, especially its Annual Reports). The results are given in Figure 2B, which shows the two important plots, the top one the plot of catch per unit effort against fishing effort, the main diagram used in analyzing the situation. The bottom one is the corresponding plot of total catch against fishing effort, which is the more useful form for explaining the situation to fishermen or administrators. The first thing to note in the upper figure is that for the data up to 1953 there are two clusters of points, one at a high catch per unit effort and a low effort for years up to 1949 and the other at a rather higher effort and lower catch per unit effort for the period after 1950. It is not surprising that a straight line gives a satisfactory fit to the points. The important thing as far as the fishery is concerned is that having drawn this line and the corresponding curve in lower figure, labelled logistic in Figure 2B, there is available quite a satisfactory description of the state of the fishery. Up to about 1950, all the points were down on the left-hand

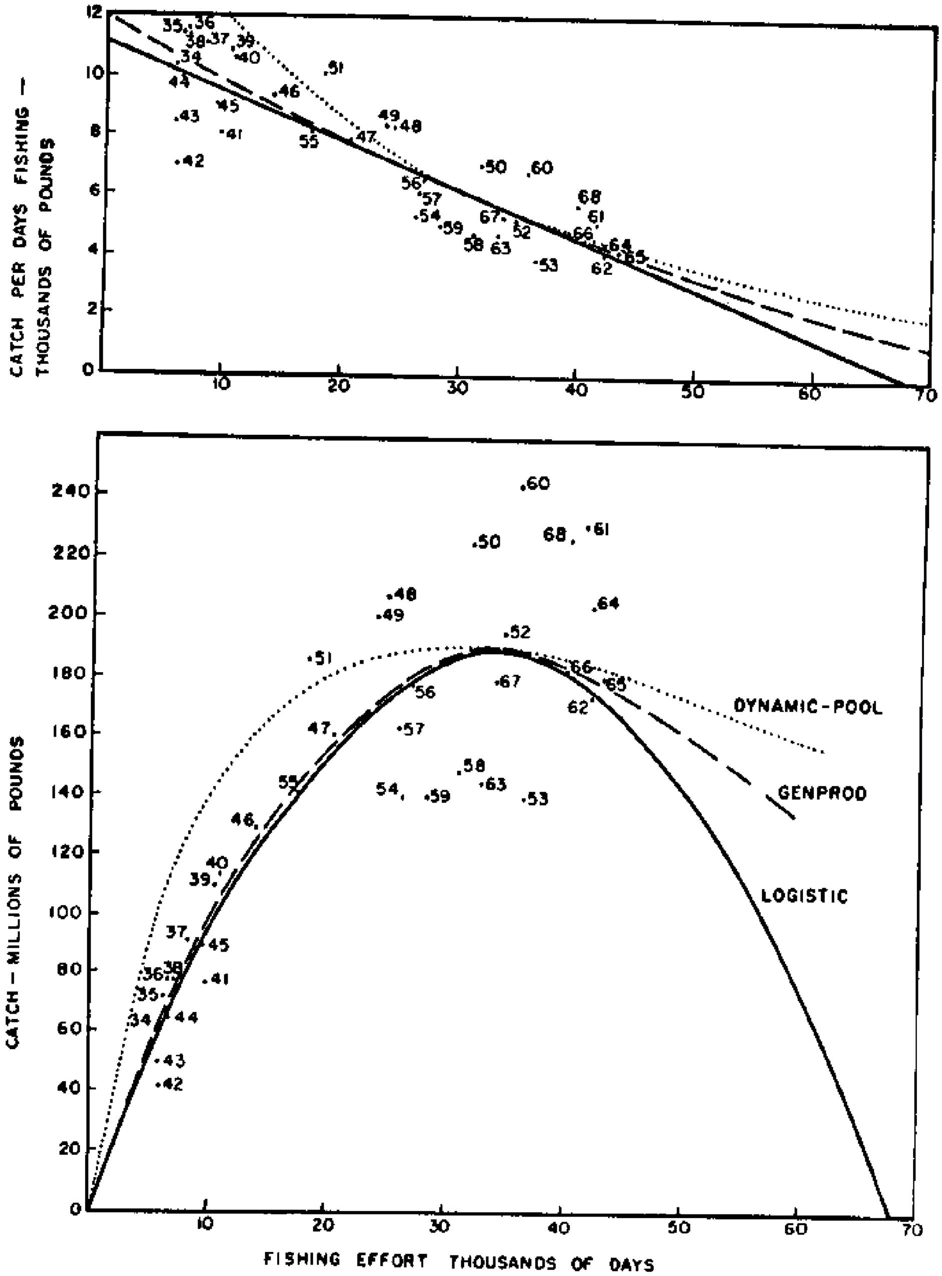

Figure 2B. The relations between fishing effort and catch per unit effort (upper figure) and total catch (lower figure) in the yellowfin tuna fishery, showing lines fitted by various theoretical models (from I-ATTC Annual Report, 1968).

Table 2A

YELLOWFIN TUNA IN EASTERN TROPICAL PACIFIC

Catches and Catches Per Unit Effort

Year	Catch 1,000 Lbs.	Catch/Day Lbs. (A)	Year	Catch 1,000 Lbs.	Catch/Day Lbs. (A)	(B)
1934	60,913	10,360	1955	140,900	7,895	
35	72,294	11,480	56	177,000	6,700	
36	78,353	11,570	57	163,000	6,200	
37	91,522	11,116	58	149,900	4,500	
38	78,288	11,463	59	145,400	5,220	
39	110,417	10,528				
1940	114,590	10,609	1960	234,200	6,817	
41	76,841	8,018	61	239,800	5,544	
42	41,965	7,040	62	172,500	4,298	
43	50,058	8,441	63	144,300	4,376	
44	64,094	10,019	64	197,700	5,166	
1945	89,194	9,512	1965	188,700	7,673	7,507
46	129,701	9,292	66	187,900	5,773	9,168
47	160,134	7,857	67	180,800	6,741	14,286
48	200,340	8,353	68	229,100	5,217	17,146
49	192,458	8,363	69	252,800	10,643	17,777
1950	224,810	7,057	1970	283,500	6,631	16,578
51	183,685	9,809				
52	192,234	6,097				
53	138,623	3,814				
54	138,623	5,546				

(A) - Catch/day of Class 4 bait boats
(B) - Catch/day of Class 3 purse seiners

Data from Schaefer (1957) and Annual Reports of the Inter-American Tropical Tuna Commission.

side of the curve of yield against fishing effort and the fishery was in a fairly satisfactory condition. It was expanding and could continue a moderate expansion without trouble. The fishery was not taking all it could from the stock and there was no need for management, or restrictions. All that was necessary up to 1950 was to fish harder and more efficiently. Also in this period, and indeed rather later--until around 1960, there was no particular difficulty in obtaining a satisfactory measure of catch per unit effort.

The data are set out in Table 2A. In this table from 1965 onwards two values for catch per day are set out: (A) the catch per day of bait boats, and (B) the catch per day of purse seiners. The early calculations were all done in terms of bait boat effort and bait boat units. Both bait boats and the purse seiners have been grouped in the Tuna Commission's statistics according to the size of the boat, running up from class 1--vessels carrying less than 50 tons of fish, up to class 6--boats carrying more than 400 tons of fish. The bait boats have been standardized in terms of the catch per effort of class 4 boats--a capacity between 200 and 300 tons--on the basis of their comparative catches. But from 1960 onwards, the bait boats became scarcer and scarcer. The use of bait boat data for measuring effort and catch per unit effort became less satisfactory. It represented an increasingly small section of the fleet, and it became necessary to change over to purse seine units. The first analysis was done on class 3 purse seiners, then the commonest--100 to 200 tons capacity--and the figures in column B are based on that class. Here again the Tuna

Commission has been unlucky because since they standardized on class 3, the size of tuna purse seiners has been increasing rapidly and continuously and class 3 purse seiners are now a comparatively small and rather unimportant part of the fleet. Most of the new seiners are in class 6 and in fact the need is becoming apparent to subdivide class 6.

Returning to Figure 2B, we see that the fit was very good in earlier years, but that in some of the later years more of a scatter has become apparent, particularly in the lower figure. This becomes even more apparent if the 1969 and 1970 data are included. Up until about 1950 it was clear that the fishing effort was less than in any way undesirable, that there was no need for any restrictions; From about 1950 onwards, however, fishing effort had, at least on this analysis, about reached the level giving the maximum sustained yield, and if it was allowed to increase there was a real danger that the catches would fall off and the fishery would move into the right-hand side of the curve with excess costs, too much effort, and a reduced catch.

This has changed the emphasis in the work of the Tuna Commission, which has been concerned principally with research activities in collecting data, studying some of the basic biology of the tunas and of the water in which the tunas live, and in developing theoretical models.

From 1950 or thereabouts onwards, the Tuna Commission was clearly coming into the management business. It had to consider the introduction of management measures and restrictions on the amount of fishing. It has been a difficult and fairly long process, from the time around 1962 when it could be shown that the effort had gone past the peak and that restrictions were necessary, until restrictions actually went into effect. The first year in which there were restrictions set by the I-ATTC was in 1966. The 1966 and 1967 points in the upper part of Figure 2B fall close to the line, but the fit for 1968 is not very good. Though the effort in 1968 was high, so too was the catch per unit effort. On the basis of the increased effort between 1967 and 1968, the catch per unit effort might have been expected to decrease but in fact there was quite an appreciable increase. As a result, there was considerable pressure from the industry to increase the quota. Because there was some degree of uncertainty about how the curve behaves at levels of effort greater than those so far observed, it was agreed that ther should be an experimental period of not more than 3 years during which the Commission would allow catches higher than the estimated sustainable yield. This would provide data on the right-hand side of the curve and thus determine more precisely the shape of that side of the curve. What did happen in both these years was that there were very high catches. The modern super-seiners were very efficient, caught very good catches, and the true effort increased very rapidly. Though there was some decrease in catch per unit effort, the catches per unit effort in both 1969 and 1970 were higher than expected.

Hence it seems that the simple straight line curve drawn through the points between 1934 and 1960 does not give very good description of what has been happening in the last couple of years. There are a number of reasons why this could have happened. One possible reason is that the fishery has expanded to the westward from the comparatively narrow coastal belt, and is therefore now fishing new stocks of fish in addition to those considered in the original analysis. This would imply that the true effort on the original stock of fish was not really so very high. The original curves describe adequately what is happening to the old stock of fish, but in addition there should be another curve describing what is happening to the more westerly stocks of fish. This doesn't seem to be an entirely satisfactory explanation because there have been a number of tagging experiments showing some degree of movement, though maybe this degree of movement is not really sufficient to insure complete mixing. Then, although the westward expansion might not involve the exploitation of entirely new stocks, it would really add to the potential yield from the available stocks.

Another important factor has been the changeover from bait boats to purse seiners, and particularly from rather small purse seiners with relatively small nets, to huge purse seiners with even huger nets, which has resulted a change in the size of fish being caught. On the whole, in a school of yellowfin tuna the smaller fish tend to be at the top near the surface, and bigger fish to be deeper, and also there is a tendency for small and big fish to keep in different schools. The large seine nets fish much deeper than the bait boats, which catch fish only at the surface. The average size of tuna caught in the fishery has changed very greatly between the pre-1950 period and the most recent period. The average weight around 1950 was between 4 and 10 pounds, whereas the average weight in 1969 was around 20 to 30 pounds, In the simple model, it doesn't matter what size of fish is taken, since the population is considered as a single biomass of yellowfin tuna behaving as a unit, but the more realistic models described later predict that a change in average size, or a change in the minimum size caught will allow the small fish to grow and will give an increase in the yield from a given amount of effort, particularly when the effort is high. This would mean that both curves of Figure 2B should be shifted upwards, particularly at the right-hand side. (A similar change has been shown for the Iceland haddock stock, Gulland, 1961). There should then be one curve for the pre-1950 situations as drawn, and another curve for the most recent situations which would be higher, particularly at the right-hand side at the high levels of effort.

A third possible reason for the departure of the recent points from the curve is that the tuna may not have read these simple models and don't really believe in straight lines. Although the straight line is a perfectly good description of what happens on the left-hand side of the diagram, at higher levels of effort, the true relation may cease to be a straight line and may become a curve. One such curve, shown as a broken line in Figure 2B, is a more general model developed from the logistic curve. This general production model, Genprod, was developed by Pella and Tomlinson, (1969). The Genprod line here has been plotted to the points as in the original data derived by the Tuna Commission. The corrections that have been made to allow for changes in the sizes of boats, in expressing the effort in terms of a given size class of seines or bait boat may not take into account all the changes in efficiency that have occurred. Though they take into account the differences between boats in the fleet during a particular year, which compare the small boat in 1960 with a bigger boat in 1960, and thus estimate the changes in fishing power of individual boats, it will be difficult to estimate changes in the fishing power of the fleet as a whole, that is year-to-year changes that apply equally to all vessels in the fleet. Studies of possible changes are being made which take into account the detailed operations of the fleet--the relative time spent steaming to and from the grounds, searching for fish, and actually fishing. It is possible that the fleet as a whole may be becoming more efficient so that the true effort in 1968 instead of being 40,000 units may have been 60,000 units. This will also affect the catch per day which must be decreased in the same proportion; instead of being 6 standard units, it would really only be 4 standard units. This would move the points in the upper part of Figure 2B downward and to the right and produce a fitted line even more curved upwards, than the dashed, Genprod, line. This would give, in the lower part of 2B, a line that comes up and may continue to increase for values of effort beyond 40,000 days (in the early units of fishing effort). The other lines shown on this diagram, labelled dynamic pool, are those obtained using the dynamic pool or Beverton Holt yield-per-recruit model and applying it to this data. It is possible that by fitting this model rather differently, and particularly using a different relation between fishing effort and fishing mortality, a line that fits rather better would be obtained.

In summary, just how well have we dealt with the problems of the tuna? The first thing that can be said is that the model has been useful. It has described the situation up to 1950 reasonably well. It could and did advise the tuna industry and other people that considerable expansion was possible in the 1930's and 1940's. It also warned them that unlimited expansion wasn't possible. Maybe the figure of 200,000,000 pounds was too low, but in practical terms of the difference in regard to advice and the action of the industry before 1950, it was perfectly adequate. However this model, or at least the straight line model, doesn't appear to be giving very useful advice in the present situation, or rather the present situation is demanding more precise estimates and better extrapolations into unknown levels of fishing effort than the model can provide.

The other question is: What is happening to the tuna industry? The framework of the regulations has been similar to that used for the regulation of several other fisheries, such as the Pacific halibut. The Commission has set a quota depending on the estimate of the state of the stock, and the desired control of the fishing effort. Until this quota is filled, everyone may fish freely and after the quota is filled the fishing is with certain exception, stopped. This is leading to precisely the effects predictable on economic grounds, and observed for halibut and for Antarctic whales--the effective season is becoming shorter and shorter and the number of boats is increasing until the fleet size is badly out of balance with the potential yield from the stock and shows signs of becoming even more out of balance in the next few years. In fact at the present rate of increase in the fleet, it is quite likely that soon the total carrying capacity of the fleet will be greater than the quota allowed by the Commission and this will lead to all sorts of trouble.

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Chapter 3

NORTH SEA PLAICE: THE SIMPLE MODEL OF BEVERTON AND HOLT

Compared with the previously described yellowfin fishery, which is a fairly young and expanding fishery, the plaice fishery in the North Sea is a mature fishery, in which the main expansion in total catches and in the fishing effort took place a long time ago. Since about 1900 the total catches have not varied very greatly. It has been a favorite fishery in the study of fish population dynamics, mainly because it is a fishery for which there are extremely good data, including a very long series of data of total catches. Data for the English catch are available from about 1900 onwards, and from 1906 onwards there are good statistical data for all countries fishing in the North Sea. These statistics have been compiled and published by the International Council for Exploration of the Sea, whose headquarters are in Copenhagen. This council was set up in 1906 and one of its first pieces of work was to arrange for the collection and publication of catch statistics. These have been published in the Bulletin Statistique of ICES since 1906 and are invaluable for the study of this fishery.

One useful thing about the North Sea plaice is that it is a well-behaved fish: we can tell its age quite easily; it is easy to tag, and it doesn't seem to have any big fluctuation. It has therefore been the subject of a number of important studies of fish population dynamics. The work of Beverton and Holt at Lowestoft and also the earliest work on fish population dynamics by Baranof in the USSR were based largely on the data for the North Sea plaice. The North Sea is one of the major fishing areas of extensive shallow water: except for the trench of deep water along the Norwegian coast and into the Skagerrak, all the North Sea is less than 100 fathoms deep. South of the Dogger Bank, which is about 20 fathoms or less, the depths are not more than about 30-40 fathoms. The plaice itself is found in shallow water with a sandy bottom and is therefore concentrated in the southeast part of the North Sea (Wimpenny, 1963).

Figure 3A shows the distribution of catches per unit effort of plaice by Belgian trawlers. The entire North Sea, in fact the whole area around the British Isles, has been divided up for statistical purposes into areas of 1 degree of longitude by half a degree of latitude--roughly 30 miles square. The statistics of trawling operations by most of the countries around the North Sea are recorded in these squares, so that the catch per unit effort in a square is available and gives a very handy measure of the abundance of fish in each square. The data of Figure 3A have been taken from the ICES Statistical Newsletters, which give more detailed statistics of catches and corresponding fishing effort than are published in the Bulletin Statistique. Similar data to those for Belgium are available for the Netherlands and are published in the ICES bulletins, and extensive data for both England and Scotland, though not published, are available in the national laboratories at Lowestoft and Aberdeen. These latter would have been better for showing the overall distribution in the whole North Sea because the Belgian data are concentrated mainly in the southeast corner. There is a certain amount of fishing by Belgium farther north for cod and haddock, but the British

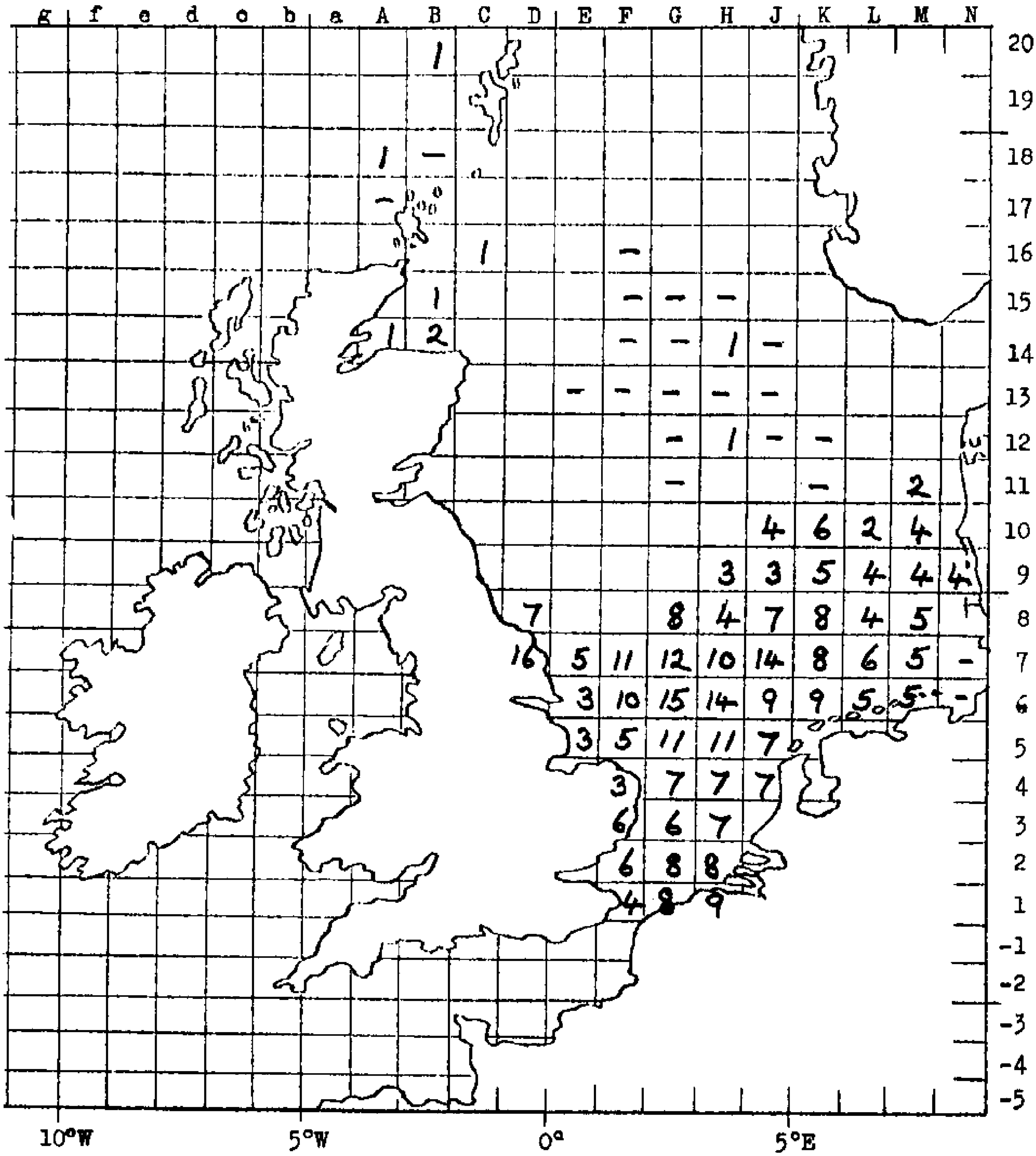

Figure 3A. Distribution of plaice in the North Sea, as shown by catch per unit of effort of Belgian trawlers (from ICES Statistical Newsletter).

fisheries cover the entire North Sea and give a very good picture of the distribution of the various species of fish.

Turning to the plaice itself, we see that the highest figures of over 10 kg per unit fishing effort occur in the region just south of the Dogger Bank. There are also fairly high figures down in the south and along the coast. Farther north the catches decrease, and north of almost $56^{O}N$ are negligible, except for a small quantity inshore along the Scottish coast and particularly in the Moray Firth.

The North Sea plaice is divided into two major spawning groups. One spawns in the Southern Bight between the Thames and the Rhine, and the other spawns in the German Bight off the north coast of Holland. It spawns in the middle of winter and the eggs from the Southern Bight spawning tend to drift slowly round north and northeastwards for a couple of months. Then the young fish turn from the orderly fish shape into flat fish shape, move down to the bottom, and are distributed along the continental coast all the way around from Holland up to northern Denmark. There are also smaller stocks, such as the one off Flamborough Head, on the northeast English coast. In the next few years the young fish move slowly offshore; at about 3 or 4 years, they become a marketable size, though still small, and at this time they are still concentrated fairly close to the continental coast. A few years later, they mature and move down to spawn and also move back in the feeding season into the center of the North Sea, so there is some degree of separation between small and large fish in the fishery. The vessels that are fishing in the shallow water not far from the continental coasts tend to catch mainly small plaice, whereas the trawlers fishing offshore at the center of the North Sea catch on the whole bigger plaice. This has some implications on the effect of fishing on the stock and the catches that can be maintained.

The fishery for plaice uses two main gears. The most important has been the ordinary otter trawl, which started out as beam trawling by sailing vessels, particularly along the English east coast. In the middle of the last century, there were steam tugs to pull the sailing trawlers out of harbor; then if it was flat calm, they could go on fishing by continuing to tow the sailing trawler with a steam tug. It didn't take very long to cut out the middleman and use a steam-boat to actually trawl. There was also a changeover from using a beam trawl to the otter trawl, and the traditional English North Sea trawling fleet built up very rapidly towards the end of the last century. The other major gear is the Danish seine type. As its name suggests, it is mainly used by Danish fishermen, but is also used by some of the English fishermen. Like the otter trawl, the seine catches the fish in a net moved along the bottom, but it relies less on brute force and the dragging of a large net a long distance, and more on the subtle shepherding of the fish into the path of the net by the long warps.

Figure 3B gives the trend in the total catches. This figure is reproduced from another important series of ICES, which is the Cooperative Research Reports, put out by various working groups of ICES which have been set up to study specific problems. In this case this is a working group set up to study the North Sea demersal

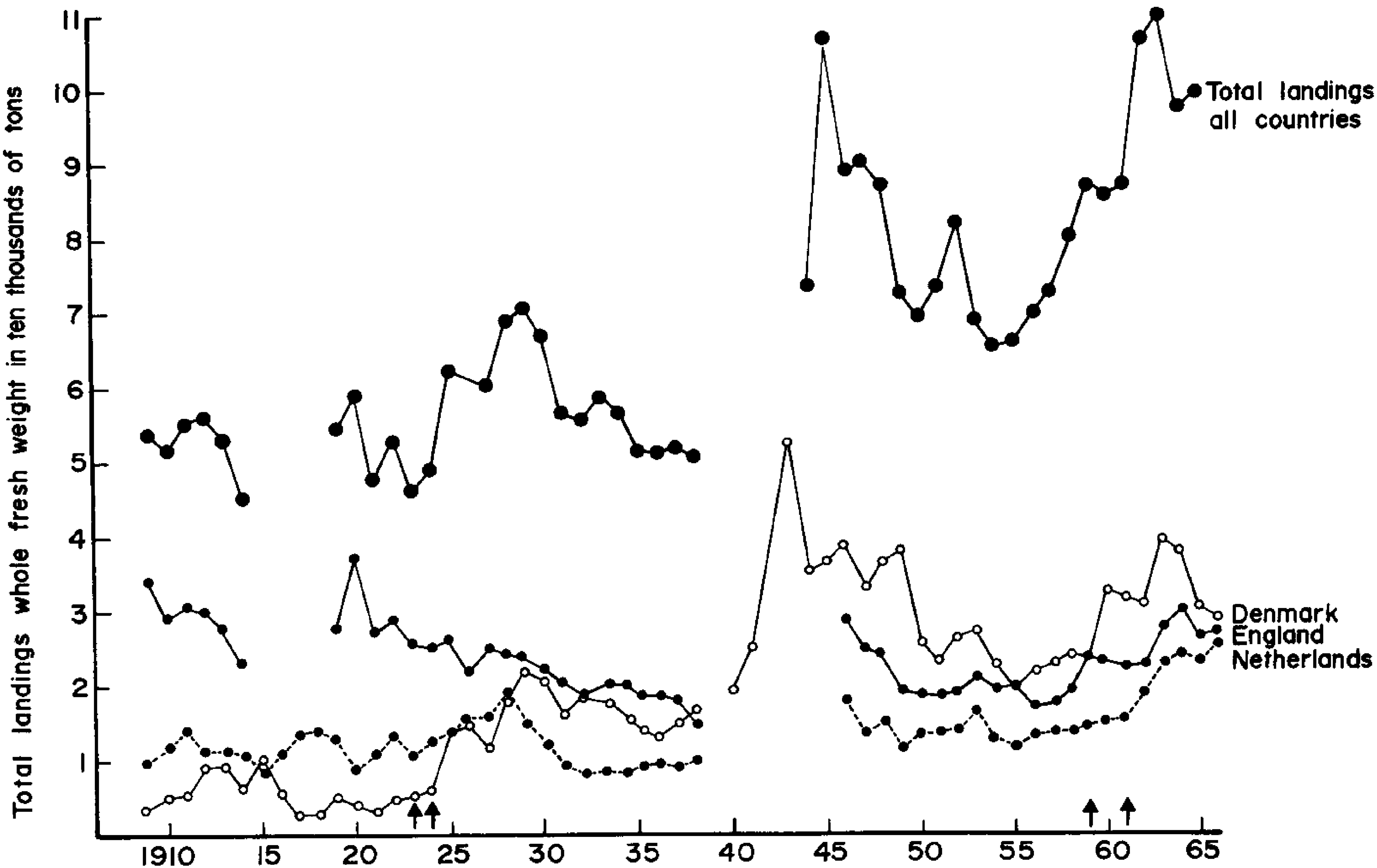

Figure 3B. Trends in total catch of North Sea plaice (from ICES Cooperative Research Report No. 9).

fisheries. The top line in Figure 3B shows the total catch by all countries combined; the catches by the major countries are shown separately in the lower lines. Back in 1910 England was the major fishing country and took over half the total catch. The English share has tended to decrease, and there have been increases by Denmark and by the Netherlands. Figure 3C shows the other two important characteristics of the fishery--that is, the trend in fishing effort as estimated by the effort of English trawlers adjusted to take in total catch, and the estimate of the abundance of the stock. For the present, let us concentrate on the period between 1925 and 1938, because this was when much of the thought and development of population dynamics in England and particularly at Lowestoft with Michael Graham, Russell and others was taking place. This was a period of stable conditions. The stock was almost constant, though the effort tended to decrease a little bit, and the total catches after reaching a peak around 1930 tended to decrease a little after that. There was a very big increase in the stock during the war when there was a respite from fishing, followed by a steady decline for several years.

At that time Beverton and Holt were doing their studies it appeared as though the fishery was returning to the same unsatisfactory prewar situation, in which period the total yield was reasonably high, but the effort was much too high and the catch per unit of effort was very low. The North Sea fishery as a whole was an extremely unsuccessful one. The fleets had built up very rapidly, both at the turn of the century when steam trawling was getting underway and also again, during and just after the first

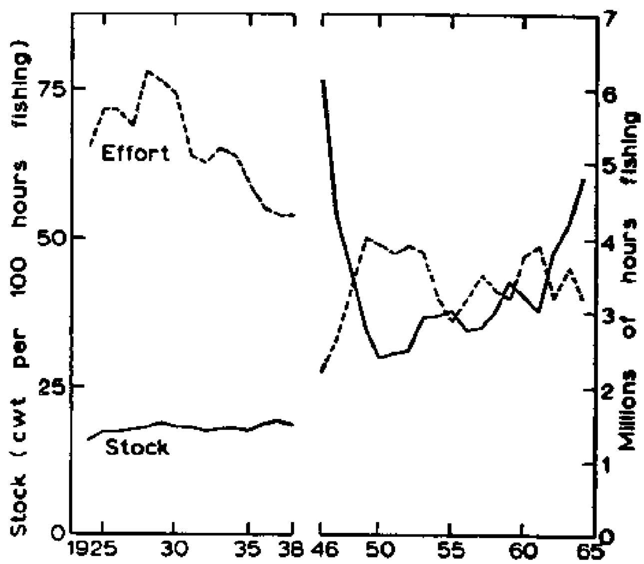

Figure 3C. Trends in estimated stock abundance of North Sea plaice, and on total effort (from Gulland, 1968).

world war. During the war, a lot of trawlers were built as mine sweepers, and then after the war the stocks had recovered during a period when there had been no fishing for 4 or 5 years; they were very abundant and the catch per unit effort was very high. The fleet therefore expanded rapidly. By 1920 the fleet of North Sea trawlers was much too big, and in fact between 1920 and about 1955 virtually no more boats were added to the fleet. The same fleet carried on, but still despite losses even in 1950 it was rather too big.

This was the situation that the English scientists were faced with. The fishery was in a bad way, and the experience of the two wars, and the recovery of the stocks during them, had confirmed that the major reason for the scarcity of fish and the difficulties of the industry was the excess amount of fishing. The scientific problem was to put their qualitative feeling into precise quantitative terms. Unlike the yellowfin fishery, with a rather steady increase in fishery during the period of study this was a rather stable fishery with not too much change during the period (1920-39) which was the source of the main data. On the other hand, there were many favorable factors--a very good long series of statistics, a convenient fish that could be tagged easily, whose age would be determined, etc.

It was in this situation that the model of Beverton and Holt was developed. This theoretical model, sometimes referred to as the dynamic pool model, involves in any particular application a number of assumptions, and the usefulness and reliability of the conclusions reached from the application will depend on the extent to which these assumption are more or less reasonable. A distinction must be made between the basic assumptions that must be true in applying this approach, and the specific assumptions that have to be fulfilled in using a particular equation. The basic assumptions are very simple and very reasonable. The first one is that all fish die once and only once. The second one is that fish grow as they get older. These are reasonable. Among the assumptions made when applying a specific model, one that is very often made and is convenient is that, though there are great differences between individual fishes, the behavior of the population can be adequately described by the average behavior of the average individual, and there is no need to take into the account the differences between individuals. Other assumptions that have to be made in a specific application include the actual form of the growth curve, of the mortalities, and of the recruitment and particularly the relations of these parameters of growth, mortality, and recruitment to the abundance of the fish population. It is important that in these models, in the first and simple approach it may be assumed, for instance, that growth is independent of the abundance of fish population and we get a certain result. As more data are produced, as more time is available to carry out detailed studies, and also as the fishery develops and requires more precise and accurate answers, it is possible to go back and instead of making the simple assumption that for instance the growth is independent of population abundance, to make some assumption regarding the form of relation between growth and population abundance.

In developing the model, the easiest way is to consider the yield from a single cohort of fish during its life span. It will come into the fishery, grow, be caught, die, and finally after some years even the oldest fish has died. It is possible to calculate what happens to this cohort of fish and what would be the yield from it under various patterns of fishing. In the steady state, given constant recruitment, the yield from a single year-class of fish will be the same as the yield in a particular year from all year-classes present during that year. The first aspect of the population to discuss is the change in the numbers of the cohort. The rate of change of numbers is proportional to the mortality rate and also to the numbers present, that is

$$\frac{dN}{dt} = -ZN \qquad (3.1)$$

where Z is the total mortality coefficient. This is fairly easy to integrate, and gives on integration

$$N_t = N_o e^{-Zt}$$

where N_o = numbers alive at time t = 0. (3.2)

(A more extensive discussion of the derivation of these equations is available elsewhere, e.g. Beverton and Holt (1957), Gulland (1968).

This is looking at total mortality. The important division in this mortality is between the fishing mortality F and the natural mortality M. In practice both these could vary with age, but an assumption that simplifies matters and is convenient to make at first, is that each is constant and the same for all ages. If desired, and data are available to show how these vary with age, it is possible to go back and instead of using a constant M or a constant F, to put some function of age or time into equation (3.1).. This makes the algebra difficult, but with modern computer facilities a tidy algebraic solution is not necessary. The normal assumption is that the mortality rates are constant, or rather that M is constant, and that F is constant above some particular age while the fish are in the fishery and is zero for ages less than this, before they come in to fishing. In practice recruitment to the fishery is not so sharp as this. For instance in the North Sea plaice, as a year-class moves offshore, it will slowly come into the fishery, and the fishing mortality on the brood of fish will increase with age over quite a span before the year-class is fully into the fishery and exposed to the full fishing mortality. But again the first assumption is that fishing mortality $F = 0$ if $t < t_c$, where t_c is the age at first capture and F = constant $t > t_c$. And this then allows some expressions to be calculated.

The first expression is the number of fish alive at t. This can be expressed as a function of the number alive at t_c, the age at first capture, the age at which the fish become fully exposed to the fishery, as the fishery currently operates, or better, at the age at recruitment, t_r, which is the earliest age at which the fish are potentially available to the fishery. This age is a fixed biological characteristic of the stock, at least for a given type of fishery, e.g. for plaice it is the age at which they move offshore from the very shallow inshore waters. The age at first capture, however, can be altered by suitable adjustments to the fishery. The obvious example is the increase of the mesh size in a trawl fishery. This will allow the small fish to escape, and increase the size at first capture. The age at recruitment t_r is therefore the first time a cohort of fish is of direct concern to the fishery, and the number of fish alive at that time, N_r, is of some significance. It is often denoted by R, the number of recruits. Between ages t_r and t_c, the only mortality operating is M, so that the number of fish alive will be given as $N_t = R\, e^{-M(t-t_r)}$, and in particular N_c, the number alive at the age at first capture is given by

$$N_c = R\, e^{-M(t_c-t_r)} \qquad (3.3)$$

and this number is often denoted by R'. Similarly, those alive at any time t greater than t_c will be given by

$$N_t = R'\, e^{-Z(t-t_c)} \qquad (3.4)$$

The next expression needed is one for the weight of the individual fish. The subject of growth and the fitting of growth curves will not be discussed in detail because on the whole it raises no particularly interesting problems. Either the age of the individual fish is fairly readily determined, in which case a set of points relating age to weight is available, and any suitable curve can be fitted to them without much difficulty, or else the age of the fish cannot be determined, in which case there is little information to which theoretical studies can be applied. Some expression for the weights of the individual fish is needed and any expression can be used that fits the data reasonably. Also if calculations have to be made on a desk calculator, it is desirable to have an expression for weight of the individual fish that makes the mathematical calculations reasonably easy. The particular form used by Beverton Holt is that of von Bertalanffy,

$$l_t = L_\infty \left(1 - e^{-K(t - t_0)}\right)$$

where L_∞ = limiting length, and K, t_o are constants. K is the measure of how fast a fish reaches the limiting length. Typically the length of a fish increases rapidly and then flattens out. It can flatten out either slowly, moderately, or very fast and these situations correspond to increasing values of K. The corresponding expression for the weight of the individual fish is

$$W_t = W_\infty \left(1 - e^{-K(t - t_o)}\right)^3 \qquad (3.5)$$

The weight of the catch can now be set out. The rate at which the catch is added to is equal to the product of the fishing mortality, the numbers of fish and their individual weight; i.e.

$$\frac{dC}{dt} = F_t N_t W_t$$

and the total catch is obtained by summing this over the life span of the cohort, i.e.

$$C = \int_{t_c}^{t_\ell} F_t N_t W_t \, dt$$

The upper limit may be set at infinity, or some rather arbitrary limiting age t_1. This is a perfectly general expression for the weight caught. All that is required for its application to a given situation is to feed into it the particular values for fishing mortality, numbers, and weight and carry out the arithmetic. Convenient ones are to set F and M constant and use the expressions derived above for numbers and weight. This gives

$$Y = F R W_\infty e^{-M(t_c - t_r)} \sum_{n=0}^{3} \frac{U_n e^{-nk(t_c - t_o)}}{F + M + nK} \left(1 - e^{-(F+M+nK)(t_\ell - t_c)}\right) \qquad (3.6)$$

It has been expanded and the integration has been carried out on each of the four terms. This gives a summation with U_n a constant in the expansion, $U_o = 1$, $U_1 = -3$, $U_2 = 3$, $U_3 = -1$. This gives the yield for any particular value of fishing mortality and also for any particular value of t_c, the age at first capture. All that is required to answer most problems is to have good measures of R, M, t_c, t_r, W_∞ , K and t_o, and to do the calculations.

In theory there would be no further need for research on this fishery. Fortunately for the continued employment of fishery scientists, however, things are never quite as easy as this. Much of the rest of our discussion will be concerned with looking at situations where there is less than complete information, and seeing how sensible and useful answers can still be obtained. The other important aspects will be to examine methods of estimating these parameters and to consider the intent to which some of these so-called constants, the ones like R, the recruitment, and M the natural mortality and the growth coefficients, W_∞ , and K, are not, as assumed here, independent of the abundance of the fish stock. Their possible variation with the abundance of fish stock can affect the calculations and result in conclusions regarding the state of the stocks, and the result of possible actions different from those obtained on the basis of constant parameters.

In relation to equation (3.6), we should keep in mind that it is just one derived expression from the particular calculations done by Beverton and Holt using certain specific formulations for mortality and growth. In particular the equations derived by Ricker (1958) differ in the arithmetic, but the basic ideas and approaches are very much the same. An important difference is that the Beverton and Holt formulation has been developed to describe a continuous fishery, that is, a fishery where fishing goes on more or less continuously right around the year, and we can reasonably assume that other things like growth and mortality also go on more or less continuously, whereas the Ricker expressions are particularly applicable when there is a seasonality in the fishery.

Now let's look at the results of this. Equation (3.6) gives the yield in terms of two major quantities. F, the fishing mortality, and t_c, the age at first capture, can best be illustrated in an isopleth diagram, such as Figure 3D. In this, the lines are lines of equal yield. The highest yield of over 400 units is taken with a high fishing rate and a high age at first capture. This is obtained by waiting until the year class is at its maximum weight and then catching it all more or less instantaneously. On the other hand, if we don't fish very hard at that high age of first capture, we lose weight fairly rapidly because the fish die before they get caught; the catches also decrease if we fish very hard but start catching at an early age because the fish are not allowed a chance to grow to a good size. The catch in numbers may be large, but consists of small fish and the total weight caught is also relatively low. The important point here is P, which is the estimated position of the interwar fishery. The combination of fishing mortality and age at first capture occurring between 1920 and 1938 gives a yield per recruit of about 200 units compared with a possible maximum of well over 400.

This describes very well what happened to that fishery. Fishing was at much too high a rate and the fish were at too small an age at first capture. At that age at first capture, if there was a reduction in the amount of fishing, we would increase the yield until the point A is

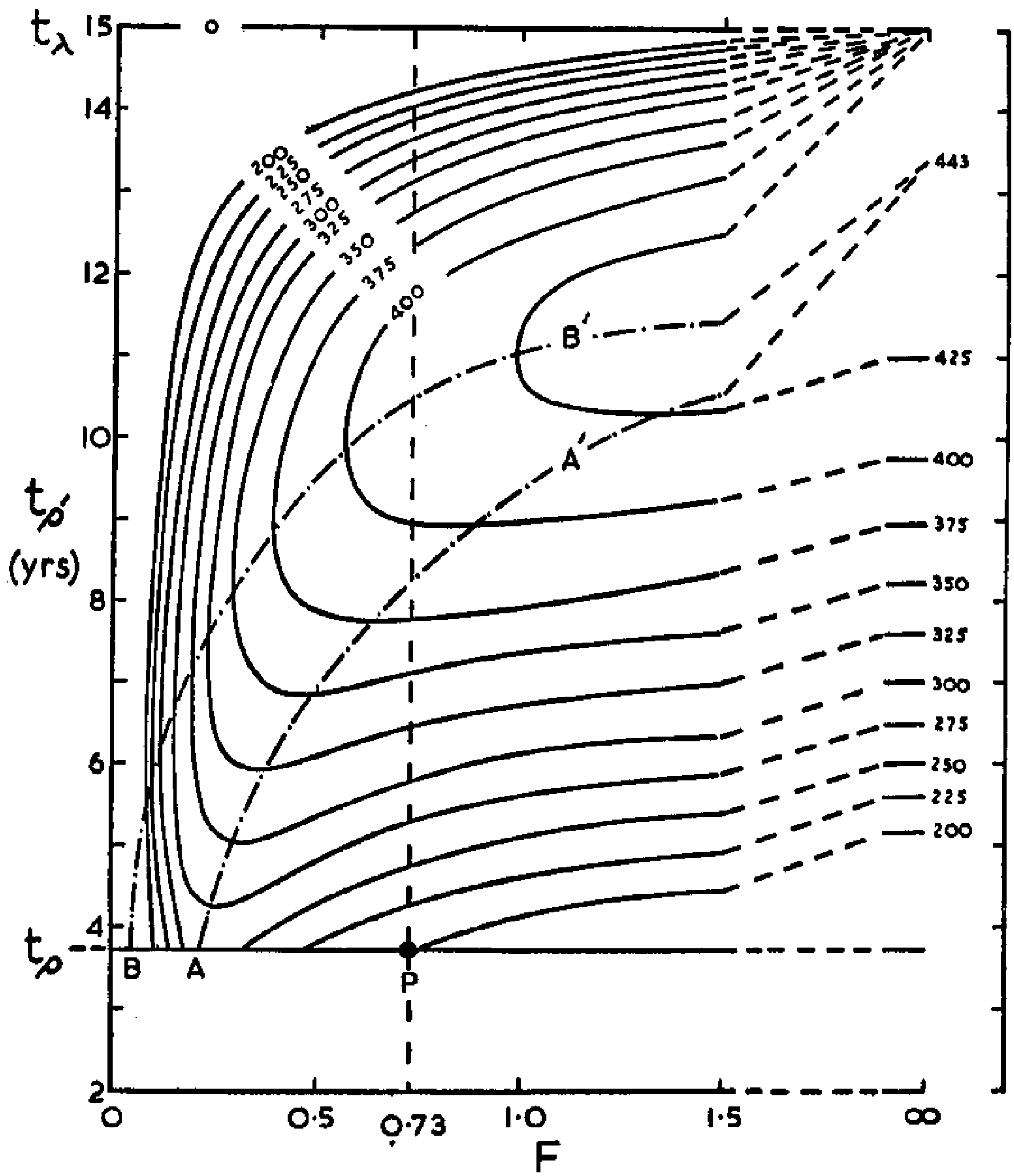

Figure 3D. Yield isopleth diagram for North Sea plaice (from Beverton and Holt, 1957).

reached where the yield is between 250 and 275 units. Also if the size at first capture were increased, it would increase the total yield.

Now consider actual events in the North Sea fishery since 1950 (Figures 3B and 3C). Until 1950 it appeared that the fishery, following the war, was falling back to the unfortunate situation of the 1930's. Around 1950, the scientists could feel satisfied because they could explain that the low catches and low stock abundance were due to excess fishing, even though nothing seemed to be done to improve matters. After about 1950, and especially from 1956 or 1957 onwards, the situation changed. The total catches started to increase steadily until a peak in 1963, which saw the highest catches ever recorded of plaice in the North Sea. Though catches have dropped back a little bit since then, it is clear that the average level of total yield in the last few years is very much higher than the yield before the war. This is certainly not due to any regulations. There have been no controls on the amount of fishing in the North Sea. There have been controls on the mesh size used, which would be expected to benefit the cod and haddock fisheries, but it is very doubtful whether this would have any effect on the size of the fish taken in the plaice fishery since even the new larger mesh would still retain all but the very smallest plaice in the fishery.

It is clear that matters have improved, but the question is: Just how well can this be explained by the theory? One thing that has happened is that there has been a big change, particularly in the English fishery, in the places in which the trawlers have been operating and in the sizes of fish caught. This is quite likely a reflection of the changeover from the old steam trawlers to a fairly modern fleet of motor trawlers, slightly larger and to some extent more willing to fish farther out into the center of the North Sea. Figure 3E shows the length composition of English landings in 1950-51 and 1963. The 1951 curve is typical of the landings at that time and in the prewar period. In 1963 there were many more big fish and considerably fewer small fish, so that there has been a big change in the effective size at first capture. An increase in yield is precisely what could be predicted from the theoretical yield curves following an increase in the size at first capture.

This has not been the only change compared with the prewar situation. As Figure 3C shows, the effort has been appreciably lower. This change is confirmed by the change in total mortality rate (Figure 3F). This shows, on a logarithmic scale, an average age-composition in the pre-and postwar periods. The slope of the two lines is proportional to the total mortality rate in the two periods -- between 1929 and 1938, and between 1950 and 1958. The decrease in the total mortality agrees very well with the decrease in the estimated fishing effort between the interwar period in the 1930's and the postwar period. Compared with the prewar period, on which the calculations of Beverton and Holt were done, there have been two major changes: the first one in time being this reduction in the amount of fishing and the second, the increase in the effective size at first capture, particularly in the English fishery. Both these things should, on theoretical grounds, increase the total yield and this is precisely what has happened.

The first conclusion from this study is that this model, the Beverton and Holt model, or the dynamic pool model, does give a very adequate description of what is going on. It does give a description of the situation, provided, and this is the big proviso, that we can estimate all these different parameters, particularly the mortality rates. Otherwise it is

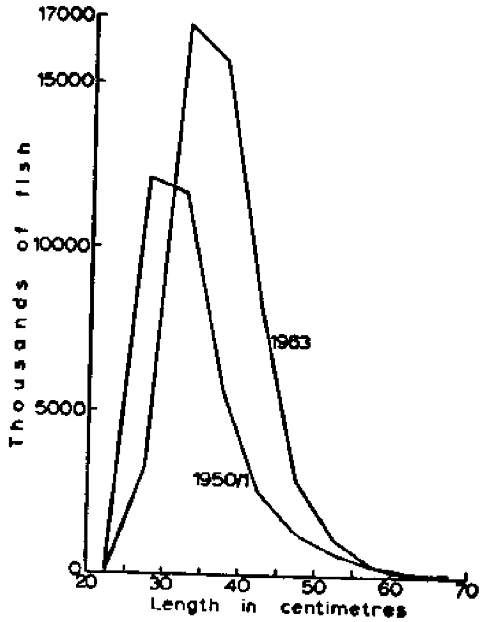

Figure 3E. Size composition of English plaice landings in 1950/51 and 1963, to show decrease in small fish (from Gulland, 1968).

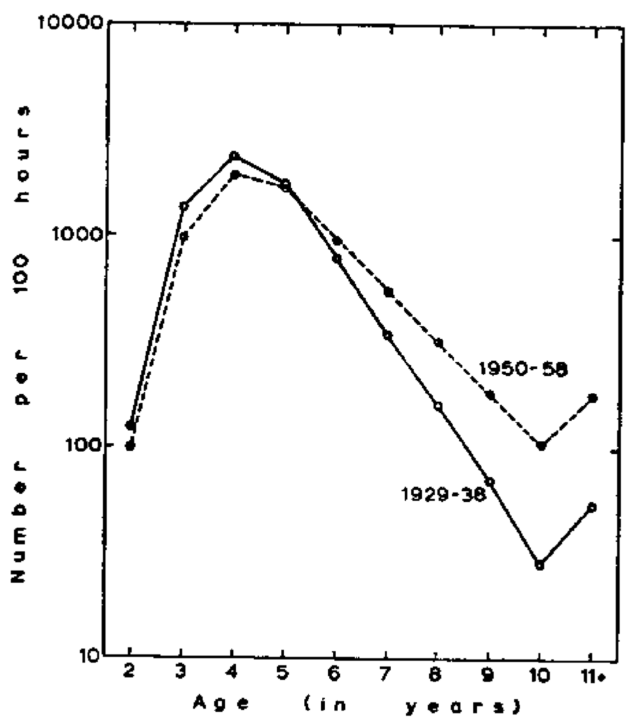

Figure 3F. Age composition of landings of plaice at Lowestoft 1929-38 and 1950-58, to show reduced mortality in the latter period.

difficult to apply them, but if we get reasonable estimates of these quantities, then we are in fairly good shape.

The other conclusion is in regard to the fishery which is also in very good shape, but here the future outlook is perhaps not so good. Both changes, the reduction in the amount of fishing and the shift to bigger fish, have been rather accidental. There has been the reduction in the amount of fishing because the English fishing industry had found in the interwar period that North Sea fishing was extremely unprofitable and had put their money into bigger trawlers going outside the North Sea up to Iceland and farther afield, but now these stocks are all heavily fished. This distant-water fishery is becoming less attractive but the North Sea fishery at the moment is profitable. It is therefore not at all improbable that the fishing effort will come back into the North Sea, and the total effort there will tend to return to the 1930 situation. The same is true of the changes in size. The reason for this change is not certain, but once started, it tends to be a self-generating change. Once a sufficient number of ships have changed over from fishing small fish to large fish, the stock can recover, the large fish get more abundant and everyone goes that way. If there is a tendency to change back and start heavy fishing on the small fish again, big fish will decrease and anyone who wants to make a living fishing has got to go where the fish are. That will be where the small fish are, so the size at first capture decreases. Thus though the outlook for the model is good, and the scientists may feel satisfied with their ability to explain what is happening, there is less cause for satisfaction in the likely future trends in the fishery.

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Chapter 4

ARCTIC COD: ESTIMATION OF MORTALITY RATES

The subject of this section follows from that of the previous section. It follows both historically in the development of the British fishing industry and also rather logically in the theory. We went very quickly through the development of the so-called dynamic pool model, particularly as derived by Beverton and Holt, and showed that this model, provided that we had good measures of the various parameters of mortality, growth, and recruitment, could provide good estimates of the yield obtained from any combination of the amount of fishing and of the age at first capture. This can give a good description of what happens to the fishery and also useful advice to the fishing industry. The practical difficulty is the need to know values of the various parameters. In describing the North Sea plaice situation, we took these parameters as being known. Now let us look at some of the methods of estimating these parameters, particularly the methods of estimating mortality.

When discussing North Sea plaice, we learned that since about 1920 there was no change in the English fishery there. A large fishing fleet had built up but stayed constant from about 1920 onwards, and after that period the initiative in the fishing industry went into other areas and into the so-called distant-water fishery at Iceland and in the Barents Sea. Let us examine one section of this cod. Figure 4A shows the major current systems in the area inhabited by this stock, from northern Norway up to Spitzbergen, and east to northern Russia. A warm current, the North Atlantic drift, the extension of

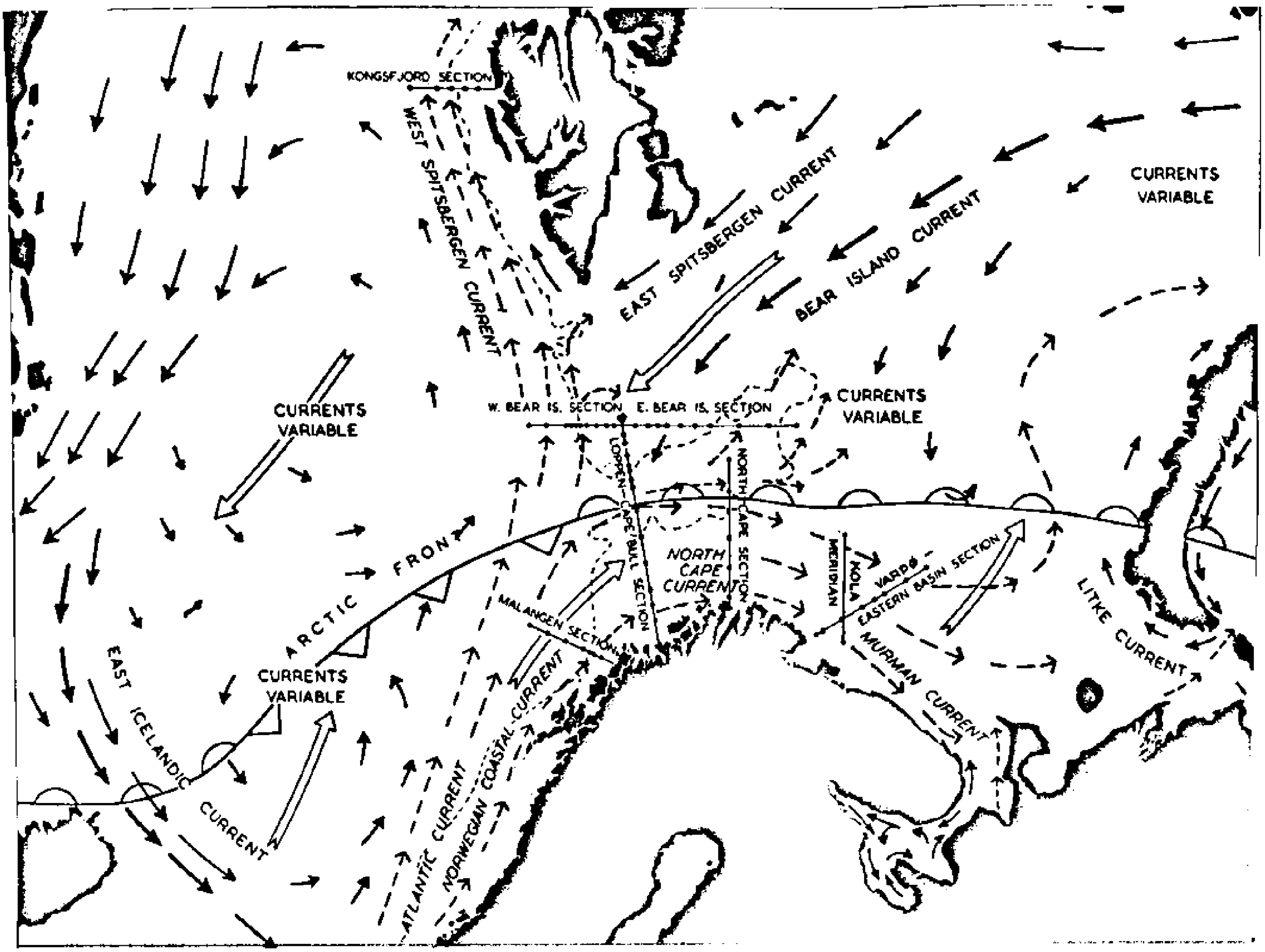

Figure 4A. The main currents and winds in the Barents Sea (from Lee, 1961).

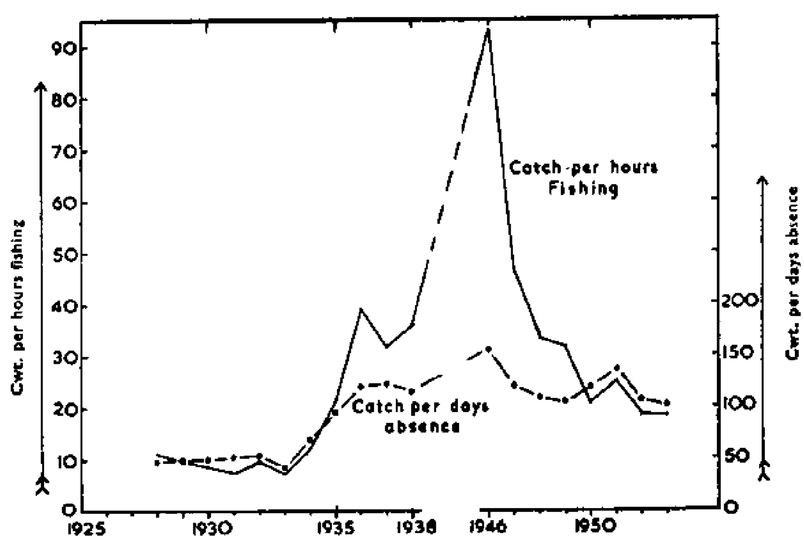

Figure 4B. Trends in catch per unit effort of English trawlers, based on different measures of fishing time (from Gulland, 1956).

the Gulf Stream, flows north along the Norwegian coast. Northwest of the northern coast of Norway, it divides into two, one section, the west Spitzbergen current flowing nearly due north, and the other coming around the north coast of Norway and the north coast of Russia. This fishery has perhaps the dubious distinction of being probably the most northerly fishery in the world.

This cod stock has a main spawning area here in the Lofoten Islands, where they spawn at about 10 years old and onward, and then the young fish drift with these warm currents and form two rather independent feeding and juvenile stocks: one in the Bear Islands--Spitzbergen area, and the other in the area of the southeastern Barents Sea, often referred to in the British fishing industry as the White Sea. Thus there are three main sections of the fishery. The oldest is the fishery on the spawning and prespawning fish along the Norwegian coast, some of it by trawlers in the offshore region along the edge of the continental shelf, and this can be a very concentrated fishery. The fish tend to be at one particular depth. The bottom slopes steeply so that this particular depth extends for not much more than a matter of a few yards, and at times the trawlers have to queue up to tow along this preferred ground. Despite the fact that the trawlers cover this ground several times a day, the fish come in from both sides, and the supply of fish is more or less maintained.

The major spawning fishery is inside the fjords on the spawning grounds and uses a variety of gears--hand lines, purse seines, and gill nets. The spawning fishery is mainly carried out by Norway inside the fjords but also by England and other countries outside on the prespawning concentration. The other big fisheries are almost entirely trawl fisheries, on the young fish and the feeding fish: one group in the Bear Islands – Spitzbergen area, and the other in the eastern Barents Sea. In these, as in all fisheries, before looking at trends in fishing effort and catch per unit effort, we ought to be careful about the measure of fishing effort that is used. The proper measure of fishing effort is always one of the most difficult things to tackle. It is best thought of as made up of the product of two terms--one is the fishing power of the gear--the size of the boat, the length of the net, etc.--and the other is the fishing time. In the English trawling industry, two major estimates of fishing time are available. The one for which the longest series of data are available is the days absent from port, that is the number of days from the time the boat sails until it returns to port. This has the advantage of being both easy and reliable. The figures are almost certainly correct, but the number of days absent will include the time spent steaming north from Hull or Grimsby up to Bear Islands and back, the time spent up there unable to fish because of bad weather, and also if fishing is very good, the time spent gutting and cleaning the fish and not actually fishing. The other measure of fishing time we have is the hours fishing, that is the actual number of hours spent towing the trawl along the bottom. This is based on interviews with the fishermen when they return to port, asking them how many tows they made, and the average duration of the tows. This is therefore not so accurate as days absent, but does give a much better measure of how much fishing is being done.

Figure 4B (from Gulland, 1956) shows the catches per days absent and the catches per hours fishing by English trawlers in the prespawning fishery along the Norwegian coast. Just after the war, particularly in 1946, there is an extremely high peak in catch per hours fishing whereas the catch per days absent didn't change very much. This general trend in both of them between

1925 and 1938 can probably be accounted for by an increase in the size of boat and in the fishing power of the gear, which has not been corrected.

After the war, just as there was an increase in the North Sea stocks, there was also an increase in the stocks of Arctic cod. They were so abundant that in a couple of hours tow or less, a trawler could fill up with fish. It was then necessary to spend a long time cleaning and gutting these fish before putting the net in the water again. As a result, although there was a very high catch per hour, which was a true measure of the abundance of fish, the catch per day didn't go up very much because it was limited to the handling capacity of the ship. Also relatively more time was spent steaming to and from the grounds and less time actually on the ground fishing, which also meant that the catch per day absent from port did not increase. Clearly the catch per hours fishing is a better measure of abundance than the catch per days absent, even though the days absent is probably recorded more accurately than the hours fishing.

Figure 4C gives the basic statistics of the fishery, combining the data from all three major sections of the fishery. The total fishing effort is calculated from the data of English hours fishing, corrected for the increase in size of trawlers. From 1946 onwards, the total catch, though fluctuating, has not shown any great tendency to increase, whereas the fishing effort increased very rapidly, up to about 1956. Despite harder fishing, the catches did not increase and the fishery was clearly in the same sort of poor situation in the North Sea. The same advice is required. The qualitative statement that the stock is being too heavily fished needs to be put into quantitative terms, and estimates need to be made of the actual changes in steady-state yield to be expected from changes in the amount of fishing.

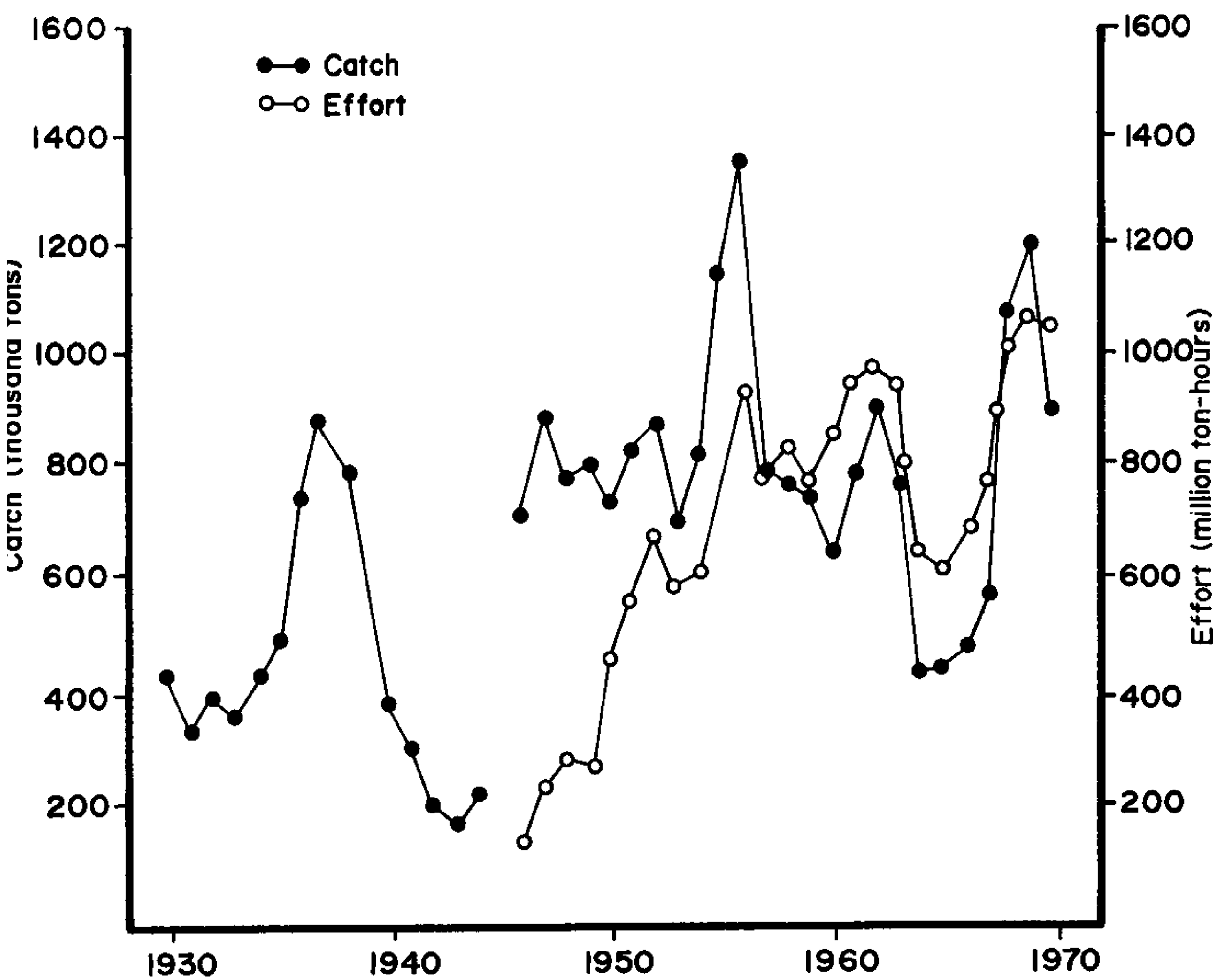

Figure 4C. Trends in total catch (dark line), and estimated total fishing effort, in English units (light line), of the Arcto-Norwegian cod stock from ICES working group reports).

Since 1956 the effort has fluctuated. The poor catches per unit effort discouraged further increases in effort, and there was a considerable drop-off between 1962 and 1964. As in the North Sea, the fishing industry found that it was better to go somewhere else. The major diversion has been by the USSR, which has in the last fifteen years or so built up a large fleet of factory trawlers. They can go virtually anywhere in the world and in 1962-64 when there was a big reduction in fishing effort in the Arctic, most of the Russian fleet was fishing over on the West Atlantic or other areas. Later, as catches in many of these areas decreased, there was a swing back in 1968 and 1969 into the Northeast Atlantic from the Western Atlantic. One of the major problems facing fishery biologists, fishery scientists, and administrators is this increased ability of fishing fleets to move to and fro and for things to happen very much more rapidly than they used to.

The buildup in the North Sea trawl fishery took place over quite a period, and during the period of heavy exploitation in the 1930's, there was virtually a stable situation, whereas the Arctic cod fishery is very far from being stable. We have a situation where the effort can drop off by 30% between 1962 and 1965, and then very nearly double again between 1965 and 1969. This makes it much more important to be able to come up with quick and precise answers. All too often now when we come up with slow answers, the situation has gotten completely out of hand and the opportunity to interfere with any success has gone. The situation in the Northeast Arctic is that the cod stock is clearly being heavily fished. To make a quantitative statement of exactly where we are on some yield curve, estimates must be obtained of the basic parameters (growth, mortality, etc) to fit into the dynamic pool model. The cod is a fairly well-behaved fish, whose age is readily determined from otoliths. Given the age and size of individual fish, there is no problem to fit an appropriate curve to these growth data.

Now let us discuss how to estimate mortality rates, both the estimation of total mortality and how this estimate of total mortality may be divided between mortality due to fishing (F) and natural mortality (M). For total mortality, the basic expression we have is that the rate of change of numbers is given by $\frac{dN}{dt} = -ZN$ or $N_t = N_o e^{-Zt}$ or $\ln N_t = \ln N_o - Z\,t$ (4.1)

Therefore if we plot the logarithms of the numbers of fish in a particular age group, or year-class against time we should get a straight line whose slope is Z. No further problem. The difficulty is that we very rarely do have estimates of actual numbers at distinct points of time, nor in fact will Z be constant. There are various ways of handling the data to overcome these difficulties. Equation (4.1) is a convenient and important relationship, and it is very important to make this type of graphical plot because though computers can churn out answers quicker and use more complex analyses than any graphical plot, making the plot to see whether in fact the points do fall in a straight line gives a more immediately comprehensible picture of what is happening and whether the assumptions being made are justified.

Arithmetically the numbers at the end of one year will be given by $N_1 = N_o e^{-Z}$ or $\frac{N_1}{N_o} = s = e^{-Z}$, where s is the survival during one year. The logarithm of the survival during one year gives the simplest arithmetical expression for the mortality. Usually there is no estimate of abundance or numbers of a year-class on a particular date. What we do have is the estimates of the average abundance over a period, and the average abundance during the first year is given by $\overline{N}_o = N_o \frac{1}{Z_o} (1 - e^{-Z_o})$ and similarly the mean abundance in the second year is given by $\bar{N}_1 = N_1 \frac{1}{Z_1} (1 - e^{-Z_1}) = N_o e^{-Z_o} \frac{1}{Z_1} (1 - e^{-Z_1})$

The ratio of the mean numbers in successive years will then be

$$\frac{\bar{N}_1}{\bar{N}_o} = e^{-Z_o} \frac{Z_o (1 - e^{-Z_1})}{Z_1 (1 - e^{-Z_o})} \qquad (4.2)$$

or, taking logarithms

$$\ln \frac{\bar{N}_1}{\bar{N}_o} = -Z_o + \ln \frac{Z_o (1 - e^{-Z_1})}{Z_1 (1 - e^{-Z_o})} \qquad (4.3)$$

Clearly if the mortality in the two years is the same, the second expression on the right-hand side of equation (4.3), reduces to 1 and we have a perfectly accurate estimate of the mortality rate during these two years from the ratio of the mean abundance in successive years. Also, if the mortalities are small, the expression again tends to 1, and the survival is well estimated from the ratio of the mean abundances. Very often there will not be estimates of the actual numbers but there will be estimates of the catch per unit effort of a particular year-class during a particular year. For simplicity, let's assume at first that the catch per unit effort is estimated at a particular point in time. Then the catch per unit effort will be given by $n_o = q_o N_o$, where q_o is the coefficient referring to the catchability of that year-class in that time. Similarly, a year later, the catch per unit effort will be $n_1 = q_1 N_1$. The initial estimate of mortality will be the ratio of the catches per unit effort at a year interval

i.e. $s = \frac{n_1}{n_o} = \frac{q_1 N_1}{q_o N_o} = \frac{q_1}{q_o} \cdot e^{-Z}$ or, again taking logarithms

$$\ln \frac{n_1}{n_o} = \ln \frac{q_1}{q_o} - Z \qquad (4.4)$$

Provided that this ratio of the catchability coefficients, q, is 1, then not surprisingly, the ratio of the catches per unit effort gives a perfectly valid estimate of the total mortality. But it does require that this ratio remain constant and in many fisheries this is just precisely what it doesn't do. There are variations from year to year in catchability, in the ratio of catch per unit effort to abundance, which applies equally to fish of all ages. For some reason the fish don't come into the fishery or the weather is wrong, or for some reason the catch per unit effort goes down even when the abundance is quite high. The coefficient can also vary with age. The assumption we made in talking about the North Sea plaice was that fishing mortality was constant with age from a certain age at recruitment onwards. This very well may not happen and there are some changes in mortality. Since we write $F = qf$, where f is the fishing effort, which has a value for the fishery as a whole, a variation of F implies a variation of q, i.e. we should write $F_t = q_t f$. The n ratio in the first term on the right-hand side of equation (4.4) is not equal to 1, so that the ratio of catches per unit effort does not give a reliable estimate of the total mortality. One way of tackling the variation of the catchability coefficient from year to year is to consider the ratio of 6-year-old and 7-year-old fish in the catches during a particular year, and this would not have in it the ratio of the catchability coefficients for different years.

Consider the number of 8-year-old fish at the beginning of 1970, supposing they recruit at 5 years old. This will be the number of recruits coming in, in 1967, reduced by the mortality in 1967, 1968, and 1969, whereas the number of 9-year-old fish in 1970 will be the recruits in 1966, reduced by the mortality in 1966, 1967, 1968, and 1969.

i.e. $N_8 = R_{67} \ e^{-Z_{67}-Z_{68}-Z_{69}}$ and $N_9 = R_{66} \ e^{-Z_{66}-Z_{67}-Z_{68}-Z_{69}}$

or in terms of catch per unit effort

$$n_8 = q_{8,70} \ R_{67} \ e^{-Z_{67}-Z_{68}-Z_{69}}$$

$$n_9 = q_{8,70} \ R_{66} \ e^{-Z_{66}-Z_{67}-Z_{68}-Z_{69}}$$

where $q_{8,70}$ is the catchability coefficient for 8-year-old fish in 1970. Then the estimate of total mortality will be Z^1, where $Z^1 = \ln \ n_8/n_9$ or

$$Z^1 = \ln \frac{q_{8,70}}{q_{9,70}} + \ln \frac{R_{66}}{R_{67}} + Z_{66} \qquad (4.5)$$

The first term of this expression will be zero if the catchability of 8-and 9-year-old fish in 1970 is the same, even if there are year-to-year changes. It may be noted that equation (4.5) gives an estimate of the mortality in 1966, when the older year-class recruited, and not of the mortality in 1969 or 1970. The same technique can be used graphically, in the so-called catch curve, in which the logarithms of the numbers of fish of different ages taken in a particular sample are plotted against age. This gives a reasonable estimate of the total mortality, provided there were no changes in recruitment, but what is also interesting is that this is not a current estimate of mortality but tends to be a fossilized estimate of mortality. We have frozen into the age composition the mortalities at various times back in the past. This can sometimes be a disadvantage. At other times, however, it can be quite an advantage. We can take age composition and by looking at it get some insight as to what has been happening in the fishery. To use the results in the model, we must have some estimate of the split between fishing mortality and natural mortality. The basic equation for this is that the total mortality is the sum of natural and fishing mortalities, and that the latter is proportional to the fishing effort.

i.e. $Z = M + F = M + qf \qquad (4.6)$

This shows that total mortality should be, if M is constant and q is constant, a linear function of fishing effort and again all we have to do is to plot total mortality against fishing effort and we will find that all the points lie on a straight line. The intercept is M, and the slope is q. Once again we can carry out the calculations on the fishery, provided we have good values of total mortality, good estimates of fishing effort, and the two quantitatives M and q remain constant.

Now let's look briefly at some of the results of attempting to estimate mortalities in different fisheries. First let's go back to the North Sea plaice. Figure 3F shows the plot on a log scale of the numbers of fish at each age, averaged over two periods from 5 years onwards, the points do lie very nicely on a straight line, and provide good estimates of the total mortality rates in this fishery at two different periods. The fishing effort in these periods was different. Equation (5.6), written for the two periods, gives two equations relating total mortality to effort in these two periods, with two unknowns, the natural mortality and the coefficient q, and can be easily solved. In fact, rather pleasingly, the answer we get as the estimate of natural mortality agrees very well with the calculations made by Beverton and Holt before the data for the second period became available. Figure 4D shows the result of plotting the logarithm of the number against age for two types of data for the cod. One line is for the 1943 year-class in successive years as it passes through the fishery. These are all based on catches by English trawlers in the feeding area around Bear Island, and the numbers have been estimated as the catches per unit effort. We can see that between 6 and 10 years, they do lie very nicely on a straight line. Whether in fact they should lie quite so nicely in a straight line at a time when fishing effort is increasing and presumably fishing mortality is also increasing is perhaps not so clear. It may not really be such a good fit to the model as all that.

The other way of analyzing the data, at least graphically, is to plot out the catch curve for a particular year. Shown in the figure are the data for the 1952 catch, shown by the broken line, and again from about 6 or 7 years old onward this falls in a reasonably nice straight line. A more detailed analysis of mortalities can be made, with a table giving catches per unit effort for each **age**, and each year. Individual estimates of mortality can be obtained from the catches per unit effort of the same year-class in successive years giving some half dozen estimates for each pair of years. These can be analyzed to see whether there is any pattern of mortality changing with age or changing between years.

Figure 4E shows the age composition in the different sections of the fishery. The three major regions of this fishery have been divided up by ICES into region IIb which is Bear Island and Spitzbergen, the most northerly region, region I, the eastern Barents Sea and region IIa, the Norway coast which includes the spawning fishery. We can see here that the age composition is rather different in the different regions. Region IIa contains mostly old fish; there is a peak at 10 years old and not many fish less than about 7 or 8 years old. In the feeding and juvenile fisheries in the Barents Sea, and around Bear Island, the peak ages are 5 to 6 years and the numbers of older fish fall off rather rapidly.

These diagrams from reports of one of the ICES working groups are good examples of how the data from different fisheries and different countries can be put together. On the whole, our best statistics of fishing effort in the feeding and immature fisheries have come from the English fishery, and for the spawning fishery from the Norwegians. The age compositions are rather equally split. For region IIb most of the age data come from the

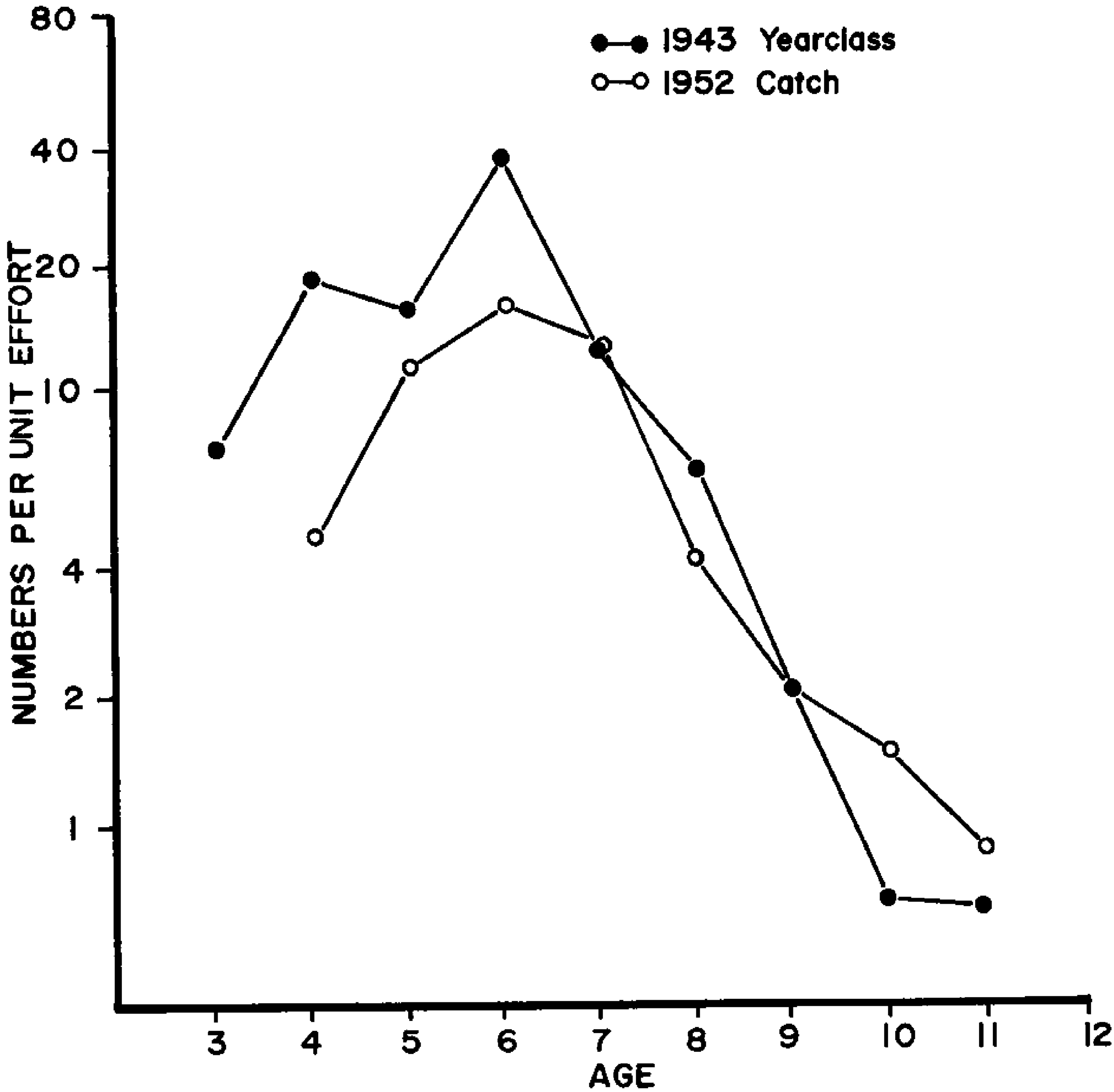

Figure 4D. Decreases in catches per unit effort of successive ages for the 1943 generation, and the catches in 1952 of cod in the Barents Sea.

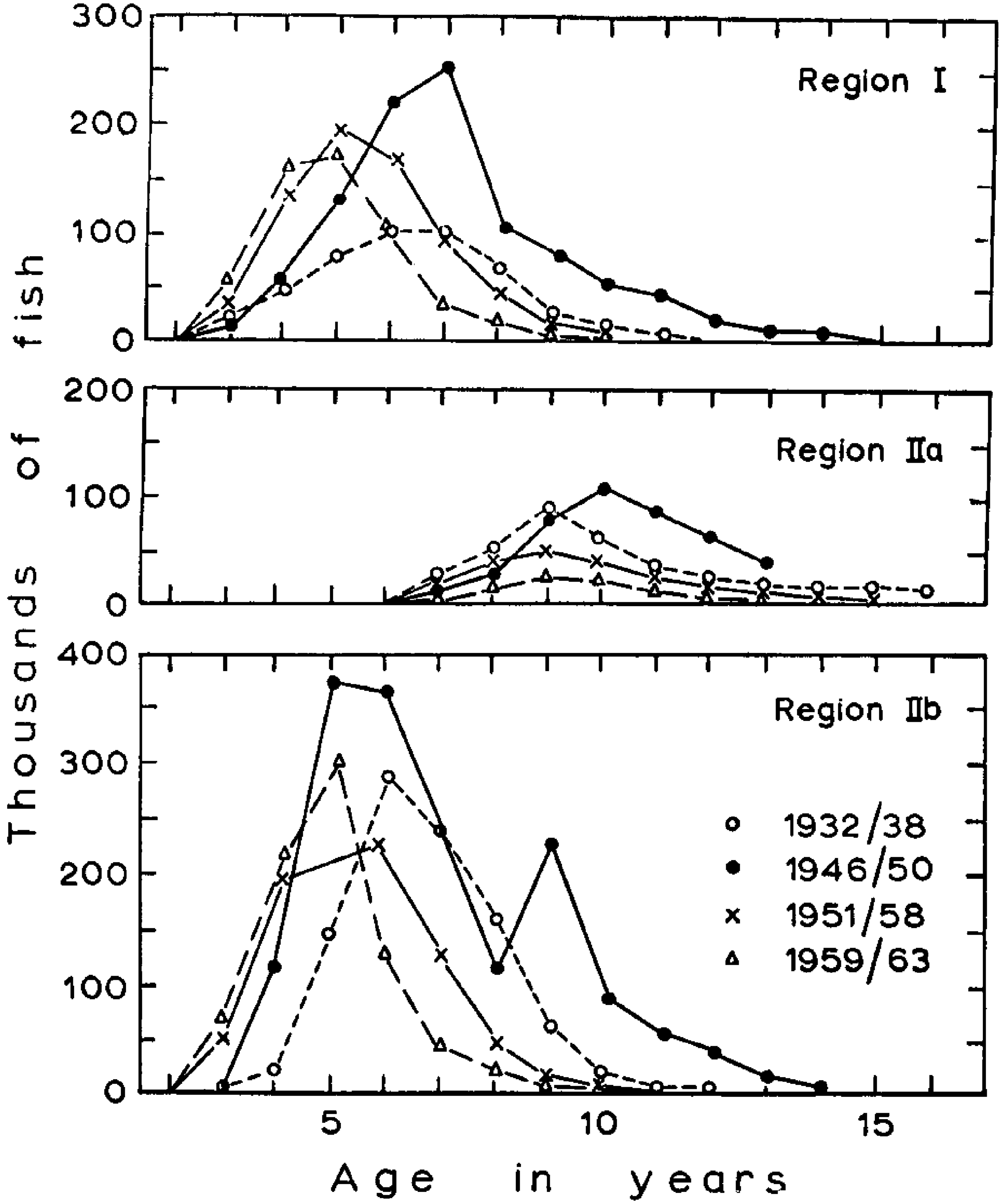

Figure 4E. Age-composition of cod catches in different regions of the Northeast Arctic (from ICES Liaison Committee Report, 1965).

English fishery but some Russian data are included, while in region 1 the age data are mainly based on Russian information but with an input of English data.

The other important thing to notice in this diagram is the differences in the age composition during the different periods. The one with the black dots and the heavy lines is the data for 1946 to 1950. This is immediately after the war, and after the period where there hasn't been much fishing, and in every area the age composition of this period shows an unusual number of old fish. This is particularly striking perhaps in the Bear Island-Spitzbergen area where the samples included a large number of fish from 9 years old and up, whereas for all the other years the old fish hardly appeared in the samples at all. The difference between the other periods is not quite so striking, but there is a tendency for the fish to get younger, and for fewer old fish to appear in the samples as time goes on. By the time we get to the 1959--1963 period, the fish being caught, especially in the feeding areas, are very much younger and smaller. This of course is precisely what we would expect, if indeed we are right in suspecting from the statistics of catches and fishing effort that this stock is becoming increasingly heavily fished and that the mortality is now a significant proportion of the total mortality. If our theory is at all reliable, we must expect the mortality rate to increase and the proportion of small fish to increase.

Figure 4E gives a quantitative way of expressing this. In this figure, the apparent total mortality in the feeding fisheries has been plotted against the estimated total effort. Though the years aren't labelled on this diagram, there is a progression upwards diagonally from 1946 and 1947 on the left to the early 1960's in the upper right. This is quite a satisfactory regression. The natural mortality as judged by the intercept is very low, and in the later period it would appear that something like 80% or more of the total deaths are accounted for by fishing. Apparently there is a satisfactory split of total mortality between fishing mortality and natural mortality which can be fed into the model.

There are possible snags which we will deal with later. We might notice the difference in the intercepts in the two parts of Figure 4F. The lower plot has been calculated for 6-year-old fish, i.e. the apparent mortality as estimated from the ratio of the catch per unit effort of 6-year-old fish is one year to the catch per unit effort of 7-year-old fish in the next year. The intercept suggests a very low natural mortality, less than 0.1; in the upper figure, based on the ratio of 7-to 8-year-old fish, the intercept, and therefore the estimated natural mortality is rather higher, about 0.2. For the moment, one may hope that this is a sampling error and doesn't mean anything, but in fact it is a sign of one of the ways the simple model here does break down and that mortality is not constant. For the present, we will assume that we have estimated total mortality as shown and can take as the natural mortality the larger of the two intercepts, i.e., 0.2. Using a high estimate of natural mortality will underestimate the effect of fishing, and give a conservative figure for the benefits to be obtained from decreasing the amount of fishing. Using this value and the growth curve, the yield curve can be readily calculated, and is shown on Figure 4G. The amount of fishing has been expressed in terms of the ratio of fishing mortality to the natural mortality rather than fishing effort. The point corresponding to the situation in 1963, is shown in the diagram: The figure gives a reasonable description of what has happened since 1946. The fishing effort down here soon reached the point where the curve bends over.

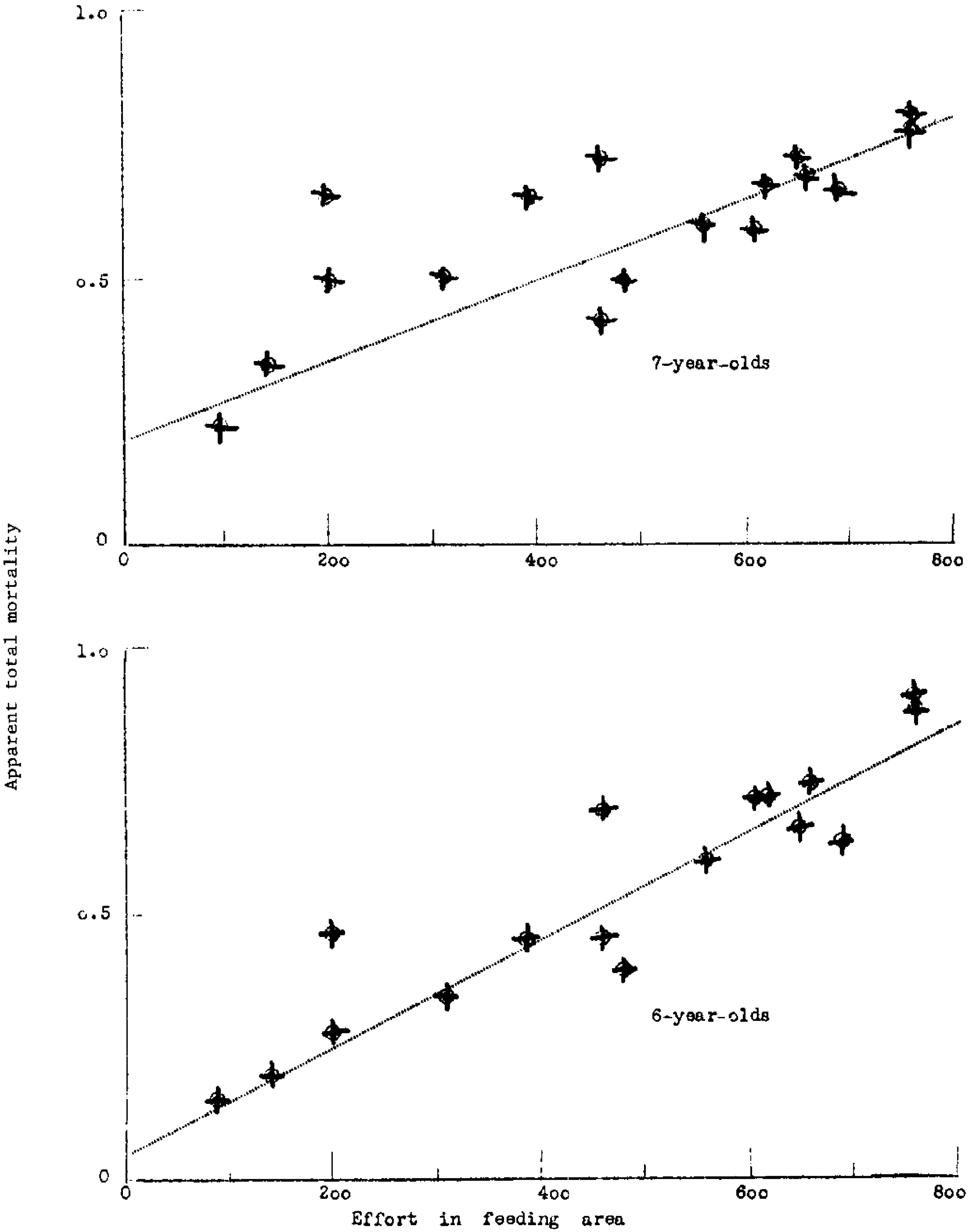

Figure 4F. The relation between the apparent total mortality coefficient and fishing effort for 6-and 7-year-old cod (from Gulland, 1965).

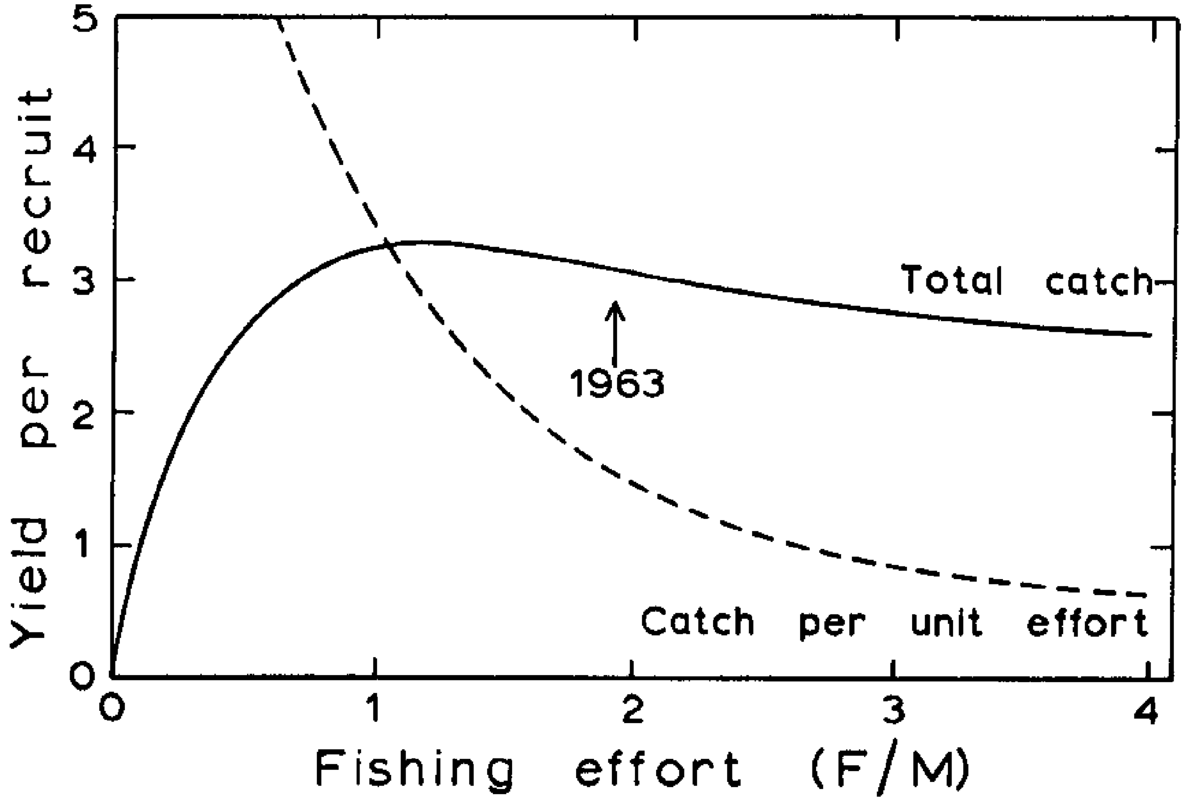

Figure 4G. The relation between fishing effort and catch per unit effort for the Arcto-Norwegian cod stock (from ICES Liaison Committee Report, 1965).

Since then increasing effort has led to no great change in the average catch. Clearly a decrease in the present amount of fishing, in addition to leading to some increase in the total catch, could also result in a substantial decrease in the total costs.

The difficulty in all these international fisheries is exactly how to cut down the amount of fishing. The English would be perfectly happy to recommend that the Russians cut down their amount of fishing because they came in after the English, and so might be held responsible for the effort having reached the present excessively high level. The Norwegians believe that both the Russians and the English should cut their fishing down because fish spawn in Norwegian waters, and are therefore to some extent Norwegian fish, but the Russians don't quite go along with either of these points of view. The situation is perhaps even more complicated than this, because in addition to the three big players in the game, which between them account for something like 95% of catch and even more, over 99%, in some years when the stocks are low, and the fishery unattractive compared with those in other areas, there are other players in the game. There are signs that the three big players can sit down together and come to some agreement on reducing fishing, but the possible reactions of the other players, which include Germany, the Faroe Islands, and France, make the success of such an agreement problematic. These and other countries have big trawlers in their fleets which fish for cod in other areas, and the Northeast Arctic is not their favorite area. They take most of their catch across in the West Atlantic, but if a conservation scheme in this area by the major players were successful and the stocks increased, then those other players would be attracted into the Northeast Arctic, and the effort would increase again.

To summarize, we have for the Arctic cod a reasonable general explanation of the main past events in the fishery and an estimate of total mortality, and a division of this total mortality between natural mortality and fishing mortality that appear generally satisfactory. Ways in which these estimates may not be too precise or reliable for detailed analysis will be discussed later. At the same time, although the analysis shows clearly the desirability of reducing the fishing effort on the stock, there are serious difficulties in doing this.

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Chapter #5

ICELAND COD: VARIATION OF MORTALITY WITH AGE. COHORT ANALYSIS

We have already considered the Arctic cod, which is one of the big fisheries in the Northeast Atlantic, and now let us look at another of the major fisheries in this area, the fishery round Iceland (Gulland, 1961).

The analysis presented previously is a nice simple answer and a fairly reasonable description in broad terms about what is going on, but life is just not quite so simple as all that. In fact this description of the Arctic cod fishery is not completely true. It was, to a large extent, a gross oversimplification, and a rather important point is the degree to which any fishery scientist has to simplify the problem with which he is dealing. Most fishery scientists are employed by governments to advise them on what is happening to the fishery and how the situation in the fisheries can be improved. If the fishery scientist worries too much about details, he will carry on for years and years worrying about slight differences between fish in different areas, variation in growth rate, the effect of currents on the distribution of fish, the effect of environmental conditions on the year-classes and so on. He can keep himself busy for years on the details and never come up with any specific answers about the state of the fishery, or concrete advice on what should be done. To come up with useful answers within a reasonable time, and this can be a difficult matter of judgment, he must make some simplifications. On the other hand if too great simplifications are made, a nice simple answer is obtained but it doesn't describe the fishery adequately.

Let us analyze the extent to which simplifications can be made, and the extent to which useful answers and advice to the industry can be forthcoming. If the simplifications are useful and powerful ones, they can be built upon and modified, provide useful initial advice in broad terms on the basis of simple assumptions, and can then go on and plug the complications into the models and give more and more precise advice. One complication is the fact that fishing mortality does not stay constant with age. It tends to vary because the large and small fish are differently distributed. In the extreme case there may be quite distinct fisheries on small fish and large fish, but even in the same fishery, the fishing mortality can vary with age.

This separation into distinct fisheries is illustrated by the rather rough diagrammatic situation of the fisheries around Iceland (Figure 5A). There are two major fisheries, the fishery on the spawning fish in the southwestern corner, which, like the fishery on spawning fish on the Arcto-Norwegian stock, is carried on close inshore, around the Westmann Islands, by a variety of gears. It is almost entirely carried out by local Icelandic fishermen, though there is also some trawling by other countries on the aggregation of fish before they start to spawn. The other major fishery is the fishery on nonspawning fish carried out almost entirely by trawlers around most of the coast of Iceland. As suggested in this diagram, it is not so much in the southwestern part, but more on the northern, northwest, and eastern sides. This nonspawning fishery is both on the young fish, the immature fish, and also on the mature fish, which after they finish spawning go back and feed.

It is largely a trawl fishery carried on principally by British trawlers, at a convenient distance from the main English ports of Hull and Grimsby. The fishermen can go up there, do their 20-day trip, and come back with what is often referred to as fresh fish. The rates at Iceland in terms of catch per hour or per day on the grounds, though better than those in the nearer waters around the British Isles, are rather less than in the more distant grounds at Greenland, Labrador, or up in the Northeast Arctic; hence the factory trawlers tend to go to these more distant grounds and the Iceland fishery is carried on by these fresh fish trawlers. There is a long history of British fishing in this area, which hit the world press some years back during the so-called "cod war," when Iceland extended her limits around Iceland from the 3-mile to the 12-mile limit. The other complication in Iceland is the immigration of mature fish from East Greenland and the southern part of West Greenland, to join the Iceland spawning fishery. So we have the three sets of fish--the immature nonspawning fish, the spawning fish that come in from the nonspawning fishery, and spawning fish coming in from Greenland. Figure 5B shows the usual basic statistics of catch, catch per unit effort, and effort.

Again, the importance of getting the correct effort should be emphasized. One very important adjustment at Iceland has been the adjustment for the increase in size of trawlers. The first trawlers to go to Iceland from England half a century ago were very small, around 200 tons or less, and steadily ever since then the size of new trawlers has increased. The mean size of trawlers increased so that for much of the period the catch per hour of the individual trawler tended to stay about the same, even though the stock abundance was actually decreasing.

The problem of measuring effort or fishing time was discussed previously. The difference the unit of effort used can make to estimated changes

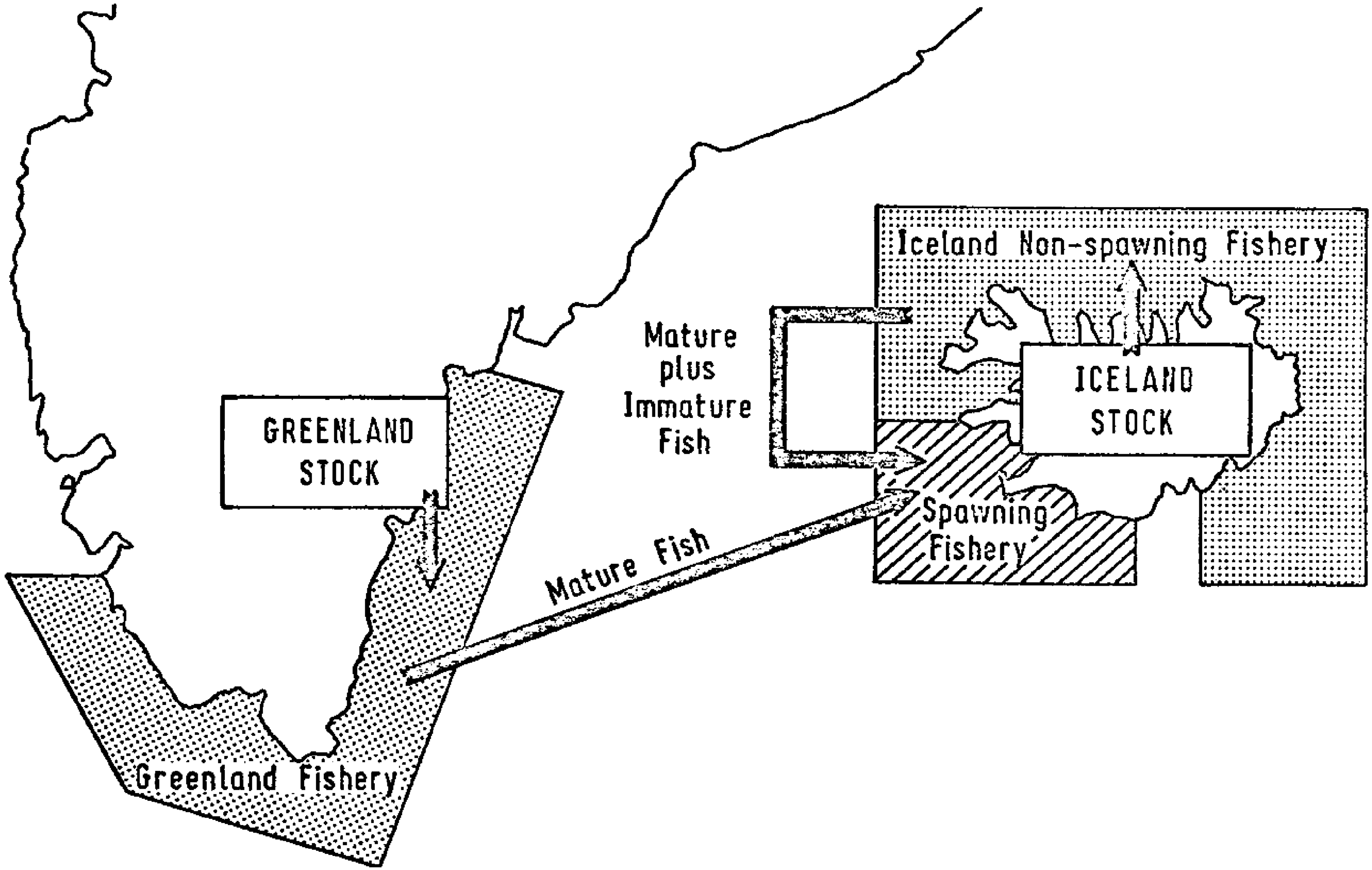


Figure 5A. Diagrammatic representation of the Icelandic cod stocks and the fishery on them (from ICES, 1970).

in abundance is illustrated by the changes of apparent catch per unit effort at Iceland between 1948 and 1951. Measured in terms of catch per day, the stock went down from 100 to 95; in 1951, it was 95% of that in 1948, and the catch per hour 65%. On the other hand, if we put in corrections for size of trawler the catch per day went down from 100 to 74%. The best estimate is the catch per hours of fishing corrected for size of trawler, or catch per ton hour--that's the hours of fishing multiplied by the mean tonnage of the trawlers, and in 1951 this was only 51% of the 1948 value. This is probably the best measure of the abundance in the nonspawning fishery, and is the measure used in Figure 2B. This shows that from 1946 up to 1960, there was a steady fall-off in the catch per unit effort. The catches started to increase, then decreased, while the effort came to a peak around 1962. It has since then fallen off, down to 1970, and there has been some increase in catch per unit effort since 1962. However, only part of the changes in stock abundance, or catch per unit effort, can be accounted for by changes in the fishing effort.

Figure 5C illustrates another of the complications of life in many of these fisheries. In our discussion so far, we have considered everything being more or less in a steady state except for the effect of fishing. But many of these fisheries are very far from being under uniform conditions. There are, in particular, great fluctuations in the year-class strength and this shows the effect of the year-classes on the catches of Iceland cod. The histograms at the bottom show the estimated strengths of each year-class in each year and the upper part shows the total landings. These have been displaced 8 years so that the year-class is plotted under the catch in the years during which the year-class makes its biggest contribution to the fishery. For example, this strong 1922 year-class gave good catches from 1928 up to 1933, but this

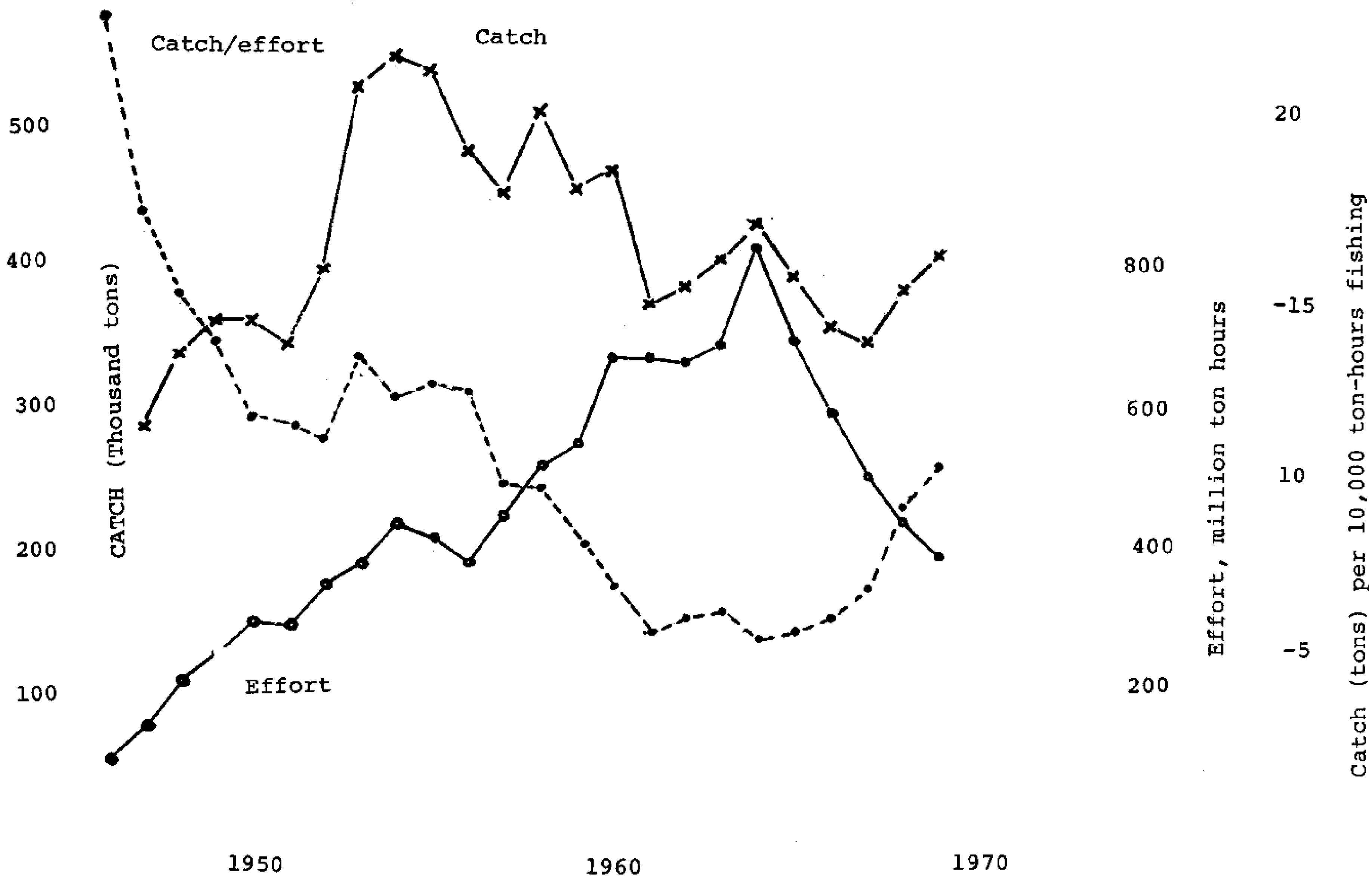


Figure 5B. Trends in catch, effort, and catch per unit effort of cod at Iceland (from ICES, 1967).

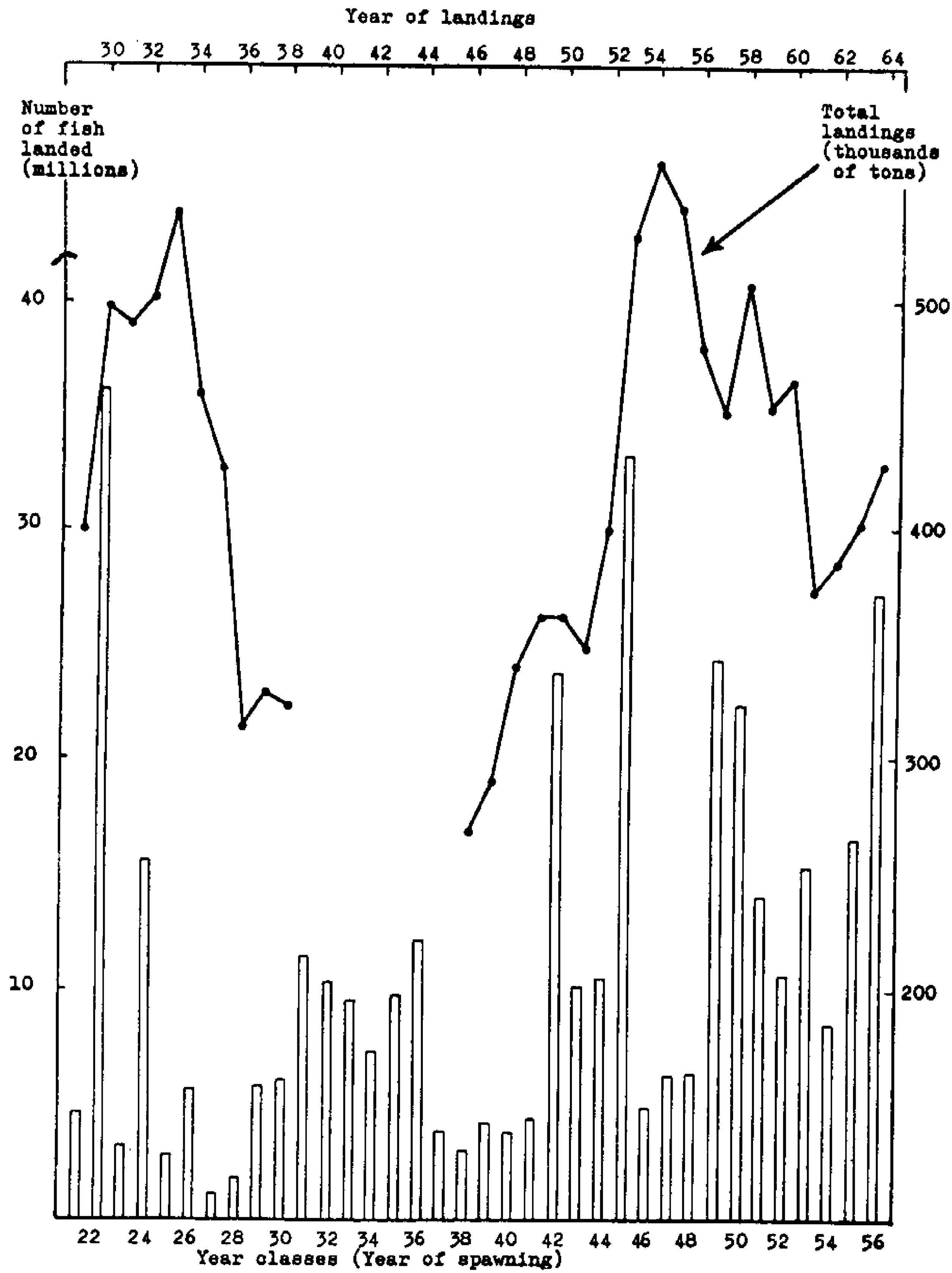


Figure 5C. The relation between year-class strength and total catch of Icelandic cod (from ICES, 1969).

outstanding year-class was followed by a succession of rather poor year-classes, which resulted in the decrease in abundance after 1933.

There are two aspects of the variation in year-class. One is that this variation can be considered as an essentially random variation, which makes the analysis of the situation more difficult. In particular, it will add scatter in any plot of catch per unit effort. Also year-class fluctuations make predictions of catches in forthcoming years more difficult. The other, and more important, question regarding the strength of year-classes and recruitment is: Just why is this 1922 year-class so strong? Has it any connection with the abundance of fish in that year? This extremely important question of the relation between stock and subsequent recruitment will be discussed later.

What is clear in many of these northern fisheries is that though there may be some underlying relationship between stock and recruitment so that the mean recruitment may be different for different stock levels, it is quite clear that the recruitment in any one particular year also depends on factors other than the spawning stock in that particular year. For instance, the 1922 and 1923 year-classes were very different in strengths though the spawning stock must have been approximately the same size. This variation in year-class strength is one reason why the results of many of the calculations particularly of the Beverton Holt model, are expressed as the yield per recruit.

Another reason is that in the expression for the yield, in addition to the parameters of growth and mortality, there is the unknown parameter of recruitment. It is easy to bring this expression for recruitment over to the left-hand side of the equation, and express the answers as yield per recruit. This leaves on the right-hand side an expression for the yield per recruit in terms of parameters of growth, mortality, etc. for which estimates should be available. Another reason for expressing conclusions in terms of yield per recruit is that in a fishery with very fluctuating year-classes, it is very difficult to say, for instance, that if fishing effort is cut by 20%, the catch will go up 10%, because for reasons that are quite independent of the fishery, a poor year-class may come into the fishery. The actual captures will not go up 10%, but may perhaps go down 10%. However it will be true to say, assuming the calculations and assumptions are correct, that the catch will be 10% higher than it would have been if the effort had not been reduced. If recruitment is average, the yield will go up 10%. If recruitment is poor, the yield may go down 10%, but if we haven't made the change, it would have gone down even more. Therefore assessments are often made in terms of yield per recruit or in terms of the difference between what would have happened if particular action had been taken compared with what would have happened if no action had been taken.

Turning to the Iceland fishery, and looking first at the spawning fishery alone, Figure 5D shows the result of plotting the total mortality rate against the amount of fishing. To reduce some of the scatter, this has been averaged for 5-year periods running from 1930 up to 1965. This results in a very satisfactory regression, giving a reasonably good estimate of natural mortality of 0.19 and a good estimate, by subtraction, of the fishing mortality on the mature fish in each of these 5-year periods. Figure 5E compares the age-composition of catches in the spawning and other fisheries. The fish come into the spawning fishery around 6 to 7 years old and are more or less fully recruited to the spawning fishery by 9 years. In contrast, the fish recruit

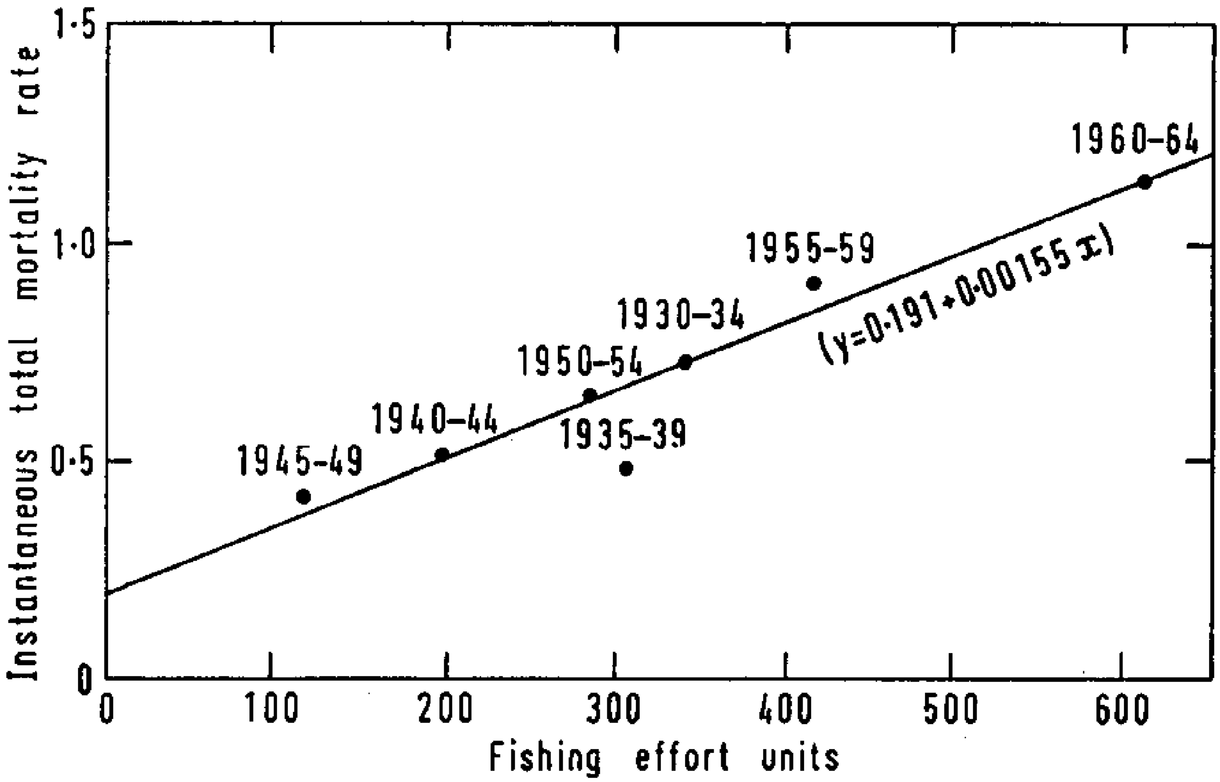


Figure 5D. Relation between total mortality and effort for the Icelandic spawning cod (from ICES, 1967).

to the English trawl fishery at 3 or 4 years old and from 5 years old onwards there is a remarkably straight line decrease in the logarithm of the numbers at each age. On the face of it, this represents a very good estimate of the total mortality of the cod stock. The problem then is that if this is really the total mortality and the actual numbers of fish do fall off at this rate, where do all these old fish come from that are in the spawning fishery? For the older age groups, 10 to 15 times as many fish are caught in the spawning fishery as appear in the trawl fishery. The number in the stock must be greater than the catches in the spawning fishery. This means that the trawlers are catching a very minute proportion of the total stock, which doesn't fit in with the fact that the trawlers are having a significant effect on the stocks. It means, in fact, that the observed decrease of numbers with age in the trawl catches can't be the real decrease of fish in the sea. It must measure to some extent the rate at which the fish are coming out of the trawl fishery. The decrease in numbers includes a term due to the decrease in fishing mortality in this fishery. The fishing mortality due to trawlers on 4-year-old and 5-year-old cod is higher than the fishing mortality on 9-or 10-year-old fish. The problem one has is just how to deal with it.

The simplest way of approaching this problem has been set out by Murphy (1965). He points out that the catch in a given year is given by

$$C_i = N_i \frac{F_i}{F_i + M} (1 - e^{-F_i - M})$$

Where N_i = numbers alive at the beginning of the i^{th} year; similarly the catch in the following year is given by

$$C_{i+1} = N_{i+1} \frac{F_{i+1}}{F_{i+1} + M} (1 - e^{-F_{i+1} - M})$$

$$= N_i e^{-F_i - M} \frac{F_{i+1}}{F_{i+1} + M} (1 - e^{-F_{i+1} - M})$$

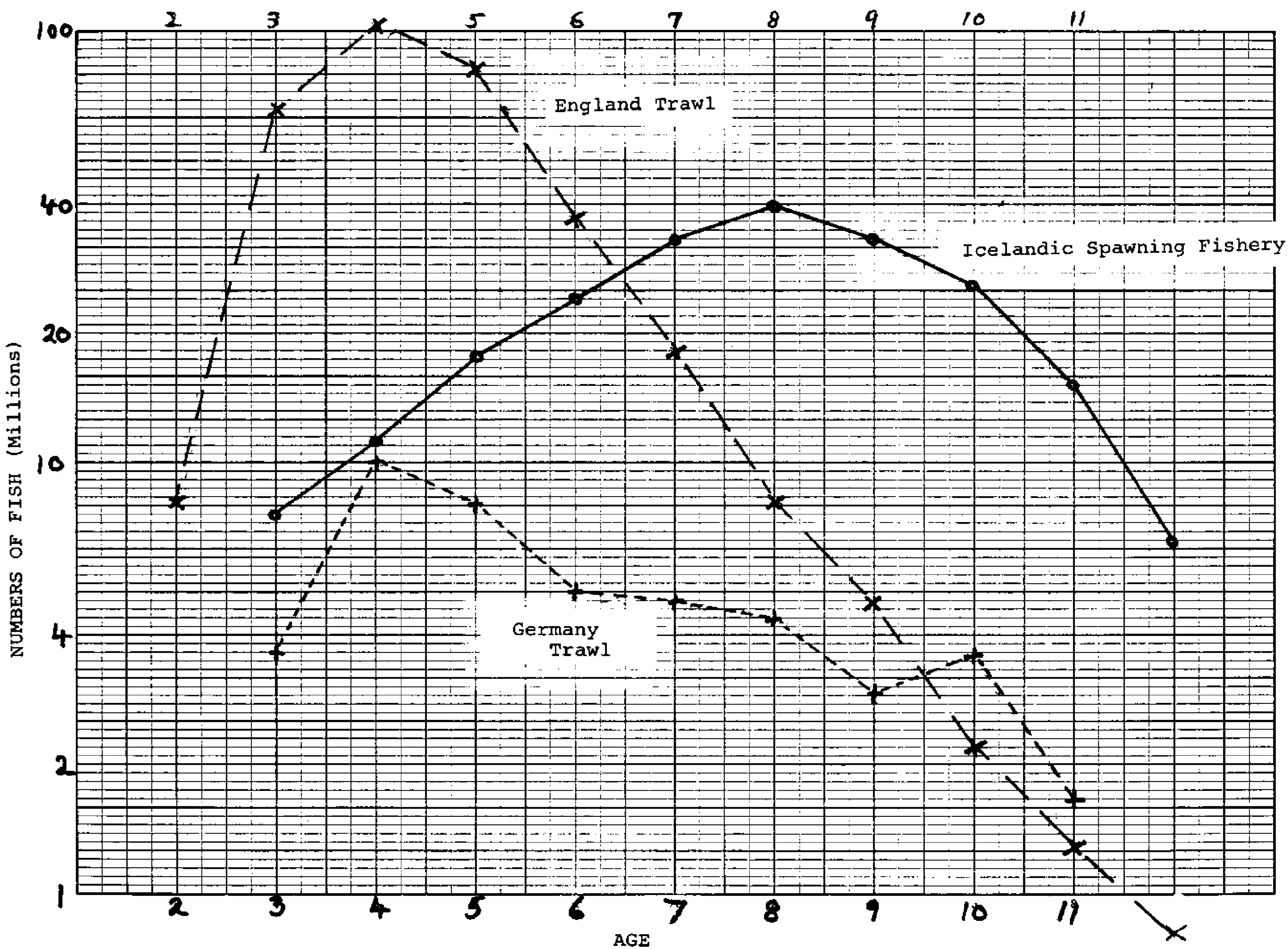


Figure 5E. Numbers of cod of each age (logarithmic scale) taken in different sections of the Icelandic fishery, 1960-1966 (from ICES, 1969).

The ratio of catches in successive years is therefore given by

$$\frac{C_{i+1}}{C_i} = \frac{F_{i+1} \ (1 - e^{-F_{i+1}-M}) \ \ (F_i + M)}{F_i \ (1 - e^{-F_i-M}) \ (F_{i+1}+M)} \ e^{-F_i-M}$$

This expression for the ratio of the catches in the two years has three unknown quantities, F_i, F_{i+1}, and M. Similarly if we go on to find the ratio of the catches in the next 2 years, this will be some expression which will include the fishing mortality in the second year, the fishing mortality in the third year, and the natural mortality, i.e., one new unknown, F_{i+2}. And we can do this for the whole life of a year-class. Each time we add another year, we add just one more unknown. So if we have a fish living in the fishery for 7 years, the ratio of catches can be calculated for 6 pairs of years, giving 6 equations and 8 unknowns, the natural mortality and the fishing mortality in each of the 7 years. If we can make some estimates from outside this system of two of these quantities, then we have the right number of equations for the unknowns and can solve for remaining mortality rates. In particular, as Murphy shows, the calculations ran readily be carried forward from a particular year, given the values of M and F in that particular year.

A slightly different approach, though ultimately the arithmetic in the end comes out to be very much the same, is the way suggested by the problem met in Iceland. The fishing and natural mortalities that seem to be occurring in the feeding fisheries imply numbers of old fish in the population that are rather less than the actual catches in the spawning fishery. This suggests that the virtual population, in the sense of Fry, i.e. fish present in the population which will ultimately appear in the catches, could provide a useful lower limit to the population. For example, if we catch a million fish of the 1968 year-class older than 4 years old, then at the end of their 4th year, there must have been at least a million fish of that year-class in the sea. The virtual population of the x year-class on its n^{th} birthday is the sum of the catches of that year-class from age n onwards i.e. ${}_xV_n = \sum_{i=n} {}_xC_i$, summed up to the oldest fish that appear in the catches.

One estimate of the survival during a year is equal to the number of fish we knew for certain were alive at the end of the year divided by the number of fish we knew for certain were alive at the beginning of year n.

i.e. $s = \frac{{}_xV_{n+1}}{{}_xV_n}$ where s = survival of the x^{th} year class.

Provided there is an adequate sampling system to determine how many fish of each age are being landed each year by the different sections of the fishery, this estimate can be calculated without further assumption. We can be sure that this expression is what we think it is. It involves none of the complications of measuring abundance or fishing effort which can make perhaps otherwise more desirable methods of measuring survival less reliable in practice. This formulation, without further modification, is most useful when the virtual population approaches the true population, that is when most of the fish in the sea will be caught sometime during their lives rather than die of natural causes. Also we can define an exploitation rate ${}_xE_n$ as the proportion of the year-class x alive on its nth birthday which will be caught sometime in the future, and this will be equal to the virtual population which is the number of fish we actually do catch divided by the number of fish alive. Clearly if the mortality rates are constant, $E = \frac{F}{F + M}$. But as the exploitation rate, E, is defined here, it makes no assumptions about the form of the mortalities. It just says this is the expectation of being caught sometime during its life.

Now if we can go on from this to carry out various calculations to express these virtual populations and exploitation rates in terms of the fishing mortalities and the natural mortalities, then provided two unknowns, usually the natural mortality and the fishing mortality or exploitation rate for the oldest fish, are assumed, the fishing mortalities at other ages can be calculated from the observed catches.

The catch during any year can be expressed as a function of the fishing and natural mortality rates during the year, and of the population at the end of the year. Thus, in a manner similar to that of Jones (1964), if it is assumed that natural mortality is constant, and some value of fishing mortality among the very old fish is assumed, it is possible for each year-class to proceed year by year backwards from old to young fish estimating the fishing mortality in each year.

Assuming that year-class x is t years old in year n,

let $${}_x r_n = \frac{{}_x N_{n+1}}{{}_x C_n}$$

i.e. r is the population at the end of the year, expressed as a proportion of the catch during the year (thus r can be greater or less than unity)

then $${}_x r_n = \frac{{}_x N_{n+1}}{{}_x C_n} = \frac{{}_x N_n \, e^{-(F+M)}}{{}_x N_n \frac{F}{F+M} \cdot (1-e^{-(F+M)})}$$

where for convenience F has been written for ${}_t F_n$.

Thus ${}_x r_n$ is a simple function of ${}_t F_n$ and M, and if given M, the function

$$\frac{(F+M)\ e^{-(F+M)}}{F(1-e^{(F+M)})}$$ is tabulated for a range of values of F, then once ${}_x r_n$ is

determined, ${}_tF_n$ can be at once read off from this table.

$$\text{Now } {}_xr_n = \frac{{}_xN_{n+1}}{{}_xC_n} = \frac{{}_xV_{n+1}}{{}_xE_{n+1} \cdot {}_xC_n}$$

$$= \frac{1}{{}_xE_{n+1}} \left(\frac{{}_xV_{n+1}}{{}_xV_n - {}_xV_{n+1}} \right) = \frac{1}{{}_xE_{n+1}} \left(\frac{{}_xS_n}{1 - {}_xS_n} \right)$$

i.o. ${}_xr_n$ is a simple function of the apparent survival during year n (as estimated from virtual populations) and the exploitation ratio ${}_xE_{n+1}$, applicable to the fish of the x-year-class alive at the end of year n.

The exploitation ratio, ${}_xE_n$, applicable to the fish at the beginning of year n will be the sum of the proportions of fish alive at the beginning of the year caught during the year, and caught later, i.e.

$$\frac{{}_tF_n}{{}_tF_n + M} \left(1 - e^{-({}_tF_n + M)}\right) + e^{-({}_tF_n + M)} \, {}_xE_{n+1}$$

Thus, if values of M and ${}_xE_{n+1}$ are assumed, estimates can be observed in succession of ${}_xr_n$, ${}_xE_n$, ${}_xr_{n-1}$, ${}_{t-1}F_{n-1}$ ..... etc. The actual steps in the calculation of mortality rates for the 1948 year-class are set out in Table 1 (values of M = 0.20, and E at the 15th birthday of 0.8 were taken).

The results of applying this technique to the Icelandic cod fisheries are given in Figure 5F which shows the estimates of the fishing mortality on different ages of fish in the Iceland fishery. The first and rather striking thing is that the fishing mortality as a whole (the full line in Figure 5F) goes on steadily upwards, reaching a mortality of about 1.0 among the oldest fish. The mortality in the Iceland spawning fishery is negligible for fish younger than 4 years and then builds steadily. In the other fisheries, the fishing mortality rises to a maximum at 4 to 5 years old, then drops off. This is because there appears to be some segregation between different sizes even for immature fish, and the trawlers tend to concentrate on the most abundant age-groups. From 8 years old onwards, there is tendency for the mortality in the trawl fisheries to increase again. This is because in addition to the main trawl fishery on the young immature fish, there is a fishery on the spawning and prespawning fish, other than the Iceland spawning fishery itself.

This improved set of mortality rates can now be fitted into our calculations of the yield. Instead of using a single mortality rate, which is constant from some age of recruitment onwards, the calculations can be done using a different rate for each age, and also computing the catch of each fishery separately. These calculations give the effect on the total yield of different mortalities and also the effect on the yield of the individual fisheries. Figure 5G shows the results that have been obtained. These have not been calculated for an entire yield curve. Each of these curves in the figure is a section of a yield curve starting from the present situation and assuming that the total mortality rate on each age group is changed by the same percentage amount, ranging from an increase of 40% to a decrease of 60%. This has been shown for each of the major fisheries separately. It is also shown for three different values of natural mortality, from 0.05 up to 0.3.

Turning first to the Iceland spawning fishery, we see that whatever the natural mortality, whichever of the three curves we look at, if we decrease the fishing mortality, we will increase yield at least for the magnitude of reductions considered here. Clearly a big enough reduction will result in a loss in yield, and in the limit, no fishing must mean no catch. The same is true for the total yield for the lower value of natural mortality which is perhaps the less likely one. If the natural mortality is as high as 0.3, however, any reduction in fishing will decrease yield, because too many fish

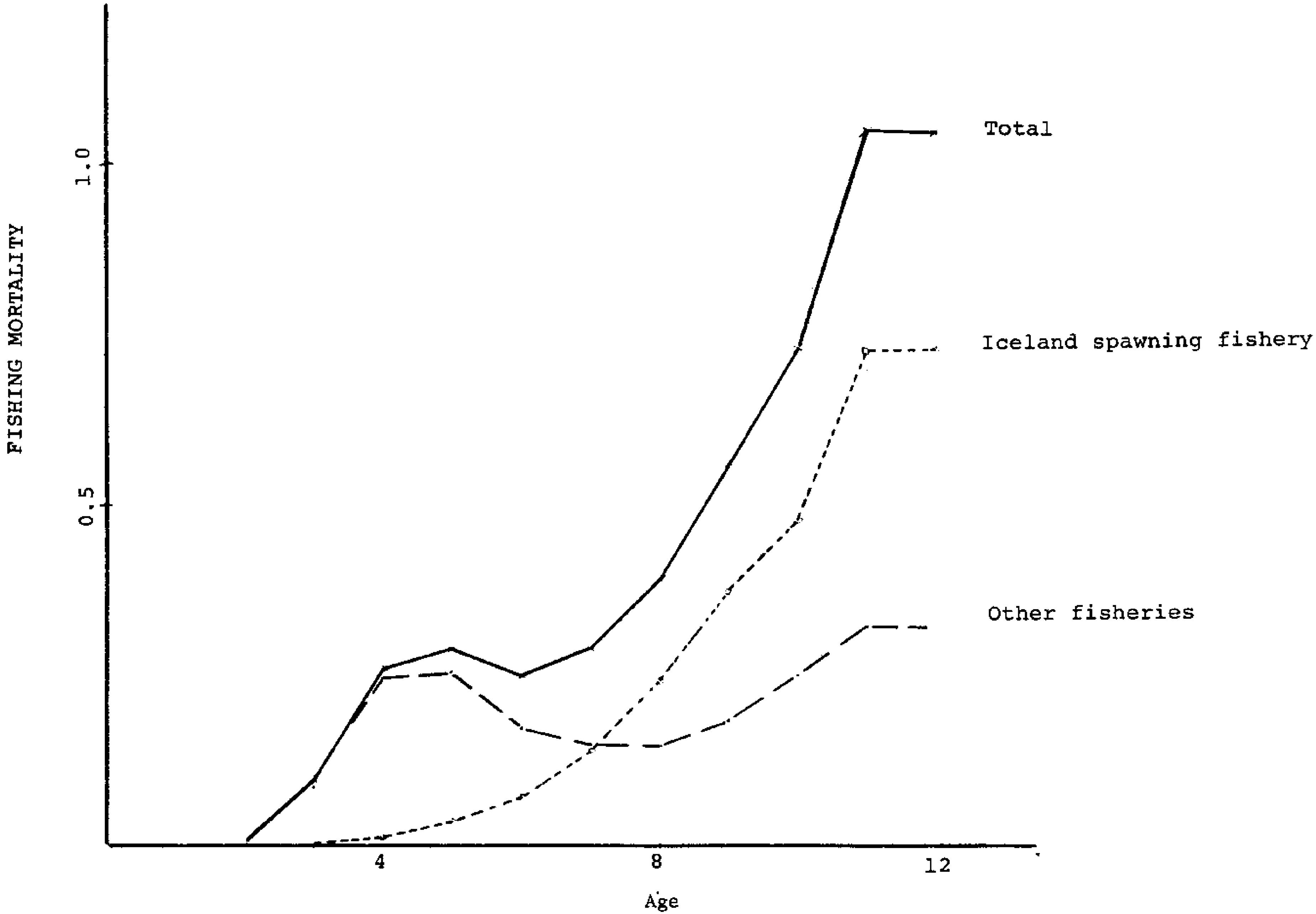
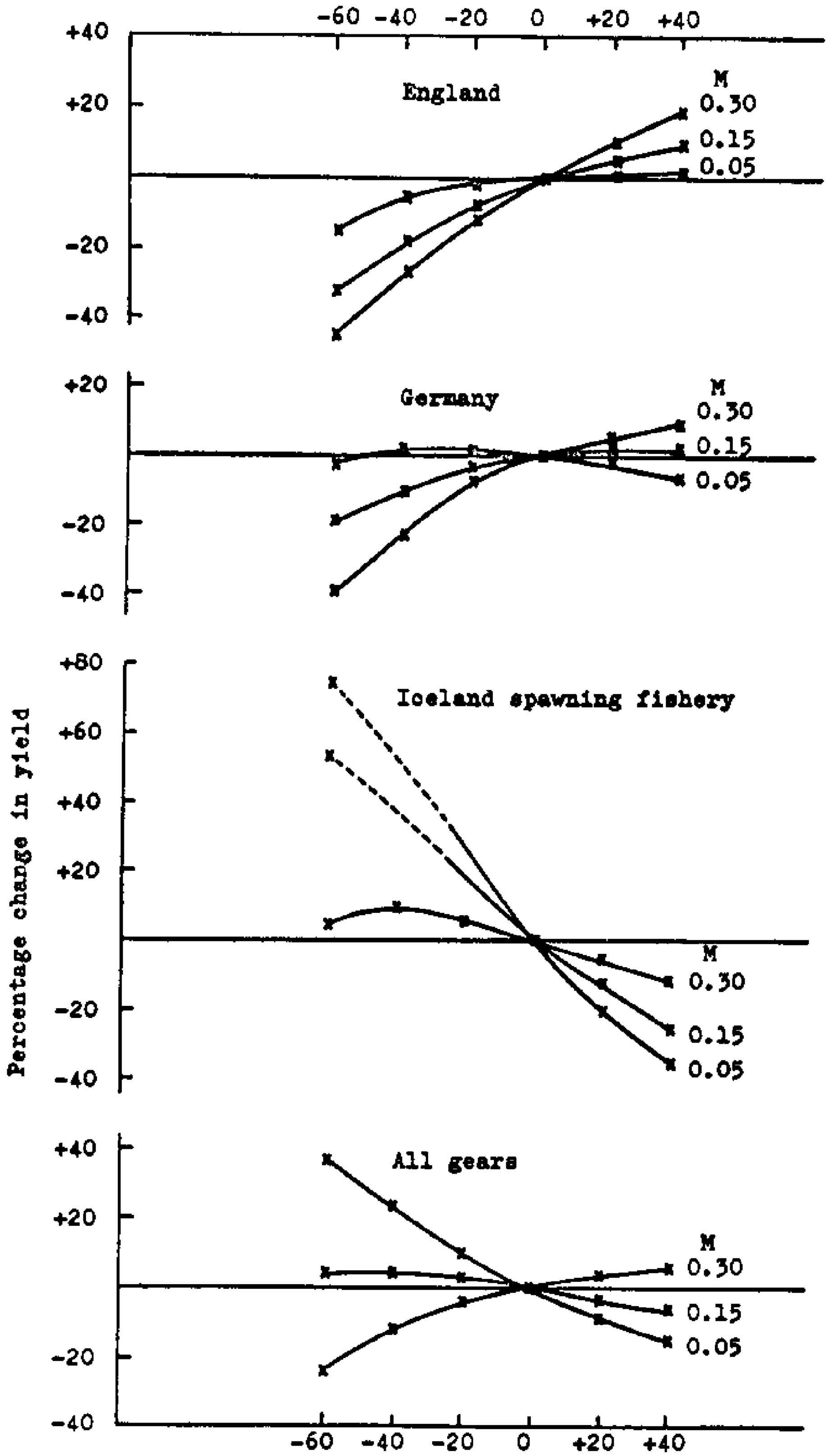


Figure 5F. Estimated fishing mortality on different ages of Icelandic cod (from ICES, 1969).



Percentage change in fishing mortality rate from mean 1960-1966 value.

Figure 5G. Effects on different sections of the Icelandic fishery of changes in the total fishing effort, for a range of possible values of natural mortality (from ICES, 1969).

will die before they are caught. Well that is all right for the Icelanders and for the total as a whole, but it is not all right for the English fishery, which is on the smaller fish. As Figure 5G shows, whichever of the assumed values of natural mortality is used, a reduction in the total amount of fishing will reduce the English catch. This, of course, makes it that much more difficult to reach agreement. Assuming that the natural mortality is low, just how much can the English fishery be penalized and lose catch to insure that the Icelanders get a lot more fish and the total increases slightly?

This is perhaps an oversimplification because the pattern of distribution of the trawlers is based on the distribution of the most abundant fish, which under the present conditions of the high total mortality, means the youngest fish. It pays the trawlers to go where these young fish are. If the amount of fishing is reduced, the relative abundance and absolute abundance of the bigger fish will increase. It would pay the trawlers to go more to where the bigger fish are, and some of the estimated loss will disappear. For instance, the German fishery is also a trawl fishery but is more seasonal and more concentrated on the large prespawning fish. This trawl fishery would, in fact, benefit from a decrease in amount of fishing provided that the natural mortality is not too high. If the amount of fishing changed, it is quite possible that the pattern of English fishing would change to something more like that of the German fishery.

There is another complication that needs to be thought about in the Icelandic fishery, and indeed in all these distant-water, North Atlantic cod fisheries, and that is the ability of the fishing fleets to move very rapidly from area to area. Looking back to Figure 4D, giving the trends in catch in fishing effort in the Arctic cod, we see that in the last few years there have been rapid changes in the amount of fishing. There was a big decrease in 1963 when many trawlers went over to the Western Atlantic; 1968 and 1969 catches were very good in this area and so was the effort; as elsewhere the catches were not very good. In 1971 the effort in the Northeast Arctic seems to have dropped off and probably when the statistics are available, they will show a sharp drop in catch. Catches at Iceland were good after a period of relatively low effort, and a large proportion of the English distant-water fleet and a number of other trawlers have gone there. The fishing effort there this year (1971) will be very high. The catches will be very high and the stock abundance, the catch per unit effort at the end of this year, will almost certainly be rather low. These interdependent fluctations of 50% or more in effort in different areas over a 2- or 3- year period, make it no longer possible or reasonable in these fisheries to sit down and look at each fishery as an independent single unit, a unit in which things do not change very much from year to year.

A system of advice is needed that treats the North Atlantic cod fisheries as a single system, and that takes into account the ability of the different fleets to move very quickly from area to area, and the big fluctuations in year-class strength in each stock. In such a system, the effort in a particular area in a given year will depend on the past history in that area, whether it has been fished very hard or very lightly, the strength of the year-classes occurring in that fishery at that time, and also the relative abundance of fish stocks in different areas. For example, the high effort in 1969 in the Arctic was due to a combination of relatively light fishing in the immediately preceding years, the presence of a couple of very good year-classes, the 1962 and 1963 year-classes, and poor fishing elsewhere. This is a problem suitable for tackling by the use of a simulation model, and such a model is being developed at the Lowesroft Fisheries Laboratory. It deals with some ten different fisheries in the North Atlantic--the two we have been considering, plus that at the Faroes, and the major fisheries on the West Atlantic. It calculates the yield along the lines already discussed--the abundance of year-classes each year in succession, from the time it recruits, being reduced by fishing and natural mortality, but added to by growth of the individuals. This model has provided a good fit to past events and can supply predictions about what will happen in the next few years. It can predict the effects of either introducing a regulation in one area without regard to events in any other area, or the effect of a comprehensive system of regulation covering the whole cod stocks in the North Atlantic.

One very important item in using the model as a predictive device is the need to get estimates of year-class strength some time before they recruit to the fishery. In some areas this is now going rather well. In the Southeastern Barents Sea, the Russians have been carrying on surveys of young cod for some 20 years. These have been extended to the Bear Island-Spitzbergen area, and are now carried on jointly by the Russians, Norwegian and English research vessels. In the first few months of life, the larval and young cod are pelagic, but in the autumn they move to the bottom and can be caught in small-meshed bottom trawls. Thus a quantitative estimate of the abundance of a year-class can be obtained some four years before it recruits to the fishery. Increasingly precise estimates of the abundance can be obtained from the catches of 1-,2-,and 3-year-old fish in the research surveys. The correlation between the estimated abundance in these prerecruit surveys and the abundance in the fisheries has been good. It appeared that the prerecruit surveys tended slightly to underestimate the abundance of the 1963 and 1964 year-classes, but not by very much. They also show that the following year-classes, 1965, 1966, 1967, up to 1968, have all been poor or very poor, but the 1970 year-class looks very good.

Thus it is possible to make a reasonable prediction for this Arctic cod fishery, that in the next few years there will be reasonable fishery on the spawning fish as the survivors of the 1963 and 1964 year-classes come through, but then there will be poor fishing until about 1974 or 1975 when this good 1970 year-class comes into the juvenile fishery.

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Chapter 6

NORTHWEST ATLANTIC COD FISHERIES: THE EFFECT OF MINIMUM MESH SIZE REGULATIONS

Next let us consider the great fishery in the Northwest Atlantic, particularly on the cod, ranging from Greenland, around Labrador, and from Newfoundland to New England. It is interesting that the cod fishermen were the first to move into the northern part of North America. Cabot discovered these grounds in 1497, and by 1527 an English fishermen visiting St. Johns in Newfoundland noted that there were "11 sail of Normandy, 1 of Brittany, 2 of Portugal, all a fishing!" In other words, within 30 years there was a thriving international fishery on these cod stocks and ever since then this cod fishery has been extremely important, both to the local people and to many parts of western Europe (Innis, 1954).

Two quotations are appropriate here. One from 1784 is a motion passed by the House of Representatives in Massachusetts that "leave might be given to hang up the representation of a cod-fish in the room where the House sit as a memorial to the importance of the cod fishery to the welfare of this Commonwealth." That is the importance in New England. The next quotation from a century later is from a French abbé who, after visiting Canada in 1871, said "It is the land of the cod fish. Your eyes and nose, your tongue and throat, and ears as well, soon make you realize that in the Peninsula of Gaspé the cod-fish forms the basis alike of food and amusements, of business and general talk, of regrets and hopes, good luck, everyday life. I would almost be ready to say of existence itself." Well that's the importance of the cod in that area.

Figure 6A shows the fishing grounds in the Northwest Atlantic, as well as the regions into which it has been divided by the International Commission for the Northwest Atlantic Fisheries (ICNAF) for statistical and other purposes. This shows the shoal area, less than 100 fathoms, over which most of the fishing takes place. Particularly obvious is the great area of shelf, the Grand Banks of Newfoundland, which has always been one of the most important fishing areas.

The fisheries in this area until recently have been of two major types: one, the vessels coming from Europe, which salt their fish on board, and which fish mainly on the off-shore waters, particularly the Grand Banks, and the other, the local fishery catching the fish inshore, salting them and drying them, and then shipping them either to Europe or to the West Indies. The new fishery, which is a development of the last few years, has been the big freezer trawler, particularly from eastern Europe, Russia, and Poland, which freezes and often fillets the cod at sea. While part of the ICNAF fisheries are carried on by some of the largest and most technically advanced fishing vessels in the world, the old fishery is still going on. There are still a few Portugese sailing schooners working off Greenland using dories, each man going out in a small boat in the middle of nowhere off West Greenland to catch cod on hand lines. This is probably among the toughest form of fishing there is.

Tables 6A and 6B present the summary statistics of the cod and haddock fisheries in the ICNAF area during the past twenty years. These have been arranged first by countries, and then according to the statistical subareas shown in Figure 6A.

The country figures illustrate how the catches in most of the traditional fisheries, such as Canada, France and Portugal, have not shown any very marked trend, though Spanish catches have increased greatly. The more striking

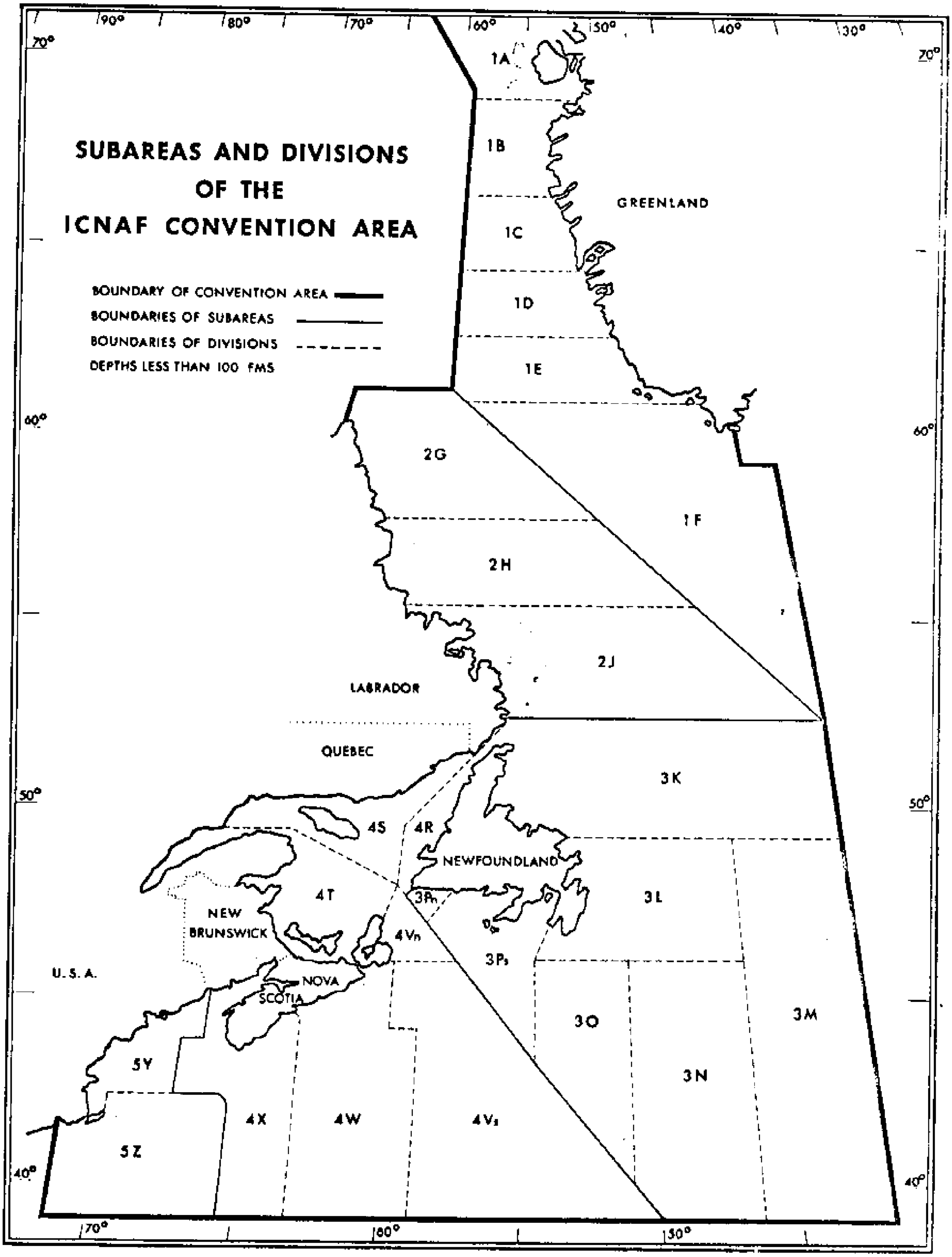

Figure 6A. Map of the area of the International Commission for the Northwest Atlantic Fisheries.

Table 6A. Catches of cod in the Northwest Atlantic (thousand metric tons round fresh) (from ICNAF Statistical Bulletin).

	Year																
	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968
Canada (M)	133	103	109	108	133	130	123	124	108	103	114	112	112	124	120	110	122
Canada (N)	219	189	246	207	220	222	165	232	228	183	206	222	204	190	188	176	201
Denmark	68	57	50	56	53	60	73	73	87	96	132	115	109	104	108	107	84
France	171	141	156	140	116	122	122	131	145	172	161	118	155	135	146	153	172
Germany, Fed. Rep.	2	3	2	7	29	11	31	21	37	99	126	140	101	152	154	172	187
Iceland	64	18	3	9	9	10	10	3	6	12	1	5	3	6	4	ϕ	ϕ
Italy	11	13	12	10	9	7	3	5	2	3	1	–	–	–	–	–	–
Norway	28	39	49	43	42	36	43	31	36	46	34	37	41	40	42	59	59
Poland	–	–	–	–	–	–	–	–	–	1	4	8	11	22	37	58	91
Portugal	161	196	195	205	225	205	179	160	185	197	218	231	210	197	202	237	219
Spain	82	98	112	96	110	110	100	124	158	197	197	209	219	225	232	280	329
USSR	–	–	–	–	3	18	6	16	103	158	101	82	129	149	110	165	246
UK	59	34	19	6	3	12	11	16	22	18	25	39	47	52	55	77	46
USA	19	15	16	15	15	15	17	18	16	19	20	18	17	16	17	20	22
Non-m [illegible]	–	–	–	–	–	–	1	–	1	–	–	–	44	51	62	71	82
Total	**1 017**	**906**	**969**	**902**	**967**	**958**	**884**	**964**	**1 134**	**1 304**	**1 340**	**1 336**	**1 402**	**1 463**	**1 477**	**1 685**	**1 860**
Subarea 1	294	242	302	265	321	269	320	234	243	345	451	406	350	359	366	430	382
Subarea 2	61	129	22	26	34	32	40	60	188	265	255	216	213	333	338	298	449
Subarea 3	328	352	472	429	392	449	294	425	471	461	289	406	581	498	499	721	733
Subarea 4	132	159	149	160	198	188	214	214	218	212	219	218	229	225	215	194	247
Subarea 5	14	11	12	12	13	13	16	16	14	18	26	30	29	42	57	42	49
Subarea NK	188	13	12	10	9	7	–	5	–	3	–	–	–	6	2	–	–
Total	**1 017**	**906**	**969**	**902**	**967**	**958**	**884**	**954**	**1 134**	**1 304**	**1 340**	**1 336**	**1 402**	**1 463**	**1 477**	**1 685**	**1 860**

Table 6B. Catches of haddock in the Northwest Atlantic (thousand metric tons round fresh) (from ICNAF Statistical Bulletin).

	Year																
	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968
Canada (M)	30	32	40	42	49	47	40	46	38	42	41	42	52	47	59	54	48
Canada (N)	5	8	24	29	35	25	17	16	14	22	21	7	5	3	2	2	1
France	–	–	2	3	4	4	3	4	3	5	3	1	1	1	1	ϕ	ϕ
Spain	40	28	20	57	32	30	20	12	13	8	7	11	7	7	6	7	10
USSR	–	–	–	–	–	–	–	–	37	40	5	7	13	129	73	8	3
USA	78	66	74	64	73	64	57	51	54	61	61	56	60	61	60	45	32
Oth-m	ϕ	1	2	3	1	1	1	ϕ	ϕ	1	ϕ	2	4	1	2	1	3
Total	**153**	**135**	**162**	**198**	**194**	**171**	**138**	**129**	**159**	**179**	**138**	**126**	**142**	**249**	**203**	**117**	**97**
Subarea 1	ϕ	ϕ	–	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ	1	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ
Subarea 2	–	–	–	–	–	–	ϕ	ϕ	–	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ	ϕ
Subarea 3	32	43	56	104	84	68	44	35	67	79	35	15	12	9	10	11	7
Subarea 4	55	45	51	43	51	48	49	53	46	47	44	51	60	85	66	49	46
Subarea 5	51	47	55	51	59	55	45	41	46	52	59	60	70	155	127	57	41
Subarea NK	15	–	–	–	–	–	–	–	–	ϕ	–	–	–	–	–	–	–
Total	**153**	**135**	**162**	**198**	**194**	**171**	**138**	**129**	**159**	**179**	**138**	**126**	**142**	**249**	**203**	**117**	**97**

increase has been in the catch by new countries, such as USSR, Poland, and Germany.

Estimates of the total fishing effort on the cod stocks in the area have not been explicitly calculated, but there has probably been a very rapid buildup in the fishing effort in this area compared with a reasonably stable fishery at a moderately high level going well back into history. The catches from Greenland (subarea 1) and the old traditional fishery at Newfoundland (subarea 3) have remained fairly stable but there was a jump in 1967, 1968 in the Newfoundland catches, when a large number of factory trawlers moved in onto a strong year-class. In contrast to this is the fishery in Labrador, which was a rather small-scale inshore fishery until about 1960, when the offshore trawling built up rather steadily.

The bottom figure shows the similar tabulations for haddock, and in particular the long-established U.S. fishery on haddock in subarea 5, which ran along at about 50 thousand tons rather steadily for a long period. The international body responsible for the fisheries in the Northwest Atlantic, ICNAF, is one of the best known and most active of the Atlantic type commissions, which are very small organizations. Its permanent staff consists only of the executive secretary and some secretarial and similar assistance, and most of the work is carried out at the national level by national research laboratories. These research laboratories have worked in very close collaboration, and most of the studies and assessments of what is happening to these fisheries have been carried out by groups set up by ICNAF and composed of members of national laboratories working together and pooling as best they can the data available to them. The work of ICNAF has been divided for many purposes into 5 panels, corresponding to the five areas, which are responsible for reviewing the scientific research, and particularly for recommending to countries any regulations that appear necessary. The other important part of the organization of ICNAF is its Research and Statistics Committee, which is a group of scientists meeting together away from the administrators to discuss the purely scientific problems arising in the area.

The problem to be considered here is rather different from the problems discussed up to now. These have been concerned with changes in the abundance of stock and in the catches following variations in the total amount of fishing, that is, considering the general yield isopleth diagram such as that shown in Figure 3D and changes parallel to the x-axis. These are perhaps the most important changes, having direct effects on the economics, the costs, and the whole structure of the industry. To be discussed here are changes in the size at first capture, that is, changes in the yield isopleth diagram along the y-axis. If there were complete information available to draw up this sort of diagram, i.e. good estimates of growth, mortality, etc., there would be no special problem. But this, of course, is not so. We rarely have in any fishery as much information as we would like.

The first consideration given in the Northwest Atlantic to possible changes in the size of fish capture was in relation to the haddock fishery in Georges Bank. This is almost entirely a U.S. fishery carried out from New England, and it had been a trawl fishery using a small mesh and with a relatively small size at first capture. For some time, it had been realized that there would be benefits from increasing the size at first capture and allowing the small fish to grow. In fact, one of the reasons for setting up ICNAF was the need to make the regulation of this fishery an international affair. Although it was mainly a U.S. fishery, there were a few Canadian fishermen and in all, or virtually all fishery regulations, if there is to be effective management, it must apply to everyone. If the U.S. wanted a mesh regulation to increase the size at first capture on this Georges Bank haddock, it was necessary that the Canadians play the game too. In principle, there are a number of ways of increasing the size at first capture, but much the simplest way in any trawl fishery is to increase the mesh size of the nets in use. This will allow the small fish to get through and keep the big fish. Then the small fish will grow and later they will be caught.

The effects of using a large mesh in the haddock fishery are shown in Figure 6B. The full line shows the length composition of the catches taken with the small mesh (2 7/8 inches stretched measure). These catches include the shaded portion, which are fish that were so small they weren't worth bringing back to port and marketing, but were discarded at sea. Obviously there was no point at all in catching these, and any measure that allowed them to escape would be bound to be beneficial. The dotted line shows the size composition taken by trawlers using the larger mesh (4 1/2 inches), and we see that virtually none of the fish caught with the large mesh were discarded. There was also some dropoff in the smaller sizes of marketable fish taken when the larger mesh was used so that these fishermen lost some small fish that they otherwise would have taken. In the long run, these should appear again in the catches and give the theoretical benefit from the mesh regulations.

In addition there is an extra benefit that is not part of the theoretical benefit coming from increasing the size of the first capture but is a mechanical benefit because a large mesh net is often more efficient than a small mesh net. It can be towed faster, and there is a better flow of water through the net.

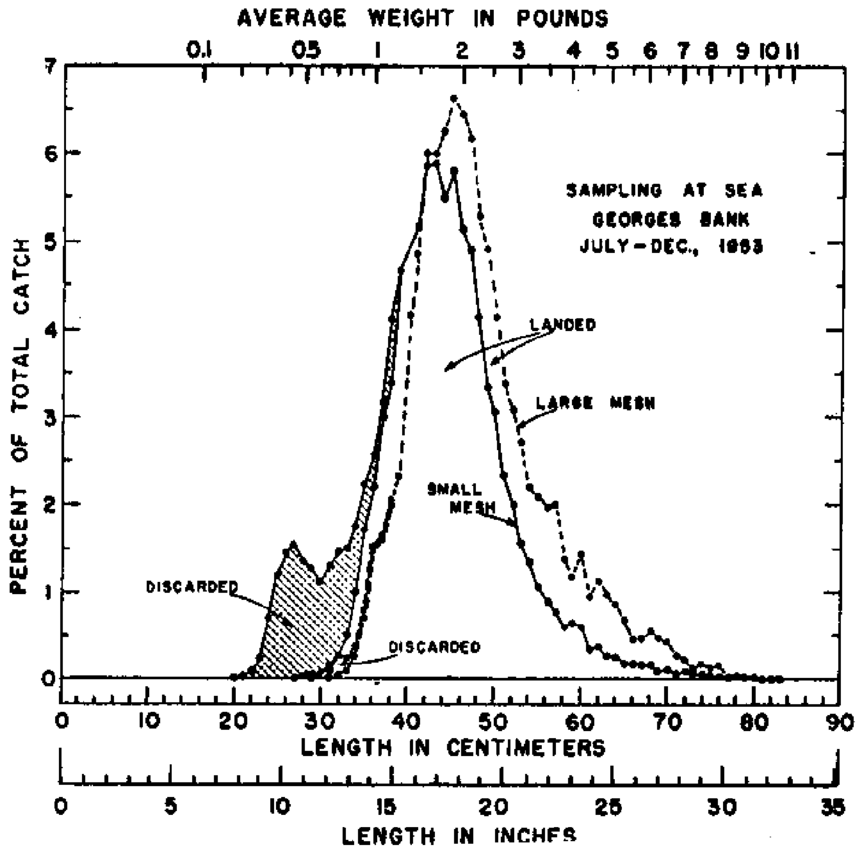

Figure 6B. Immediate effect on catches and landings of the use of a larger mesh size in the Georges Bank haddock fishery (from Graham, 1954).

Figure 6B, which compares the catches of trawlers using large or small meshes during the period soon after the large mesh was introduced, shows that the large mesh caught more large fish than the small mesh. The increase we would expect from the population dynamics analysis will not occur until the small fish that are released by the larger mesh have had time to grow and to increase the numbers of medium to large fish in the population. The catches of these fish by large and small-meshed nets should then increase equally. The benefit gained from using a large mesh in the haddock fishery could be calculated fairly readily because this was a well-studied fishery with statistics going back a long time and with good information on size composition, mortality rates etc. The question then arose for the other fisheries in the ICNAF area as to whether too small a mesh was being used and whether regulations should be set to increase it. The tendency of most fishermen is to use a small mesh because a they can see the small fish escaping and to see a fish escaping through the net while it is in the water always upsets a fisherman, especially because a fish in the water looks bigger than it really is. This discourages the use of a large mesh and tendency is to use a smaller mesh than is really the most effective in the fishery. This concern about the fish that escape through the meshes is increased by the fact that the selection of fish by the meshes of a trawl is not an abrupt knife-edged selection such that all fish below a certain size get through and all fish above that size are kept. There is a rather wide range.

Figure 6C shows two typical selection curves, in which the proportion of fish retained by the net is plotted against the length of fish. While for the net without chafer some fish as small as 40 cm long are retained, some as large as 65 cm will escape. Over this quite wide range, some fish get through and some fish are kept in the nets. These observations on which Figure 6C

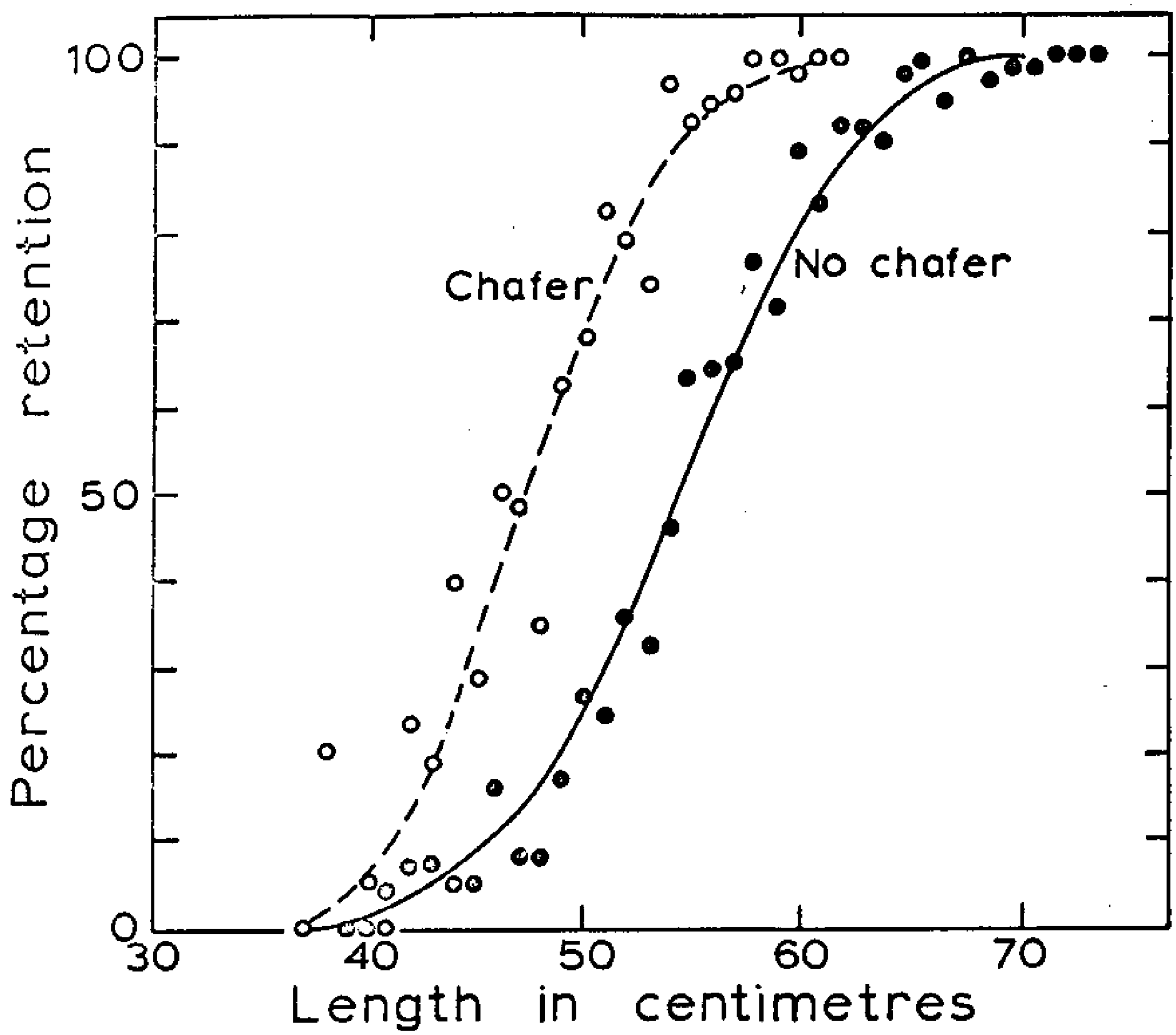

Figure 6C. Selection curves for cod, using a trawl with and without a chafer (from ICES, 1966).

was based were obtained by placing a large cover of small-meshed netting over the back of the cod-end and seeing what fish get through. The numbers retained in the cod-end can then be expressed as a percentage of the total numbers of fish that entered the net, i.e., the sum of those retained in the cod-end and in this cover over the back. This isn't an entirely satisfactory technique because the presence of the cover must change the flow of water through the net and thus affect selection to some extent. However, comparison of results obtained by fishing two boats side by side, one with a small mesh and one with a large mesh, shows that the covered cod-end technique gives a reasonably good measure of the selection.

Figure 6C also shows the effect of using a chafer to cover the top of the nets. These chafers are put on by the fishermen to cut down the wear and tear, particularly when the nets are hauled either up the stern ramp or over the side of the ship. With a chafer in place, the selection curve is moved to the left, and many more small fish are retained.

Turning to methods of assessing the effects of using a large mesh, other than by computation of mortalities, growth and recruitment, the simplest method has been set out by Allen (1953). Considering the desirability of releasing fish of weight W_c, this would be a good idea if the total weight of those fish which are ultimately caught were greater than the weight if the small fish released. The expected weight of those released, when ultimately caught, will be the product of the probability of being caught, E, and their average weight, which will be equal to the average weight in the catch, $\bar{W}$. The condition for the release of fish W_c to increase the catch is therefore $E\bar{W} > W_c$. If $E\bar{W} < W_c$, then we better keep these fish, because if we let them go, the survivors caught will not in total have as great a weight. The inequality could, if wished, be expressed in terms of value, which might often be a good idea because small fish often are less valuable per pound than bigger fish. The great advantage of this expression is that it makes no assumptions as to the form of the fishing mortality rate. It just assumes that once small fish are released, they are typical members of the population and if caught their average weight will be the same as that of the rest of the catch.

This inequality provides guidance as to whether or not we should increase the size at first capture, but clearly we would also like to know what the quantitative effects would be. To do this, the expressions can be modified to take into account the numbers of fish released. The immediate loss, or the weight of fish released, will be given by $W_R = N_R w_c$, where N_R is the number of fish of weight w_c which are released. The weight of the fish caught later will be $EN_R\bar{w}$, and the net long-term gain will be $N_R w_c - EN_P\bar{w}$. To apply this to an actual situation, it is necessary to consider the range of sizes of the fish released. Let us write

$N_1 = \sum n_{1,\ell}$ = numbers of fish in the original catches, summed over all length groups.

and $W_1 = \sum n_{1,\ell} w_\ell$ = weight caught with the small mesh, where w_ℓ = mean weight of fish length ℓ.

Immediately after increasing the mesh size, the numbers of fish caught will be given by

$$N_K = \sum n_{1,\ell} \frac{r_{2,\ell}}{r_{1,\ell}}$$

where $r_{2,\ell}$, $r_{1,\ell}$ are the proportion of fish of length ℓ which are retained by the large and small meshes, respectively. The weight in the catches immediately after the introduction of the larger mesh will be given by

$$W_K = \sum n_{1,\ell} \frac{r_{2,\ell}}{r_{1,\ell}} w_\ell$$

We can get an expression for the loss, L, that is the immediate drop in catches by using a larger mesh, as a proportion of the initial catch.

$$L = \frac{W_1 - W_K}{W_1} \quad \text{which}$$

is probably an overestimate of what the fishermen would actually lose because of the increased catching power of a larger mesh.

The number of fish we release, N_R, will be given by $N_R = \sum n_{1,\ell}(1 - \frac{r_{2,\ell}}{r_{1,\ell}})$

and of these a number EN_R will ultimately be caught later.

This will increase the catches taken with the larger mesh approximately by a proportion $\frac{EN_R}{N_K}$ where N_K is the number being taken with the larger mesh immediately after the change, before any of the fish have grown and appear in the large mesh catches. More precisely the long-term increase in the catches with the large mesh compared with these immediately after the change will be given by

$$Q = \frac{EN_R \, e^{-M\Delta t}}{N_K}$$

The term $e^{-M\Delta t}$ is included because if we are considering a very substantial increase in mesh size, there will be a period during which the released fish are still not really accessible to the larger mesh, but will suffer some natural mortality so we should reduce this number that are released for the natural mortality in this period Δt between the time when they are released from the small mesh and the time that they become available to the larger mesh. The long-term catches with the larger mesh will therefore be equal to the W_2 where

$$W_2 = W_1 \ (1-L) \ (1+Q)$$

One of the powerful things about this expression is that it can be readily applied to complex fisheries where there are a large number of gears operating or where there are appreciable national differences between the sizes of fish taken even with the same type of gears. For these fisheries, we could do the initial calculations of N_1 N_K N_R and L for each fishery separately. The numbers released for each fishery are added together to get the total numbers released, and this total is used to calculate a single value of Q for all sections of the fishery.

Another thing that can easily be done in this formulation is to calculate the interim effects. There will be a period between the time we increase the mesh, and the time that expression for the long-term effort holds good during which the fish we release are growing up through the fishery. The long-term situation will not strictly apply until all fish in the fishery have been exposed to the larger mesh for all their lives, and this can be seven or eight years for many of the cod stocks in the Northwest Atlantic. It is rather important if we are telling fishermen or administration that something will happen in the long-term and the long-term is six or seven years into the future, that information be given also about events in the interim period. Quantitatively it can be stated that, because most of the fish are young fish whose catches will soon reach the long-term state, the overall catches during the interim period will be more like the long-term situation than the situation immediately after the mesh change.

But we can put this into quantitative terms. Again by summing for different length groups we can say that at any interim time the first fish released by using the larger mesh will have reached a length l_T. Catches of fish larger than this will still be the same as immediately after the mesh change, while catches of fish smaller than l_T will have increased to the long-term situation. That is, the interim catches are given by $C_T = \sum_{l=0}^{\ell_T} (1+Q)\, n_{K\ell} w_\ell + \sum_{\ell=\ell_T}^{\infty} n_{K\ell} w_\ell$

This approach has proved very useful in ICNAF and indeed in many other similar areas, because all that is required is an estimation of the selectivity of the trawls being used, data on the size composition of the catches in the different fisheries, and some estimate of E. This last is the trick question. It is not easy to get estimate E; but quite often we can estimate the effects of a mesh change for a range of values of E, and within the likely range of E the answer we get about whether or not we should increase mesh size is the same.

Table 6C shows the results of applying the method to the cod at West Greenland. This shows the estimated effects of changing from the mesh size generally in use in 1960-- 4 1/4 inches--up to 4 1/2,-5,-5 1/2-or 6-inch mesh. The first significant calculation is the immediate loss, that is the proportion of the trawl catches released by the larger mesh size. Assuming that we have good data on the sizes of fish being caught, the calculation involves no assumptions concerning the effects on the stock. For example, if the mesh size were increased up to 5 inches, the trawlers from Portugal, Spain, and France, which catch more small fish than those from other countries, would lose 4% of their catches by weight, the English would lose 2% and the German trawlers, which fish mainly on large mature fish, would lose only 0.08%.

The fisheries with hook and line, of course, wouldn't be affected by changing the mesh size of trawls and the catches in the fishery as a whole would decrease by 1.7% immediately following the increase in mesh size. The long-term effects were calculated for the range of values of E, varying from 0.4, to 0.6 corresponding to possible divisions of the reasonably well-estimated total mortality in this period of about 0.35, between natural mortality and fishing mortality.

Provided the true value E in fact lies in this range, the total catch is bound to increase following an increase in mesh size, as would the catches by hook and line. German fishermen would also be bound to benefit, as would

Table 6C. Assessments of the effect on the fisheries for cod at West Greenland of changes in the trawl mesh size.

Mesh size change inches	l_c cm	t_c yr	Gear groups	Percentage change in 1957—58 landings: Immediate	Long-term change for			
					0.4	0.5	0.6	E
					0.14	0.175	0.21	F
					0.21	0.175	0.14	M
From 4¼ to	52	5.2						
4½	53	5.3	Trawl a	— 0.5	— 0.3	— 0.1	+ 0.1	
			Trawl b	— 0.2	+ 0.3	+ 0.5	+ 0.6	
			Trawl c	0	+ 0.3	+ 0.5	+ 0.6	
			Trawl d*	— 0.3	0	— 0.1	+ 0.2	
			Line	0	+ 0.4	+ 0.5	+ 0.8	
			Total	— 0.2	+ 0.1	+ 0.2	+ 0.3	
5	54	5.4	Trawl a	— 4	— 1.8	— 1.1	— 0.4	
			Trawl b	— 2	+ 0.5	+ 1.2	+ 1.8	
			Trawl c	— 0.8	+ 1.6	+ 2.3	+ 2.9	
			Trawl d*	— 3.2	— 1.0	— 0.4	+ 0.2	
			Line	0	+ 2.3	+ 3.0	+ 3.5	
			Total	— 1.7	+ 0.5	+ 1.2	+ 1.8	
5½	55	5.5	Trawl a	— 9.7	— 4.6	— 3.3	— 2.0	
			Trawl b	— 5.3	— 0.7	+ 0.6	+ 2.0	
			Trawl c	— 2.8	+ 2.5	+ 3.0	+ 5.3	
			Trawl d*	— 8.3	— 3.1	— 1.8	— 0.5	
			Line	0	+ 5.3	+6.6	+ 8.0	
			Total	— 4.4	+ 0.9	+ 2.2	+ 3.6	
6	56	5.7	Trawl a	— 18.0	— 8.7	— 6.6	— 4.6	
			Trawl b	— 11.0	— 2.7	— 0.6	+ 1.7	
			Trawl c	— 5.5	+ 3.3	+ 5.5	+ 7.8	
			Trawl d*	— 14.0	— 6.0	— 4.0	— 1.9	
			Line	0	+ 9.5	+ 11.5	+ 13.6	
			Total	— 7.3	+ 1.3	+ 3.4	+ 5.6	

a = Portugal, Spain, France; b = UK; c = Germany; d = Norway, Denmark, Iceland. * = Estimated.

the English; but it is likely that the trawl fishery by Portugal, Spain, and France would lose a little because they capture relatively small fish. The other thing is that these numbers look rather small in terms of percentage of change — only up to perhaps a 5% increase. However, the total catches have been around 400 thousand tons, and 5% of that is 20 thousand tons, worth a couple of hundred dollars or more a ton. The value of the annual increase in catch therefore comes out to very much more than the total expenses of the research including operation of the commission. It may not be a big thing, but at least it is a cost-effective operation.

However, mesh regulation is not tackling the real problems of the fishery that exist now and will increase even more in the future, the imbalance between the amount of fishing needed for effective harvesting of the stock and the amount of fishing that actually goes on.

This is rather a difficult problem to tackle, since the adjustment of the total amount of fishing requires direct interference with the activities of the fishermen, telling some or all of them that, for all or part of the season,they will not be allowed to fish. This is difficult enough in a single-nation fishery,but becomes even more difficult for an international fishery, where countries have very different interests. There has been, therefore, in the North Atlantic at least in the past concentration on the rather more tractable problem of mesh regulations. It has enabled the Commission to find their feet in operating regulations in the international fishery. Though control of the mesh size used is less difficult than controlling fishing effort, since it only involves telling fishermen to use a rather minor modification of his gear, it does in practice usually raise a number of difficulties, for example when there are a variety of different species being exploited in the same area (ICES, 1960). Enforcement of the regulations is a special problem in these international fisheries. Fishermen are willing to follow regulations only if they believe that all other fishermen are obeying them, and they are particularly suspicious of the activities of foregin fishermen. Enforcement of the regulations, therefore, requires some method of assuring fishermen in one country that those in other countries are obeying the regulations. This is now being achieved in the North Atlantic by a system of international inspection. Under this a duly authorized fishery inspection vessel of one country can stop a fishing vessel of any other and inspect its nets to see whether they are of the regulation size. If they are not, the facts are reported to the government of the fishing boat for suitable action.

Another problem of enforcement is illustrated on Figure 6D. The regulations are usually phrased to require that a gauge of the standard dimensions should pass "easily" through the meshes, without defining "easily." For scientific purposes, a more precise measurement is obtained by the use of a pressure gauge which extends the meshes diagonally with a fixed force, but the pressure-type gauge has been held to be unsuitable for legal proceedings in some countries. Figure 6D compares measurements with this pressure gauge and the wedge gauge used for enforcement. For meshes above the legal size (70 mm), the agreement is good, but when as shown by the pressure gauge the mesh size is less than 70 mm it is clear that the inspector has increased the pressure with which he inserts the gauge into the mesh, and hence has obtained a legal size.

Despite these difficulties, effective control of mesh size is coming into operation in the major bottom-fish trawl fisheries on both sides of the North Atlantic. The problem now is to move on to tackle the more difficult task of controlling the amount of fishing.

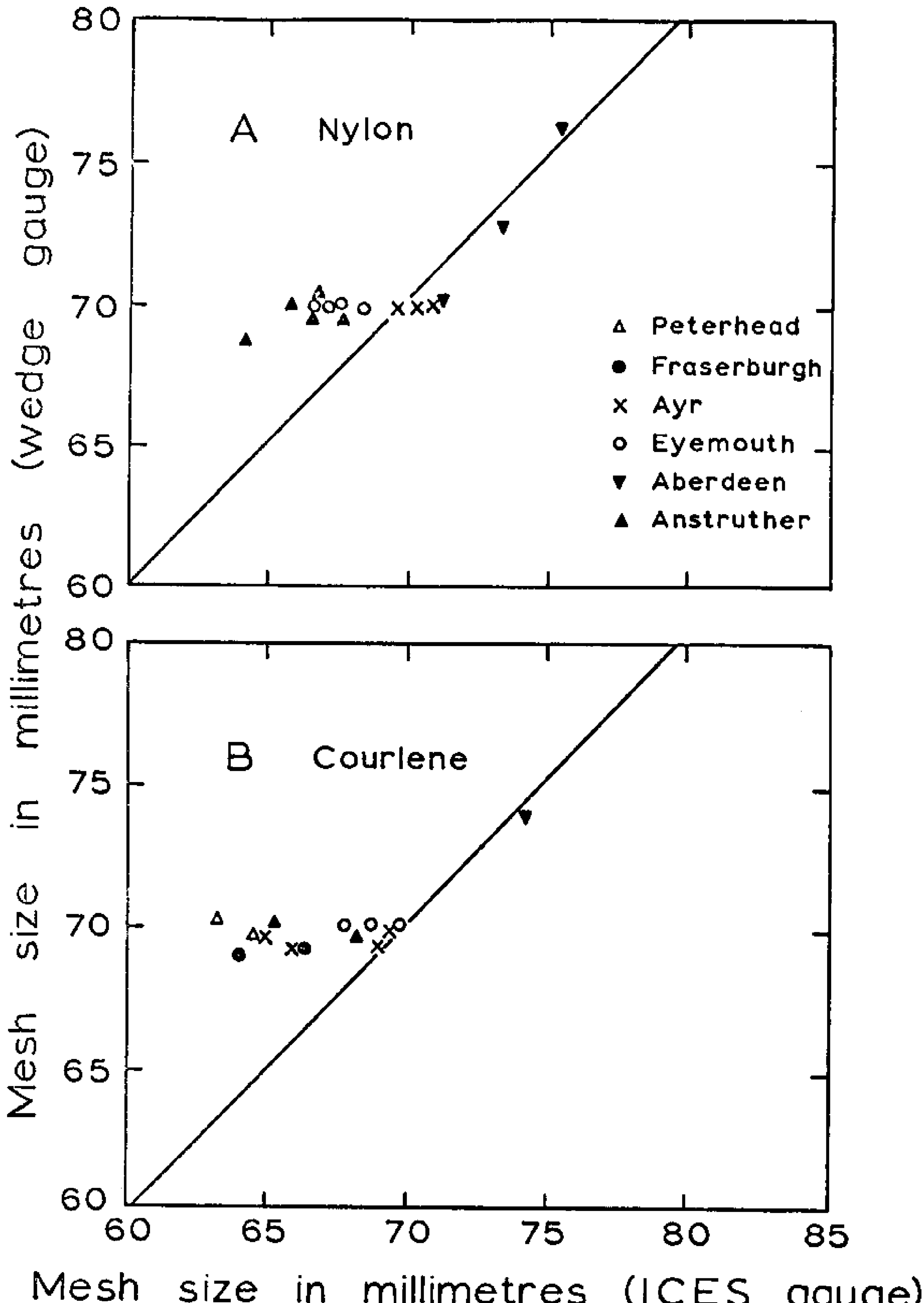

Figure 6D. Comparison of mesh measurements made with a wedge gauge and a pressure gauge (from ICES, 1966).

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Chapter 7

NORTH SEA HERRING: EFFECT OF JUVENILE FISHERIES; ANALYSIS OF TAGGING DATA

The last three chapters have been concerned with cod fisheries in different parts of the North Atlantic. We will now turn our attention to the other major species in the North Atlantic, namely the herring. The herring fishery goes back some 2,000 years and over much of this period has been very important to many parts of northern Europe. Between the 12th and the 16th century, the Hanseatic League dominated economic affairs in northern Europe and around the Baltic, and many of its activities were based on the herring. Some historians have even suggested that the decline of the Hanseatic League can be associated with changes in the movements of herring, and particularly the failure of the herring to migrate into the Baltic.

One of the features of herring and of other clupeoid species has been large fluctuations taking place over periods of a century or so. There have been substantial changes in the clupeoid populations during the last few years, some of which are obviously due to fishing. For others the situation is less clear, and one of the more difficult and important problems in fishery research is to determine the extent to which these fluctuations are natural, or due to fishing. These changes make herring stocks more difficult to deal with than those of the cod and similar demersal fish for which changes, other than those due to fishing, are more often relatively minor.

There are large numbers of herring stocks in the North Atlantic and for the moment we will concentrate on the group of herring living in the North Sea, which has long supported herring fisheries. For a long time this herring fishery was the source of rivalry between the countries bordering the North Sea, particularly between England and Holland during the 17th century. Within the North Sea, there are a number of separate stocks of herring. The identification and separation of stocks is an important early step in any fishery investigation, and has been a major problem in North Sea herring research. Any analysis of a fishery must take account of whether this fishery is exploiting a single stock, or a number of stocks, or only part of a stock. If it is exploiting several stocks, we need to consider whether the different stocks are behaving differently, while if the range of the stock extends outside the area of the fishery being analyzed, it will be necessary to look at the events outside the fishery and take them into account when making the analysis.

In the North Sea, three spawning groups of fish have been identified, and their distribution is shown in Figure 7A. The so-called Downs group spawns at the entrance to the English Channel, the Dogger group in the central North Sea, and the Buchan in the north. In addition to these local stocks, the very large Atlanto-Scandian stock, which is a more northern stock spawning off the Norwegian coast and migrating to and fro between Norway and Iceland, enters the northeastern North Sea along the Norwegian coast. The big problem in the North Sea is the mixing between these various stocks.

The main spawning areas, shown as dotted in Figure 7A are quite distinct, as, to a lesser extent are the main nursery areas for young fish cross-hatched. The young herring from this Downs spawning drift north and northeastwards in the same current system as the young of the North Sea plaice do, to arrive in similar nursery areas off the Dutch and Danish coasts until at about 3, they join the adult stock and move into the main feeding area in the north-western North Sea, as shown by the diagonal stripes. In this feeding area, there is a mixture of fish from different spawning groups, and it is this mixture that often causes difficulty. Figure 7B shows the distribution of the major herring fisheries during 3 periods of the present century-before 1914, between 1919 and 1939, and from 1946 up to about 1960. Before the First World War,

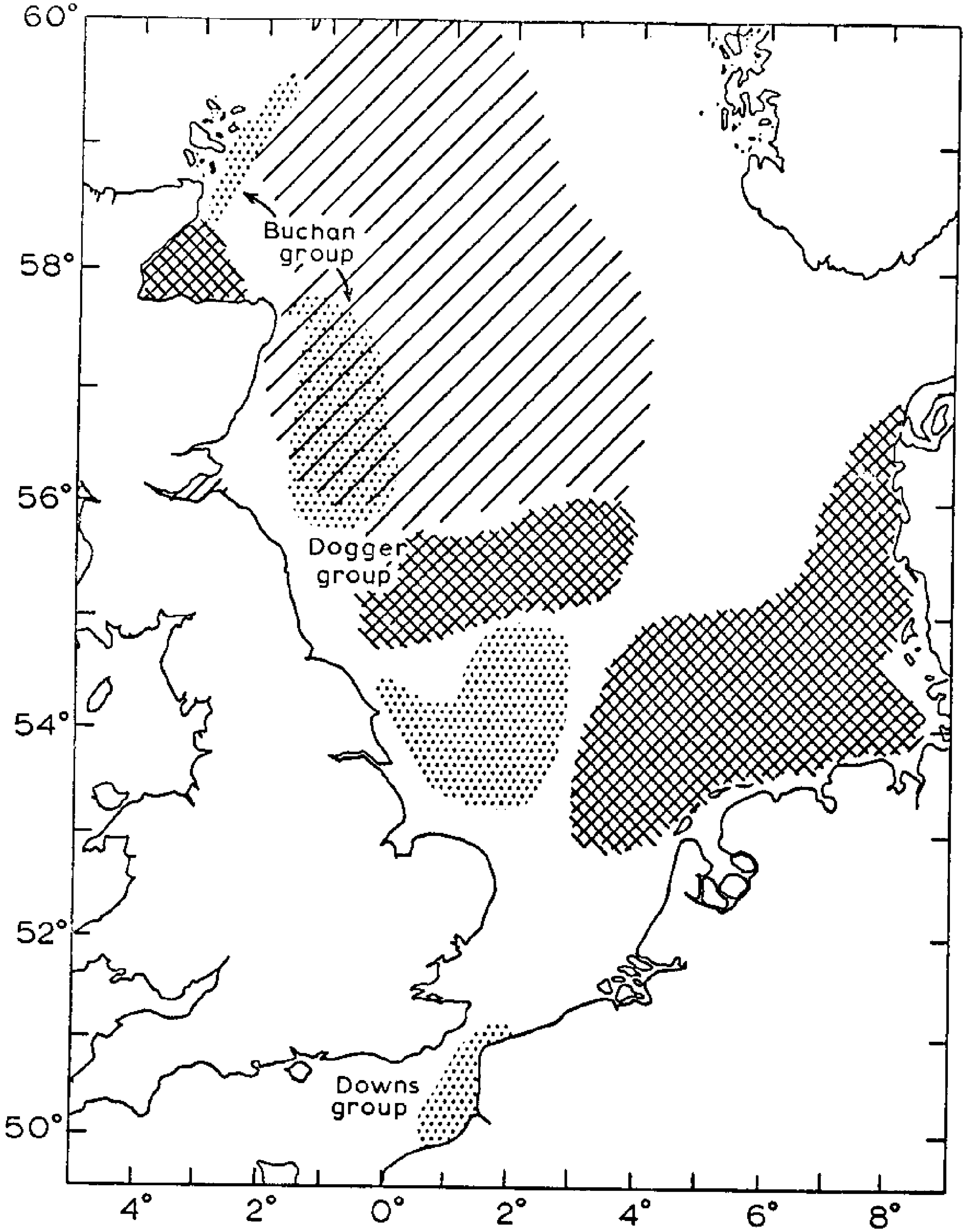

Figure 7A. Location of feeding, spawning, and nursery areas of North Sea herring (from ICES, 1966).

the herring fisheries were almost entirely carried out by drift nets, that is, long lines of gill nets several miles long, in three main areas-the so-called East Anglian fishery in the autumn, which is on the Downs fish as they are moving south to spawn in the English Channel, and two summer fisheries, on the Buchan grounds and off Shetland. After 1920, the drift net fisheries remained much the same, but in addition a trawl fishery on concentrations of herring on the bottom built up in two areas -- on the Dogger area in the late summer and on the Fladen earlier in the summer. Then after the last war, from 1946 onwards, the situation became even more complex. Again there were still the same drift net fisheries and the prewar trawl fisheries, but a new development was the so-called Bloden industrial fishery, on small herring before joining the adult stock. These were used entirely for processing into meal and oil. This fishery grew up in the nursery area between the Dogger Bank and Denmark.

Since 1960 there have been further changes, not shown in the diagram, especially the introduction of the modern purse seines with power blocks. These purse seiners, mainly from Norway, have quite revolutionized the North Sea herring fishery, and the situation which was originally complicated has become even more complicated. However, we will consider only the events up to about 1960.

Figure 7C shows the trends in catches during this period. The total catch (full line) has fluctuated from year to year, and tended to increase during each period, (before World War I, between wars, and since 1945). The peak catches in each period have been much the same, about 500,000 to 700,000 tons. Since 1960 we have had the purse seines coming in and the catches have tended to increase rather rapidly. By 1965 they had gone up to one and one-half million tons. Then the stocks declined and the catches went down to about 800,000

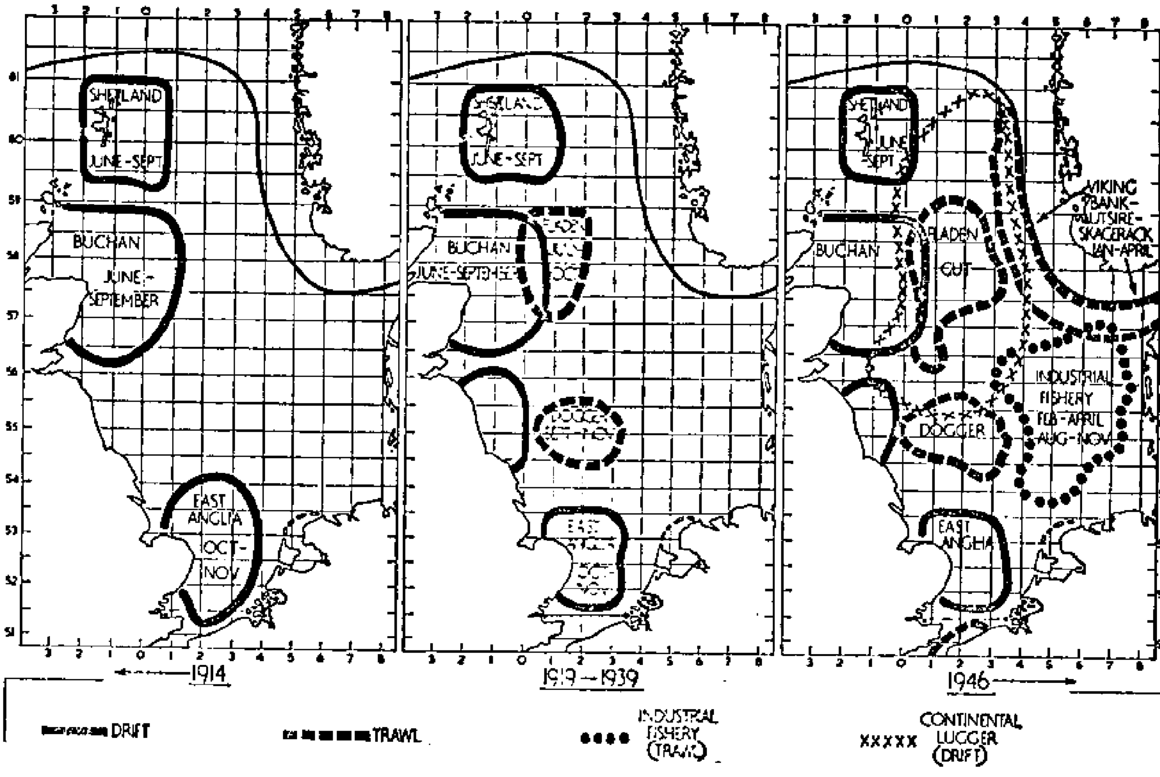

Figure 7B. Changes in the distribution of herring fisheries in the North Sea (from Parrish and Saville, 1967).

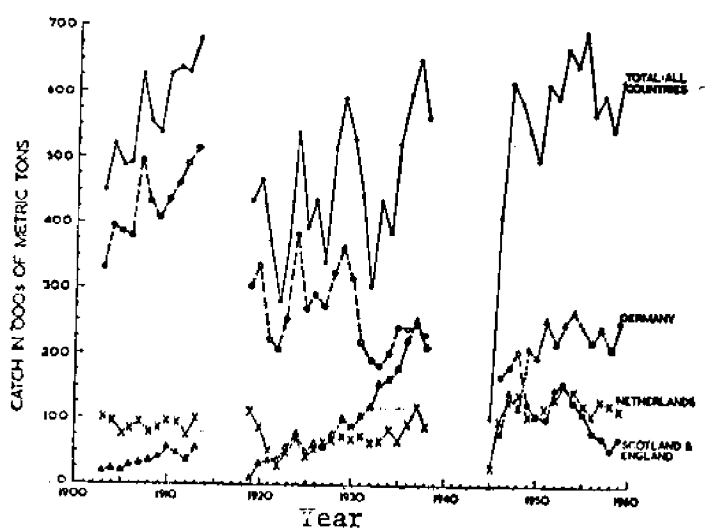

Figure 7C. Trends in total catch of herring from the North Sea (from Parrish and Saville, 1967).

in 1968 and .even lower since then.

The figure also shows separately the catches by the three main herring fishing countries during this period, United Kingdon, Germany, and the Netherlands. Up to 1913 the United Kingdom was by far the major country. This fishery was mainly for salt herring to export, especially to eastern Europe. After World War I, particularly after the Russian Revolution, this market failed and the British share tended to decrease. At the same time, the German share increased rather sharply between the wars with the introduction of herring trawling, which was mainly a German fishery. During this whole period, the Dutch share tended to increase,though less strikingly.

In a complex fishery like this, it is difficult to calculate the total effort, or the catch per unit effort of the fishery as a whole, and in any case because of the mixture of different stocks, the latter may not be a very meaningful statistic. It is better to look at catch per unit effort in a single part of the fishery, and if possible in a part of the fishery that exploits a single uniform stock. Such a fishery is that in the southern North Sea on the Downs herring on their way to spawn. The catch per unit effort in this fishery is shown in Figure 7D. For some years after the war, frcm 1947 until 1953, there was a period of fairly constant catch per unit effort; but after 1953, there was a decline in both catch per unit effort and in total catch. We have to be a bit careful about what this decline in total catch means. In Figure 7D we have data showing total catch taken in the southern North Sea, which are pure Downs stock. But in addition to this catch, fish from the Downs stock are taken during the summer feeding fisheries farther north in the North Sea where they are mixed with fish from other spawning stocks. It is difficult to estimate what precisely is happening to the total catch of this stock, and also what is the total effort. If we knew the total catch, it would be very easy, from the catch per unit effort in the southern North Sea, which we know is a catch per unit effort on the pure stock, to calculate the total effort, provided a reliable measure of effort is being used in the southern North Sea. The measure used for catch per unit effort is the catch per drift net per night, but it is possible that the efficiency of the drift nets has changed.

Both the type of nets in use and the same number of nets have remained about the same. However, it is likely that the boats are able to search for the fish more effectively or at least boats are interfering with each other less. During this period, and in fact during the whole period since 1913, there has been a steady decline in the numbers of ships operating in the East Anglian fishery. At the peak of the fishery in 1913 when there were over a thousand boats operating. All these boats were using several miles of drift net and were working in a fairly restricted area, and hence it is not difficult to imagine that there must have been some interference and competition for fish between these ships. As the numbers of ships decline, this interference gets less and the individual net becomes more effective. It is also likely that with the increasing use of radio and, later of echo sounders, they could concentrate on the fish much more effectively. When ships were very numerous, the later ships arriving on the grounds had to shoot their nets where there was space between the earlier arrivals, rather than where they believed fish were most abundant. So it is quite likely that the effort has become more effective and the true decline in catch per unit effort is greater than marked here. Whatever the extent, it is clear that the catch per unit effort in the southern North Sea has declined.

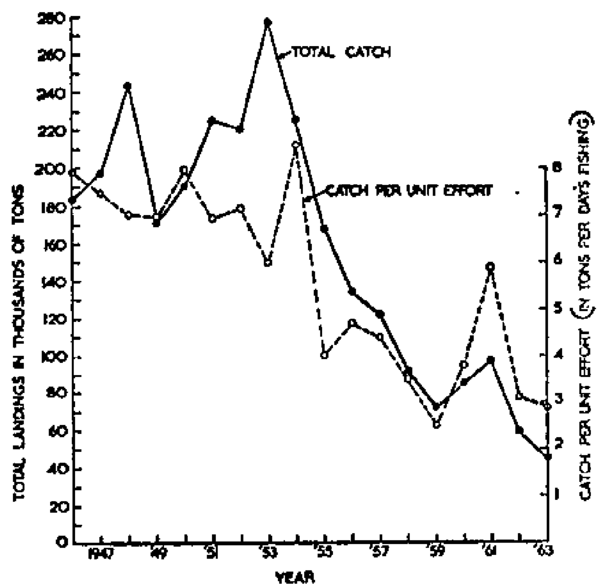

Figure 7D. Trends in catch and apparent catch per unit effort in the southern North Sea herring fisheries (from Parrish and Saville, 1967).

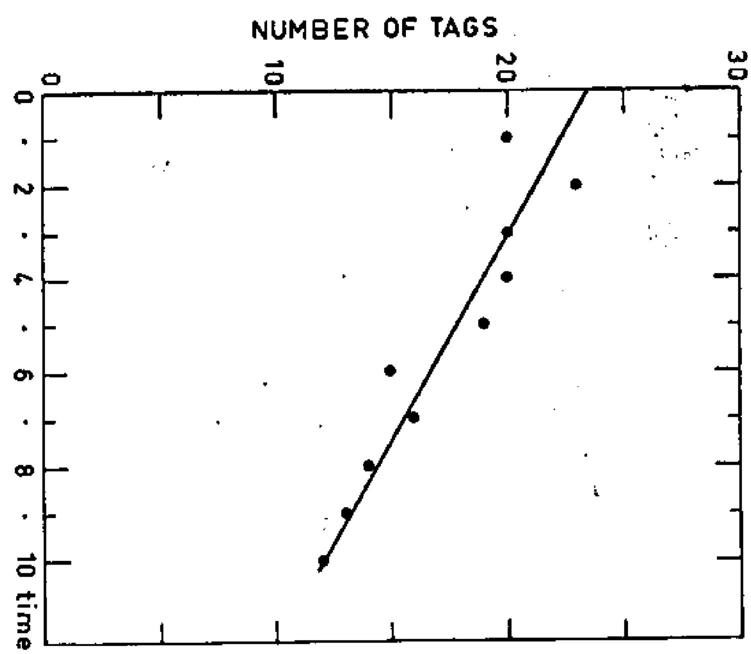

Figure 7F. Decline in the return rate of tagged herring with increasing delay between capture and tagging (from Aasen et al., 1961).

Another change in this fishery is illustrated in Figure 7E which shows the age composition of the fish caught in the East Anglian drift net fishery on the prespawning fish, and the Sandettie fishery on the spawning grounds. These two age compositions look very much the same for any given year, even though one can be quite sure that at least the drift net fishery, like any gill net fishery, must be to some degree selective. However, the range of sizes of these fish is not very great, and most of the fish are well within the selection range of the gear. The most obvious effect is that when there are lots of old fish, e.g., between 1951 and 1955, the trawls tend to get more of them than the drift nets. Presumably the older fish are too large to be fully vulnerable to the drift nets.

The other thing that this figure shows is the increasing steepness of the right-hand side and the decrease in old fish until by the 1956 to 1960 period, the last period shown here, the fishery was mainly on 3-year-old fish. This shows that there had been a very great increase in mortality in this stock, which cannot be immediately accounted for by increased fishing because the nominal fishing effort on herring in the southern part of the North Sea had not changed much and if anything had decreased. At least the nominal effort has decreased, and we would have to put in a large correction for increased efficiency to get anything except a decrease. But there has been increased fishing in the northern and central North Sea, on the feeding fish, which might account for an increased fishing mortality on this stock depending on how much of these catches are of the southern, Downs, stock.

The other thing about this diagram that was particularly upsetting to scientists at Lowestoft was that the ratio of 3-year-olds to 4-year-olds has changed. In the typical prewar and immediate postwar period, the 4-year-olds were more abundant than the 3-year-olds. Though it is not apparent in Figure 7E, which shows the average during 5-year periods, there are fluctuations in year-classes

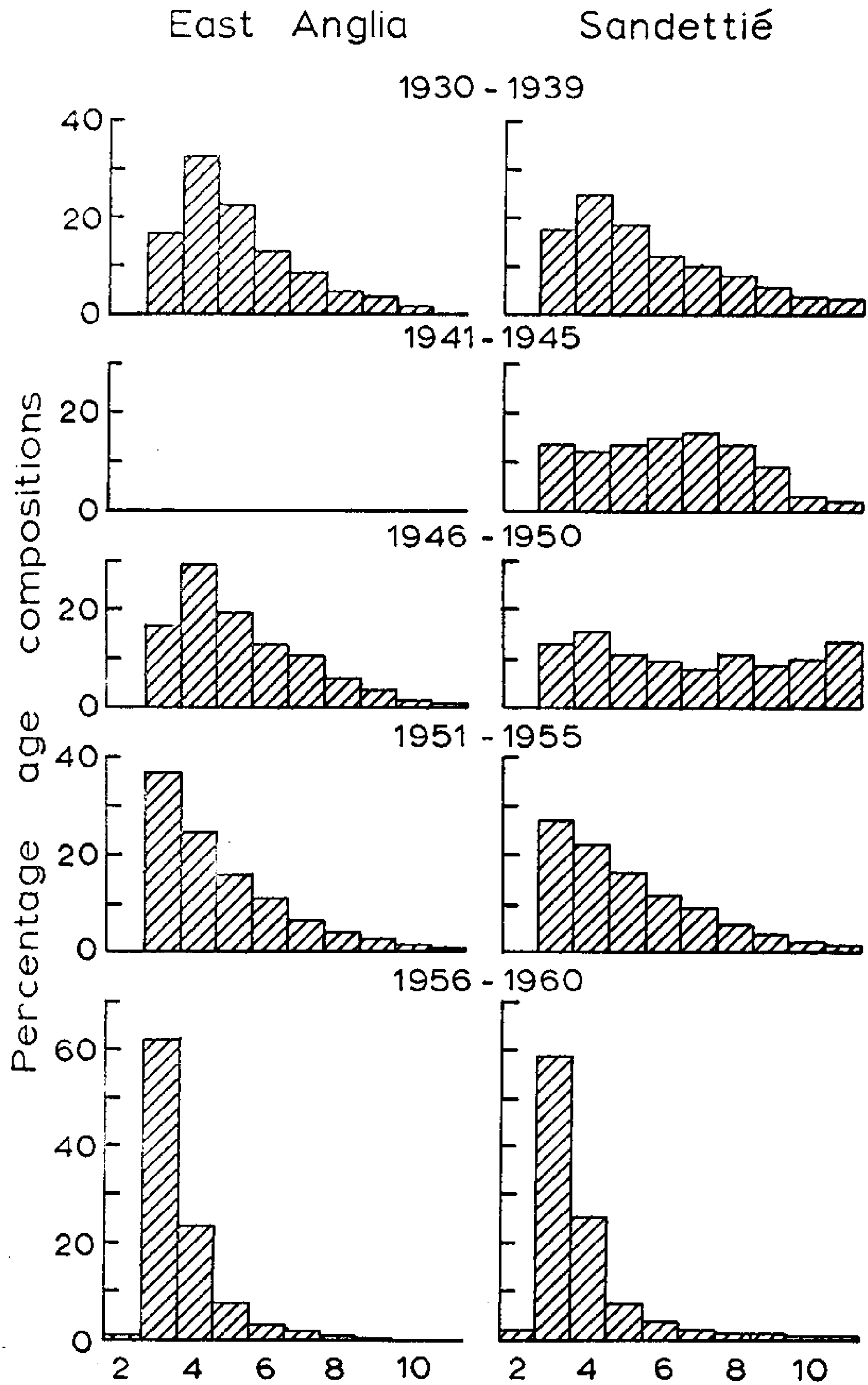

Figure 7E. Changes in age-composition of the herring catches in the southern North Sea (from ICES, 1966).

in this stock, though much less than in some other herring stocks. When the ratio of 3-year-olds to 4-year-olds in the following year was consistent as it had been in the 1930's, we could forecast how many 4-year-olds and 5-year-olds there would be in the fishery from the abundance of 3-year-olds and 4-year-olds of the previous year. This was of some significance to the fishery because the 3-year-olds and 4-year-olds come through at different times, with the 3-year-olds coming through earlier.

By knowing the age composition, we can have a forecast in rough terms of both the abundance and of the timing. If there is a good set of 4-year-olds and older fish coming through, it is going to be a good late fishery. If it is mostly 3-year-olds, it is going to be an early fishery. This change in the ratio between 3- and 4-year-olds upsets this forecasting method. In the most recent period, the large number of 3-year-olds suggested very good fishing on 4-year-olds, which did not occur.

One possible reason for the change in rates has been a growth change in the herring. Recently the herring have been growing faster and maturing earlier. Instead of only the bigger fish of a year-class maturing at 3 years old, and the rest at 4, now most if not all, mature at 3. Another factor quite strongly put forward by some people as being responsible for both the decline in catch per unit effort and the changing rates of 3- to 4-year-old fish was the new industrial fishery for juvenile herring on the so-called Bløden Ground. One method of determining the effect of this industrial fishery would be to estimate the proportion of the juvenile stock that is being taken, by a means of a tagging experiment. Tagging has always been a favorite occupation of fishery biologists. It is very easy to go out and tag some fish and the problem of using the information from the tagging experiment may not have to be faced for some years until all the tags come back. Mathematicians also have been playing around with tagging results and producing maximum likelihood or other estimates of population size or mortalities. For these reasons, there has been much literature on tagging, including fishery aspects (e.g., ICNAF, 1963) and statistical aspects (e.g., Cormack, 1968).

If we are going to tag, there are probably three things we wish to find out: one is estimates of population size and mortality; the second is estimates of migration and mixing; and the third is estimates of growth. In theory, a tagging experiment can show any or all of these. In practice, one can be rather doubtful whether it will show much. If we tag a fish, it will probably not behave the same way as an untagged fish. In particular it may not grow as fast as an untagged fish, and there is good evidence of differences in growth of tagged and untagged fish for a number of species. On the other hand, for other species, or with suitable tagging techniques, tagged and untagged fish may grow the same amount and we can get a good estimate of growth from tagging. The possible influence of the tag on the behavior of the fish also applies to the estimation of migration and mixing, but the main practical difficulty in the estimation of movement is that tagged fish will only be returned from areas where fish are being caught and any estimate of movement or mixing must include a correction for the probability of being caught in different areas.

The biggest use of tagging has always been to estimate mortality or population size and the basic theory of this is very simple. For example, to estimate the exploitation rate, E, the proportion of fish in the sea that we will at one time or another catch, we just tag 100 fish and if we get 36 back, the value of F is 0.36 and that is that. Alternatively, to estimate the population size, the simple Lincoln index or Petersen method can be used. This assumes that if some fish are caught after the tagging experiment, whether by commercial fishing or by research workers, the ratio of tagged to untagged fish in this catch will be the same as that in the population. Supposing we tag a number N, and a catch C includes n tagged fish, then the population P can be determined from the relation $\frac{N}{P} = \frac{n}{c}$ or $P = \frac{N\,c}{n}$ (7.1)

Any competent statistician will be able to suggest improvements to eliminate bias, or to produce estimates that are not too greatly affected by certain complexities, such as mortality between the time of tagging and the later catch. Any competent fishery biologist will show that in any case many of the necessary assumptions don't hold at all. Several of the failures in the assumptions result in the number of tags we get back into our hands from the experiment being smaller than it should be. There are three occasions when these losses might take place. The first is that when we tag fish, the act of catching the fish before tagging, or of putting the tag on can be fatal. The number of live tagged fish in the sea soon after the tagging work will be less than the total tagged. Even if the fish survives the act of tagging, the fact of having a tag on it may increase its mortality by making it more visible to predators, or less able to get food. These losses will be continuous while the tagged fish are in the sea. The third group of losses will occur after a tagged fish is caught. The tag may not be seen, or if it is seen, the fisherman just puts it in his pocket, and does not report it. Often the first and third types of losses are grouped together since they will only affect estimates of population size or fishing mortality, but will not affect estimates of total mortality rate.

A complication in large widespread stocks which support many of the major commercial fisheries is that the tagged and untagged fish will not be completely mixed for some time after the time of tagging. It is impossible to spread tagged cod or herring evenly throughout the North Sea. We are only physically capable of tagging in one place at a time and though we can move around the

North Sea it does take time, so that immediately after we tag, the tags are concentrated in one spot and subject to a rather different fishing mortality than the untagged population. We can, of course, wait till the tagged fish are nicely mixed up with the untagged population and use only the returns from then onwards, but if the mixing rate is low this means we may have to throw away a very large proportion of the data.

Let us return to the herring, and the so-called Bløden tagging experiment, which was one of the nicer examples of international collaboration in fishery research. It was organized by ICES, and financed by contributions from seven of the countries with important interests in the North Sea herring - France, Netherlands, Germany, Denmark, Poland, United Kingdom and USSR. These contributions were used to hire a purse seiner for tagging, to buy tags, pay rewards, and pay for some of the analysis. In addition to this, there was a great deal of work carried on by the national research ships to survey the distribution of fish before and after the experiment. In relation to the tagging experiment on the Bløden ground, described by Aasen et al. (1961) the scientists were in some ways, rather lucky. Many of the losses could be eliminated, or at least measured. It was possible to use an internal tag, that is a small steel tag inside the gut cavity. Once the fish had settled down after the tagging, there probably was not much difference between untagged and tagged fish so that there were few losses in the intermediate period in the sea. The tags were returned from the processing factories, where they were detected by magnets along the production lines. These found most of the tags, but of even more advantage, they could put some dead tagged herring into the beginning of the production line and see how many tags were returned from a known number of tagged fish entering the factory. This knowledge of the discovery percentage can be used to estimate the actual number of tagged fish caught, from the number returned.

In this experiment this return rate was quite high, ranging, in the Danish factories, between 88% in 1957 and 91% in 1958, and 74% and 64% in the German factories. What was more problematical in this experiment was initial loss, that is how many of the fish with tags inside were in fact swimming happily around the sea two or three days after the tagging experiment. This is, of course, extremely difficult to measure, but there are various indirect ways of finding out. One approach is illustrated in Figure 7F, which shows the effect on the rate of return of the time between capture and tagging. These fish were caught in a purse seine. Each set of the purse seine resulted in a large number of fish which took some time to tag. The fish tagged towards the end of each batch which had been in the net for some time were less lively than those tagged almost immediately after being caught. The fish tagged were grouped according to the time between when they were caught and when they were tagged and released. Figure 7F shows that the numbers returned from each group decline quite sharply. The return rate of the ones that were held longest was only about half of those that were tagged immediately after capture.

Another effect that can be measured is the differences between the people tagging the fish. It was found that some seemed to be better at handling fish than others and one tagging team would have a consistently higher percentage of returns than another. Presumably this is because the team with lower returns was more clumsy. One can then correct the number of fish tagged to the number that would have had to be tagged by the best team immediately after capture to give the observed number of returns. This gives an estimate of the number of tags effectively released.

The summary results of the experiment that was carried out are shown in Table 7A. For each batch of fish, the right-hand part of the upper half of the table gives the numbers of tags that were actually used, and the estimate of the number of fish that were effectively tagged, taking into account the various losses at tagging. The body of the table gives the percentage of the fish effectively tagged which were returned each week, and these were the first data we looked at. What we hoped was that the percentage returned each week would be a fairly constant figure equal to the fishing mortality per week on the stock, and perhaps declining slightly with time as there was some mortality of the tagged fish or changes in the fishing rate. Instead of a constant figure, however, we found highly variable figures. From one experiment, (1957 II) we got nearly 5% back, within a week, but less than 1% in total during the following weeks; whereas in another experiment (1957 III), we had a low rate that went on more steadily.

These great differences, both in the week-to-week pattern of different liberations and in their overall level, made it very difficult to use these results as an estimate of the fishing rate on this stock. We were then at a common stage in any piece of work, when the way of tackling the problem doesn't give sensible answers and it is necessary to scratch around for something else. In this case the clue to further progress lay in the positions where the boats had been fishing. We had for this fishery very good estimates of where each boat in the commercial fleet had been fishing. When it came back to the factory with its load of herring, the skipper was asked where he had been fishing and this was recorded in areas of approximately 15 miles square. What we didn't know was where the tagged fish had been caught, because the tags were detected towards the end of the production line at the factory. The day on which the tag was discovered was not necessarily the same as that at which the fish was delivered to the factory, and it was virtually impossible to tell which of some 50 boats delivering herring to the factory was responsible for a particular tag. While we had no direct evidence of where the tagged fish went, we did have a good idea where the fleet as a whole had been fishing relative to the liberation positions of each batch of fish. And when we looked at the data on the position of the fleet relative to the tagging positions, we found a close connection between this distribution and its changes, and the pattern of returns. For instance, there had been a high concentration of boats around the liberation positions of the second batch of 1967 during the week of tagging, but the boats moved off somewhere else in later weeks, and there were few boats fishing near where the fish had been released.

On the other hand, there had never been much fishing near the release point of the third batch of fish released in 1957, and few tagged fish were caught. This suggested that we should look, not at the numbers of tags returned, or the numbers of tags returned per fishing effort of the fleet as a whole, but that we should compute a fishing intensity on the tagged fish taking into account the fishing positions of the fleet. We did this by making some assumptions as to the pattern of movement and dispersion of the fish and each week, in each of the 15-mile squares adjacent to the tagging position. From this it was possible to calculate the average fishing intensity in terms of hours fishing per square on the population of tagged fish each week.

The figures in the lower half of this table were then calculated, as the returns per thousand fish tagged per unit fishing intensity. Though these figures are still not absolutely constant, they are much more uniform than the figures in the upper part of the table. These dashes indicate where there had been no fishing in the immediate vicinity of the release point. This reasonable constancy between different batches gave us a feeling that we were getting somewhere, and that the estimates we were obtaining were fairly meaningful. The next step was to calculate a mean q (catchability coefficient), using the usual relation F = qf where f is expressed in units of fishing intensity of 100 hours per square. For the 1957 experiments, the mean q was 14.6. So that unit fishing intensity (100 hours fishing per square) would catch 14.6 of every thousand fish, or 1.46% of the population. Similarly from the 1958 experiments pooled, we get a q of 12.5 but we suspected that one group of fish was behaving abnormally and moved outside the fishing area because not only was the value of q from this group low, but some of the tags were returned from a factory in northern Denmark, most of whose boats hadn't been fishing in the main Bløden area. Discarding the data from that experiment gave a mean value for 1958 of 16.6. From this value of q, a fishing mortality rate each week could be calculated for the part of the total population present in each square, using the data on the number of hours fishing in that square.

It would be possible to compute an overall fishing mortality by taking the average of these fishing mortalities weighed according to the abundance of fish (or catch per unit effort) in each square. Alternatively, the value of q could be used, with the value of catch per unit effort, to calculate the abundance of fish in each square. During 1957 the catch from each hundred hours of fishing in a square would be 1.46% of the population in that square. During 1957 the average catch per hundred hours of fishing was 122 tons. That must be 1.46% of the population in a square, which must therefore have been $\frac{122 \times 100}{1.46}$ or 8,400 tons of fish. Then we knew from the distribution of the fleet that this stock of fish extended over approximately 50 squares. The extent of the North Sea over which the fish were distributed was determined partly from the records of the commercial fleet and partly from echo surveys made by research vessels. Our best estimate of the total population in 1957 was therefore 50 times 8,400 or 420,000, and similarly in 1958 we got two estimates, depending on which of those two figures of q we took, of 735,000 and 555,000 tons.

Table 7A shows that these analyses were done for returns during a period of 6 to 8 weeks after the experiment. There were some returns after that. By that time it was difficult to estimate a reliable figure of fishing intensity, but it was likely that the tagged fish were becoming fairly well mixed with the population. Equation 7.1 could therefore be used to estimate the total population size. The equation can be used once, grouping all returns after mixing has occurred, or data from different parts of the fishery can be treated separately. If mixing is complete the ratio, c/n, or the proportion of tagged fish in the catch, should remain constant.

We found that if we plotted the catch against the number of tags for each week from the eighth week after tagging onwards, we got a fairly good proportional relationship suggesting a constant ratio of c/n, and that probably there had been reasonable mixing. Taking the slope of this line as the best estimate of c/n, the estimate of the population from late returns in 1957 was 465,000 tons. Similarly, the estimate of the 1958 stock from the late returns in the Danish fishery was 695,000 tons. We also got returns from the German fishery in 1958 which could be used in the same way to give an estimated population of 492,000 tons. The general agreement between these figures was reasonably pleasing. While the estimates are not completely independent since they do depend on knowing the number of fish effectively marked, they are otherwise independent. Therefore, they probably give a fairly reasonable estimate of how many fish there were in this stock.

Table 7A. Returns of tagged herring from the Bløden ground experiments.

(a) Returns per 1000 fish effectively tagged.

Liberation	Weeks after release								Numbers Tagged	
	0	1	2	3	4	5	6	7	Total	Effective
1957 I	7.0	17.7	16.0	2.7	1.3	2.3	0	3.3	4040	(3000)
1957 II	44.9	6.8	1.1	1.7	0	1.7			1989	(1760)
1957 III	1.0	1.0	2.7	0.7	0	0			3900	(3000)
1958 I	13.9	69.7	4.3	8.6	4.3	5.4	6.4	2.1	1500	930

(b) Returns per 1000 fish tagged per unit fishing intensity.

Liberation	Weeks after release							
	0	1	2	3	4	5	6	7
1957 I	8.9	27.6	29.1	10.3	2.4	9.7	-	10.8
1957 II	10.6	48.7	0.9			24.4		
1957 III	16.7	33.3	33.3	-	-	-		
1958 I	2.5	29.7	2.3	16.9	6.4	2.9	18.4	24.9

Expressing the catches as a proportion of this stock shows that the Blφden fishery was, in 1957, taking about 17% of the stock and in 1958 18.5% of the stock. Now this answer pleased no one because it was sufficiently large to say that this fishery was having some effect on juvenile stock and hence on the fisheries on adult fish later on. On the other hand, it wasn't sufficiently large to account for all the changes that had taken place in these mature fisheries, particularly the changes in the fisheries on the Downs stock in the southern North Sea. In fact, in the last few years these changes in the North Sea herring fisheries have become more pronounced, and a number of other difficult problems, such as the relation of recruitment to adult stock, are having to be carefully examined. However, a knowledge of the effect of the juvenile fisheries remains an important element in understanding events in the North Sea fishery, and plans are currently under way to update and revise the estimates discussed here by another larger-scale tagging experiment in the Blφden fishery.

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Chapter 8

ANTARCTIC WHALES: ANALYSIS BY SEVERAL METHODS

Now let us get outside the North Atlantic and away from fish and talk about the big whale fisheries in the Antarctic. Instead of using a fishery to illustrate one particular technique or principle in population dynamics, let us look at the application to the whale fishery of a number of different techniques and see how they fit together and whether we get more or less the same answer when using different methods.

Whales occur all over the world's oceans. The oldest whale fisheries were in the Northern Hemisphere for the right whales, so called because they were the right whales to go after. These are the least active of the baleen whales, and supported very important British and Dutch fisheries in the Arctic between the 16th and 19th centuries. The whale fishermen were among the first to go up into these northern areas around Greenland and Spitsbergen. The next great period of whaling was the development of the New England sperm whale fishery which spread all over the world. These are good records of the distribution of these catches (Townsend, 1935), and it is interesting to see how closely the distribution of the catches of sperm whales agrees with the distribution of the main centers of primary production in tropical and subtropical areas. Both these fisheries were open-boat fisheries in which the fishermen stopped the sailing boat when they saw a whale, lowered a small rowboat and went after the whale, hurling harpoons into it until they caught it.

The modern age of whaling started with the Norwegian development of the harpoon gun, and the great age of Antarctic whaling started with both the harpoon gun for catching the large rorqual and also the floating factory ship which enabled fishermen to go anywhere in the Antarctic and process the whales without much trouble. Antarctic whaling is carried on all around the Antarctic, but the distribution of whales and hence of whaling is not uniform. The South Atlantic sector has always been one of the most productive areas for whales, just as it appears to be the area with the greatest primary production and also a high zooplankton standing stock. In the rest of the Antarctic, the primary production does not seem to be so high; and to some extent this fits in with the production of whales. There are fewer whales south of the Indian Ocean, and particularly south of the Pacific where the primary production is low.

Whales carry out north to south migration, feeding in the Antarctic during the Antarctic summer, and spending the winter further north in warmer waters. This migration north and south tends to separate the whales into separate stocks in each chunk of the Antarctic and in each ocean. Theoretically we should carry out analysis on each stock separately, but because the events in different stocks have followed more or less the same pattern, we will not separate out the events on different stocks on the same species of whales.

The catches of the three main species of baleen are set out in Table 8A. The first favorite of the whalers was the blue whale, the biggest of the lot, which grows up to nearly 100 feet long. The next one in size is the fin whale, growing up to about 80 feet, and the smallest of the three is the sei whale. The industry has concentrated on these species in the same order: In the 1930's the biggest catches were of blue whale. After the war, particularly in the 1950's, the biggest catches were of fin whales, and after 1963, the concentration was on sei whales. This switch from one species to the other was a good measure of what was happening to the stock of the preferred species. In the 1930's, the blue whale was being depleted and it was necessary to turn to fin whales to maintain the catches. Similarly in the 1960's the fin whale was depleted and it was necessary to turn to sei whales to keep the industry going.

Because of this decline of blue whales, which became apparent back in the 1930's and because of the past history of whaling on the right whales, where the stocks had nearly became extinct following intensive whaling, there was great concern even among the industry about the conservation of the whales as long ago as 1935. Though much is said now about the failures of the International Whaling Commission, and indeed the Whaling Commission has been far from entirely successful, there have been times during the history of Antarctic whaling when conservation of whales has been moving quite strongly in the right direction. One of the favorable periods was before and just after the war, soon after the time when it first became apparent that the blue whale stocks were decreasing. During that period there was much discussion between the main whaling countries about how to conserve the whales, and after a number of international meetings, the present International Whaling Commission was set up in 1946. Almost as soon as it came into being, it set up controls on the total amount of whales caught. These controls turned out to have three failings. One was that the control was on the total amount of whales, calculated in terms of a single unit, the so-called blue whale unit, which took no account of the balance between species. Two fin whales or six sei whales count as one blue whale. There was no specific protection for individual species but just for the whales as a whole. The second thing wrong with the regulations was that there was no way of revising it very quickly as time went on. Between 1947 and 1962, there were some minor adjustments to the quota, but it remained between 14,500 and 16,000 BWU. This quota is not very different from the combined sustainable yields from the different stocks, provided each is maintained at its optimum level. Initially the difference between the quota and the sustainable yield from the contempory stocks was not large.

Table 8A Baleen whale catches in the Antarctic (from Chapman, 1971).

Season[1]	Blue[2]	Fin	Sei
1925	5,703	4,366	1
1926	4,697	8,916	195
1927	6,545	5,102	778
1928	8,334	4,459	883
1929	12,734	6,689	808
1930	17,487	11,539	216
1931	29,410	10,017	145
1932	6,488	2,871	16
1933	18,891	5,168	2
1934	17,349	7,200	0
1935	16,500	12,500	266
1936	17,731	9,697	2
1937	14,304	14,381	490
1938	14,923	28,009	161
1939	14,081	20,784	22
1940	11,480	18,694	81
1941	4,943	7,831	110
1942	59	1,189	52
1943	125	776	73
1944	339	1,158	197
1945	1,042	1,666	78
1946	3,606	9,185	85
1947	9,192	14,547	393
1948	6,908	21,141	621
1949	7,625	19,123	578
1950	6,182	20,060	1,284
1951	7,048	19,456	886
1952	5,130	22,527	530
1953	3,870	22,867	621
1954	2,697	27,659	1,029
1955	2,176	28,624	569
1956	1,614	27,958	560
1957	1,512	27,757	1,692
1958	1,690	27,473	3,309
1959	1,192	27,128	2,421
1960	1,239 (917)	27,575	4,309
1961	1,744 (739)	28,761	5,102
1962	1,118 (716)	27,099	5,196
1963	947 (220)	18,668	5,503
1964	112	14,422	8,695
1965	20	7,811	20,380
1966	1	2,536	17,587
1967	4	2,893	12,368
1968	-	2,155	10,357
1969	-	3,020	5,776
1970	-	3,002	5,857
1971[3]	-	2,888	6,151

(1) 1932 refers to the 1931/32 season, etc.

(2) Including catches of pigmy blue whales; estimated catch of true blue whales shown in parentheses (1959/60-1962/63).

(3) Preliminary data.

As time went on, the fin whale stock declined because more than the sustainable yield was being taken. As the population went down, the sustainable yield also went down and the gap between what was taken, which for fin whales stayed more or less constant over quite a long period, and the sustainable yield got greater and greater until in the 1960's the catch was several times the sustainable yield. The stock was being depleted at an ever-increasing rate.

The third problem about the regulations as set in this period just after the war was that there was no allocation between countries. The quota was achieved, and the industry controlled, in the same way as the halibut and yellowfin tuna fisheries were controlled. That is, everyone is free to fish until the quota is taken and then everyone stops. And it is easy to see what happens in this situation. If there is a limited season and fishermen have to get what they can in this season, they have to build more factory ships and send more catchers with each factory ship to make sure of getting their share during the season. This means that the season gets shorter and shorter. It is then necessary to build even more boats and even more factory ships to maintain the share and the season gets shorter still. Though the stock may or may not be in good shape, it is almost certain that the economics of the industry are very far from being in good shape.

The length of the Antarctic season shrank from 120 days, which is the practical whaling season in the Antarctic, to about 40 days in the 1950's. There was a great deal of argument in the 1950's about how a more rational system of economic exploitation of the whales should be carried out. Finally in June 1962, an agreement was signed between the countries on how the quota could be divided, with agreed percentages going to each country. This enabled the industry to get back onto a sound economic footing, but meanwhile, unfortunately, it was getting on an increasingly unsound biological footing. It was an unfortunate accident that in the late 50's much of the attention that should have been paid to the declining stocks and to the need for more sensible biological regulations was diverted to the other, and to the industry more visible, problem of putting in more sensible economic regulations.

Another rather serious situation was the absence of any population biologists working on the analysis of the whale stocks. While there were some scientists working on the biology of whales, they were few and had no expertise in the quantitative aspects of population dynamics. This made it difficult for the scientists to give clear quantitative advice as to what was happening and particularly to point out clearly the effects of alternative actions, such as an immediate reduction in the catches, or letting things go as they had been going without changing the quotas.

Another shortcoming in whaling research was the failure to identify the simple basic elements in the story, and to ignore the complications. Inevitably the nice simple picture given by the population dynamicist will omit many of the real complexities of the whaling industry and whale biology. Some of the whale biologists were good at pointing out these complexities, though often without suggesting methods of dealing with them. The result would be that no clear advice could be given to the Commission. As has been emphasized before, it is important to make the right sort of simplifications, the ones that enable us to simplify matters to get to the really important picture without spending too much time worrying about the unnecessary complications. The difficulty always is to determine which of the complications are important and which are unnecessary. This can be illustrated by one of the unnecessary complications that cropped up in whaling analysis. This was connected with getting the right measure of effort and of catch per unit effort. One of the fortunate things about the whale problem was that a lot of the basic data were available. Data are available in the records of the Bureau of International Whaling Statistics of every single whale caught from about 1930 onwards, its species, the position where it was caught, its size, its sex, and if it was a female, the size of the young foetus if there was one.

The Bureau also has good information on the operations of the factory ships. Each factory ship has with it a number of catchers which varied during the period from 4 or 5 up to twenty or more, and the simplest measure of effort usually taken, is the number of catchers day's work, that is the number of days the factory ship has been operating, multiplied by the number of catcher vessels attached to this factory ship. Of course not all catcher days are equal. In particular, if visibility is bad or the weather is stormy, it is more difficult to catch whales than when the visibility is good and the sea is calm. The quantitative effect of weather is shown in Table 8B, which gives the catches of one particular Norwegian expedition in terms of numbers of whales caught per day for different wind strengths, using the Beaufort scale and different visibilities. When the visibility was good and the weather calm, they got 60 whales in a day, but catches declined as the weather got worse, down to only one or two whales per day. In fact, it would probably be impossible to work in force 9. These records are taken at noon at the factory ship. It may have been blowing force 9 at noon but perhaps one catcher caught a whale earlier in the day before the weather got too bad.

The table also expresses the catches under any weather conditions as the ratio of those catches to those under standard weather conditions which were taken as force 3 to 4 with good or moderate visibility. Very similar values for these ratios were found for other expeditions. The average of these ratios for different expeditions was calculated to give a correction factor for any

Table 8B. Catches by Expedition KOSMOS IV, season 1960-1961, as a function of weather at noon on day of operation.

(a) Average catch per day

WIND	0	1	2	3	4	5	6	7	8	9	10+
VISIBILITY											
Good (A)	63·5	—	38·0	25·2	19·8	20·5	5·6	2·5	1·0	—	—
Moderate (B)			33·5	18·2	16·8	11·6	7·8	3·0	1·0	1·0	
Poor (C)					11·0	0·7	2·5	2·5	—	3·1	

Mean catch in standard conditions $= \frac{1}{4}(25{\cdot}2 + 19{\cdot}8 + 18{\cdot}2 + 16{\cdot}8)$
$= 20{\cdot}0$

(b) Average catch as a percentage of standard catch

WIND	0	1	2	3	4	5	6	7	8	9	10+
VISIBILITY											
Good (A)	317	—	190	126	99	102	28	12	5	—	—
Moderate (B)			168	91	84	58	39	15	5	5	
Poor (C)					55	4	12	12	0	15	

Table 8C. Fishing effort, catch per unit effort, and catch of fin whales in the Antarctic.

Season	T	Catch	Total C.W.D.	Corrected effort in 10^3	c.p.u.e.
1931/32	254	1,136	5,149	1·308	0·869
1932/33	254	4,434	16,150	4·102	1·081
1933/34	254	5,471	13,725	3·486	1·569
1934/35	254	11,694	18,143	4·608	2·538
1935/36	295	9,176	16,173	4·771	1·923
1936/37	295	13,291	17,656	5·209	2·552
1937/38	295	26,412	24,905	7·347	3·595
1938/39	316	19,477	25,954	8·201	2·375
1945/46	316	7,701	8,253	2·608	2·953
1946/47	328	12,870	14,414	4·728	2·722
1947/48	347	18,864	17,747	6·158	3·063
1948/49	399	17,081	18,902	7·542	2·265
1949/50	424	17,876	18,128	7·686	2·326
1950/51	454	17,296	18,952	8·604	2·010
1951/52	473	20,312	16,902	7·995	2·541
1952/53	494	20,964	17,101	8·448	2·482
1953/54	498	24,675	15,630	7·784	3·170
1954/55	511	25,608	16,619	8·492	3·016
1955/56	513	25,102	14,893	7·640	3·286
1956/57	545	25,502	15,429	8·409	3·033
1957/58	570	25,067	16,344	9·316	2·691
1958/59	599	25,687	16,275	9·749	2·635
1959/60	630	26,271	21,269	13·399	1·961
1960/61	642	27,299	23,998	15·407	1·772
1961/62	657	26,364	29,952	19·678	1·340
1962/63	703	18,636	22,504	15·820	1·178
1963/64	709	13,853	20,407	14·469	0·957
1964/65	715	7,306	17,475	12·495	0·585
1965/66	743	2,314	13,122	9·750	0·237
1966/67	757	2,885	11,760	8·914	0·324
1967/68	769	2,152	9,785	7·523	0·286
1968/69					

combination of wind and visibility. This could be used to express the effort in any period in standard units, independent of weather. This standardization was calculated once for a series of years and found to be very constant from year to year, but since then has not been used.

The effect of weather was an unnecessary complication when considering the measurement of effort, but there were two more important complications. One was the fact of this change from species to species. The catch per day of any species was as much a measure of the interest of the industry in that species as a measure of its abundance. For instance, the catch per day of sei whales gave virtually no information on the abundance of sei whales up to about 1960 when the industry first got interested in sei whales. Over most of the period considered here, fin whales were an important species. After about 1950 blue whales ceased to be very important, being less than 10% of the catch, and sei whales became important only after about 1965. The catch per unit effort is therefore a good measure of abundance of fin whales from about 1955 until about 1964. This is shown in Table 8C which gives, in the last column, the catch per unit effort of fin whales. From 1931 until the 1953-1954 season, this tended to increase, with fluctuations, from less than 1 whale per unit effort up to 3 whales. This was clearly not a real increase in fin whales. It is because the industry was transferring its attentions from blue whales to fin whales.

There is an overlap of distribution between these species, on the whole the blue whale being further south, often right down on the ice edge, the fin whale further north, in a zone of generally worse weather, which is one reason why the weather factor was mentioned, and the sei whales furthest north almost out of the true Antarctic. Thus, as the industry changed its preference from blue whales, to fin whales, to sei whales, the broad geographical zones in which they worked changed. They also changed their tactics within these broad geographical areas. When there are a lot of blue whales about, catchers may not chase after a fin whale on the horizon, but wait for a blue whale to appear. When there aren't any blue whales, they will go after every fin whale they can see. As a result, there is an apparent increase in catch per unit effort over a long period which means nothing, but between 1955 and 1964, this number, hopefully, should be a reasonable measure of the abundance of fin whales.

The other complication is that during this whole period the catchers were getting more effective. In some ways this is difficult to measure, but one thing we can definitely measure is the tonnage of the boats, which is getting bigger; they increased from about 250 tons in 1930 to nearly 800 tons now. It is not immediately obvious what the theoretical relationship between tonnage and the catching power should be, but the bigger catcher can go faster, it can work in worse weather, and it has more equipment, such as sonar to follow the whale under water. It is bound to be a more effective operator. Comparisons of the numbers of whales killed by catchers of different tonnages working with the same expedition in the same season give a good correlation of about 0.7 between tonnage and catch per day. The horsepower of the boat might also be a measure of fishing power because the bigger the engine, the faster the boat can go. That gave a correlation coefficient of only about 0.5. Within a season the fishing power, the catch per day, was proportional to the tonnage of the boat, so the measure taken to correct for the increasing efficiency was to multiply the number of catcher day's work each year by the average tonnage of the catchers in operation during that year to give a corrected effort, as catcher-ton-days. This was divided into the total catch to get the best available measure of catch per unit effort which should be proportional to the abundance of whales.

One is never quite satisfied with this estimate of fishing power, because the changes we want to look at are the changes of efficiency from year to year, whereas the changes we can measure easily are the differences in fishing power of boats fishing at the same time, i.e., in the same year. On the one hand, it is quite likely that these estimates of comparisons within a year overestimate the difference between large and small boats because the good gunners will go in a modern big boat and the poor gunners will go in the small old boat, so that the big boat will look as though it is doing better than the small boat, even if gunners with equal skill in the two boats would get about the same catch. That is an error one way. There is equally likely to be an error the other way because some of the big increases of efficiency are not in the size or speed of the boat, but an overall increase in the whole operational efficiency from year to year affecting all boats equally.

Anyway, we have, we think, some measure of abundance that is reasonable to put into the calculations and we can start using some of the population models already developed. The simplest one is the Schaefer model based simply on the analysis of catches, catch per unit effort, and effort. If we look at the corrected effort, we see that the effort was steadily increasing over this period. There was no one time when there was a reasonably steady state. Also since whales are long-lived animals, we would expect there to be some time lag before a steady state is reached so that we can't just use the simple analysis of plotting catch per unit effort or catch in one year against the effort in that year. We have to make some allowances for the changes in population, and the simplest way is the way that Schaefer developed for the yellowfin tuna.

The basic equation is that the sustainable yield or the net rate of natural increase is equal to the catch plus the change in population. If the population is going down, it is the catch less the reduction in the population. To do this, we must have an estimate of population size. Thus the first analysis concerns the estimation of the population size in absolute terms. One method is the so-called DeLury method developed on freshwater fish, but it works quite well for whales. Figure 8A shows the results for humpback whales off western Australia. The rationale of this method of estimating population size is that if a population of whales does not change except for removals by man, i.e., the net effect of births or recruitment and natural deaths can be ignored, the population at any moment will be the initial population less the accumulated catch, i.e., $N = N_o - \sum C$ and we will get a straight-line relationship between the population and the amount of catch coming down to zero when we have caught them all.

If the catch per unit effort is proportional to abundance then we have $n = qN = q(N_o - \sum C)$, and plotting n, the catch per unit effort against the accumulated catch should give a straight line, with the intercept being the original population. We see that for this population of humpback whales the relation is reasonably good. Of course the basic assumption that there are no net additions or losses other than the catches does not hold quite true, but it is a reasonably easy matter to make some allowances for this. Equations to do this have been presented in reports to the IWC. In fact, the development of the study of the whale populations is quite easy to follow because most of the papers are set out in the various reports of the International Whaling Commission. These are issued annually and in addition to the formal reports of the activities of the Commission and its meetings, there have been over the last ten years or so appendices giving the results of the scientific work and giving most of the basic papers that have been used in the analysis. In particular, we should refer to the work of the committee of three, or later four, which came out in the 14th annual report. In these reports, we find formulas for adopting this DeLury method to take into account the fact that in addition to the catches there are some recruits coming in and there are some deaths by natural mortality. But if these are not too great a proportion of the total catch and the main decline is the removals by man, we can get a reasonable first estimate of the population of whales from the direct and simple application of the method.

Another estimate of absolute abundance of whales which involves no great assumptions is just to go out and count them. Whales have to come up to breathe, and when they come to the surface, we can see and count them. In theory we can sail a research vessel to and fro across the Antarctic counting all the whales in the path of the ship. Suppose we can see all whales for two miles on each side of the ship, which steamed for 5,000 miles, we have looked at a strip four miles wide and 5,000 miles long. Then if 50 whales were seen in this strip, the density of whales would be 2.5 per 1,000 square miles. This density can be multiplied by the total area of the Antarctic inhabited by whales to give an estimate of the total number of whales in the Antarctic. And surprisingly enough this has been yielding quite reasonable answers. It doesn't take very advanced statistics to show that it is inefficient to wander at random all over the Antarctic. We can divide up the Antarctic by some reasonable system of stratified sampling and concentrate most of the activities where whales are most abundant.

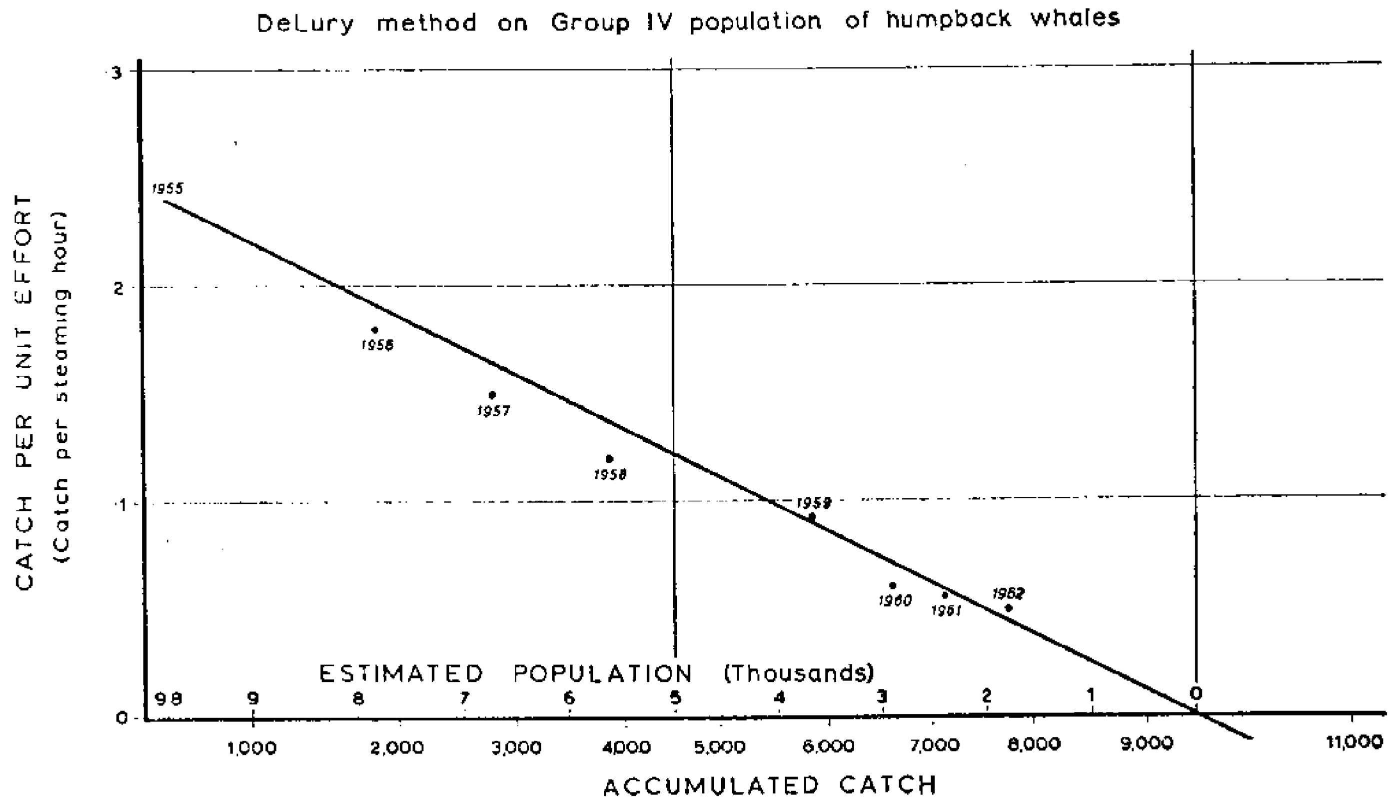

Figure 8A. DeLury estimate of stock size of humpback whales, Area IV.

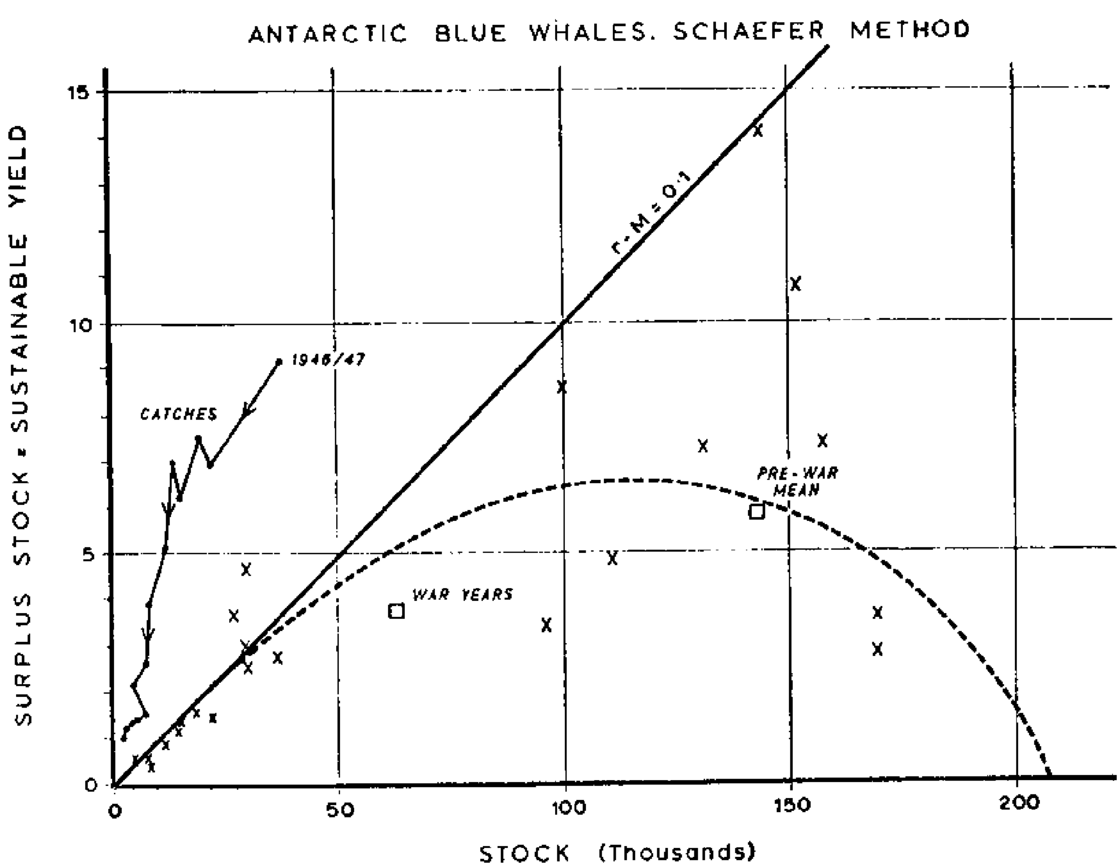

Figure 8B. Relation between stock abundance of blue whales and sustainable yield, as estimated by the Schaefer method.

A practical question is whether we do in fact see all whales within two miles of the ship or whether we are missing a lot. We can get some insight into this by considering the range at which a whale is first seen. Few whales are detected at more than half a dozen miles, and the number seen increases with decreasing detection range, reaching a peak at about 3 miles, but then falling off. Presumably this is because whales that come closer than 3 miles are likely to have been already seen before they come so close to the ship. Probably, therefore, most whales that come as close as 2 miles will be seen.

Despite the rather imprecise nature of the estimates by these methods, there tends to be reasonable agreement between different estimates. The DeLury method gives a population of blue whales of about 9,000 in 1953, dropping down to about 5,500 in 1958. Sightings of whales from research ships in the prewar period gave an estimate of about 33,000 blue whales in the 1930's. Between then and 1953 some 40-50,000 whales were caught. Taking into account some recruitment into the population, the estimate from sightings is not inconsistent with the DeLury estimate. Some additional and rather encouraging estimates of the number of blue whales have been made from sightings by Japanese survey vessels in the last three or four years and run from about 4,000 up to 10,000. While the reliability of the individual figures may not be too great, and an apparent year-to-year increase in these estimates is probably not significant, there is at least good evidence that there are quite a lot of blue whales about now, probably more than there were back in 1960.

Given an estimate of absolute numbers for some period, this can be compared with the catch per unit effort for that period to give a measure of the catchability coefficient Using this, the changes in population size can be estimated from the catch per unit effort. This enables us to apply the Schaefer model, taking into account changes in population size. The results, for the blue whale stocks, are shown in Figure 8B. In this the sustainable yield was calculated as the change of stock as estimated from the catch per unit effort, plus the catch. Since these estimates are on differences of catches per unit effort, which are rather variable, the estimates of the sustainable yield each year, shown by crosses, are very scattered, especially for the prewar years. These years have been pooled, and also an estimate of the sustainable yield during the war years has been obtained from the changes between 1939 and 1946, to give the points shown in the figure.

Using these and the basic assumption of the method that the curve must come down to zero at the estimated initial, unexploited stock of slightly over 200,000 animals, we are more or less forced to draw a curve that can't differ very much from this dotted curve. One of the interesting things from the theoretical point of view is that it is rather difficult to draw this curve as a symmetrical parabola. It has to be drawn with a rather flatter left-hand limb, dropping off rather more sharply on the right-hand side.

Figure 8B also shows the catches taken during the postwar period plotted against the stock size. The catches were two to three times as great as the sustainable yield. Though the catches came down over the period, they came down in proportion to the stock so that the rate of depletion of the stock remained the same. The catches remained some two to three times the sustainable yield almost until in 1963 when they stopped catching blue whales altogether.

Before leaving this model, and looking at the application of the other, dynamic pool method, in detail, let us look at the slope of the left-hand limb of the curve in Figure 8B. The slope of this line is the net rate of increase of a small population as a proportion of the population, and as drawn has a value of about 0.1. Now for fish, it is very difficult to see any theoretical limit to the slope of this line, but for whales it is very easy to estimate an upper limit to this slope. The slope is equal to the recruitment rate less the natural mortality. The natural mortality could theoretically take any value, but there are limits set to the recruitment by the slow breeding rate of these whales. At best, they only have one young every other year so for a sex ratio of unity the gross reproductive rate is at 0.25. Death of young animals between birth and reaching maturity must be appreciable. Also the interval between births may be larger than the theorctical value of 2 years. Thus the actual number of whales recruiting to the mature stock will be much less than 0.25 of the parent stock, perhaps 0.15; subtracting a natural mortality of 0.05 gives a net rate of natural increase of 0.10, and perfect agreement with the line in Figure 8B.

Unfortunately more precise calculations of the net rate of natural increase of fin whales show that this is appreciably less than 0.10. As the two species have nearly the same reproductive rates and probably also have similar mortality rates, it does look as though this line is too steep, and that this isn't quite as good a story as we hoped.

Looking at the dynamic pool model, we can go through the usual calculations of mortality rates. One of the classical ways of estimating mortality rates is the catch curve, that is looking at the age composition of a sample of animals taken in one year, and comparing the number of animals of successive ages. This ratio of successive age groups taken in the same year depends partly on the initial numbers of these two groups, and partly on the difference in the total mortality. Unfortunately in whales it is quite clear that as the population changes, the number of recruits is bound to change so the catch curve analysis is not too suitable for direct application to most samples. However, whales are very long-lived and the far right-hand side of the catch curve for fin whales will include very old animals, born in a period when the population had not been depleted by exploitation, and wasn't changing too much. This part of the curve should give a reasonable estimate of mortality; it is also a fossilized estimate of the mortality way back when those year-classes were recruiting to the fishery. This was at a time when there was very little exploitation so it is an estimate of natural mortality. These estimates for the fin whale stocks are shown in Table 8D, separately for males and females, and for samples taken in different areas. The average of these estimates of natural mortality comes out at about 0.04 or 4% a year.

Another way of estimating mortalities is to calculate the ratio of catches per unit effort of the same year-class in successive years. The results for fin whales are given in Table 8E, again separately for different areas and also for different periods. In the early periods the estimates are low; in fact, several are negative, suggesting that the increase in catch per unit effort due to increased attention being paid to fin whales, rather than blue whales, was greater than the real decline in numbers from all causes of mortality. The estimates of mortality steadily increase to high values of about 0.35 during the 1962 to 1967 period. From these estimates of total mortality, we can subtract the estimate of natural mortality, which is reasonably good, to give estimates of fishing mortality.

So much for estimates of mortality rate from age composition. We also have some marking data for whales. Marking data are always very difficult to interpret and whale-marking data are no exception. Not only is the number of returned marks less than the number of marked-whales killed, but the number marked is unknown. The marking technique is to fire little metal cylinders under the blubber of the whales and hope these are found when the whale is being processed. One cannot

Table 8D. Natural mortality coefficients of fin whales estimated from older ages (from IWC, 1970)

Area	Male	Female
I	0.021	0.043
II	0.024	0.053
III	0.043	0.066
IV	0.042	0.061
V	0.040	0.037
VI	0.033	0.035
Whole	0.034	0.057
Average of I-VI	0.034	0.049
Best estimate for both sexes:	0.0415	

Table 8E. Estimated total mortality coefficients of fin whales by Area by period (from IWC, 1970).

Period	Area I	II	III	IV	V	VI	Whole	Average of I-VI
1932/34 ..		-0.275	-0.644	-0.123			-0.353	-0.348
1935/38 ..		0.046	0.145	-0.082			0.071	0.036
1946/49 ..		0.078	0.060	0.122	-0.021	0.402	0.102	0.128
1950/53 ..		0.043	-0.020	-0.041	-0.076	-0.155	-0.020	-0.050
1954/57 ..	0.420	0.106	0.081	0.073	0.040	0.137	0.095	0.143
1958/61 ..	0.202	0.211	0.210	0.292	0.269	0.322	0.226	0.251
1962/67 ..	-0.148	0.403	0.429	0.445	0.519	0.498	0.443	0.358

be sure that when a mark is fired at a whale, it has gone firmly into the whale or not. So marking is not too good, but there is one technique that will give meaningful estimates and this is illustrated in Table 8F. Whales were marked before the war and also between 1955 and 1956. Marks were returned from both groups between 1957 and 1960, but at different percentage rates. From 2,205 prewar marks, 16 marks were returned in this 1957 to 1960 period, whereas of only 262 marks put in after the war, 8 were returned. Presumably the situation regarding loss of marks at the time of marking was the same for the two situations. Also we are comparing the returns at the same time under the same conditions, so the return conditions for these two sets of marks were certainly the same. If we got 8 back from 262 whales marked after the war and 16 from prewar marks, the same number of returns would have been obtained if 16/8 x 262 = 524 whales had been marked after the war. The lower returns from the prewar marks must be due to mortality of these whales between the two tagging periods. This total mortality is equal to 1 - 524/2,205 or in terms of mortality coefficients 1.43, as shown in the table. This is the total mortality during 20 years between the two periods of marking, and gives a mean annual mortality of 0.07, in fair agreement with other estimates.

Given the estimates of population size, and the age composition, it is possible to calculate the number of the recruits each year, and express these as a proportion of the parent population. These estimates of recruitment, natural mortality, and fishing mortality (or numbers caught) can be used to build up a model describing what happens to the population the results of which are illustrated in Table 8G (from Chapman, 1971). We have a certain stock size. We lose so much from catches, giving the stock at the end of the season, allowing also for natural mortality and adding on the recruits from the parent stock so many years back, the stock size at the beginning of

Table 8F. Comparison of the percentage return in 1957 to 1960 of whales marked before and after the war (from IWC, 1964).

Area			Prewar	Postwar	Ratio of %	Zt	Z
II	N	Number marked	2205	262			
	n	Number returned	16	8			
		% returned	0·726	3·053	0·238	1·43	0·071
III	N	Number marked	749*	300			
	n	Number returned	21	31			
		% returned	2·80	10·33	0·271	1·30	0·065
Other Areas	N	Number marked	289	276			
	n	Number returned	3	54			
		% returned	1·038	19·56	0·053	2·94	0·147
Total	N	Number marked	3243	838			
	n	Number returned	40	93			
		% returned	1·233	11·10	0·111	2·20	0·110

*Marking in 1934/35 and 1935/36 seasons only

Table 8G. Reconstruction of Area II-V blue whale stock, 1933/34-1957/58 (from Chapman).

Season	Initial Stock Size ('000's)	Catch '000's	End of Year Stock Size ('000's)	Recruitment ('000's)	Stock Size Beginning of Next Season ('000's)
1934	100.0	17.3	79.4	8.0	87.4
1935	87.4	16.5	68.1	8.0	76.1
1936	76.1	17.7	56.1	8.0	64.1
1937	64.1	14.3	47.8	8.0	55.8
1938	55.8	14.9	39.3	8.0	47.3
1939	47.3	14.1	31.9	7.0	38.9
1940	38.9	11.5	26.3	6.1	32.4
1941	32.4	4.9	26.4	5.1	31.6
1942	31.6	.1	30.2	4.5	34.7
1943	34.7	.1	33.2	3.8	37.0
1944	37.0	.3	35.2	3.1	38.3
1945	38.3	1.0	35.8	2.6	38.0
1946	38.0	3.6	33.0	2.5	35.5
1947	35.5	9.2	25.2	2.8	28.0
1948	28.0	6.9	20.2	3.0	23.2
1949	23.2	7.6	15.0	3.1	10.1
1950	18.1	5.0	12.6	3.0	15.6
1951	15.6	6.3	9.3	2.8	12.1
1952	12.1	5.0	6.8	2.2	9.0
1953	9.0	3.6	5.2	1.9	7.1
1954	7.1	2.6	4.3	1.5	5.8
1955	5.8	2.0	3.6	1.2	4.8
1956	4.8	1.1	3.6	1.0	4.6
1957	4.6	.7	3.7	.7	4.4
1958	4.4	1.1	3.2	.6	3.8

the next year and so on. This gives a description of what has been happening. Also running on into the future, it can predict what will happen under various patterns of exploitation.

In conclusion, we may say that though none of the various methods applied is very good, several of them are reasonably independent, and give more or less the same answers. We can therefore be reasonably satisfied that we are understanding what is happening to the whale stocks. There is good agreement between the scientists about the broad details. There is less agreement on fine details, such as the precise value of the present population size and of the net recruitment rate. These disagreements about details are becoming important to the Whaling Commission now that the catches set by the Commission are close to the estimates of sustainable yield. The quotas have been brought down from the earlier excessive levels of two or three times the sustainable yield, and the Antarctic whale stocks are no longer declining rapidly. The range of uncertainty concerning the present sustainable yields are some hundreds of animals, but now that the quota is within this range, the imprecision in the estimates means a doubt whether the stocks are being allowed to rebuild slowly, or whether they are still being depleted, though very slowly. Better estimates are therefore needed.

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Chapter 9

HARP SEALS: ESTIMATION OF STOCK SIZE. SUSTAINABLE YIELD FROM NON-EQUILIBRIUM STOCK

There are a number of species of seal in the Northwest Atlantic but the one supporting the main commercial fishery is the harp seal. The major commercial catches of this species are of the newly born pups from the breeding patches on the ice. As always in any population, we have to see what different stocks are being exploited.

There are two main concentrations of breeding animals: (1) the animals that come into the Gulf of St. Lawrence and breed on the ice inside the Gulf, the Gulf herd; (2) animals that breed on the ice along the so-called front at the edge of the open Atlantic off the north of Newfoundland and off Labrador. The Gulf herd is exploited entirely by Canadians, and the Front herd both by Canada and by Norway. Sealing vessels from both Canada and Norway go into the ice after the seals, but the Canadian catches include seals killed by landsman from the small communities along the Newfoundland and Labrador coasts.

In addition to the main fishery on these pups, catches of other ages of animals, both mature and immature, are taken at various times and places. During the summer the seals migrate northwards and some are taken along the coast of Greenland by the local population and small numbers are taken in the Canadian Arctic, though most of the seal catches in the Canadian Arctic are of other species. It seems therefore that there might be two distinct and independent stocks, the Gulf stock and the Front stock, but an interchange of tagged animals between these breeding groups and some other evidence suggests that they are probably mixing to some extent. It may be that once an animal starts to breed, it breeds either in one area or another and sticks there, but there is some interchange of younger animals. For most purposes, we can take as our first working approximation that there is just one population of seals breeding in the Northwest Atlantic.

The other thing that we do at the beginning, apart from stock separation, is to look at the catches. These are shown in Figure 9A. In this figure the histograms show the average annual catches of pups from 1820 onwards except for the blank period between 1920 and 1940, where the data are missing. There were up to 300,000 pups taken each year for quite a period in the middle of the last century. Then catches dropped off rather steadily. Also shown in the figure are the estimates of population size circles which are reasonably good for the period from 1950 to 1970. This diagram includes the forecast of where we might go in the future from 1980 onwards.

An important point is that there was a decrease in population from 1950 onwards and probably an increase in the 1940's after there had been low catches during the war. It is also fairly certain that there was a decrease from the beginning of last century when there might have been perhaps 1 million breeding females. Though it is possible to consider the entire population of harp seals in the Northwest Atlantic, it is for many purposes more convenient to think about the population of breeding females because these are the ones that produce the pups which are the main harvest. They are the easiest group within the population to estimate.

As always there are a number of different ways we could go about estimating the total population of seals. One of them is tagging. This can be used to obtain an estimate of the number of pups, which will be almost exactly equal to the number of breeding females. The method is to go into the breeding patches just before the sealers go there, and to mark a large number of pups. The usual Petersen or Lincoln index method can be applied to the returns of these marked pups in the weeks immediately following, when the commercial harvest takes place. It is difficult to get an accurate estimate from this method because there isn't much mixing between the time of marking and the time of

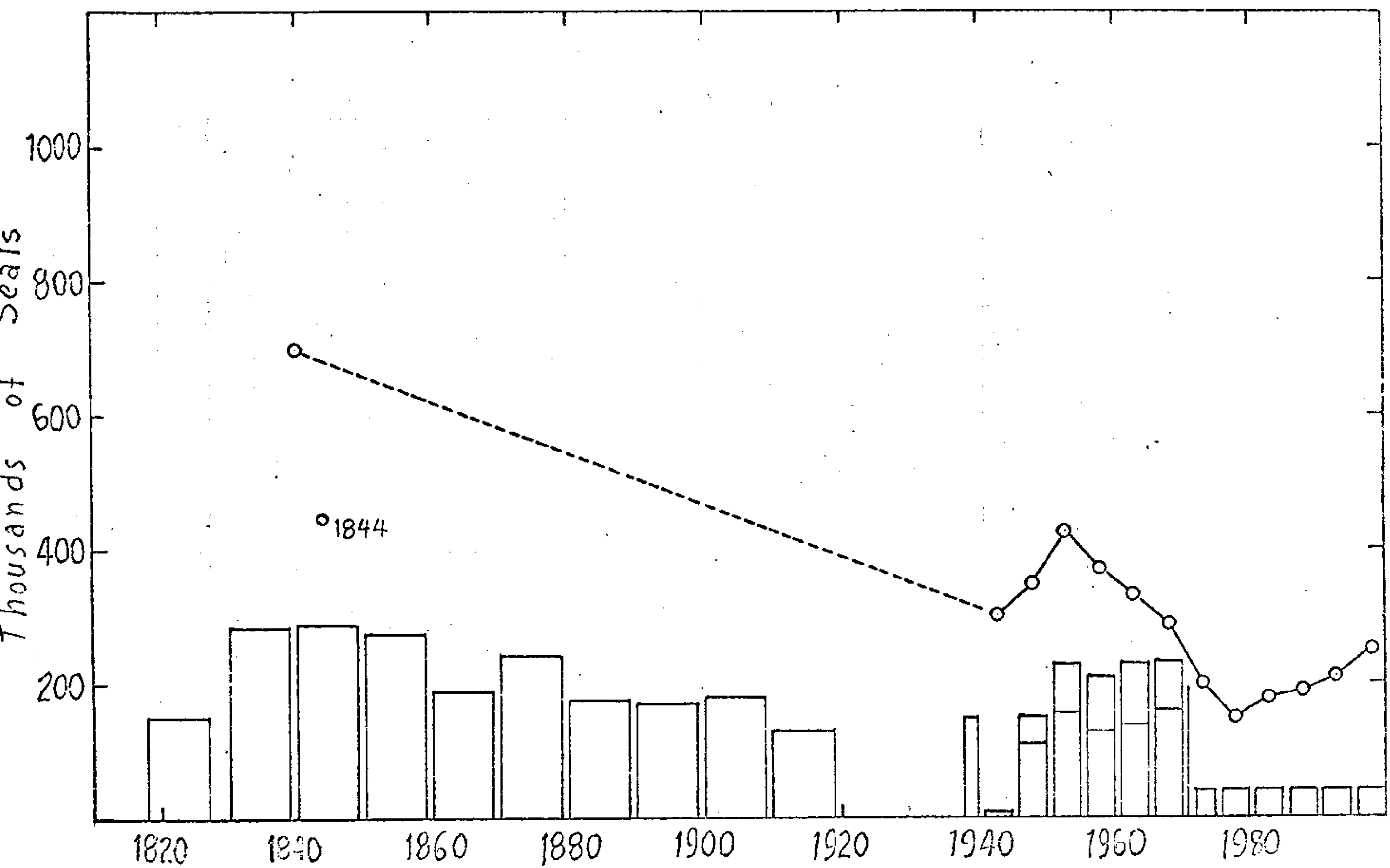


Figure 9A. Summary catch records of harp seals in the Northwest Atlantic, and estimated trend in the population (from Ricker, unpublished).

recapture. Either the sealers go into the patch of seals that we marked and we get about 100% returns, or they miss the patch of seals and we get no returns.

The ability to spread the marks through the population is limited by the short period between the time the pups are born and the time the open season starts. In the Gulf of St. Lawrence in the winter, it is not very easy to get about, and it is not very easy to spread tags during these few days. Another way of counting the pups or the females is by aerial survey. We can fly over these breeding patches and see the seals on the ice and in theory count them in the same way that we can count whales from a survey ship. It seems that the seal pups are in rather confined patches, rather than evenly spread all over the ice and if we can find these patches, in theory it is not too difficult to fly over them with a camera and photograph the seals on the ice. The total number can either be counted directly or estimated from the number of seals per unit area and the total area of these patches. In practice the weather may interfere and make a photographic survey impossible. Even when the survey can be made, it is not certain we are seeing all the seals. They may be on the ice and we miss them, or more likely they are down in the water and off the ice.

However, on the whole this gives a reasonable first estimate and also a figure that will if anything be an underestimate. If we counted 100,000 seals, there must have been at least 100,000 there, though we are never quite sure whether it might have been 150,000 or upwards. But the method for estimating this population that has been most promising in terms of giving a good numerical estimate and one that we can have some statistical confidence in, is based on a type of approach common in game animals, which is to watch the change in ratio when one particular group of animals is selectively removed.

In terms of game animals, the usual thing to observe is the change between males and females, before and after a hunting season when the hunting has been almost entirely on males (Paulik and Robson, 1969).

The technique used here is to look at changes in different year-classes. The principle here is that because of the short season and the bad weather, the catches can vary quite greatly from year to year. If the weather is good and the ships find the patches, the proportion of pups caught can be extremely high. On the other hand if the sealers can't find these patches during the short open season, the proportion removed may be small. These changes in the number of pups that survive can be observed in the age composition in later years. If a large proportion of a year-class is removed, there will be a hole where that year-class ought to be in any subsequent age composition. The difficulty in applying this to the harp seals is that until they mature at about six or seven years, it is very difficult to get a representative age composition of the population. One sample from an area may consist mostly of young animals and be dominated by 1-and 2-year olds. Another sample at a slightly different time or place may consist mainly of 3-and 4-year-old animals, or of adults. So it is not easy from a single sample to find out which are good or bad year-classes. But what we can do is to compare from year to year under fairly uniform conditions of sampling, the proportion of, say 1-year-old animals in the samples.

Table 9A sets out some of the basic data. The animals were sampled in two places, at St. Anthony and on the front icefields. The one-year-old animals were relatively more common in the latter area, but the relative changes from year to year in the ratio of 1-year old to older animals is the same in both areas. Neither ratio tells much about the absolute value of the ratio of 1 to older animals in the population, but changes in this ratio from year to year might be expected to be proportional to the changes in the ratio in the population. These changes are clearly related to the catches of pups. Denoting the number of pups born by N, catch of pups by C, actual proportion of 1-year-old animals in the population by P, and observed proportion by p, we have

$$P_1 \propto (N_1 - C_1) \text{ and } p_1 \propto P_1 \quad \text{or in terms of two year-classes}$$

$$\frac{N_1 - C_1}{N_2 - C_2} = \frac{P_1}{P_2} = \frac{p_1}{p_2}$$

The assumption may be made that, for a population of long-lived animals and not a very high total mortality on the adults, the number of pups born in the two years shown will be equal.

$$\text{i.e.,} \quad \frac{N - C_1}{N - C_2} = \frac{p_1}{p_2} \quad \text{or} \quad N = \frac{C_1 p_2 - C_2 p_1}{p_2 - p_1}$$

This is easily extendable. Instead of using a pair of years, we can use a series of years and plot the index, p, against the removals. This looks rather like the DeLury plot. The more we remove, the lower value of P, hopefully the points will lie on a nice straight line, and the intercept on the x-axis will occur when there are no survivors of that year-class, i.e., when we have removed the lot. The intercept is therefore equal to the total production of pups. Again, like the DeLury method as applied to whales, once we have a first estimate, we can make some corrections. Once we know the population and the removals, we can get some idea of how the population changes and we don't necessarily have to assume that the adult population in the two years is the same. We can make some corrections for the fact that it must be changing.

Table 9A. Age 1 and age 4 and older groups in the samples taken at St. Anthony and the Front icefields (from Ricker, unpublished).

Year-class	Total catch (thousands)	St. Anthony sample: Age 1	St. Anthony sample: Age 6 and older	St. Anthony sample: Ratio	Front icefields sample: Age 1	Front icefields sample: Age 6 and older	Front icefields sample: Ratio
1966	257	18	261	0.069	176	377	0.467
1967	280	7	165	0.042	84	332	0.253
1968	158	87	68	1.280	62	33	1.879
1969	235	41	434	0.094	105	186	0.565
1970	217	39	285	0.137	-	-	-

This now gives us an estimate of population size. Altogether three methods of estimating population size have been used--from tagging, from aerial survey, and from the changes in year-class strength. It is rather cheering that the figures came out in reasonable agreement. They all tended to come out to about 300,000 pups in 1971 with rather higher estimates for earlier years. While there are no meaningful measures of catch per unit effort in this fishery from which to study changes in abundance, the subjective opinion of the sealers and of the biologists working closely with the industry is that the seals are becoming scarcer. This agrees with the changes in the estimated population numbers. Given the population size, the question that industry and government ask next is how many of these can or should be removed? This is not a completely defined question.

The number that should be removed cannot be determined until the objective is defined. In the case of seals it is a rather open question what the long-term objective should be. The long-term objective of seal lovers is presumably that no seals should be killed, and this would result in a very large seal population. The long-term objective of the fishing industry or the marine resources industry might be rather different. These seals eat a large quantity of fish, many of which are of present or potential commercial value. Also several species of seals are an intermediate host for parasites that spoil the value of the fish. The more seals we have, the less fish we will have and the less valuable the fish that we do catch will be. Thus the fishing industry would be in favor of reducing the seal population to a low level. The sealing industry long-term objective should be a population giving a high sustained yield, intermediate between the population sizes that might be desired by fishermen or conservationists.

In the absence of other guidance, a reasonable objective for present management of the seal herd might be to maintain the status quo, i.e., to take the sustainable yield. This leaves the problem of estimating the value of the sustainable yield.

Suppose we have a number of females, N, then the number that are dying in a year will be $N \cdot m$, where m is the total death rate. In the steady state this number must be balanced by young females coming into the breeding population. Of the existing N females a proportion, q, will produce pups during the year, and of these qN pups qN - C, where C is the catch, will survive the first few weeks, and of those a proportion p will survive the 7 years or so until maturity. That is the total number of adult animals produced. Of those almost exactly half will be females, so the condition for a sustainable yield is that

$$mN = 1/2\ (qN - C)p \qquad (9.1)$$

or

$$C = \frac{pq - 2m}{p} N \qquad (9.2)$$

This gives a formula for the catches that we can take in terms of the adult population, the mortality on the adult population, the proportion of the adult females that produce pups, which is fairly high in the seals (about 0.9) and the proportion of the pups that having survived the initial harvest will then survive to maturity. We have an estimate of N, so that all that is needed to be able to estimate sustainable yield and advise on how many pups can be caught is an estimate of these two mortality rates.

Figure 9B shows how the total mortality rate in the adults may be estimated by plotting the numbers on log scale against age for two sets of samples, one taken in 1968 and one taken in 1963. One of the difficulties in this type of catch curve is that these are fossil mortalities applying to some period in

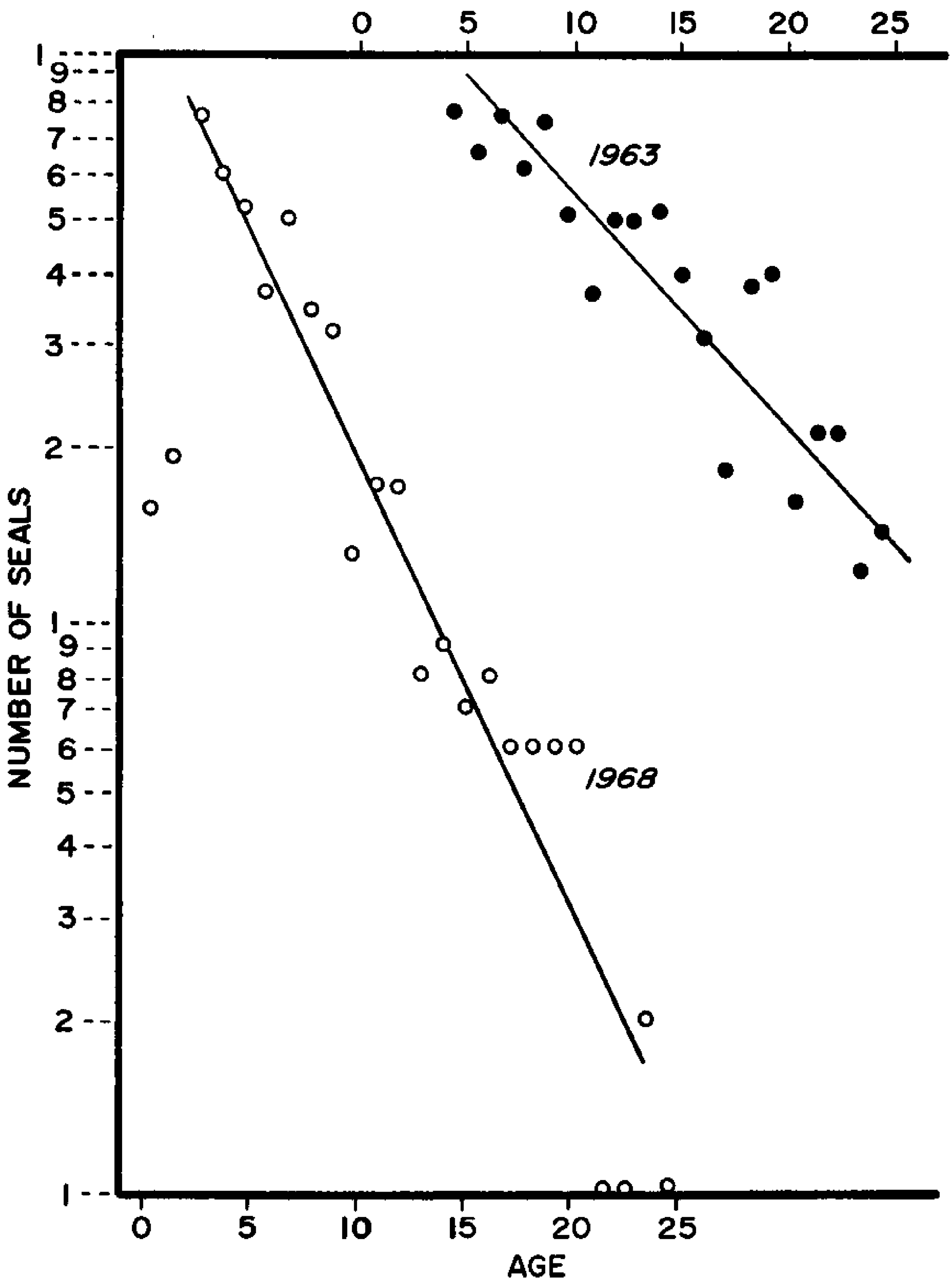


Figure 9B. Estimation of mortality rates in the harp seal (from Sergeant, 1968).

the past. The second and more serious problem is that the initial numbers of animals in each year-class should be the same, and we have good reason to suppose that the initial numbers have been changing and that the numbers have been falling, particularly in recent years.

Despite this, catch curves such as those in Figure 9B give us our first estimate of total mortality. The 1963 samples showed survival of approximately 90% or a 10% annual mortality, while in 1968 samples appeared to be 83% or the mortality 17%. The difference is probably not meaningful, and is a matter of sampling. The older animals in the 1968 sample are from the same year-classes as in 1963, and should therefore exhibit the same slope.

It is rather difficult to get a more accurate estimate of the total mortality in this stock. Sampling is limited by the difficulties of getting samples, and the age determination, which is done from the teeth, becomes increasingly unreliable with older animals. Still this does give us some estimate of survival rate, or mortality rate among the adults.

We also can get some estimate of the survival from pups to adults by using the age composition to know how many recruits are coming in each year, and also knowing how many pups survived the pup harvest seven years earlier. Table 9B gives the sustainable yield of pups from a female population of 300,000 adults assuming that 90% of them produce pups each year, calculated for different values of pup survival ranging from 30 to 60%. The first thing we see is that if the survival of pups is low and the adult mortality is high, there is no sustainable yield--whatever we do about the pup harvest the population is going to decrease. This is not utterly improbable, though clearly the population could not continue in existence if the natural death rates were as high as this. However the mortalities in the table include the effects of hunting of

Table 9B Sustainable yield of harp seal pups (thousands) from a population of 300,000 adult females (adapted from ICNAF, 1971).

Adult mortality	Survival of pups to maturity			
	30%	40%	50%	60%
8%	110	150	174	190
10%	70	120	150	170
12%	30	90	126	150
14%	-	60	102	130
16%	-	30	78	110

adults and juveniles, and this could be appreciable.

The range of values in the table can be considered in two ways. One is that there is great uncertainty concerning adult mortality and pup survival so we could possibly be anywhere in this table, but probably under present conditions the adult mortality is 12 to 14% and the pup survival 40%. This would allow a sustainable harvest of 60 to 90,000 pups assuming the adult and juvenile mortalities do not change. However the table could also be considered as showing the effect of changing these mortalities, which would be possible by controlling the hunting of adults or juveniles. Though the main harvest is of the pups in the breeding patches, appreciable numbers of both adults and juveniles are also taken. For instance, if at the present there is a 12% adult mortality and a 40% juvenile survival, by eliminating the hunting of adults, the adult mortality might be decreased to 10%. Also the survival of pups could be increased by reduced hunting, perhaps to 60%, so that we could increase our pup take from 90,000 up to 170,000 animals. In this way, we balance out the reduced harvest of older animals with the increased harvest of pups, and if we wish, we can express the take of adults or juveniles in terms of pup equivalents.

It is easy to show that removing one adult from the population is more harmful than removing one pup so that an adult is perhaps three pup equivalents and a juvenile perhaps 2 pup equivalents. This is of some significance to the industry because in terms of the value of the skins, the pups are more valuable than adults or juveniles, which is really quite fortunate. There is no real conflict of short-term and long-term interests over which ones to take; for both maximum value of the immediate harvest and long-term benefit of the stock, the harvest should concentrate on the pups.

When these figures were presented to the ICNAF, at its last session, showing how many pups could be taken without further depleting the stock -- on the order of perhaps 90,000 downwards--it was a bit of a shock to the industry. Recent catches have been of the order of 200,000 and up, and being told to go down to 90,000 or lower is not the sort of thing that sits well. It was very difficult to get any agreement, but it was suggested that a compromise solution might be considered. This might be to make some firm commitment to go down in a succession of steps to reach the sustainable yield level within some fixed period of years. Within such a commitment, there could be a wide choice of exactly how fast to come down. Some of these choices are shown in Figure 9C. In this diagram there are two sets of curves, the upper one being the population, and the lower one being the catches. Up to 1971, is pure history; from 1971 onwards, these are estimates of how we might go.

So far as the population is concerned, we have very little choice up to 1978. The number of pups killed in the future has no effect on the breeding population until 1978. All the 1978 breeding population is now alive and there is nothing we can do about it so far as the pup harvest is concerned. The population is known up to 1971 and is virtually fixed up to 1978. One strategy is to manage the pup harvest so that the breeding population from 1978 onwards stays constant. This is strategy E. Under this strategy, the catches have to be reduced very sharply immediately and to be further reduced in the next few years as the population declines, because to ensure a constant adult population after 1978, a constant number of surviving pups must be left from a decreasing number born between now and 1978.

The other strategy illustrated is to consider progressive reductions in the catch of 20,000 each season, stopping at different levels, for policy A at 100,000 pups, for B at 80,000, C at 60,000 pups, etc. Under all these

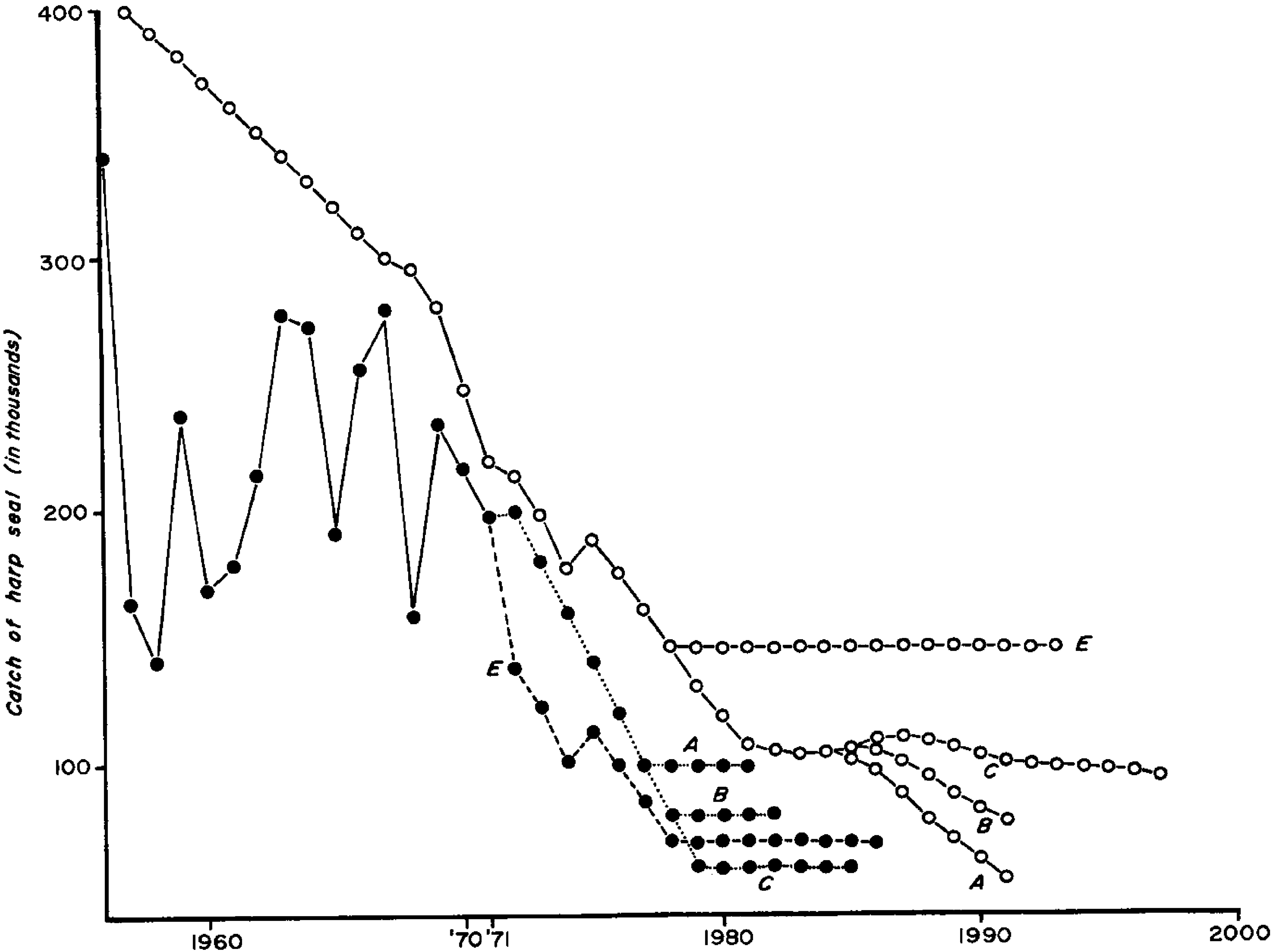


Figure 9C. Projected trends in harp seal catches and population sizes under different management regimes.

policies, the population declines at first and will continue to decline past the level maintained by policy E. If the reduction in catches is continued long enough, the decline in the population will slow down, stop, or even be reversed. Under policy A the reduction in catch will be stopped too early, and the decline in population will continue. So it will under policy B. Under policy E, holding catches at 60,000 per year, population fluctuates with at first no very obvious decrease at around 100,000 animals; but the adult population required to maintain indefinitely a yield of 60,000 is rather over 120,000 females. Under this policy the population will decrease though for some time only slowly. The catches will have to be reduced to around 40,000 animals to bring them to a level that can be maintained indefinitely.

Other policies, such as coming down steps of 30,000 or 40,000, can also be examined. What we find is that the slower the reduction in catch, the lower will be the population and the sustainable yield when we finally reach an equilibrium position. By coming down in steps, rather than immediately to the sustainable yield level, the long-term interest has been sacrificed to the short-term interest of taking larger catches in the next two or three years.

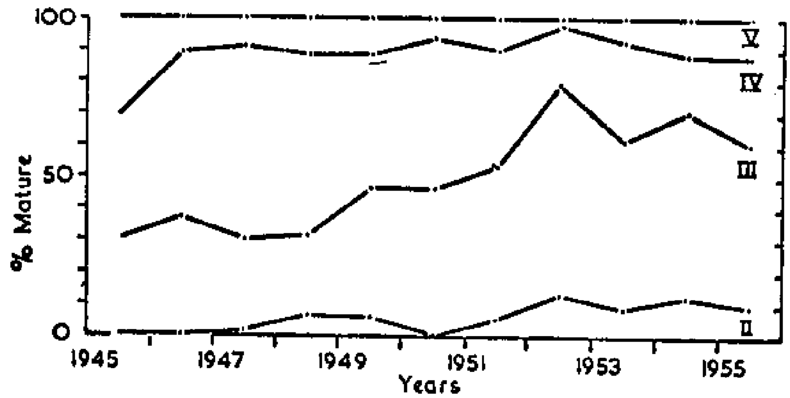
In many ways, this is just like whale situation. There was the same problem of a declining population and a belated realization that the catches have become way out of balance with the amount the present stock can maintain. Catches had to be reduced very sharply, and the industry concerned found it difficult to agree. This led to delay and the situation had become worse. In the end, the industry, whether it is on seals or fin whales, is stabilized in a very much worse situation than might have been achieved if there had been, earlier and more quantitative scientific evidence, and particularly, when good evidence did come forward, quicker action on that evidence. Now to return to some more general comments on population dynamics. So far we have really only been juggling around with some figures concerning the exploited stock. We haven't looked very thoroughly at the dynamics of the stock itself--just how it manages to stay where it is without either going into extinction or becoming so common that we can walk across the Atlantic on harp seals' backs. We can start looking at some of these problems in relation to equation (9.1), for a sustainable yield. This states that the population is stable if the number of mature female seals dying is equal to the number of young females reaching maturity for the first time. Or, writing $\bar{M}$ as the mean mortality rate during the immature stage, which lasts a period T years, equation (9.1) becomes

$$mN = 1/2\ (qN - C)\ e^{-\bar{M}T} \qquad (9.3).$$

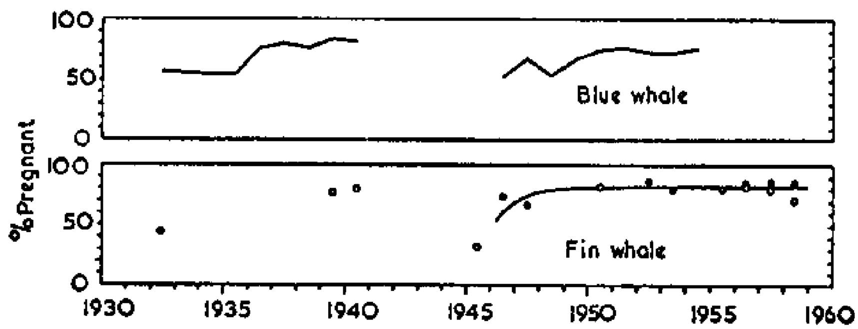
As shown in Table 9B, this equation has a solution with a positive value of C under most likely conditions of sealing, i.e., there is some positive catch that can be taken without depleting the stock. Now suppose we stop sealing altogether. This will then give C = 0, and the right-hand side of equation (9.3) will be bigger than the left-hand side, and the number of adult deaths will be less than the number of young ones recruiting to the adult population. The adult population will continue to increase at a constant rate, in the absence of exploitation, unless one or more of the parameters, m, q, $\bar{M}$ or T, in equation (9.3) change. Similarly, the condition for a steady state in whales is that the fishing mortality F, should satisfy $F = r - M$, where r is the recruitment rate, and can be written as $r = 0.5 \cdot q \cdot e^{-\bar{M}T}$, virtually the same as the equation for seals. In the exploited population $\tau > M$, and if catching is stopped the population will increase and continue to increase until one of the parameters changes.

Obviously something in this system must change, and it would be very satisfying if we could make some observations and see. So far as seals are concerned, there is not much evidence of this, but the problem is that this stock has been exploited for a longtime, and many of these changes will already have taken place, and cannot now be detected. In addition to the existence of any change, the pattern of changes with population is of interest. The net rate of increase is equal to r - M. If we plot r - M against population size, it should be 0 at maximum population size, and high at low populations. A straight line sloping downwards would be the simplest relation. If the ratio is such a straight line, this gives the logistic growth pattern for the population, and the parabolic relation between sustainable yield and population we get from the Schaefer model. But there is no particular reason why it should be a straight line. There is considerable opinion that M doesn't change very much, perhaps increases rather slowly with population size, and that r falls rather quickly from its value in the unexploited population, as the population is initially reduced from this level, but thereafter does not change much. The plot of r - M against population is not then straight, but is a curve concave upwards. The curve of sustainable yield against population will then not be a parabola but be skewed over to the right, like the observed curve for blue whales in the Antarctic (Figure 8B). For fin whales, there is some biological evidence of changes taking place. Biological data of fin whales were being collected in the fairly early period in the 1930's when the stock wasn't greatly depleted. From these data Laws (1962) has demonstrated changes in the mean age at maturity, (an increase in the proportion of 3- and 4- year-old animals that are mature) and an increase in the percentage of mature pregnant females in the catches (Figure 9D).The latter change occurred in both the blue whales and the fin whales, though a rather unexpected thing seems to have happened that doesn't fit too well. The pregnancy rate in both species increased before the war, when certainly the population was decreasing, but seemed to drop down again during the war to a lower level in the immediate postwar period, even though there was, on the basis of our analysis, not much change in population between 1939 and 1946. But in general the changes are in the right direction. We are getting more recruits per female when the population is low, though the recruitment rate does not seem to have changed since 1950 even though the fin whale stock has declined much further. This gives us some feeling that we can explain what is happening to these mammal populations, not only what is happening as a direct result of exploitation, but also some of the reactions within the population.

We still do not have any satisfactory reason why it should mature earlier or why there should be a higher pregnancy rate, and this means going on into more information on the behavior of the animals, on their food supply, etc. But at least we feel we are getting somewhere. We are also getting somewhere in relation to the management of these stocks and are getting some confidence in the governments and the industry that scientists and population dynamics experts know what they are talking about. This was helped in the case of whales by one lucky accident back in 1964 when there was argument about the quota. The catches in the previous year had been nearly 19,000 fin whales. The industry was willing to cut the quota down but only to the equivalent of about 16,000. We said that would not be helpful because the most that would be caught with the existing equipment was 14,000. Nevertheless the quota was set at the equivalent of some 16,000 whales. The actual catch was almost exactly the 14,000 predicted.



—Changes in the percentage of sexually mature female fin whales in different baleen age groups.



—Variations in the relative fertility of blue whales (all areas) and fin whales (white, area II; black, areas III and IV).

Figure 9D. Changes in maturity and pregnancy rates of Antarctic whales (from Laws, 1962).

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Chapter 10

PERUVIAN ANCHOVY: DEFINITION OF EFFORT; PROBLEMS OF STABILITY

In discussing the anchovy fishery of Peru, let us look first at the catch statistics, (Figure 10A) rather than at the background of the fishery, because in this fishery the catch statistics have a special significance. The first significance is the sheer magnitude of the fishery. Present catches are of the order of 10 million tons, making this the biggest single species fishery in the world in terms of weight. Perhaps this is slightly misleading on a world scale because this is not a particularly valuable fish. All the catch goes for reduction to meal and oil, for a price of around $15 a ton. In terms of the value of the catch and the number of boats and complexity of the fishery, the cod fisheries in the North Atlantic are bigger.

However, the anchovy fishery has an influence in Peru which is different from the influence of fisheries in virtually any other country, with a few exceptions such as Iceland. Roughly speaking the Peruvian catches are 1 ton of fish for each person in Peru, man, woman, and child. This may be compared with catches in some other areas such as Indonesia, where the catches there are roughly 1 ton of fish per fisherman. Even at $15 a ton, 1 ton of fish for every single Peruvian has an important impact on the economy.

The other thing that should be emphasized is the rate at which this fishery developed. In 1955 catches were only a few thousand tons and barely visible, as shown in Figure 10A. After 1955 the catches rocketed; between 1958 and 1962 the annual increments were approaching 2 million tons. In other words, the increase in Peruvian catches from one year to the next over this period was greater than the total annual catch of nearly every other fishing country in the world. After 1962, it tended to flatten out.

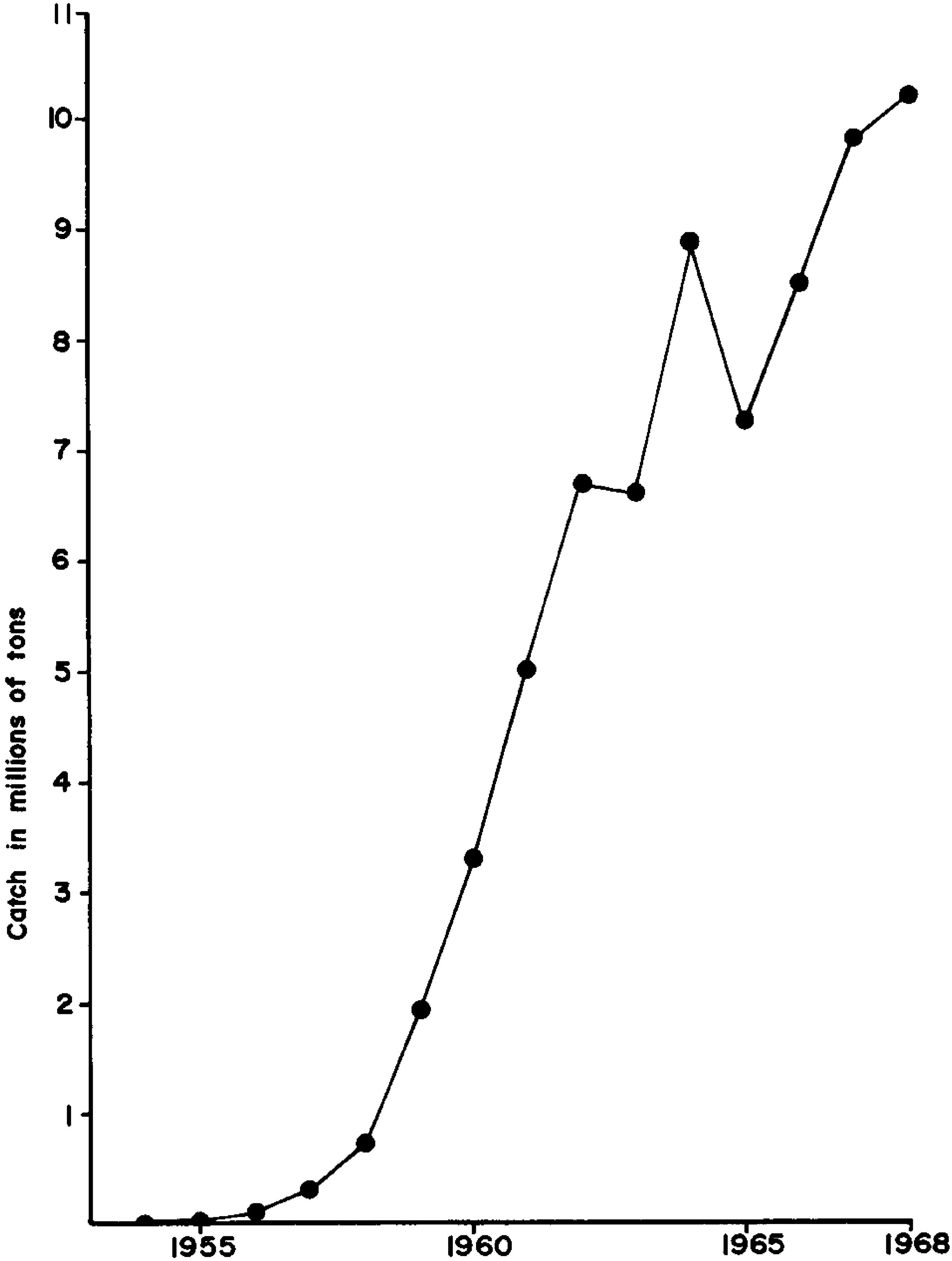


Figure 10A. Total landings of anchoveta in Peru.

Up to now we have considered fish stock in a vacuum, as a mass of fish swimming around the sea, without considering their environment. The only thing that has been looked at has been the impact of fishing on the stock, but the main determinants of the general abundance of a stock of fish are factors other than fishing. The Peruvian anchovy fishery takes place in one of the richest areas in the sea. It is in an upwelling zone where the cold Humboldt Current coming up the west coast of South America tends to swing away from shore and cold, nutrient-rich waters from the subsurface zone move up to the surface. This gives high primary production of plants all along the upwelling zone from Peru down into northern Chile. Similar systems occur elsewhere, off California, Northwest Africa and Southwest Africa, but the phenomenon is most marked and most productive off Peru.

Figure 10B shows some observations of primary production in the Peruvian area during two surveys by Peruvian research vessels. We note that particularly in the spring, there is very high production in the inshore area, becoming less farther offshore. The other thing we note is that the primary production is patchy, that this is not really a simple uniform upwelling system with the pattern of primary production the same all over the area, but there are variations within the area. The phytoplankton is one food element of the anchovy, and is also eaten by zooplankton, which in turn are part of the food of the anchovy. The distribution of anchovy is therefore directly or indirectly governed by the distribution of plants.

The distribution of fish can be examined by a number of methods, other than by fishing. Figure 10C shows the distribution of anchovy eggs and anchovy larvae. Both indicate thick dense populations of anchovy along the coast. The

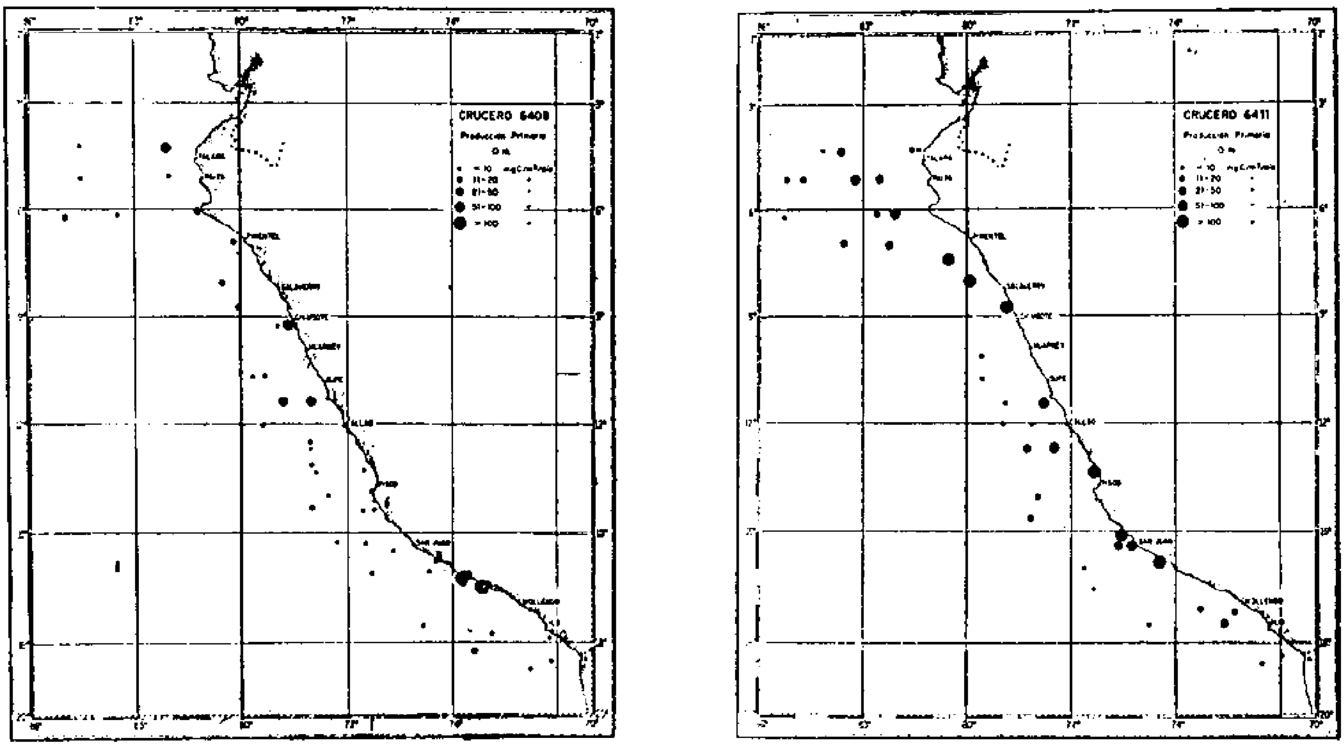


Figure 10B. Distribution of primary production off the coast of Peru in winter and spring 1964 (from Guillen and Izaguirre, 1968).

distribution of older fish is shown in Figure 10D, which gives the distribution of echo traces, most of which will be of anchovy. The echo traces are patchy and again are concentrated in the coastal area. Figure 10D also shows the distribution of one of the predators of the anchovy--birds.

This gives us a general qualitative picture of the system in which the anchovy lives. In theory it could be turned into quantitative terms to estimate how many anchovy we would expect to get, assuming that anchovy is the main consumer in this system. The observations of primary production are in terms of carbon fixation per unit volume. From this the primary production per unit surface area can be determined. Multiplying by the total area gives the total primary production. This can be multiplied by the efficiences of conversion of plants to anchovy, which though not quite the ecological efficiency, is something like it, and converted from carbon to wet weight to give the production of anchovy.

This calculation has been done by Cushing (1969), who got 20.15 million tons, and by Ryther (1969), who got 21.6 million tons. This seems splendid, especially when compared with the actual catches. One might expect that, allowing for the maintenance of a breeding stock and deaths by natural causes, the maximum catch that can be taken is somewhat over half the total production. The anchovy catches have indeed flattened out at around 10 million tons.

Unfortunately the details of the calculations are much less consistent. The equation for anchovy production is (primary production per unit area) x (area of upwelling) x (ecological efficiency) x (carbon/wet weight conversion) = anchovy production. Cushing's calculation is $236 \times 479 \times 10^3 \times 0.01 \times 17.85 = 20.15 \times 10^6$ and Ryther's $300 \times 60 \times 10^3 \times 0.12 \times 10 = 21.6 \times 10^6$ tons.

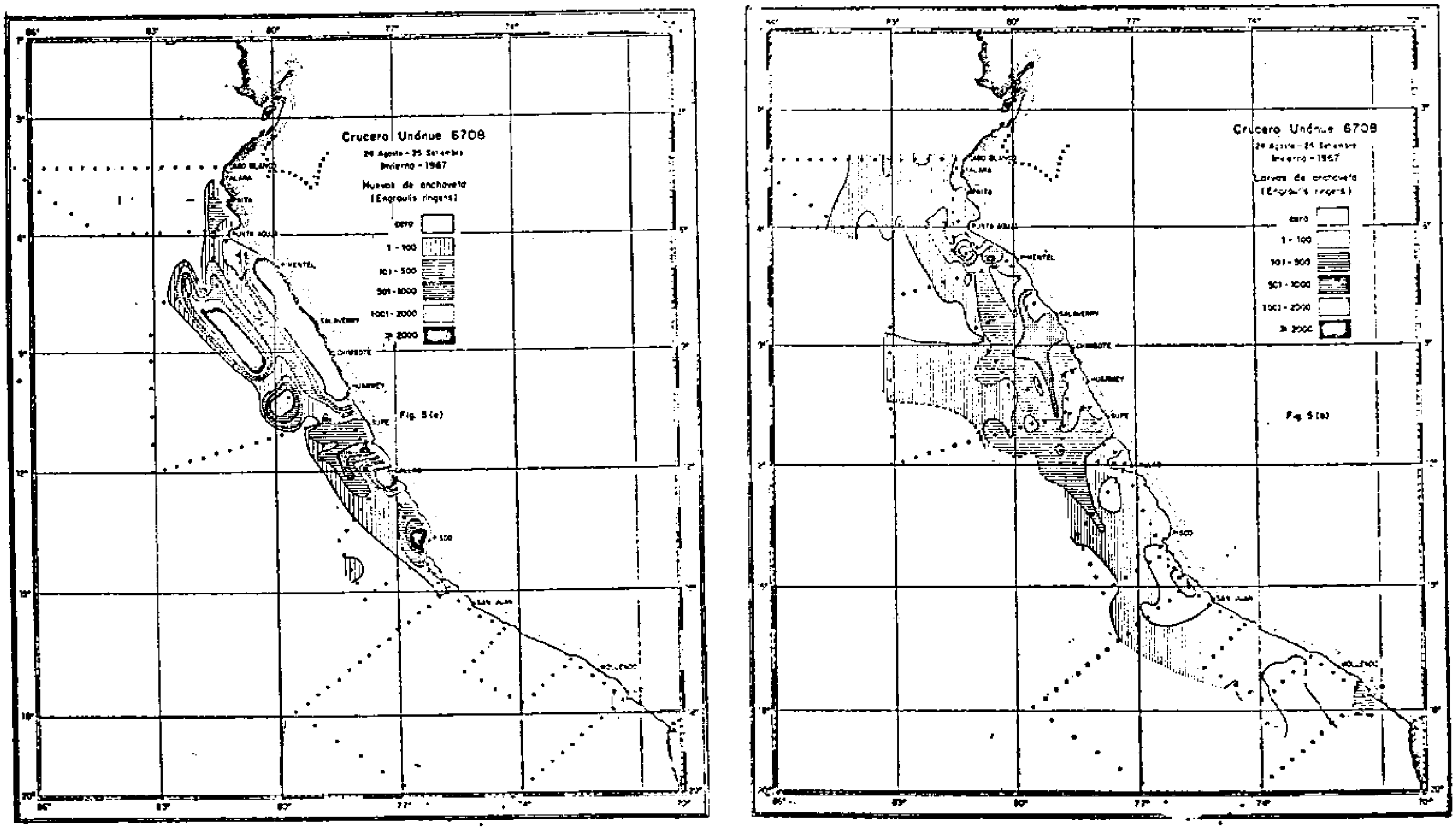


Figure 10C. Distribution of eggs and larvae of anchoveta in the winter, 1967 (from Zuta and Mejia, 1968).

The figures for production per unit area are similar, but the two authors used very different figures for the total area. The uncertainty is how far offshore the upwelling system extends. There was also considerable difference in the value used for ecological efficiency. One question is the number of links in the food chain. Do anchovy feed directly on plants, or is the main chain plants, zooplankton and then the anchovy, i.e., anchovy are mainly zooplankton feeders. The other question is the value of figures put in these links. Cushing thought that most of the anchovy's nourishment came from zooplankton and that there was a 10% ecological efficiency at each stage, giving an effective value of 0.01, whereas Ryther thought that anchovy got large proportions of food directly from plants, and that the efficiency in each link was about 20%. Hence he used an effective figure of 0.12.

This is a bit less pleasing. Instead of good agreement, there is considerable disagreement in details. If these two authors had changed around the figures they used, they would have come up with very different figures for anchovy production. Therefore it is not possible yet to start with the basic primary production and estimate very closely the likely catches of anchovy. However, by looking at what is governing the population dynamics of the plants in terms of nutrient supply, the rate at which the upwelling is occurring, the grazing on the plants by zooplankton and fish, and combining this with similar studies on the zooplankton and anchovy, there seem to be the beginnings of a description of a complete system. Soon it may not be necessary to deal with fish populations just in a vacuum and handle only the fishing mortality and the effects of fishing.

Similar calculations have been done in the North Sea in the 1960's in relating primary production to fish stocks, and there the books seem to balance

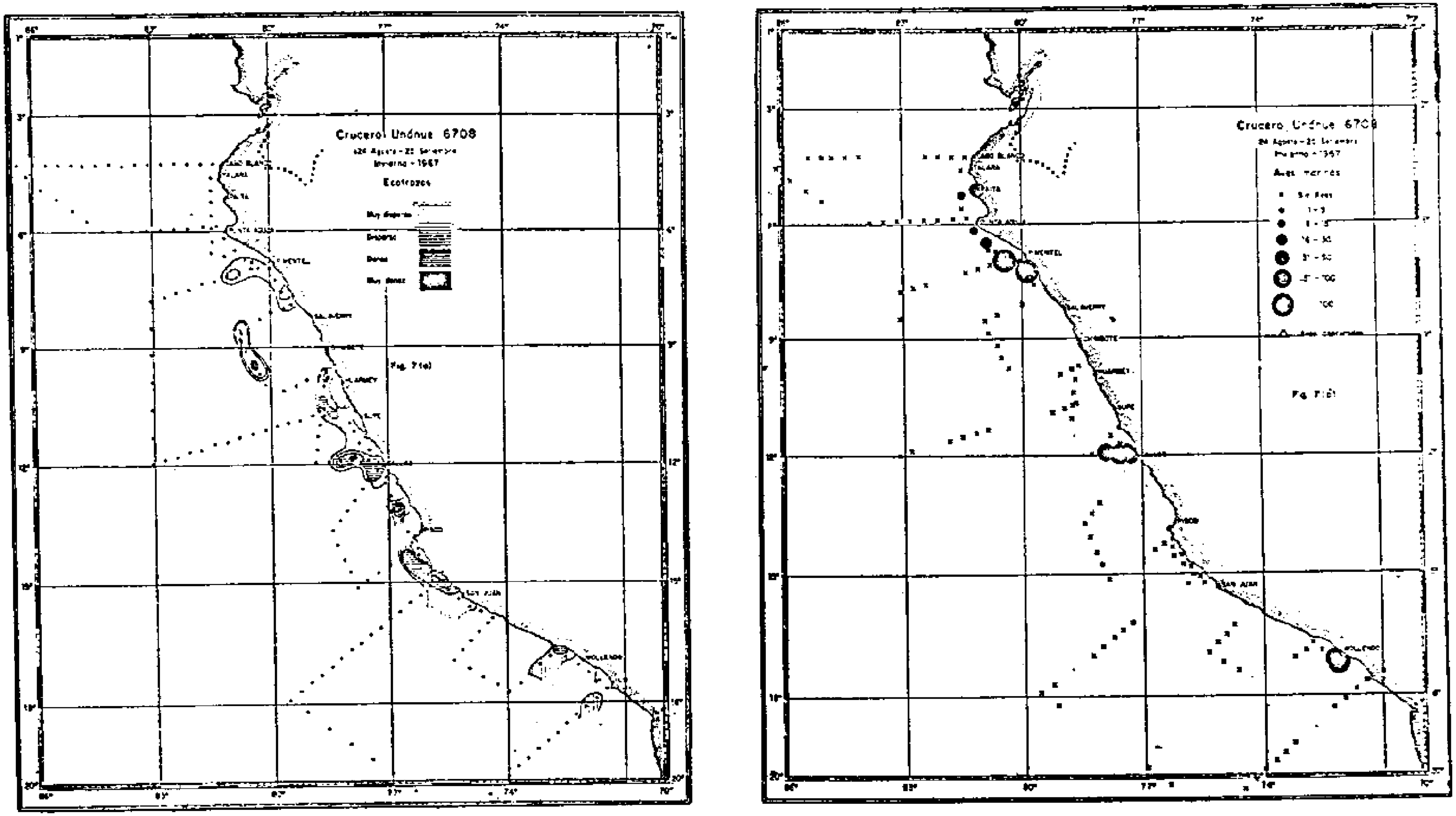


Figure 10D. Distribution of echotraces and of birds off Peru in the winter 1967 (from Zuta and Mejia, 1968).

reasonably well. The total primary production and the production of the commercial species of fish are reasonably well known. These fish are farther along the food chain than anchovy. They certainly aren't plant eaters. A lot of them aren't even zooplankton eaters, but are secondary carnivores. The fish are therefore at least two stages removed from the primary production. If the ecological efficiency is assumed to be 10%, then the fish production can be no more than 1% of the plant production; this is in fact about the value it is calculated to be from studies of the fish populations. This shows that much of the fish production is not more than two steps removed from the plants, and that the ecological efficiency is at least 10%. Further study of the North Sea situation requires more detailed and laborious studies of the production of the intermediate stages--zooplankton and benthos.

The basically simpler system off Peru, only dealing with plants, and to some extent zooplankton and anchovy, offers a hope of an earlier development of a quantitative descriptive model of the complete production process. Even the existing description, with the large difference in the detailed values used by Cushing and Ryther, may provide useful answers. Whatever figures for the area of upwelling and ecological efficiency are taken in the range given earlier, the production of anchovy must almost certainly be as much as a million tons, but not as much as a hundred million tons. If we were thinking of building up an anchovy industry from scratch, as in 1955, this could give us some guidance as to what was likely to happen. We would not expect anything serious to happen to the stocks while we were removing only say 100 thousand or half a million tons of fish, but something would happen to the stocks once we got up into several million tons of fish, as indeed happened.

Let's return now to the fishery and discuss first the separation of stocks. The anchovy and the fishery on it extend over a wide range from northern Peru down to northern Chile. They are small fish, and it would be unreasonable to expect there to be complete mixing of these fish within a generation over the whole length of coast. On the other hand, the fishery does operate more or less uniformly all along the coast, even though there are concentrations of plants and factories. The historical development of the fishery has been fairly similar in all parts. Though there may not be a one single well-mixed stock in Peru, if there are different stocks it is likely that the events in different stocks will be very much the same, so that usually the anchovy in Peru is treated as a single stock.

The other thing we must consider is the basic statistics of catch and fishing effort, and this is an occasion where FAO can take some credit. Very early in the development of the anchovy fishery, there was set up in Peru a project financed by the United Nations Development Program carried out by FAO to assist Peru in the study of the anchovy, and this included assistance in setting up a research institute at Callao. The most important thing this project did was to ensure the start of a collection of basic statistics on the catches and on the fishing effort. And it is the existence of this series of reasonably good statistics that has made the detailed studies of the populations dynamics of the anchovy stock possible. Of course as soon as we look at either the catch statistics or the effort statistics, we start running into the usual sort of problems. The catch data come from the records of deliveries to the factories and several things can go wrong. First, the landings are not the same as the catches. The boats can carry only a fixed quantity of fish, as much as they can load on board, and if catches are very good and they put their purse seine around a big shoal of fish, it may not be possible to load all the fish on board and what's left in the school will be just dumped. These fish will be removed from the stock but not appear in the landings.

Another feature of the fishery is that there has been a fixed price, but the real value of the anchovy varies during the year. There is, of course, a variation in the size. The recruits come in at the beginning of the year, a few in December but mainly in January and February, so that in January and February there is a large proportion of small fish in the catches. Now these small fish don't seem to be of such good quality. They are more easily damaged in the process of being pumped from the net into the boat and again from the boat into the holding tanks for the factory and then finally actually from there into the cookers. As a result, from a ton of small fish caught, only a proportion of that actually reaches the factory and from that proportion the yield of meal per ton of fish delivered is less than from the bigger fish. Because of the fixed price, the records of the quantity landed of these small fish (peladilla) are adjusted downwards in proportion to the reduced yield of meat.

It seems that these various losses and discrepancies in proportion to the catch have remained reasonably constant. As the stock has gone down, the necessity to dump extra fish that couldn't be carried has become less. On the other hand, the pressure on the stocks has reduced the average size, so that more peladilla are being caught. The recorded catch has always been less than the actual catch, but the ratio of the two probably hasn't varied too much. Thus throughout the analysis, we will be dealing with Peruvian tons weighing perhaps 1.2 real tons.

Also, as in most fisheries we have great problems in getting a real measure of fishing effort. As always, we can look at fishing effort as a product of fishing time and the power of the individual vessel. We have a range of possible measures of fishing time. We can just take the number of boats operating and hope that the amount of fishing done each year by boat is the same, which is unlikely. Especially early in the development of the fishery, almost anything that would float fished for anchovy, but the less efficient vessels were used only at the peak of the season.

Other measures are the number of trips made, or, better, the number of hauls, or the time spent searching for fish. The number of trips is a convenient statistic, because it is one that can be readily measured, though in Peru there is a complication. All the records are based on the deliveries to the factories, so there are immediately available data on the number of trips during which fish were delivered at the factory, but the number of trips during which no fish were caught are not so well recorded. The numbers of trips, even including trips without catch, may not be too closely related to the real fishing time, because if fish are abundant the ship may go straight out, put its net around a fish, catch a load of fish and come back; whereas if fish are less abundant, then it may have to go a longer distance, spend more time steaming to where it thinks the fish are, spend even more time searching up and down for fish and perhaps make half a dozen hauls on small shoals of fish until finally it has a load of fish to bring back. The catch per trip in the two cases may be the same, though the abundance, and also the catch per haul, or the catch per hour spent searching for fish is quite difficult. The number of trips is therefore not a good measure of fishing time. Unfortunately, in Peru we do not yet have a better measure, but we are hoping to institute measures to record the number of hauls made or the time searching, both of which should be better measures of fishing time.

The other changes are changes in fishing power. The obvious one, as in most developing fisheries, is that size of boat has increased. To correct for this, instead of number of trips we can record the number of trips times the gross registered to give tonnage, GRT-trips. In addition to increased size, there have been other changes in the fleet. The most obvious has been the introduction of echo sounders, power blocks, and fish pumps. Echo sounders help to find the fish more easily, and the power block and the fish pump permit the net and the fish to be handled more quickly and thus to give more hauls per trip. Corrections have been made for these changes in efficiency, due to the power block and echo sounders. The changes occurred between 1960 and 1966, and the effort during this period and later has been increased by a factor varying from 1.033 in 1960-61 season, to 1.200 in the 1965-66 season and later, to standardize in terms of the earlier measures of effort.

But even with these adjustments, it is doubtful whether we have made the full corrections for increases in efficiency, that is the increase in fishing mortality caused by one unit of nominal effort, the corrected GRT-trip. Some measure might be obtained from better information on the searching time, and number of hauls made per trip, but other changes, such as improvements in the skill of the fishermen, are less easy to measure directly. An indirect method has been obtained from some analysis by Tony Burd in Lowestoft, using the cohort analysis. There are data on the age composition of these anchovies and even though there may be inaccuracies in the age-determinations, it seems to give a consistent story. Using this cohort analysis, the existing long series of age composition data plus some rough estimates of natural mortality and fishing mortality on the oldest fish, we can estimate the number of fish in the sea at each age. Taken with data on the catches, these figures give estimates of fishing mortality without reference to the fishing effort. With the fishing mortality F, the records of the fishing effort, f, can be used to calculate a value of q (where $F = qf$) and this is what has been done in this fishery.

At the last meeting of the FAO Panel of stock assessment experts, (Anon, 1971), we calculated this value of q from the cohort analysis and the effort data. The results are shown in Figure 10E. The first area looked at was this central area, where there was a steady increase in q, which seemed to be a reasonable quantitative measure of how much more efficient the fishery was getting. In the northern area the most recent years showed the same clear trend, but there was an aberrant point in 1962. However, even with that point, there is a reasonably clear trend of increasing efficiency in the northern area as well as in the central area. It would be nice in this fishery to look at these fluctuations and see whether these fluctuations in q can be correlated with fluctuations in oceanographic conditions. We would expect if the conditions are such that the fish are distributed in a small area, that q, the efficiency of the fleet as a whole, would increase. If the fish are dispersed by hydrographic conditions, then we might expect the q to go down. It seems as though that there has been increase in effectiveness of the recorded units of an unknown amount over and above the increases in fishing power accounted for by increase in tonnage or increased use of echo sounders and power blocks.

In most of the analyses presented here, however, it has been assumed that the efficiency had been constant, and the evidence from the cohort analyses has been ignored for the time being. The interpretation of the results should always keep this assumption in mind. That gave us our basic statistics of catches and fishing effort as shown in Table 10A. These are given for the total country and also split between north, central and southern areas. In the last column the efficiency factors used to correct for echo-sounders, etc., are given, increasing from 1 off the table up to 1.2 in 1965. The tabulations

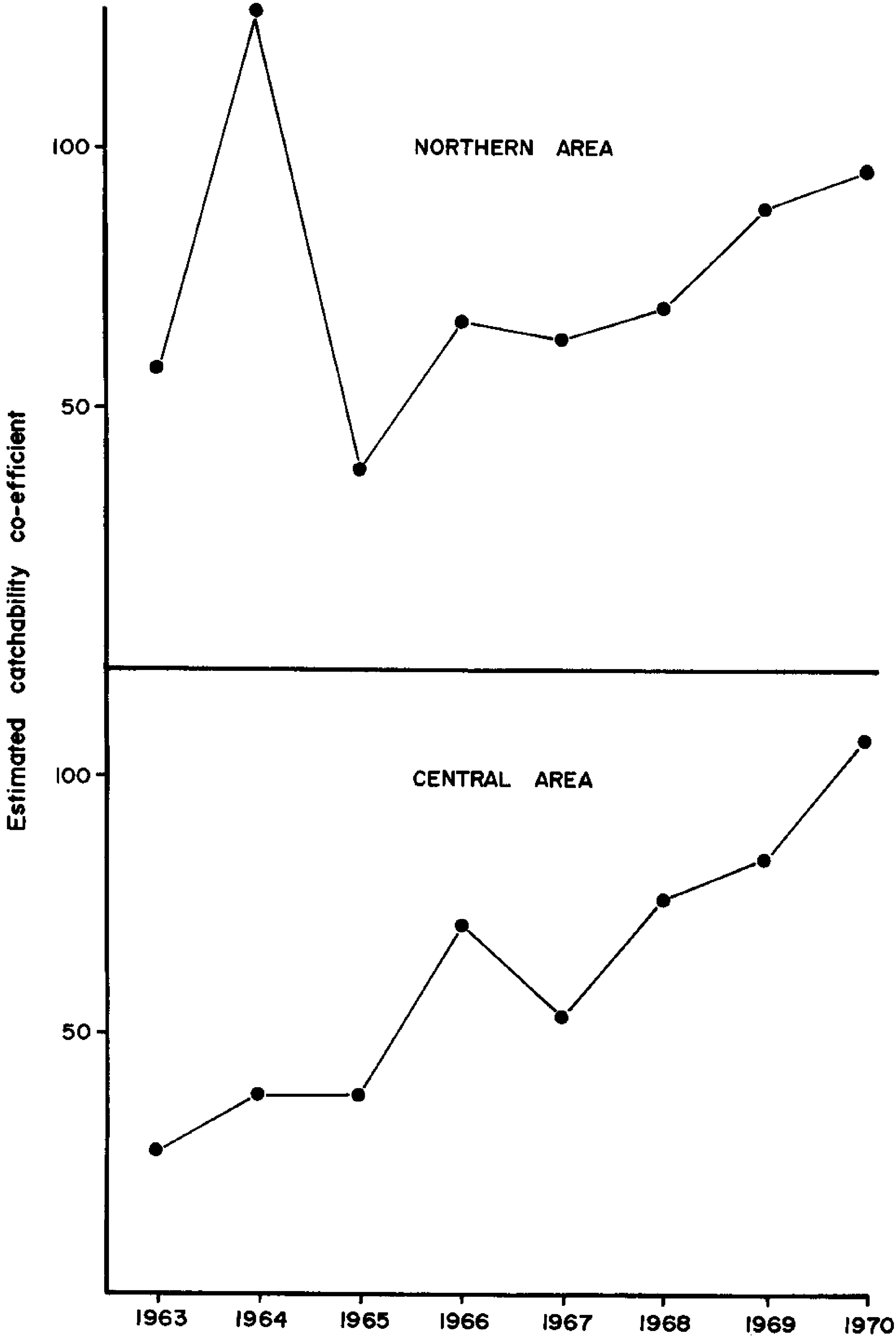


Figure 10E. Trends in the efficiency of the fleet as estimated from cohort analysis (from FAO, in press).

Table 10A. Statistics of the Peruvian anchovy fishery by fishing seasons--September to August. Total catch in thousand tons and estimates of effort (corrected as explained in text).

	Total country			North			Central			South			
Season	Catch	Effort	CPUE	Catch	Effort	CPUE	Catch	Effort	CPUE	Catch	Effort	CPUE	Correct. factor
960/61	3 934.3	7 134.1	0.551	1 114.6	2 293.3	0.486	2 680.6	4 604.0	0.582	139.1	236.8	0.587	1.033
961/62	5 501.6	9 128.7	0.603	1 827.1	2 872.3	0.636	3 457.3	5 843.4	0.592	217.2	413.1	0.526	1.075
962/63	6 906.7	14 446.8	0.473	2 245.9	4 311.6	0.521	4 235.8	9 301.7	0.455	425.0	833.5	0.510	1.120
963/64	8 005.8	21 284.8	0.376	2 702.5	6 091.3	0.444	4 590.3	13 450.0	0.341	713.0	1 743.5	0.409	1.145
964/65	8 036.6	21 374.0	0.376	2 965.7	6 817.8	0.435	4 394.1	12 736.4	0.345	676.8	1 824.3	0.371	1.170
965/66	8 095.6	22 740.6	0.356	2 705.9	7 622.4	0.355	4 565.9	13 008.3	0.351	823.8	2 190.9	0.376	1.200
966/67	8 242.3	18 947.8	0.435	3 003.3	6 283.1	0.478	4 757.4	11 193.8	0.425	481.7	1 312.6	0.367	1.200
967/68	9 817.8	20 800.4	0.472	3 728.9	7 212.7	0.517	5 356.4	12 285.4	0.436	732.4	1 389.8	0.527	1.000
968/69	9 967.7	23 453.4	0.425	4 251.4	9 468.5	0.449	5 042.6	12 450.8	0.405	673.8	1 524.4	0.442	1.200
969/70	10 054.5	24 493.1	0.435	4 018.3	9 132.5	0.440	5 207.9	13 633.6	0.426	821.3	1 812.5	0.457	1.200

of effort and catch per unit effort include these correction factors. As we see, the trends in the three areas are generally similar, so we can, for most purposes, just look at the trends in the total country. There is a general rapid increase in effort up to 1963, with an increase in catch and a falling off in catch per unit effort. Since that period the fishery has been under regulation. There have been closed seasons and other regulatory measures aimed at conserving the stock and also aimed at maintaining the price of fish meal. As a result the catch and nominal effort have not changed much.

One of the things that we know is happening over and above the fishery, which might be taken into account before carrying out the detailed analysis, is the change in the population of birds, as shown in Figure 10F. Also for comparison, the catches of anchovy are shown. We know that birds are one of the main predators on the anchovy. We also believe that we know the magnitude of this predation in quantitative terms. We certainly know the population of birds, because this has been quite closely studied. Before the anchovy fishery developed, the main industrial use of the resource was through guano produced by these birds. They were therefore of some practical importance and were quite closely studied. Among other things regular estimates of the population of birds and also reasonable estimates of how much food was eaten by a bird per day or per year were obtained. The figure shows that the population of birds has fluctuated widely and that these fluctuations were not uniform and not immediately related to the changes in the fishery. We might therefore expect to get a more uniform answer and a clearer picture of the effect of fishing on the stock if we made some allowance for birds, and particularly according to Schaefer (1970) this may be done by including the predation of birds as one element of the fishery. We split up the total mortality between F fishing and M natural mortality, but instead of including the effect of birds in M, which would add to the variability of the natural mortality, we include

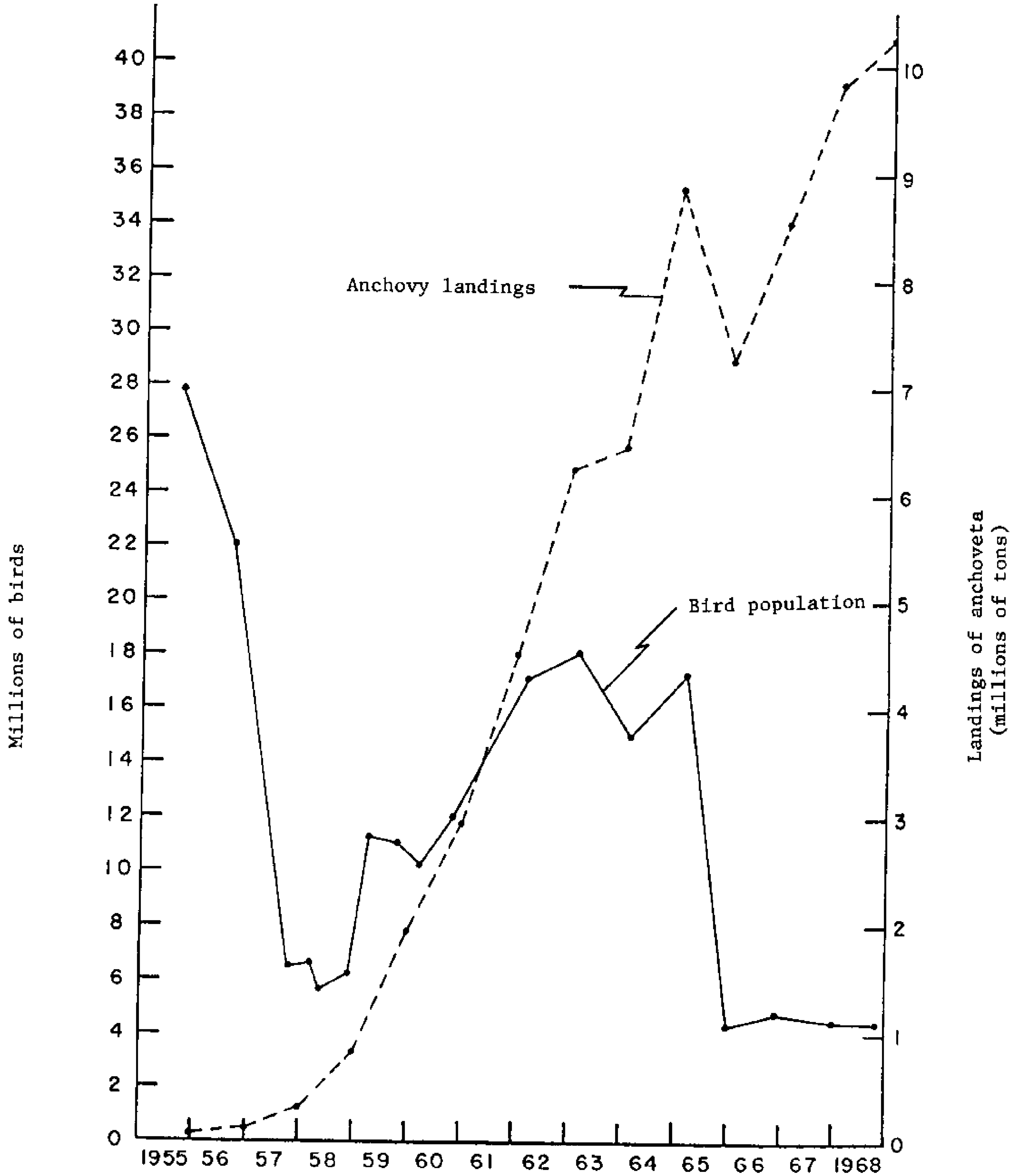


Figure 10F. Changes in the population of birds, compared with the landings of anchoveta (from Schaefer, 1970).

the effect of birds in F. This keeps the nonfishery elements in the analysis more constant. Using the observed population changes in birds and the consumption per head, we can obtain an estimate of bird catch, which can be added to man's catch to give the total catch. Also using bird catch and man's catch per unit effort, we could calculate a bird effort, in terms of gross registered ton trips. Normally this will not be done explicitly, but the bird effort will be included in the estimate of total effort.

We have now at last got our basic elements of catch, catch per unit effort, and effort, and we can do some plots. Figure 10G shows a typical result, taken from Schaefer (1970). The upper figure shows the plot of sustainable yield against effort, the lower one the plot of catch per unit effort against effort. The values for each year are plotted separately, and we can see that there was a cluster of points for the most recent years when it seemed the regulations had stabilized the catch, and also the nominal effort. The two curves shown were fitted by the Genprod model. One, (full line), is obtained by fixing the value of m, a parameter in the Genprod model, at 2. This value gives the logistic curve as a special case of the Genprod model. The other, dotted line, is the best fit with the Genprod model allowing m to take any value. The actual value which fitted best was 0.95, giving a rather curved relation between catch per unit effort and effort and a flatter curve of catch against effort. These relations were obtained using the effort data without any extra correction. A whole variety of curves can be drawn through these points because they are not very scattered, except for one or two early points.

However the situation is very different if corrections are made for changes in the efficiency of the fleet. What will happen is that the most recent points

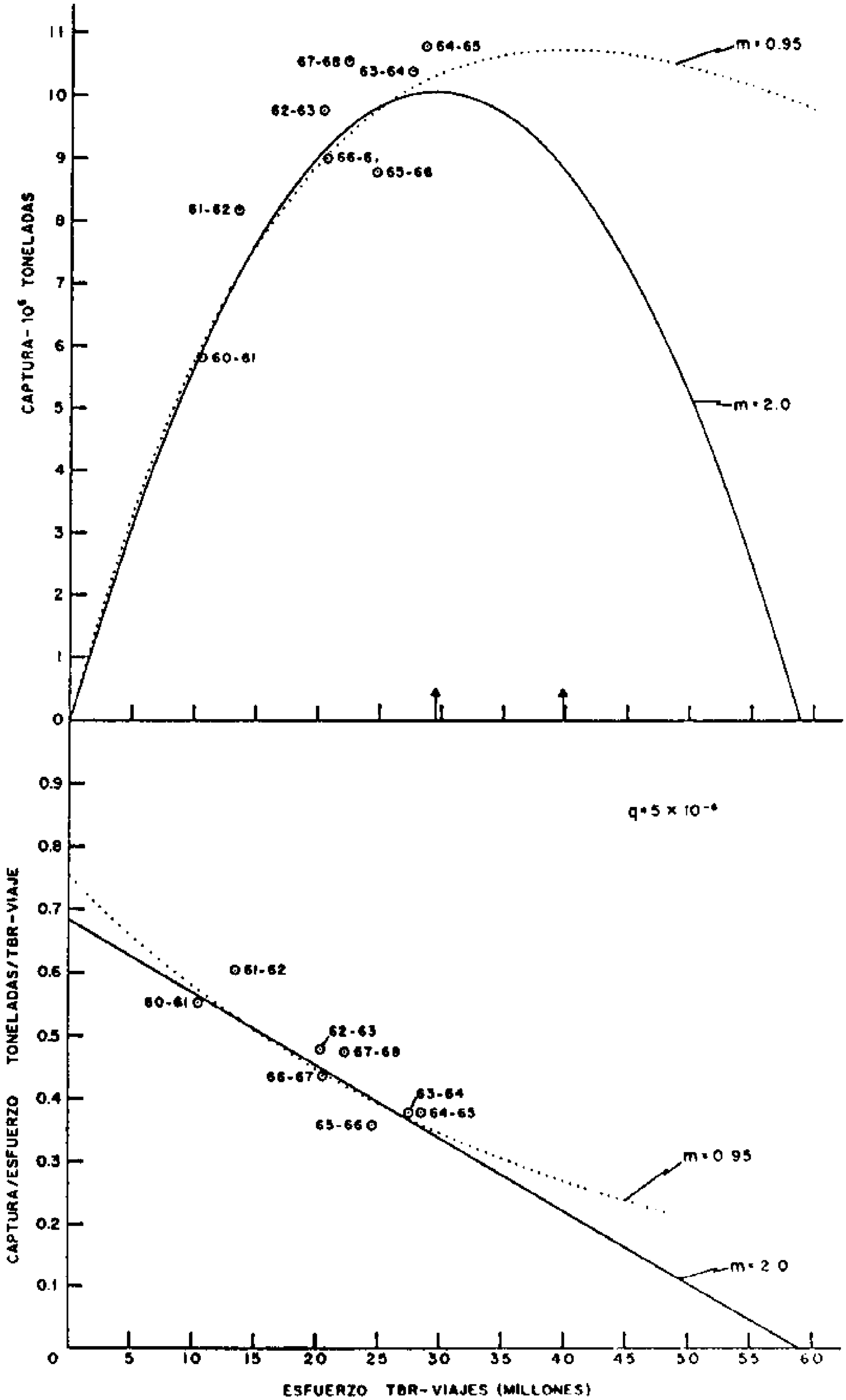


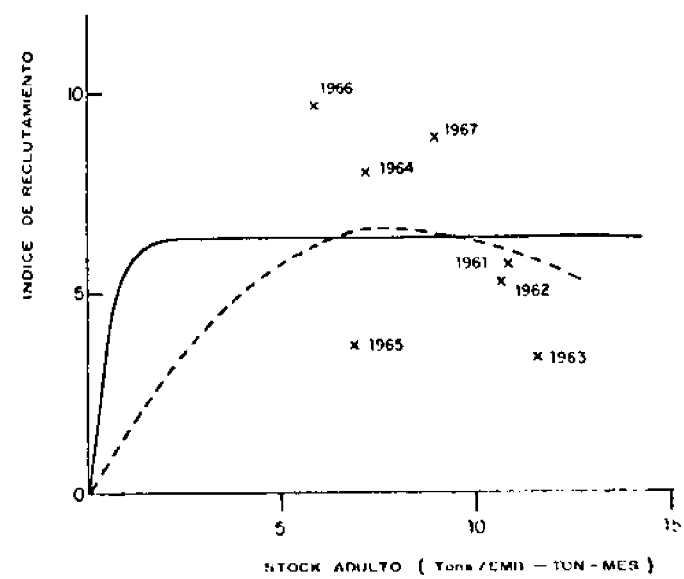
Figure 10G. Relations between effort and catch and catch per unit effort (from Schaefer, 1970).

will on the catch effort plot move out the right. Similarly, on the catch per unit effort/effort plot, they will move to the right since the effort has been underestimated, but the catch per unit effort will be less, so they will also move down. If various curves are fitted to these adjusted points, we find that the wider spread of points will give a better discrimination between the possible curves that might be considered. Some fit well and some not so well, and in particular we find that the parabola predicted by the logistic curve doesn't fit at all well.

One of the curves that fits best is the hyperbolic function suggested by Dr. Murphy in Hawaii in which the curve of catch against fishing effort is a hyperbola. As the amount of fishing increases, the curve flattens out and the catch approaches some asymptotic limit. Though this fits the observations well, it means that the yield as a function of the population increases linearly with decreasing population. The more we decrease the population, the greater will be the sustainable yield. Now this is quite possible over a small range of population. It is, in fact, what the Schaefer logistic model tells us at the highest levels of population, at the bottom right-hand part of the curve. What is impossible that if there is no population, we get the maximum sustainable yield. At some stage this curve suggested by the hyperbolic model must turn around and come down to 0. What it means in terms of the plot of catch against fishing effort is that at some fishing effort, instead of the catch remaining high, it will plummet down. And this is really quite a worrying thing. The history of many clupeoid fisheries, particularly some herring, the California sardine, and others, is of this instability. We have a nice big fishery; then suddenly after a couple of years, we have no fishery at all. And though in California the state hasn't been disturbed by the absence of its sardine fishery, this is not the situation in Peru. If the anchovy fishery disappeared, the whole economy of Peru could be in grave trouble. Therefore we are worried whether or not this fishery is going to become unstable. If in fact the fishing effort is increasing as it might well be, there might rather suddenly be a sharp decrease in stock and catches.

The most meaningful way of looking at this is to look in terms of stock and recruitment. These models in which the catch stays high at high levels of effort imply that even if we fish very hard, there will still be good recruitment. Despite a low level of adult stock, the number of young will remain high. This does not seem impossible in Peru. The young fish and the adults feed on the same things, a mixture of zooplankton and phytoplankton--and in addition anchovy eggs are not uncommon in the stomachs of anchovy, so there is a direct predation by adults on the young generation. We might expect, both from reduction of this predation and from the reduction of competition between adults and young, a better survival of eggs and young anchovy at lower stocks, and this better survival might within limits make up for the reduction in the number of eggs produced. So we might expect within limits that even at low stocks, we would get good recruitment.

The observed relation between adult stock and subsequent recruitment is plotted in Figure 10H (from Gulland, 1968). The upper figure shows the available estimates of recruitment plotted against the adult stocks, and we see that they are scattered and that there are a variety of curves we can fit to these points, two of which have been put in. The solid curve is drawn assuming that the recruitment is constant over the observed range of adult stock, but must bend down to the origin. The other, dotted, one is the relation between stock and recruitment that must exist if, given our knowledge of growth and mortality in the adult fish and thus of yield per recruit, the



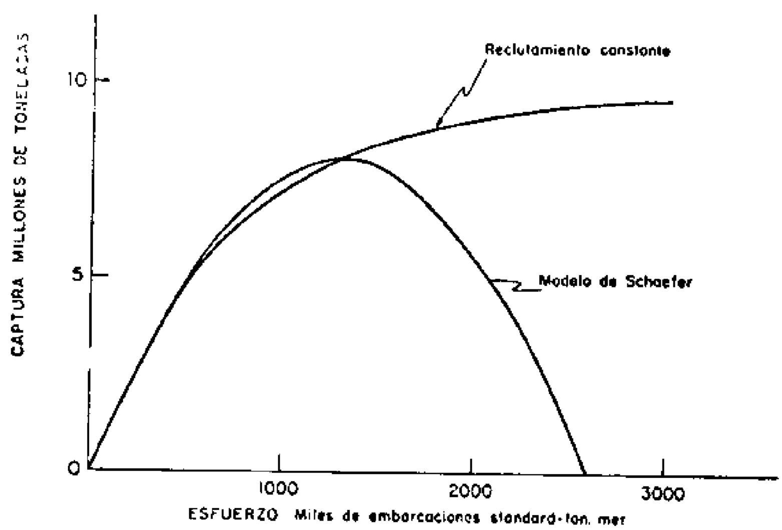


Figure 10H. Possible relations between stock and recruitment in the Peruvian anchovy, and corresponding yield curves (from Gulland, 1968).

relation between total catch and effort is to be the Schaefer curve.

The lower curves show the corresponding relations between fishing effort and total catch. The solid curve has been calculated as the yield per recruit, using the estimated values of growth and mortality of the adult fish. If recruitment is constant, this curve will (except for a change in scale) be the relation between total catch and effort. The dotted line is the relation between catch and effort already determined for the Schaefer (logistic) model.

We see that the points in the upper figure fit both curves equally badly. There are no observations at very low adult stocks, but it is the possible recruitment at these levels which is of greatest concern. If the effectiveness of the fishery is increasing, the stock is likely to be further reduced, despite the regulations. Will this result, as it might, in higher recruitment at moderately low adult stocks, or will recruitment stay the same, or will it go down? In the first case, the yield might go up to 15 million tons. In the second, it might stay at 10 million tons, but if we are unfortunate it might go down to 5 million tons or less. And these are big differences to have to distinguish on the basis of information as inconclusive as that shown in Figure 10H. Some improvements might be made by getting better estimates of recruitment and adult stock taking into account the changes in efficiency and other changes in the fishery, such as a tendency to fish harder on the younger fish. None of the adjustments so far made seem to produce any clearer picture of the stock/recruitment relation. In fact, the observed scatter of points about any likely regression line is small compared with the scatter obtained in similar plots for other clupeoid stocks. The variation in recruitment is not more than 2:1, compared with 5:1 and up to 100:1 for some such as the Atlanto-Scandian herring.

We have two major problems in Peru. The scientific problem is to understand a bit more about the stability of the population, which is best approached by looking at the stock recruitment relationship. The other problem in Peru, which is deeply tied up with the results of the biological analyses, is the general economic efficiency of the industry. We are dealing with an industry that has approached the maximum yield the resource can stand, and there are controls to insure that the resource isn't being depleted too much; but there have been few controls in the past on the buildup in the industry itself. There is a lack of balance between the size of the industry both ashore and afloat and the possible yield from the resource. The processing plants at present working in Peru could, without much strain, process the entire world's fish catch and turn it into fish meal. Because of this big overcapacity in vessels and plants they must have all the usual complex of efficiency-reducing measures. Fishing is allowed at most five days a week. There are two long closed seasons, one in the Peruvian winter, in July and August, and another in the summer, in January and February. The latter season is a good thing because this is when the small fish are abundant, and there is a benefit in increased meal production from stopping fishing and allowing them to grow. There are other regulations being put in, all of which, while protecting the stock from overexploitation, also reduce the efficiency of the industry. Careful consideration is now being given to other measures to control the capacity of the industry, and thus increase the net economic return that Peru obtains from this large resource (Anon, 1970a).

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Chapter 11

ATLANTO-SCANDIAN HERRING: STOCK AND RECRUITMENT

Let us return now to the North Atlantic and discuss the Atlanto-Scandian herring. This is the more northern herring group in the Northeast Atlantic. It spawns along the Norwegian coast and then migrates across between Norway and Iceland. Its main feeding ground in the summer is the Norwegian Sea between Norway and Iceland. There seems to be just one major spawning stock off Norway. There are a number of stocks around Iceland; and in addition to the large Atlanto-Scandian stock, there are some local stocks at Iceland which are fished often at the same time in similar places. Generally speaking, however, we needn't worry too much about stock separation in this group, provided we keep the local Icelandic stocks out and also the North Sea stock, which comes close to the Atlanto-Scandian stock when the big Atlanto-Scandian stock is coming to the Norwegian coast.

We have for this fishery a long series of data going back to 1750, shown in Figure 11A, which gives the catches of herring in Norway and western Sweden. The bottom part shows the catches off western Norway; the center part shows the catches in the so-called Bohuslän fishery, on the west coast of Sweden, which may or may not be from the same group of fish. This fishery had peak catches around 1790 and again about 1890. These peak periods in the Bohuslän fishery fit in between the two periods of the herring fishery off western Norway, one about 1850, and one about now.

It is a striking feature of this herring stock and of several other herring stocks that there are big fluctuations over periods of about a century, which may or may not have anything to do with fishing. Some of the other fluctuations that have been observed are certainly not due to fishing. The relative abundance of the anchovy and the sardine off California can be studied by an examination of the scales in bottom deposits (Soutar, 1967). These show big fluctuations in the relative abundance of these two species over long

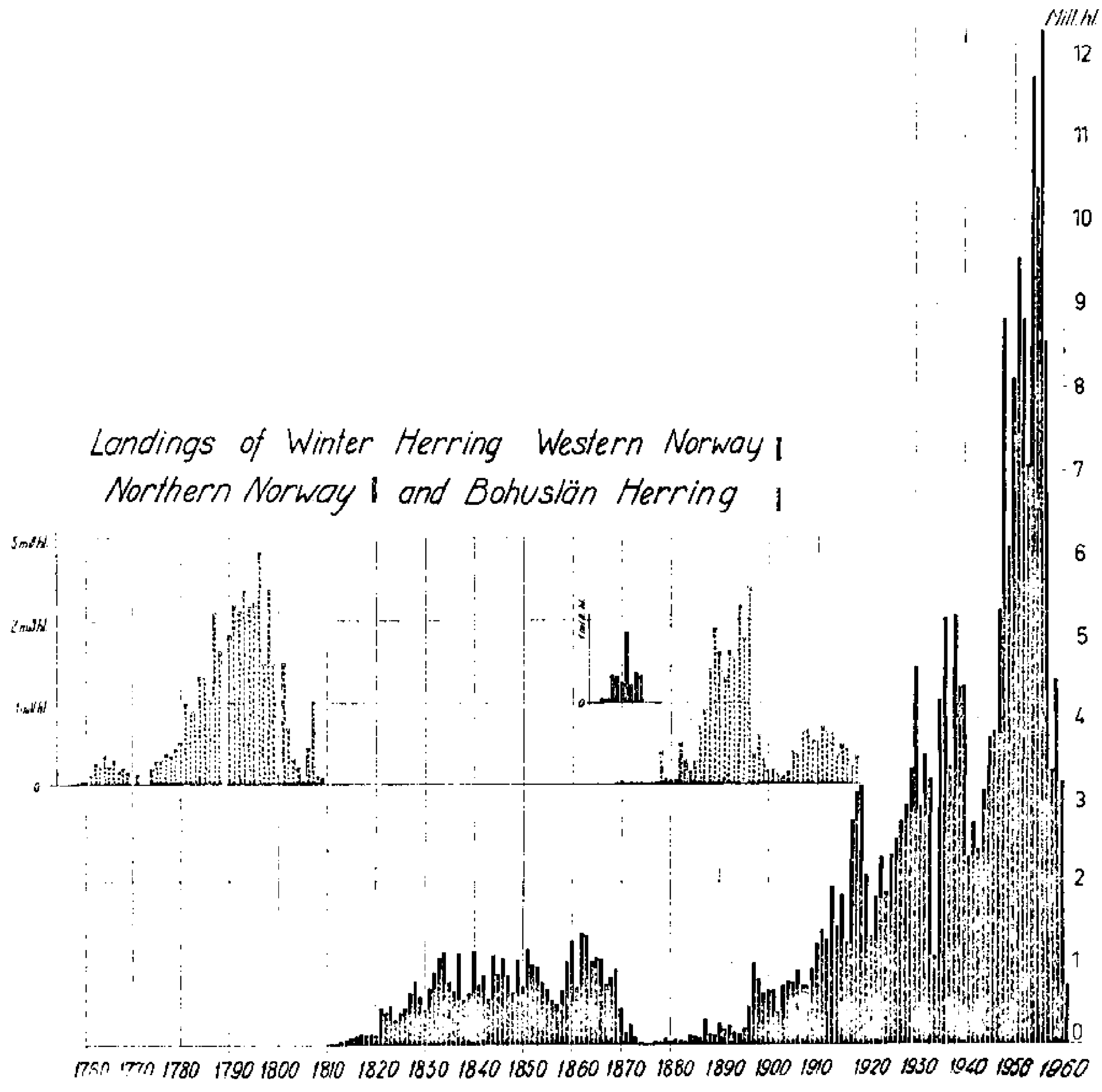

Figure 11A. Long-term changes in catches of Norwegian herring (from Devold, 1963).

periods well before any fishery started. Again in Japan the Hokkaido herring has gone through similar fluctuations. One of the problems in herring population dynamics, particularly when we suspect we may be coming to the end of a herring period, is whether decline in the stocks is due to fishing, in which case we might hope to do something about it, or whether it is something in the nature of the animal, in which case we just have to hope we can develop a fishery on some other species. Besides changes in total stock, there are other changes going on which aren't due just to fishing.

Figure 11B shows the distribution of herring catches around the west coast of Norway in each year from 1946 to 1961. Back in 1946 the catches were fairly evenly distributed between these four main areas along the west coast of Norway. However, the center of the Norwegian herring fishery has moved farther north. Also the fishery has become later, and in the last few years, the west coast Norwegian herring has been entirely in the northern part of the western coast. So there certainly have been changes going on in this herring stock which have nothing to do with fishing. We have had ancient fluctuations and changes in the pattern of the distribution of the fish.

Looking at the recent history of the fishery in more detail, Table 11A gives the catches over the last twenty years. The biggest contributor to the catches has been Norway. Icelandic catches remained fairly steady at a rather low level until about 1963 when Iceland developed its big purse seine fishery and took very large catches. The other big contributor to the fishery has been the USSR, which really started fishing in 1950 and developed its fishery thereafter. There are three sections of this fishery, corresponding roughly to the three countries. The Iceland fishery by purse seine is mainly around Iceland, the Norwegian fishery takes place along the Norwegian coast, and is now mainly by purse seine, though it was partly by drift nets, and the Russian fishery, almost entirely by drifts nets out in the open Norwegian sea

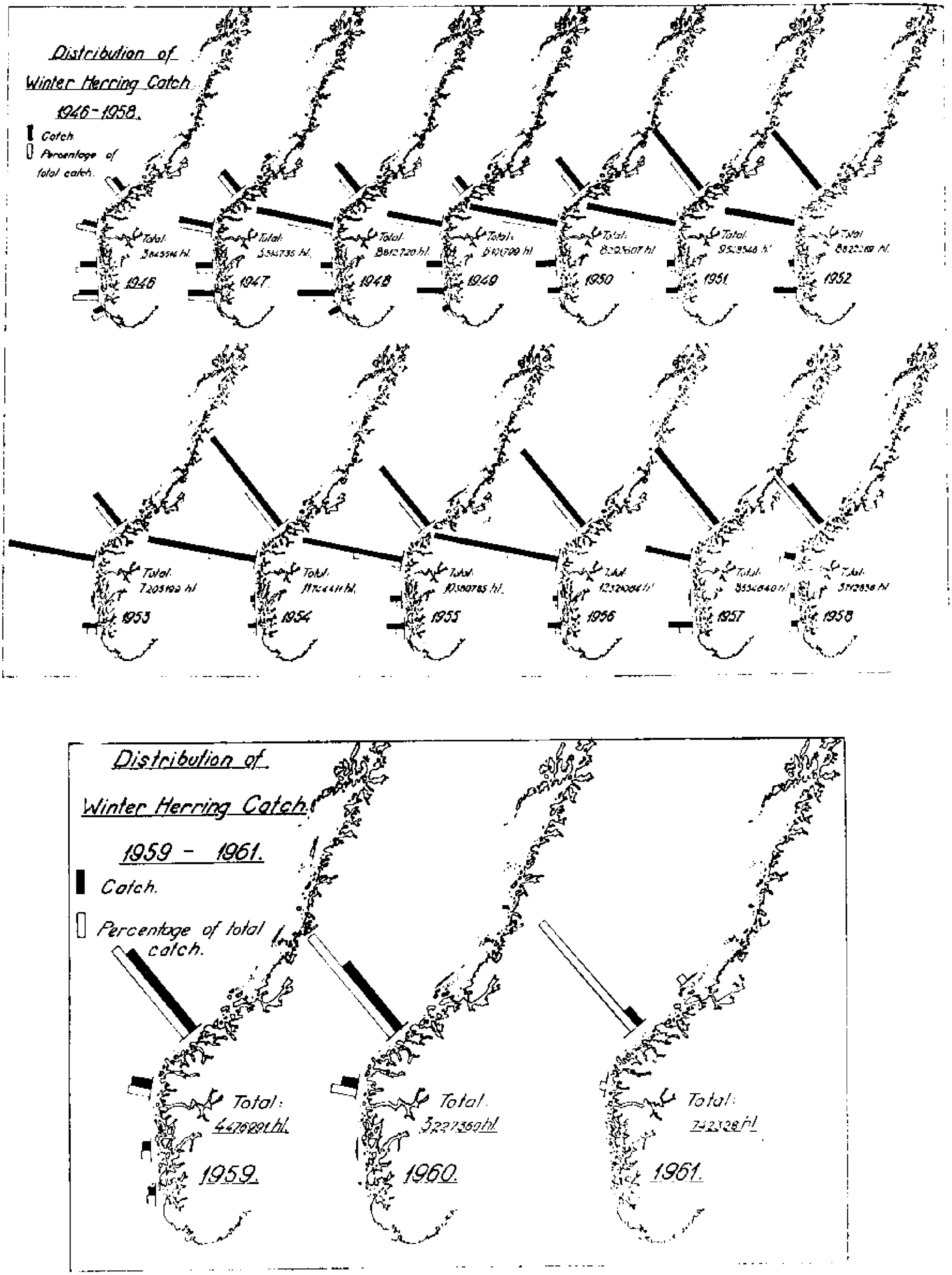

Figure 11B. Changes in the position of herring catches along the Norwegian coast (from Devold, 1963).

Table 11A. Total Catch. Catch (in thousands of tons) of adult and pre-recruit Norwegian spring-spawning herring 1950-1970.

Year	Iceland	Norway	USSR	Faroes	Germany	Total
1950	30.7	781.4	14.0	-	-	826.1
1951	48.9	902.3	43.0	-	-	994.2
1952	9.2	840.1	70.0	-	-	919.3
1953	31.5	692.2	110.0	17.0	-	850.7
1954	15.2	1 103.6	160.0	27.6	-	1 306.4
1955	18.1	979.3	207.0	13.1	-	1 217.5
1956	41.2	1 160.7	235.0	23.7	-	1 460.6
1957	18.2	813.1	300.0	17.0	-	1 148.3
1958	22.6	356.7	388.0	17.7	-	785.0
1959	34.5	426.9	408.0	13.7	-	883.1
1960	26.7	318.4	465.0	11.0	-	821.1
1961	85.0	111.0	285.0	16.9	-	497.9
1962	176.2	156.2	209.0	9.8	-	551.2
1963	177.5	130.4	330.0	12.9	-	650.8
1964	367.4	366.4	365.8	19.3	-	1 118.3
1965	540.0	259.5	489.2	31.5	5.6	1 325.8
1966	691.4	497.9	447.4	60.2	26.1	1 723.0
1967	359.3	423.7	303.3	34.9	9.7	1 130.9
1968	76.2	55.7	124.3	15.4	1.8	272,4
1969	0.6	15.6	3.2	4.4	0.3	24.1
1970	0.0	20.3	0.0	0.6	0.0	20.9

\- No information available

between Iceland and Norway. Looking at the total catches, we see a cycle. It built up to a peak of nearly one and a half million tons in 1956, then declined to about half a million tons, increased again to an even higher peak in 1966, and then declined very sharply from 1967 to 1968 and down to only 20 thousand tons in the last two years.

The next thing we ought to consider is effort and catch per unit effort. Our difficulty, as always, is to get a measure of effort and catch per unit effort. What we do know is that the purse seiners operated by Iceland and Norway have become increasingly efficient. It is difficult to use their catches per unit effort as a consistent measure of abundance. Though purse seiners have been used by both Norwegian and Icelandic fishermen for a long time, their effectiveness has been revolutionized by the introduction of power blocks, for handling increasingly large nets, and sonar, for detailed tracking of the schools of fish. A modern purse seiner is many times more effective than the old purse seiners operating in 1950.

A better measure of effort can be obtained from the drift net fishery of the USSR. The gear used in this fishery has not changed much, though there have been some changes in the seasonal pattern of the fishery. The catch per unit effort at some fixed season is therefore likely to be a more consistent measure of the abundance of fish than the catch per unit effort for the fishery over the whole year. Table 11B gives therefore the catch for February, as well as for the fishery as a whole. We see that the catch per unit effort in the February fishery had some changes similar to those in the total catch. It declined from 1959 to 1962, increased again to reach a peak in 1966, and then declined. From these measures of catch per unit effort, it is quite easy to calculate estimates of total effort, calculated as the total catch divided

Table 11B. Catches per effort of the USSR drift-net fishery (a. from the annual catch; b. from the February catch = spawning fishery only).

Year	a. Catch per drift-net (total catch) kg	b. Catch per drift-net (February catch) kg
1958	53.5	131.0
1959	63.3	132.0
1960	60.2	115.2
1961	44.4	76.0
1962	57.3	56.1
1963	61.6	87.2
1964	66.4	108.0
1965	94.4	113.6
1966	79.0	115.0
1967	56.3	55.3
1968	28.3	26.3
1969	24.2	38.9
1970	0.0	0.0

Table 11C. Estimates of total effort in drift-net units.

Year	Total Number of Nets in Millions (Total Catch)	Total Number of Nets in Millions (February only)
1958	14.65	6.00
1959	13.95	6.70
1960	13.64	7.13
1961	11.21	6.55
1962	9.62	9.83
1963	10.56	7.46
1964	19.84	10.35
1965	14.04	11.68
1966	21.68	14.89
1967	20.09	20.45
1968	9.11	9.77
1969	1.0	0.63
1970		

either by the catch per unit effort in the entire Russian fishery or in the Russian fishery in February. The pattern of change in total effort is quite different from the fluctuations in catch and catch per unit effort. There was a steady increase in total effort up to a peak in 1967 and a very sharp drop in 1968, and again in 1967. (Table 11C)

In addition to these figures of catch per unit effort, which provide indices of relative abundance, there are for this fishery direct estimates of actual abundance. These are shown in Table 11D. They come from two sources. One is from tagging data using the techniques already described. In this case, it seems that the tagging data are reasonably reliable. For the North Sea herring, the tagging technique was steel tags placed in the body cavity, and detected by magnet at the processing plants. There were some checks that the shedding of these tags by herring was rather small. These fish are large, considerably larger than the North Sea fish which run up to not much over 20 to 25 cm. These are running up into the 30-to 35-cm range and more. These big herring are reasonably easy to handle. The extensive migration back and forth between Iceland and Norway should mean that there is good mixing of the tagged and untagged fish during the migration. A lot of this tagging was done at Iceland and the estimates of population were based on the recoveries in the fish meal factories in Norway so that the estimates of total abundance from the tagging data are probably reliable. Most ways in which tagging data can go wrong don't apply to these estimates. The relative changes in population size obtained from the tagging data agree with the trends in catch per unit.

We also have for this fishery another estimate of absolute abundance, obtained from a combination of echo surveys and underwater photography. This was carried out mainly by Russian scientists on the herring (Fedorov et al, 1964) during the winter when they are fairly inactive. They form large patches

Table 11D. Estimates of absolute abundance of adult stock of Norwegian spring-spawners 1952/53 - 1967/68 (in million tons).

Years (Winter Season)	From Tagging Data	From Echo-Surveys and Underwater Photography
1952/53	12.5	-
1953/54	12.1	-
1954/55	13.9	-
1955/56	12.0	-
1956/57	9.4	-
1957/58	6.6	-
1958/59	5.0	6.0
1959/60	-	-
1960/61	-	3.1
1961/62	-	2.5
1962/63	-	2.8
1963/64	5.0	3.3
1964/65	7.7	6.8
1965/66	6.6	-
1966/67	4.0	-
1967/68	-	2.0

in the open Norwegian Sea, and it is possible to steam ships across these patches and with an echo sounder estimate the area of the patches and the average depth, and thus obtain the total volume of echo traces corresponding to the volume of the herring schools. Then we can lower a camera into these patches and photograph them. Hopefully, because herring are not very active and don't react much to the camera, the density of herring in front of the camera will be representative of the average density in the schools. Then the total number of herring is the product of the number per unit volume and the volume observed from the echo surveys. Knowing the mean weight of the individual fish provides an estimate of total herring abundance (in tons) from this combination of echo surveys and underwater photography. And it is pleasing and perhaps a bit surprising how well these two sets of figures in Table 11D agree.

For several years, there are estimates from both methods. The earliest was in 1958-1959, when tagging data gave 5.0 million tons and echo surveys 6.0 million tons. Considering the various errors that might crop up in both these methods, this is quite remarkable agreement and agreement that went on reasonably well in successive years. Putting these two estimates together, we can see the trend in population, of a high value in 1954, a decrease to a trough in 1961-1962, a further increase to a second peak in 1965, and a decrease thereafter. So we have some picture of what is happening to this stock.

Just how much can this be accounted for by fishing? It certainly seems that this latest decline fits in with the observed changes in effort during this whole period. The total fishing effort on the stock has been increasing and one would expect to see the lowest abundance in the most recent years, which indeed we do get. But it doesn't explain this trough in this middle period when fishing was no more intense than it had been before or afterwards. So we don't have a complete explanation of what happens.

We do get a better explanation if we look at the age composition, which is shown in Table 11E and which is one of the more remarkable age compositions that we are likely to see. It gives the estimated numbers of herring, in millions of fish, of each year-class taken each year between 1962 and 1970. The numbers vary very greatly. Outstanding are the contributions of the 1959 and 1960 year-classes, and also to a lesser estimate the 1950 and 1951 year-classes. Practically nothing appears for many of these year-classes between 1952 and 1958.

The other feature shown in the table is that these Atlanto-Scandian herring are very long-lived fish. The strong 1950 year-class was still making some contribution to the catches in 1970 when 20 years old, and the 1959 year-class was still about the biggest contribution to the fishery in 1970. Not only is this a long-lived fish, but it is a fish with extremely variable year-classes. The difference between the 1958 and 1959 year-class is something like 100 to 1. This fluctuation in year-class strengths has been a conspicuous feature of this herring stock ever since data were first collected on age composition back at the beginning of the century (Hjort, 1914). During the past fifty years the number of good year-classes in this Norwegian herring fishery has been relatively few. Over this period something like half the total catches have come from three or four outstanding year-classes.

Given this fluctuation in year-class strength, some of these changes in the fishery are quite easy to understand. We had, when these 1950 and 1951 year-classes came in, a good fishery. Then as they were fished out and died,

Table 11E. Total catch in numbers of Norwegian spring-spawning herring in the adult fisheries (millions).

Year-class	YEARS								
	1962	1963	1964	1965	1966	1967	1968	1969	1970
1948	64.1	60.6	43.2	52.1	8.8	0.0	0.0	-	-
1949	49.3	79.8	46.1	70.2	14.9	1.9	-	-	-
1950	959.3	932.7	771.6	703.0	392.7	64.3	5.4	1.1	0.2
1951	138.9	174.1	151.9	137.7	96.9	14.3	4.1	0.2	-
1952	59.8	92.5	83.2	106.9	72.1	14.3	3.6	0.3	0.1
1953	64.1	107.7	96.3	100.5	69.1	17.5	1.8	0.2	0.1
1954	13.3	9.3	29.3	40.0	11.0	8.9	2.6	-	0.1
1955	20.2	18.3	24.9	19.1	26.1	8.5	2.5	0.3	0.1
1956	6.5	3.5	3.0	7.4	17.4	3.5	0.8	0.2	0.1
1957	2.0	1.7	1.5	14.9	14.4	5.7	1.1	0.3	0.1
1958	1.4	4.9	13.1	19.5	38.0	8.9	2.0	-	0.1
1959	255.7	408.9	1 917.7	2 195.8	2 868.3	1 718.2	345.9	36.3	28.2
1960	49.8	38.2	307.6	570.4	1 290.6	1 135.0	134.8	33.5	26.7
1961	-	-	90.2	245.9	459.1	422.2	93.9	11.6	13.2
1962	-	-	2.2	12.1	26.5	27.0	14.3	0.7	1.0
1963	-	-	-	45.1	80.6	25.7	15.2	2.9	3.3
1964	-	-	-	-	-	-	-	-	0.4
1965	-	-	-	-	-	-	-	0.2	0.3
1966	-	-	-	-	-	-	-	-	1.3
1967	-	-	-	-	-	-	-	-	0.2
Total	1 684.4	1 932.2	3 581.8	4 340.6	5 486.5	3 475.9	628.0	87.7	75.5

the stocks went down to a minimum around about 1963 and about then the strong 1959 year-class began to come in and gave very good catches in 1965, 1966, and 1967. Thereafter it was fished out, and there haven't been any good year-classes coming in to replace it. So the changes can be explained by a combination of heavy fishing in the most recent years and fluctuating year-class strengths. So far as the harvest of a single year-class is concerned, we have two choices which don't seem to make much difference to the total weight of herring caught from that year-class. We can either do as we did to some extent on the earlier year-classes, which was to fish rather slowly and get a moderate annual yield spread over a large number of years, or we can do what we did with the 1959 year-class, which was to fish very hard for three or four years reducing the stock by the end of that period to a low level. The total yield from that good year-class would be about the same in either case.

The curve of fishing effort against yield per recruit for this herring is a rather flat curve because most of the growth of the herring has been done by the time it recruits to the main fishery. The yield per recruit cannot be reduced much by very intense fishing, but such fishing will result in a very low level of stock abundance, especially when no good year-class has recruited to the fishery recently. This is the present situation.

The big question is what is going to happen now. Can we just sit back and in a certain number of years' time, get another big year-class coming in, when conditions are right, as would presumably have occurred if we hadn't fished hard? Or will the fact that we have now reduced the spawning stocks to a very low level affect our probability of getting a good year-class? Is it possible that this heavy fishing on the 1959 year-class ruined our future prospects of the fishery?

This is a very difficult question and a not infrequent one. Let us turn from the herring to another fishery, across to the other side of the Atlantic on the haddock. The figures of catches of haddock in the Northwest Atlantic were given in Table 6A. For a long time the catches in sub-area 5 (Georges Bank and adjacent areas), were reasonably constant at around 50,000 tons. In 1965 the good 1962 and 1963 year-classes came in. They were very heavily fished, particularly by Russian boats, at the end of 1965 and the beginning of 1966. The catches reached a peak of 150,000 tons in 1965, and have since declined steadily. The most recent catches are not shown, but those in 1972 will be only about 12,000 tons. We can predict the 1972 catches fairly precisely for two reasons. One is that all the fish to be caught in 1972 are now alive, in fact have been alive for several years, and also there is a quota regulation in this fishery which will limit the catches to about this level.

This Georges Bank stock is not the only stock of haddock in the Northwestern Atlantic which is in trouble. A more northern population of haddock, in sub-area 3, south of Newfoundland has declined even more drastically. The catches in this fishery in the 1950's were good up to 100 thousand tons but then steadily declined to only 7 thousand tons in 1968. In both these fisheries, there have been big changes in year-class strength. Sub-area 3 haddock had an outstanding year-class which supported the fishery in the 1950's, since when there has been virtually no further year-class on this stock.

The recent changes in year-class strength on Georges Bank are shown in Figure 11C, plotted on a log scale. There are big fluctuations, but no obvious trend until 1963. There are in recent years two estimates of year-class

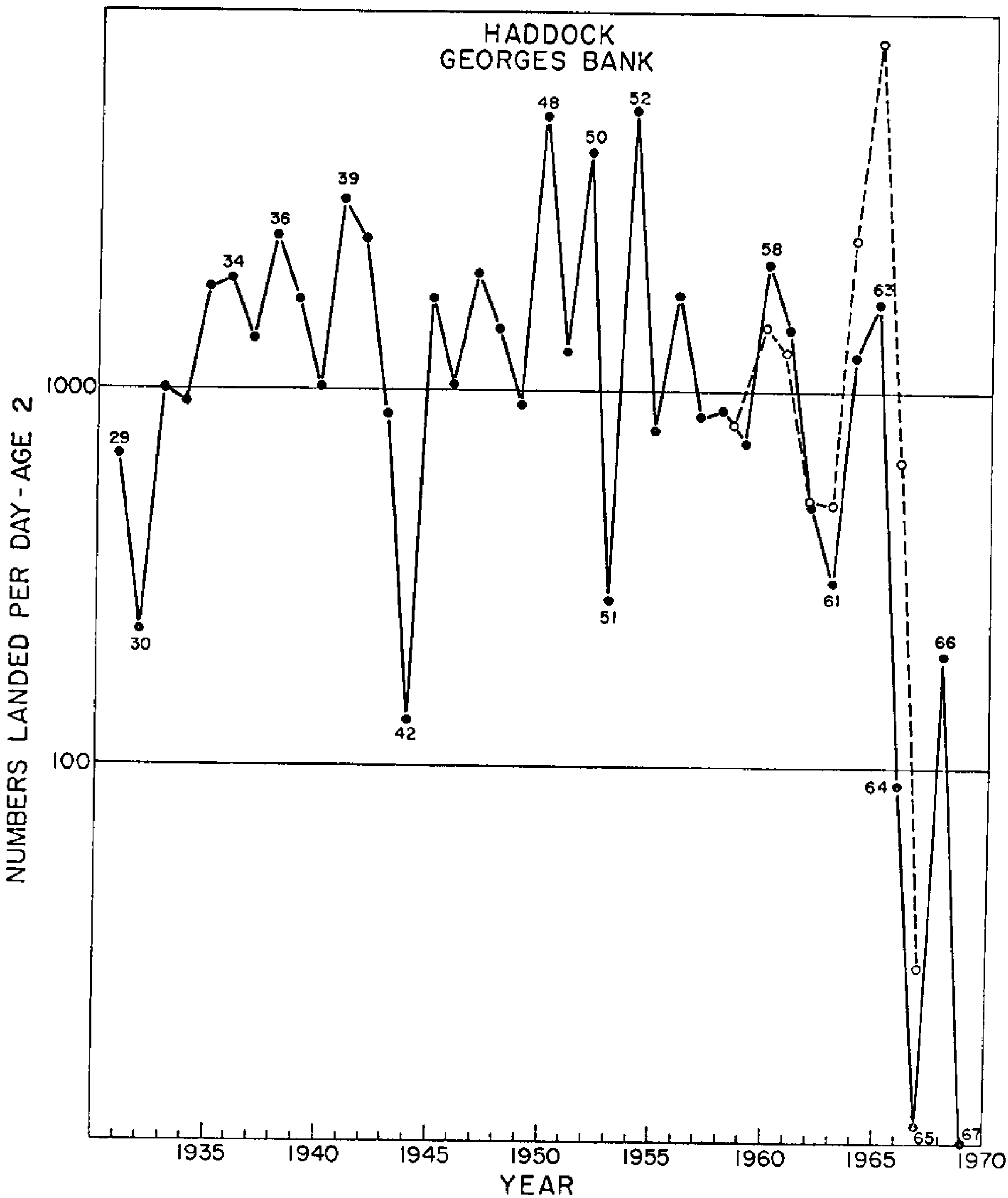

Figure 11C. Strengths of year-classes of haddock on Georges Bank (from Grosslein and Hennemuth, 1970).

strength. The old estimates were based on the numbers landed per day at age 2, shown as a solid line. The most recent, and probably more precise, estimates are coming from research surveys of young fish (broken line). On both these estimates 1963 was a good year-class and probably an outstandingly good year-class. 1964 was the lowest recorded up to that time. 1965 was even lower. 1966 was poor, 1967 was poorer still, and the most recent year-classes on Georges Bank have been even worse. There are now regular surveys on Georges Bank with research ships using bottom trawls with small-meshed trawls to retain the small fish. The earliest that the abundance of a year-class can be measured is in the autumn of their first year of life, when they move to the bottom. The 1970 survey, covering a large number of stations all over Georges Bank, caught only 3 fish of the 1970 year-class.

It is perfectly obvious what is happening to the stock. This fishery is now getting a run of poor year-classes. It seems that this haddock stock is further along the way than the Norwegian herring stocks. In the Norwegian herring, though there hasn't been a particularly good year-class since 1960, this gap is not a particularly long gap in relation to the history of the fishery. We often get gaps of up to 10 years between one really good year-class and the next one. But for the Georges Bank haddock, the present situation is unprecedented. Though there have been runs of three or four rather poor year-classes, there has never been such a long period of poor year-classes. Between 1966 and 1970, there have been seven bad year-classes in a row. It is now becoming a very urgent question exactly why these poor year-classes have turned up. It's not too easy to find a complete and simple answer. Part of it may be due to fishing; in the most recent years, heavy fishing has reduced the spawning stock on Georges Bank to a very low level, and this might account for the poor year-classes from 1968 onwards. But in

1964 and 1965, the spawning stock was quite good, virtually the same as the spawning stock that gave the outstanding 1963 year-class.

These are therefore two quite separate questions about year-class strengths. One is why do we get these fluctuations? Why, from virtually the same spawning stock, do we get a difference of over 100 to 1 in the herring and at least 100 to 1 in the haddock stocks? The second question is what relation, if any, is there between the average level of recruitment and the spawning stock? These two are among the major problems that scientists are now being faced with in fishery research.

The second question, what is the relation between average recruitment and spawning stock, is the more important question though often more attention has been given to the cause of these year-to-year fluctuations arising from a similar spawning stocks. One reason why a lot of attention has been placed on these fluctuations is that they are a very outstanding feature of many fisheries and might be related to a large number of possible environmental factors.

Therefore if a physical oceanographer or a plankton specialist wishes to interest himself in fishery matters, one of the first things he looks at, if he is working around Georges Bank, is whether in 1963 there was any unusual feature of the environment that might explain the outstanding year-class. A physicist might look at water temperature to see if 1963 happened to be an odd year and then try to see if this odd record might be related to high fish survival. The difficulty with this approach is that given sufficient ingenuity and a sufficient number of records, it is almost inevitable to find something odd that might be good for haddock. What we really need is to examine more carefully exactly what is changing the survival of these eggs or young fish in relation to environmental conditions.

The other question of the relation between recruitment and abundance of the adult stock is linked to the question of the stability of the population and the kinds of reactions in relation to marine mammals, where the density of the population seems for these mammals to influence the age at maturity and the breeding rate.

Figure 11D gives a general theoretical description of just how this stock recruitment relationship is connected with the stability of the population. The curve shows the probable general form of the relation between the adult stock and the mean recruitment, though the detailed shape probably varies from stock to stock, and is generally unknown. One point we do know is that if there is no stock, there are no recruits so the curve must start at the origin. We would also expect that to begin with, the number of recruits would increase in proportion to the stocks but as we get larger stocks, the curve will bend over. There are various theoretical models to explain how this curve might bend over and the sorts of shape it might take. Good discussions are given by Ricker (1954), and in the papers presented at the symposium held in 1970 at Aarhus in Denmark (ICES, in press). On some hypotheses the curve will have a maximum at a moderate level of stock, and large adult stocks may actually produce a smaller number of recruits. On others the curve will be approximately flat over the likely range of adult populations. But in any case the curve shown illustrates the impact of the stock-recruit relationship on the stability of the stock.

Figure 11D also shows the adult stock that will result from a given recruitment. Assuming the mortality rates and the growth are constant, the stock will be proportional to the recruitment, as shown by the straight lines in the figure. If there is no fishing, there will be a large stock from a given recruitment, as in the right-hand line. Where this line intersects the curve, the recruitment from the stock will give rise to the same initial stock, and the population will be stable. Suppose for some reason we got a stronger recruitment in one year, perhaps because of exceptionally favorable conditions when the fish were young, this would then give an above-average adult stock that would spawn and give a recruitment nearer the stable level, and thus by successive steps at each generation the stock would return to the stable situation.

Now what would happen if we started fishing? From a given recruitment the resultant adult stock would be smaller. The size of the stock will still be proportional to the recruitment but with a smaller constant as shown by the left-hand line. Where it cuts the stock-recruit curve will give the new stable position. For the situation shown in the figure, the new position involves quite a large change in adult stock, but only a small change in the recruitment. The actual change in any particular situation will depend very much on the shape of the curve. As drawn here, the effect of fishing will be to reduce the recruitment to some extent, but a flatter curve would result in little change in recruitment for any moderate amount of fishing. It is possible that this curve has a peak at some moderate level of adult stock, and that beyond a certain size of adult stock there is interference between the adults so great as to actually reduce the number of young produced. In that case, we would find that a moderate amount of fishing would increase the recruitment.

These situations do not cause any great concern to those involved in the fishery. The worrying situation is when the stock-recruit bends over, and the line relating recruitment to the resulting stock is sufficently steep (i.e., fishing reduces the stock produced by unit recruitment to a sufficiently

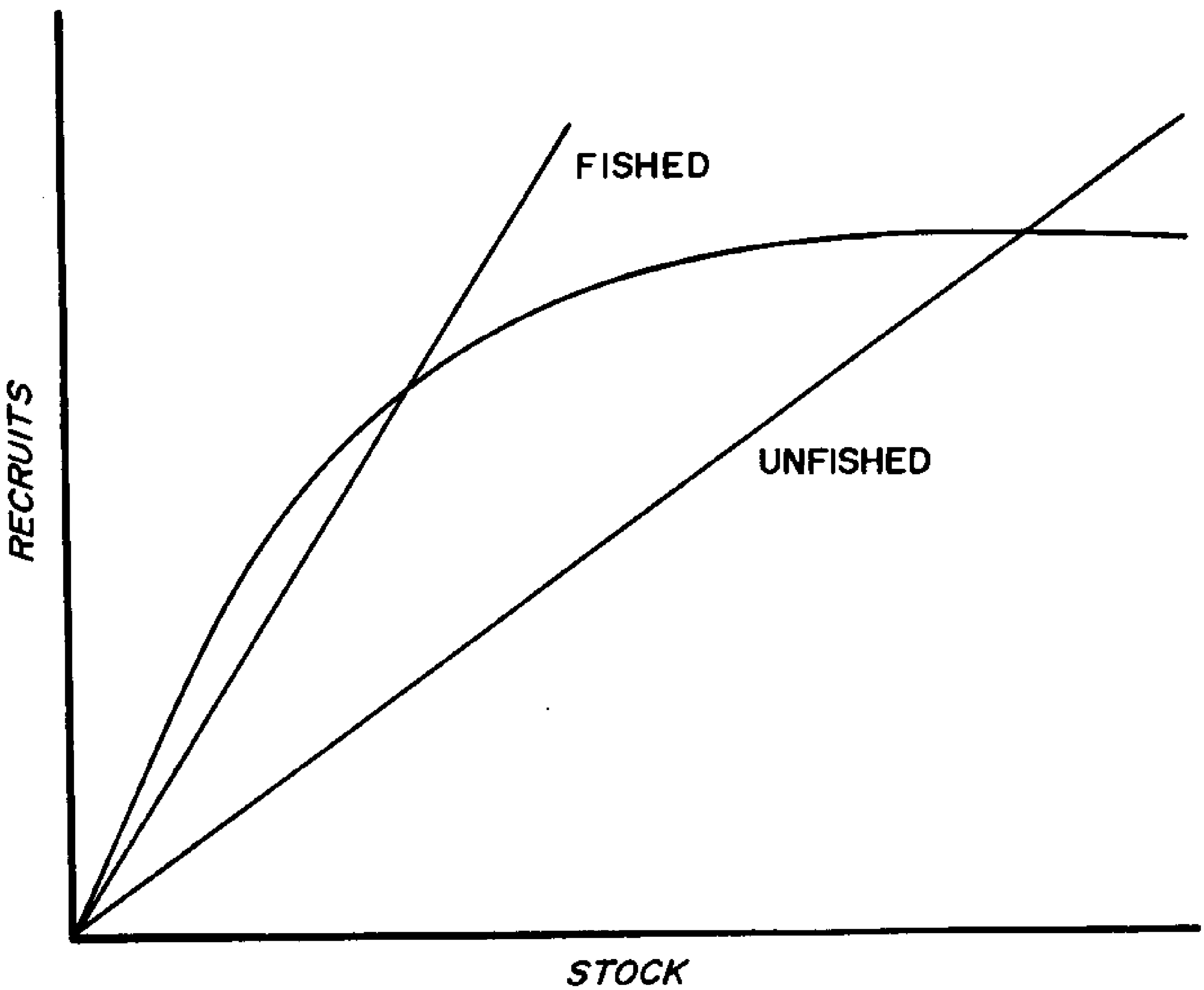

Figure 11D. A possible stock-recruitment relation, and the stable population under fishing, and without fishing low level) for the points of intersection of the line to fall down on the left-hand slope of the curve. For instance in the figure a slight increase in the rate of fishing will bring the point of intersection rapidly down the left-hand limb of the curve. A not very great increase in fishing will make the line so steep that is will not intersect the curve at all. This means that there is no stable position and that we cannot maintain that fishing rate for long without pushing the stock right down to commercial extinction, when we have to stop fishing and turn to something else.

The big question is just what is the shape fo this curve for Atlanto-Scandian herring, or for the Peruvian anchovy or for the Georges Bank haddock? One way is to plot the pairs of points for recruitment and parent stock for each year-class and see what happens. The other approach is to consider just what this curve implies. One thing it implies is that the survival of eggs to recruitment must decrease as the curve bends over and this is best shown in the next picture. Figure 11E shows, in the upper part, the relation between stock and recruitment for the plaice stock in the North Sea. The adult population is taken as the catch per unit effort near the spawning areas, and the recruits are estimated from the catch per unit effort of 4-year-old plaice by Lowestoft trawlers. This shows a great scatter with no trace of any trend, and it does look very much as though the average recruitment over the observed population sizes is virtually constant.

In the lower part of the figure, the estimated survival from eggs to recruitment has been plotted against adult stock. If the number of eggs is proportional to the adult population, dividing the number of recruits by the adult population will give an index of survival from eggs to recruits. This figure shows a very clear curve, with lower adult populations being clearly associated with higher survival of the eggs and young fish. A statistical

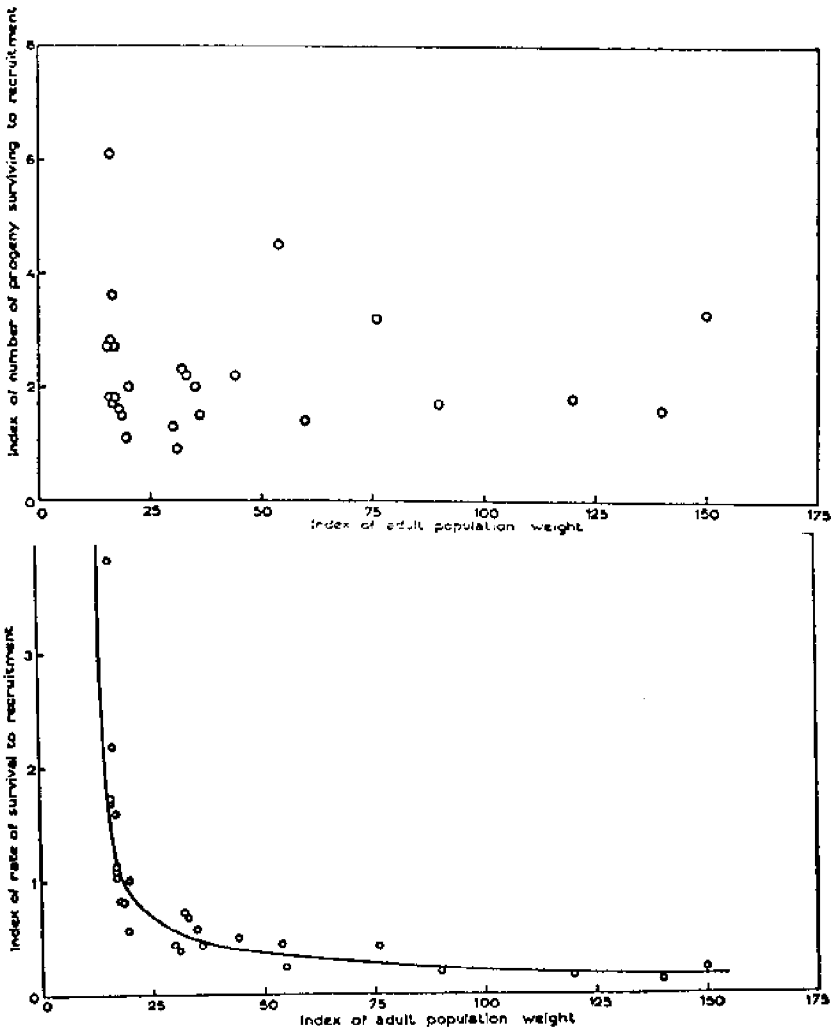

Figure 11E. The relation between adult stock of plaice in the North Sea, and the year-class strength (above), and survival to recruitment (below) (from Beverton, 1962).

objection to this procedure is that if we get a series of pairs of random numbers and divide one by the other, we get something like this relationship. This objection is valid only if the main causes in variation of the estimates were random variations inherent in the estimation procedures, rather than real changes in adult stock or recruitment, which hopefully is not true.

Having established that survival does vary with the density of adult stock, the next step is to examine this survival in detail, in relation to the density of adults of young, and in relation to possibly relevant environmental factors, especially the abundance of food.

But this immediately gives us a whole set of new questions. Up to now virtually all the data are nice convenient data referring to the commercial fisheries that can be collected without too much trouble or expense. In a typical fishery, such as the North Sea plaice the data on the distribution abundance and composition of commercial-sized fish obtainable from the fishery are far more extensive than any sampling we could expect to do ourselves. Once we want to look at the survival of the young fish and indeed their population dynamics in general, we have to start collecting samples with plankton nets. This immediately adds to the expense and difficulty of the operations, and for many fisheries the research resources are not available. This means that all that is available to aid the study of the stock-recruit relation is the commercial data and the only analysis it is possible to make is to plot the observed pairs of stock and recruitment against each other.

Figure 11F illustrates some of the difficulties encountered when applying this to Georges Bank haddock. Obviously the curve drawn does not fit the points too well. If only science was concerned, no curve should be drawn; however in the actual situation some advice had to be given to the industry and the

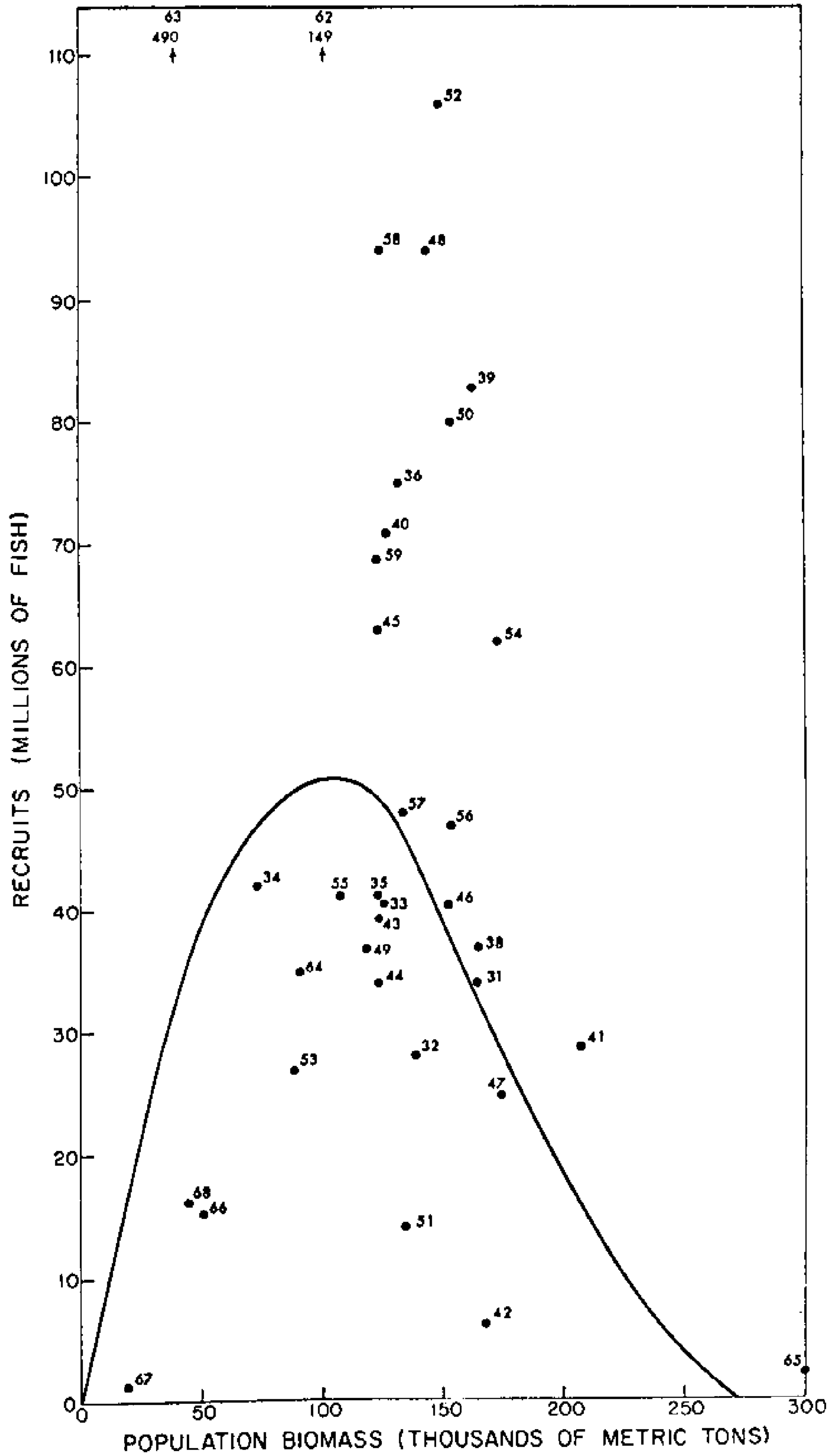

Figure 11F. The relation between stock and recruitment in the Georges Bank haddock (from Grosslein and Hennemuth, 1970).

government about what was happening to the haddock stock, and what action should be taken. To give advice, the scientist must have some concept in his mind as to the nature of the stock-recruitment relation, and this curve is probably as good as, or better than, any other.

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Chapter 12

WORLD TUNA FISHERIES: TWO-STAGE FISHERIES

Let us consider now the fishery with the biggest geographical extension--the tuna fishery. We have already considered part of the tuna fishery in the discussion about the development of the Schaefer model and the surface fishery for tuna in the Eastern Tropical Pacific. Figure 12A shows the distribution of fishing by Japanese longline vessels. Within each 5-degree square, it shows the number of yellowfin caught per hundred hooks. The zeros show where there has been fishing for other species of tuna, but no yellowfin were caught. The longline fishery extends all over the warmer seas of the world and, in some years, as far north as 60°N off Norway and as far south as 50°S in the southern Indian Ocean. For yellowfin, the concentrations, shown by high hooking rates, are all in the tropical regions. In this particular season, there was a high concentration off East Africa, a few scattered along the tropical upwelling area, and a few more off Central America.

Figure 12B shows the distribution of the hooking rate of albacore, another of the major species. While the yellowfin is a purely tropical species, the albacore is more a subtropical and temperate animal, and we see two zones of high catches, either side of low catches along the Equator. The zone in the Southern Hemisphere between 15° and 30°S is most marked, but there are also some high catches in the Northern Hemisphere between 30° and 40°N. These are two of the main species. The other tuna species that are important in this fishery are the two species of bluefin, especially the southern bluefin, both of which are mainly temperate species, distributed

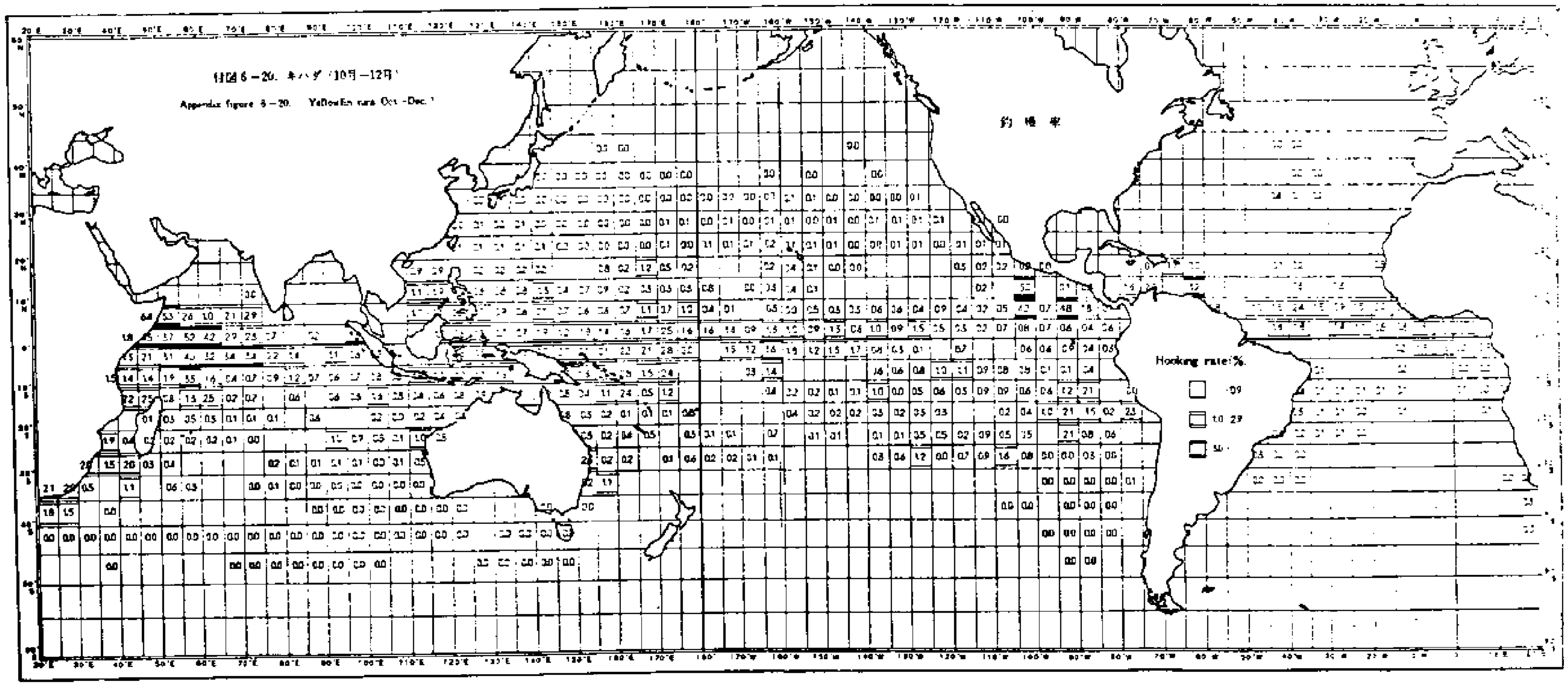

Figure 12A. Distribution of hooking rate of yellowfin tuna in the Japanese longline fishery, October to December 1968 (from Anon., 1970).

in higher latitudes than albacore, and bigeye, which is mainly a tropical species with distribution similar to the yellowfin. In addition in this fishery, the longliners get a smattering of skipjack, a number of various species of billfishes, and odd sharks. So it is a multispecies fishery.

We run into difficulties, as we often do in a multispecies fishery, but in some ways it is a fairly simple fishery. There are virtually only three countries taking part: Japan, Republic of Korea, and Taiwan. The gear has been fairly standard. The statistics are in detail extremely good, as shown in the figures. These are available by month, by these small squares, and within each square we can get the number of hooks set and the number of fish of each species caught. The Japanese data are published regularly each year, and a similar collection of data is now being made for the Korean and Taiwan fisheries. The only practical difficulty we have is that there are two sets of statistics. These very detailed statistics are based on logbook records; and these tuna vessels are away from home a long time. They may go into the Atlantic, land in various ports around the Atlantic for some time, and then finally go back to Japan after being away for perhaps two years. The time lag between when the skipper puts his notes in his logbook and the time that logbook can get into the hands of the scientists to be analyzed and checked and finally published can therefore be very long. Also these detailed statistics are always in terms of numbers of fish, whereas what we usually like to look at, especially for commercial and economic purposes, is weight of fish. Separate statistics available for the tuna fisheries are the commercial data obtained when the ship lands her catch in the various ports,

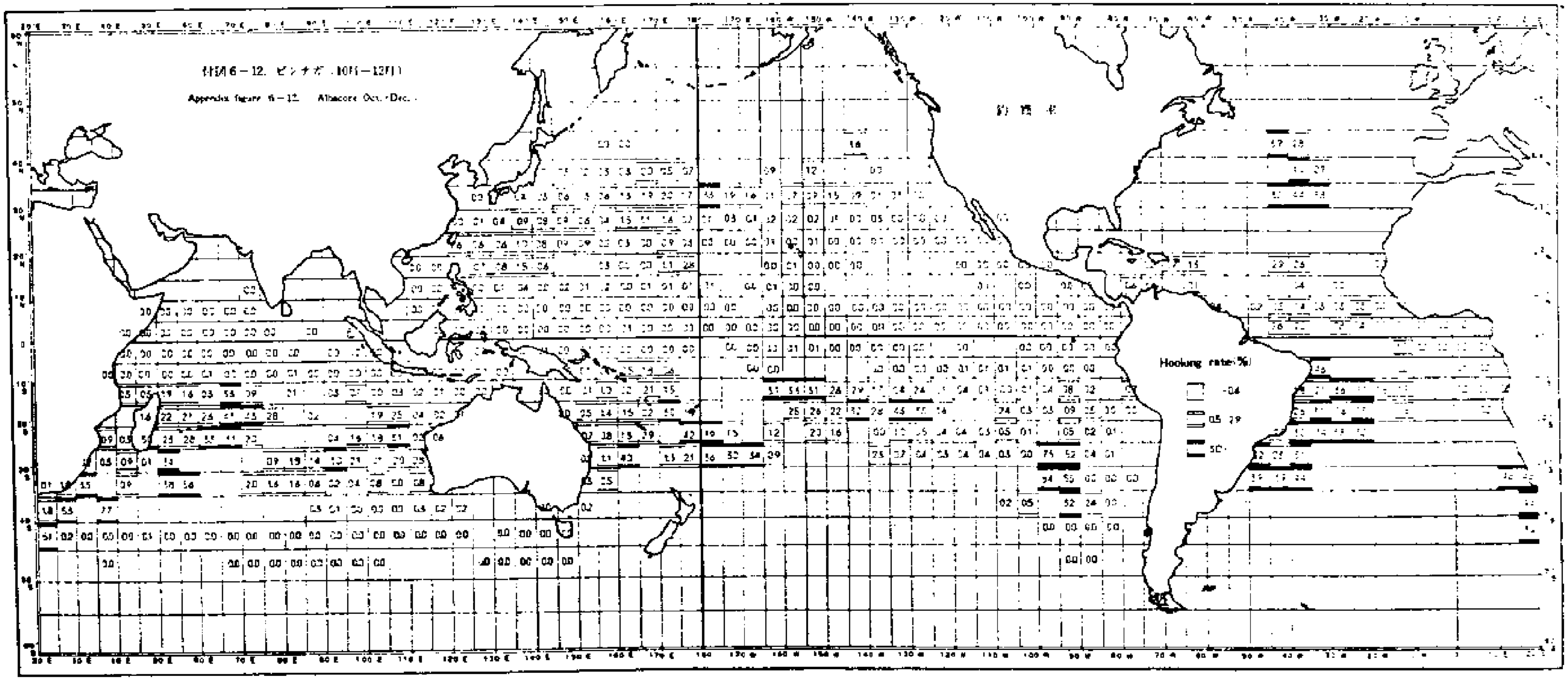

Figure 12B. Distribution of hooking rate of albacore in the Japanese longline fishery, October to December 1968 (from Anon., 1970).

either away from home or back in Japan. These give the weight landed, but without very much detail of exactly where the ship has been fishing.

One of the problems in detailed analysis, particularly when we get around to management, is fitting these two sets of statistics together. The detailed ones, though rather out of date, are extremely good for scientific analysis. They are lovely data to work on if we want to study the distribution of tuna, changes in numbers, changes in distribution, or the relation of the distribution to environmental conditions. This is fine, but if we want to give up-to-date advice about what is happening to the tuna in the Central Atlantic as of the second half of 1971, this isn't so good because the information is out of date and also we want to deal in weight and the detailed data are in terms of numbers. One of the problems we run into, as always, is whether these catch per unit figures are proportional to the abundance. The number of hooks put out might be expected to be an extremely good measure of effort. The gear has remained reasonably standard. The increases in efficiency that have taken place have been more a matter of enabling more hooks to be put out, and recently enabling the setting and recovery of the hooks of these longlines to be mechanized to a greater extent. Each set of the gear involves putting out and recovering some 50 miles of line, which has required a lot of skilled labor.

It would seem that, unlike a purse seine or trawl fishery where there are clearly great changes in the efficiency of the simple unit of effort, these longline data using catch per 100 hooks would provide a catch per unit effort figure that is a good estimate of population size.

One minor change that has taken place is the change in bait. The favorite bait has been saury, but saury stocks have been going down and catches have been declining for reasons that aren't too clear and so it has been necessary to change in some cases from saury to a less popular bait. This has meant that hooks are less efficient, but it is possible to put in a correction for that. The thing that has lately been worrying us is that in relation to other data on the abundance of large tuna, obtained from some of the surface fisheries, the catch per unit effort of the longline fisheries seems to drop off more quickly than we would expect. This seems also to be a common experience in some other fisheries using a stationary gear, which depend on the fish to be foolish enough to give themselves up, e.g. gillnets in African lakes. When we first start fishing in an area, the hook rate is very high, but soon goes down rather rapidly. The hook rate may quickly go down to a quarter of its original level, but this may not mean that the actual stocks have gone down to a quarter. What this implies in terms of q, the catchability coefficient (where $F = qf$) is that in these fisheries q may well be a function of the stock abundance. We remove the more stupid or hungry animals and bring the mean q of the population down. In the other fisheries we've been talking about, trawl and purse seine fisheries, q is probably independent of the population abundance but changes from year to year as technology improves. However the evidence of this change in q is not conclusive, and for the present will be ignored. With this reservation, we have a good set of catch and effort data for the tuna longline fishery.

One thing we must always do is look at the stock separation. In a wide-ranging fishery like this, there is no worry about the fishery exploiting only part of the stock; except for a gap in the South Pacific, there is no unfished area in which a group of fish might be present to upset the calculations. So in some ways we needn't worry too much about stock separation. However, one item about stock separation is that so far research and more particularly the management of tuna have been on a regional basis. The Tropical Tuna Commission in the Eastern Pacific and the International Commission for the Conservation of Atlantic Tuna are the two bodies which have tuna management as their prime responsibility. In addition, there are two FAO bodies that include tuna management among other fishery matters which are their concern. These are the Indian Ocean Fisheries Commission and the Indo-Pacific Fisheries Council, which covers mainly Southeast Asia and the West Pacific.

If we are looking at management in terms of these regional bodies, the question arises: Do the stocks obey these boundaries or do they extend wider? So far as the tropical species are concerned, we have no trouble in the Atlantic. The tropical tunas can't get out of the Atlantic into the other areas and the situation is much the same in the Indian Ocean, but there may be some migration right across the Pacific. This would mean that if we try to study or manage just the Eastern Pacific, without reference to the events in the Western Pacific, we run into trouble. The temperate and subtropical tunas have greater opportunity for interocean movement. The most interesting stock in this respect is the southern bluefin, which spawns mainly in the southeastern Indian Ocean. Its juvenile stages, during the first two or three years of life, are fished in the coastal waters around Australia, from New South Wales on the Pacific Coast to Western Australia. The bluefin then moves into the more open ocean and carries out very large long-range migrations.

There have been fish tagged in Australia in waters caught south of Africa and probably the same stock moves into the Atlantic. So it seems we have a single stock of southern bluefin extending at least from Eastern Australia right across the Indian Ocean into the South Atlantic. Any management will have to take into account the vast extent of this particular stock.

Having got our measure of effort and the catch statistics and having decided what stocks we should examine, we can then for this longline fishery look at the usual plots of catch and effort and see what they look like. Figure 12C gives the relation between effort and catch per unit effort in the longline fishery for the yellowfin in the Atlantic. In this particular case, there was some concern that there might be two separate stocks because in the detailed statistics of the fishery there was some suggestion of a gap in the Central Atlantic with an eastern concentration and a western concentration. So in this case it seemed reasonable to see what was happening on the East Atlantic and the West Atlantic separately, as well as looking at the Atlantic as a whole. In each of the two areas separately and for the Atlantic as a whole, there was the same pattern of declining catch per unit effort with increasing effort, with about the same percentage decrease in both areas.

Figure 12D shows data for albacore in the Indian Ocean. The lower figure shows the trend of catch per unit effort with time. It illustrates a very common occurrence in many fisheries: when the fishery starts, the catch per unit effort increases as the fishermen learn the best places and times to fish and the best detailed adjustments to their gear. The increase in catch per unit effort during this early period is not representative of changes in the stock abundance.

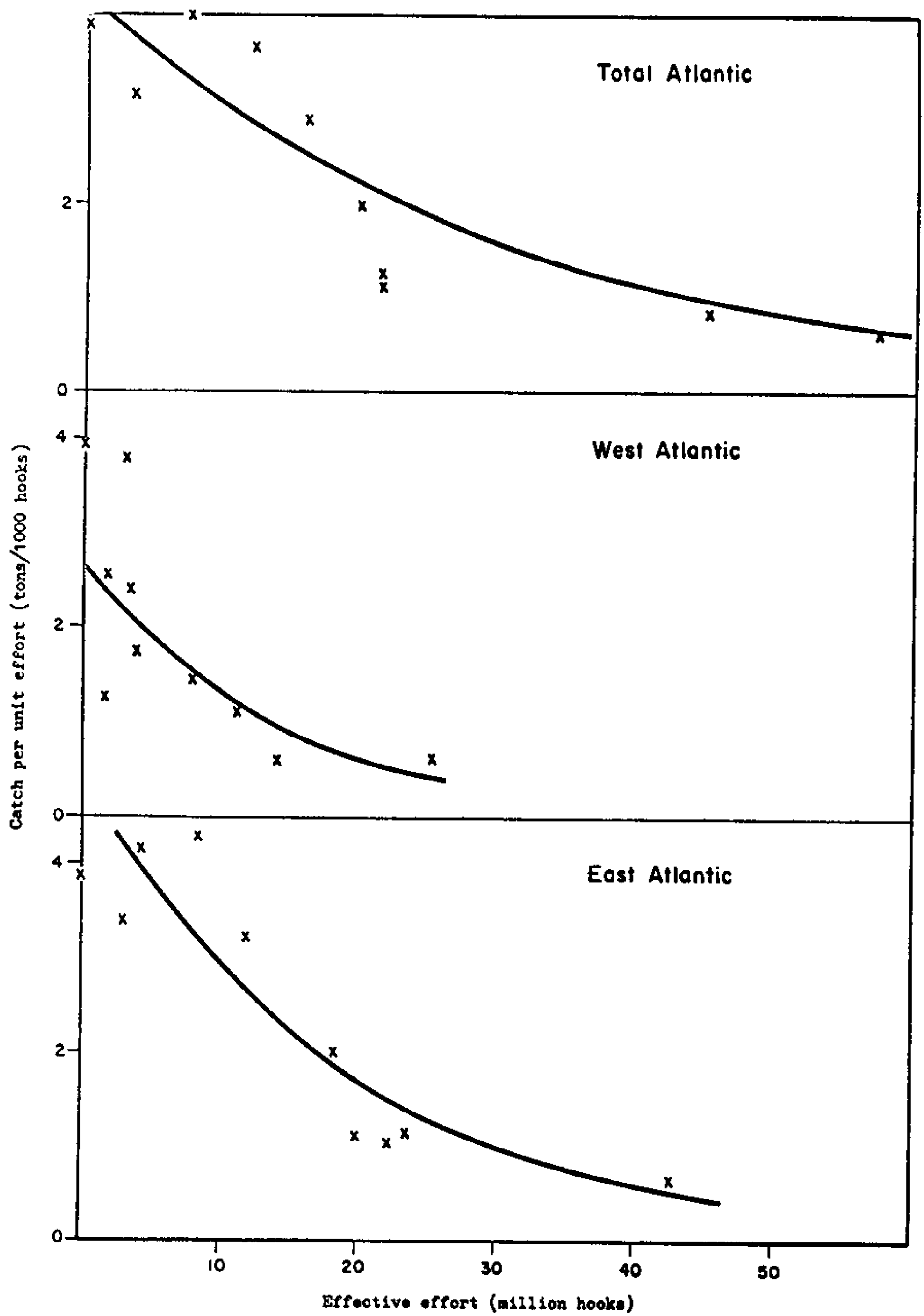

Figure 12C. Decline of catch per unit of effort with increasing effort in the Atlantic longline fishery for yellowfin tuna (from FAO, 1968).

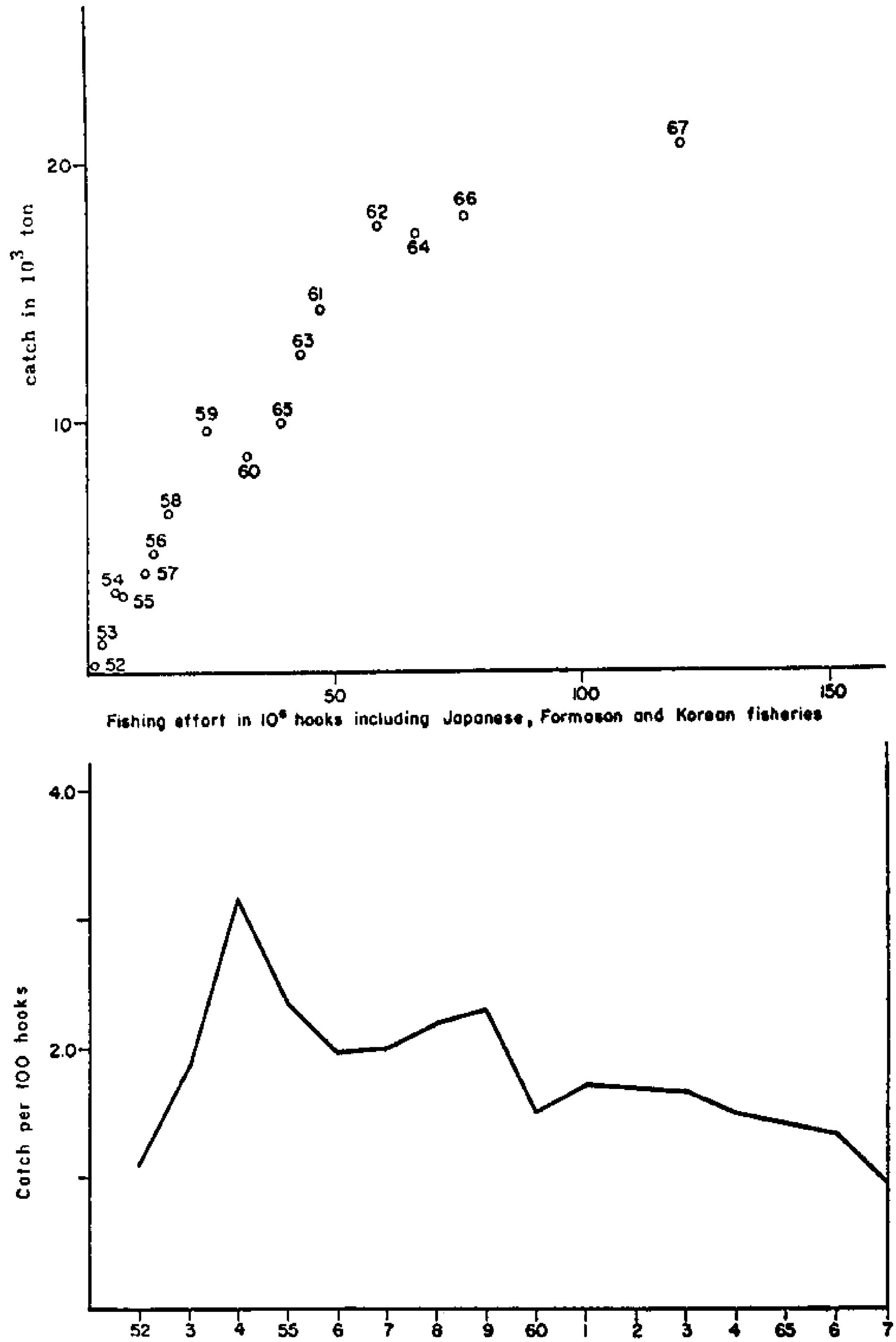

Figure 12D. Relation between total catch and total effort (above) and decline of the catch per unit of effort (below) in the longline fishery for albacore in the Indian Ocean (from FAO, 1969).

After this initial rise, there has been a very steady decline in catch per hook of albacore from a peak in 1954. This results in the curvature in the upper part of Figure 12D, which is the direct plot year by year of the total catch against the total number of hooks. This increases more or less proportionately in the early years, and then as the catch per unit effort decreases, it curves over up to 1967, suggesting that the stock is now reduced so much that we are going to get little further increase in total catch if the effort is increased beyond the present level.

The snag in any analysis of simple catch and effort data in a multispecies fishery is that, unless the species are perfectly mixed, the catch per unit effort of one species is unlikely to be a completely valid measure of abundance of that species. The species are distributed in slightly different areas. The albacore is in the temperate and subtropical areas away from the tropical belt, where the yellowfin are. A change in the main concentration of fishing from the temperate areas to the equatorial belt would result in decline in catches of albacore and increased catches of yellowfin. The observed decline of albacore catch per unit effort may have been due only to a change in the pattern of fishing rather than any real change in albacore abundance, since the unit of effort employed was just the total number of hooks used in the longline fishery in the Indian Ocean. We had the same problem for whales. For whales there was one simple solution, because there were periods when the whaling industry in the Antarctic concentrated almost entirely on one species. During each period when they were concentrating on a single species, the total effort and the catch of the preferred species could be used to obtain a good index of abundance of that species.

This is not so in this longline fishery. There have been switches from species to species, but in each year significant catches have been taken of all the main species. On the other hand, we have the data on the detailed catches and fishing effort by five-degree squares as shown in Figures 12A and B. Within each five-degree square, the catches are more likely to be predominantly of one species. Also, there is less opportunity for the fishermen to change their tactics. When they are fishing in a particular spot, they are fishing for tuna and hope for the best. A switch in interest from, say albacore to yellowfin, involves bigger changes in the area fished to, say tropical areas from subtropical areas, though since bigeye often swims deeper than yellowfin, a switch from yellowfin to bigeye might involve a change in the rigging of the gear, rather than in area. We can treat the catch per unit effort in a square, therefore, as being a good measure of the abundance of each of the species in that square. So we can write, using the obvious notation, the number of fish in a particular region $N_i = A_i \, q_i \, {}^{c_i}/_{f_i}$, where A_i is the area of the region. This expression will usually not be upset by changes in fishing tactics from one species to another. Summing over all the regions, the total number of fish will be given by

$$N = \sum N_i = \sum A_i \, q_i \, {}^{c_i}/_{f_i}$$

or, if the catchability coefficients, q, are the same in each region, the average density can be written as

$$D = \frac{N}{A} = q \frac{\sum A_i (c_i / f_i)}{\sum A_i} \qquad (12.1)$$

when A = total area = $\sum_i^7 A_i$, i.e., the overall density is proportional to the weighted mean of the catches per unit effort in each region, the weighting factors being the area of each region.

It may be noted that the simplest, but unreliable, measure of density, the total catch divided by the total effort, may be written

$$D^1 = q^1\ {}^C/f = q^1 \frac{\sum_i^7 c_i}{\sum_i^7 f_i} = \frac{\sum_i^7 f_i\ (c_i/f_i)}{\sum_i^7 f_i} \qquad (12.2)$$

i.e., it also is the weighted mean of the catches per unit effort in each region, but the weighting factors are the amounts of fishing in each region. This measure is therefore not independent of the pattern of fishing.

The ratio of these two indices, D^1/D, can be defined as the concentration index, which is a measure of the extent to which the fishermen are concentrating in the best areas for that particular species.

This is distinct from another possible measure of the concentration of fishing on the highest densities of fish, which is the correlation coefficient between the abundance of fish and the fishing effort in each region. It might be that there was a perfect correlation between fish abundance and fishermen, but if there is not very much difference between the density of fish in different areas, the fishermen would not be much better off than by fishing randomly, and we will get a low concentration index. On the other hand, there might be great differences in abundance of fish from area to area. The distribution of fishermen may not be highly correlated with the distribution of fish, but their catches will be substantially better than with random fishing, and we will get a high concentration index. Figure 12E shows trends in the concentration index for three of the main species in the Japanese

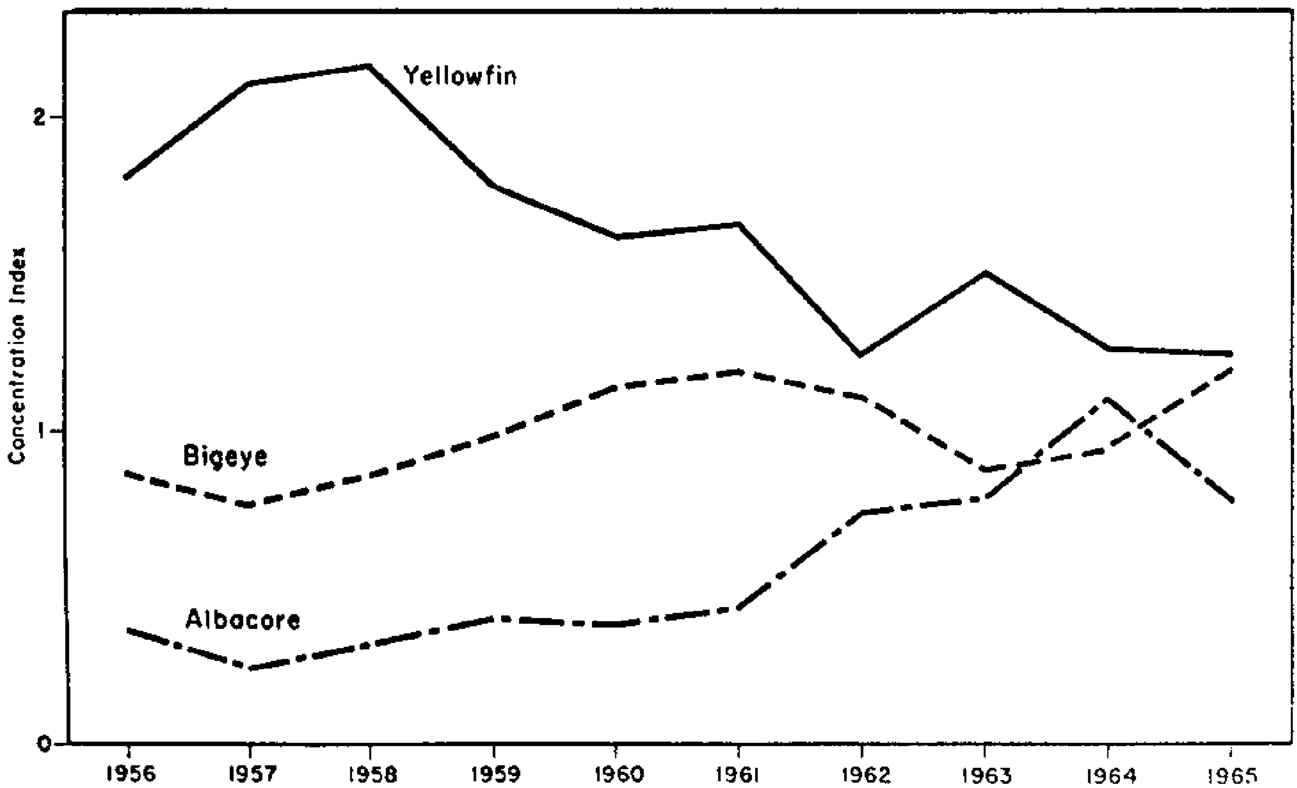

Figure 12E. Changes in concentration index for different species of tuna in the Japanese longline fishery in the Atlantic (from FAO, 1968).

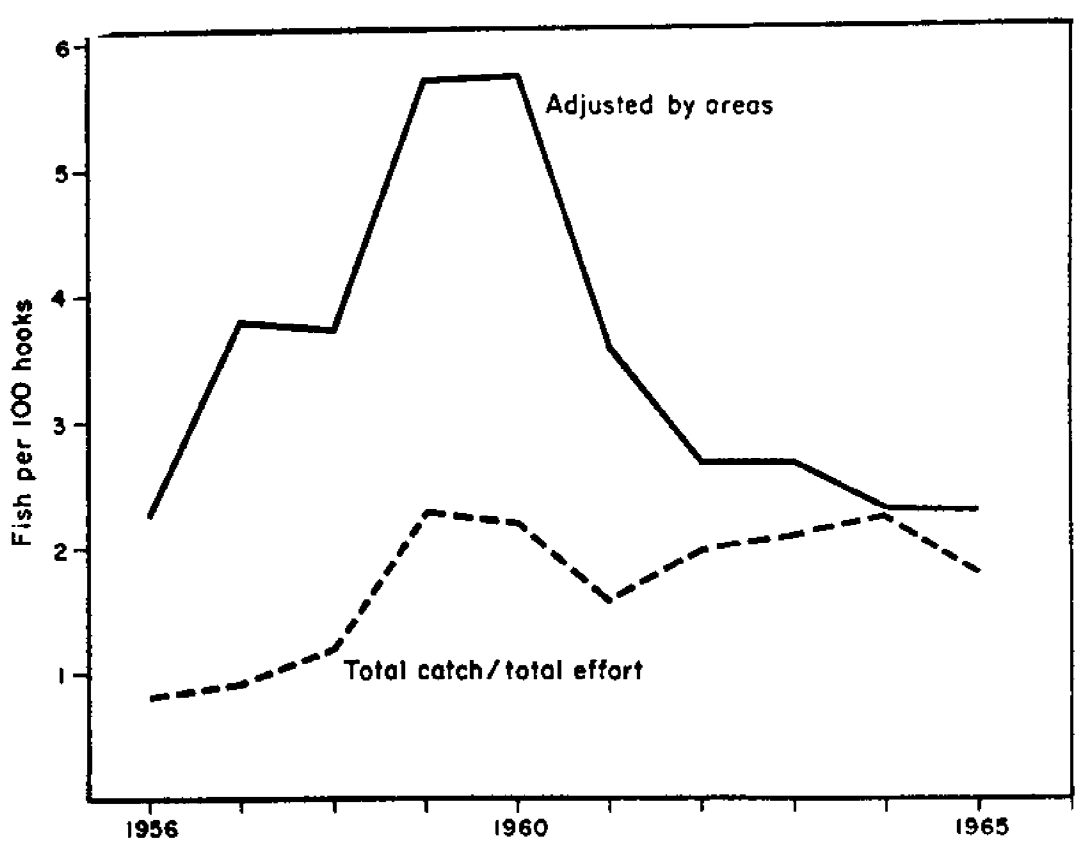

Figure 12F. Trends in catch per unit of effort of albacore calculated by two different methods (from FAO, 1968).

longline fishery in the Atlantic. Initially this fishery concentrated on yellowfin, but as the catch per effort of yellowfin declined, they switched from yellowfin to other species, particularly to albacore, and the concentration on albacore increased. The interesting thing is that in the early years the effective concentration on bigeye was greater than on albacore, even though the fishermen weren't particularly interested in bigeye, because bigeye are in the same tropical zones as yellowfin.

Figure 12F shows the indices of catch per unit effort of albacore in the Atlantic. One is total catch divided by total effort (broken line), which suggests that there has been a fairly regular increase in the abundance of albacore. The other index, using information on the regional distribution of catches and effort (full line), suggests an increase in the early years but then a big decrease. We still obviously do not have a measure of catch per unit effort which gives a true index of abundance of albacore. Probably the reason for the shortcomings of the adjusted index is that, for ease of calculation, instead of the five-degree squares, a small number of larger areas were used. The calculations were done on the basis of the total catch divided by the total effort in each of these big areas and within each area there was still opportunity to switch attention from yellowfin to albacore.

Well this has given us a nice picture of what is happening to the longline fisheries. We can draw up yield curves for the longline fishery for each of the main species in each ocean and get a fairly consistent picture rather familiar to anyone working on a well-developed fishery. These longline fisheries have reached the stage where not much further increase in total catch can be expected. There are differences of detail from area to area, and from species to species. On the whole, the bluefin and yellowfin are most heavily fished, the albacore perhaps not so much and the bigeye less still. There is some opportunity for expansion on the bigeye fisheries.

But this only gives a very incomplete picture of the situation in the world tuna fisheries as a whole because all this analysis is done on the longline fishery and there are other important fisheries on the tuna. If they were catching precisely the same sizes of fish as longline fisheries, it would be easy to include this effect in the analysis. The estimates of total catch and total effort would include the contribution to the effort made by the other fisheries. If the longline fishery is likely to provide the best measure of effort, the effort of the other fisheries can be standardized in terms of number of hooks in the usual way, i.e.,

$$\text{effort}_1 = \text{catch}_1 \times \frac{\text{effort}_2}{\text{catch}_2}$$

where suffixes 1 and 2 denote catches and effort by other years and longline, respectively. The trouble is that these fisheries do not operate on the same groups of fish.

Figure 12G gives the composition of yellowfin caught in different fisheries in the Atlantic; the two bottom curves refer to the sizes of longline fish. We see they are all very big fish, with a fork length well over 100 cm. As we might expect, there is a difference in size composition between the early longline fishery in 1955-1960, and 1965 and 1967; there has been a shift downward in the average size. They are catching marginally smaller and probably younger fish, presumably as the very big and very old fish are reduced by increased mortality, but they are still very much bigger fish than caught in the various surface fisheries. There is a range of different surface

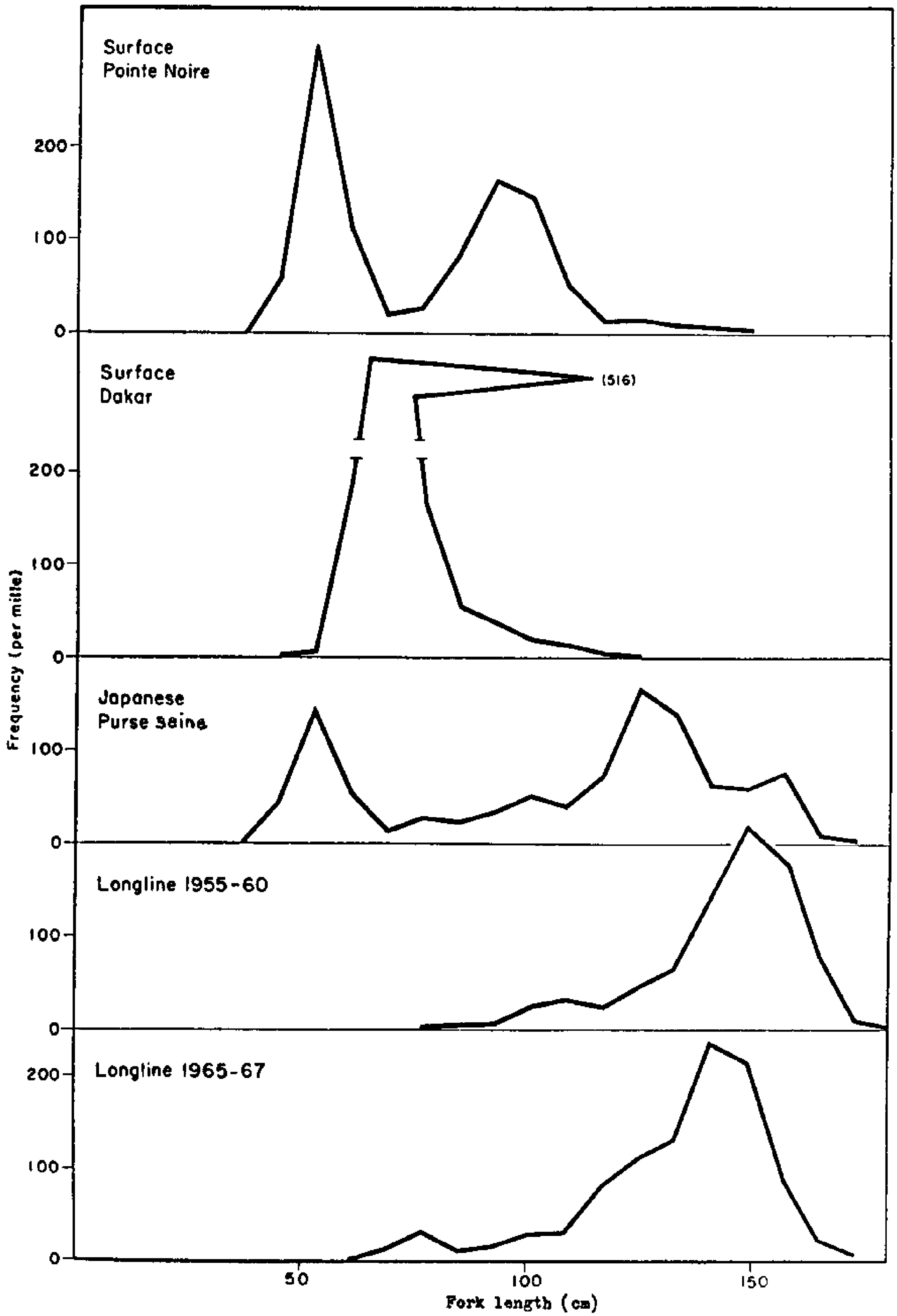

Figure 12G. Size composition of yellowfin tuna caught in different fisheries in the Atlantic (from FAO, 1968).

fisheries. One of the newest ones is the Japanese purse seine fishery, using the same techniques as the modern U.S. fishery. This catches a lot of big fish as well as a number of small fish. The older and longer established fisheries are those using smaller purse seines and live bait fishing off the African coast, mainly by French fishermen but also by local African fishermen. The upper two curves in Figure 12G show the size composition of tuna landed from surface fisheries at Dakar and at Pointe-Noire at the mouth of the Congo. These catch very much smaller fish.

The same difference appears in Table 12A, which shows the data sizes of yellowfin caught in the Eastern Tropical Pacific. Again the longline fishery catches large fish mostly from 100 cm, up to 150 cm, while in the surface fishery the biggest numbers are between 50 and 70 cm with a few bigger ones. To simplify matters, but not too outrageously, it is almost permissible to think of these as two distinct fisheries. The young fish recruit to the surface fisheries and after being exploited in those fisheries, at about 80 cm, the fish leave that fishery and then after a gap recruit at about 100 cm into the longline fishery. We can treat this in the same way that we treated the cod at Iceland, where there was some distinction between the spawning fisheries and the trawl fisheries on the younger fish, but that was a more continuous fishery. The cod were always being exploited, and it was easier there to analyze the situation in terms of an essentially uniform fishery in which there are some changes in fishing mortality with age. Here we simplify in the opposite direction, to say there is one uniform fishery over a certain band of ages by the surface fishery: then that stops and the fish recruit to the longline fishery. Such a two-stage fishery is not uncommon and in such a system the effect of fishing can be considered in four ways. The effect we have just looked at is the effect on the longline fishery of the longline fishery itself, which can be studied by an analysis of the catch and effort data of the longline fishery. A similar analysis can be done for the surface fishery on the younger fish, as done with Schaefer for the yellowfin tuna in the Eastern Tropical Pacific. We can also think about the relation between parent stock and recruits, which is the extent to which the longline fishery, by changing the number of adults may affect the recruitment of young to the surface fishery some years later.

Finally we can consider the effect of the fishery on the younger fish on the recruitment to the fishery of the older fish. This last problem is quite general and can be illustrated by reference to the Atlanto-Scandian herring, the fishery for which was simplified earlier. A previous chapter has described the main fishery on this stock, which is on the older, mature fish. In addition to this fishery, there are two groups of other fisheries by the Norwegians: one on the so-called small herring, which is 0- and 1-group fishery, mainly in northern Norway, and the fat herring fishery on slightly older but still immature fish, between 1 and 4 years old, in western Norway.

Both these fisheries have been going on a long time but have increased recently with a better market for meal and oil. One of the concerns, particularly by the Russians, who fish only the older fish, is the effect of these small and fat herring fisheries on the recruitment to the main stock. In this fishery, we have good estimates of what is happening to the adult stock; just as in the cohort analysis, we can back-calculate from the older fish in the fishery to estimate, for example, how many fish there must be in the younger

Table 12A. Comparison between estimated number (in thousands) of yellowfin caught in I-ATTC areas 05, 06, and 07 by the surface fishery and caught in the whole area east of 130°W by the longline fishery (from Suda and Schaefer, 1965).

Length	1958 S. F.	1958 L. F.	1959 S. F.	1959 L. F.	1960 S. F.	1960 L. F.	1961 S. F.	1961 L. F.	1962 S. F.	1962 L. F.
41- 50	16.9		4.9		23.5		13.9		62.9	
51- 60	574.3		142.2		362.7		754.6		1,025.5	
61- 70	501.5		585.1		527.2		1,026.8		1,867.3	
71- 80	172.6		488.0		418.9		442.1		740.2	
81- 90	91.1	0.1	465.9		222.1	0.6	380.1		332.8	0.2
91-100	91.6	0.2	321.0	0.1	234.4	2.9	183.2		132.3	0.7
101-110	130.8	1.5	126.4	0.7	256.9	0.4	162.5	1.2	45.2	11.8
111-120	56.9	5.7	56.7	2.5	159.7	2.7	163.1	4.8	74.4	10.1
121-130	11.7	8.6	7.3	7.9	112.1	7.7	162.7	21.7	94.3	13.6
131-140	1.9	8.1	2.7	9.2	90.9	15.3	87.8	40.9	25.3	22.8
141-150	0.8	17.9	0.5	9.2	21.9	20.4	92.2	32.6	26.3	28.0
151-160		17.6	0.2	7.8	3.9	11.9	75.0	14.9	29.9	24.1
161-170		5.9		2.6	3.4	4.1	16.3	3.9	9.5	7.5
171-180		0.2		0.1	0.4	0.3	4.3	0.3	0.9	1.0
TOTAL	1,650	66	2,201	40	2,438	66	3,564	120	4,467	120

Table 12B. Estimates of the effect on the longline and total yellowfin catch of a surface fishery taking 3 million fish (30,000 tons) (from FAO, 1968).

T	2	2	2	1.5	1.5	1.5	2	2	1.5
M	0.8	1.0	0.6	1.0	0.8	0.6	0.8	0.8	0.6
$\frac{F}{F+M}$	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.6	0.75
W	40	40	40	40	40	40	45	40	30
Loss to longline fishery ('000 tons)	18.2	9.7	27.2	20.2	27.2	36.6	20.4	14.5	27.5
Change in total catch (tons)	11.28	20.3	2.8	9.8	2.8	-6.6	9.6	15.5	2.5

age groups. If we make some assumption about the natural mortality in the years before they recruit to the adult fishery, we can back-calculate from the adult fishery, to estimate how many fish there were alive at any previous age. Table 12B shows some calculations for the Atlanto-Scandian herring. The first two columns give the actual catches of recent year-classes in the juvenile fisheries. The stock abundance has been expressed as the numbers at 3 years old, back-calculated from the numbers in the adult fishery. These estimates agree well with the indices of abundance of 0-group, shown in the last column. These indices of the number of herring in their first year of life were obtained from echo surveys, which, while not providing an absolute measure in terms of numbers, could give an index of young herring from the extent of the echo traces and the density of them. The numbers of fish caught, particularly in the small herring fishery, are as big as or bigger than the estimated stock abundance. So it is possible that we are removing a significant proportion of the stock in the young herring fisheries. The problem is that the numbers refer to different ages and we don't really know what the natural mortality is on these younger herring. It could be extremely high, particularly in these younger ages. Thus the 24 billion fish of the 1959 year-class estimated to be alive at 3 years old might be the survivors of a much larger number, perhaps of the order of 1000 billion, when in the small herring fishery (0- and 1-group fish). Then the small herring fishery would not have removed a significant proportion of the stock, even though the numbers caught are very large.

For the Norwegian herring analysis, this is about the present position. There is reason to suppose that the small and fat herring fisheries, because of the large numbers concerned, could be having a significant effect on the recruitment to the adult stock, but the evidence is by no means conclusive. A worrying thing in this table is that although when stock abundance is low, the catches are also low, the variation of catches is less than the variation in either measure of stock abundance. There is some suggestion that these young herring fisheries catch a larger proportion of the year-class when the year-class is weak. And this is not what we want to do when recruitment fluctuates and we are worried about falling recruitment to the adult stock. The probable reason for increased fishing mortality on weak year-classes is that the extent of the distribution of the young fish, as judged by the echo traces, changes with the strength of the year-classes. When the year-class is very abundant, the echo-traces of young herring are spread well out from the coast and the fisheries operate only on the fringe of the total distribution of the year-class. When there is a poor year-class, what fish there are, are close to the coast and vulnerable to the fishery.

While this gives some qualitative description of the likely impact of the fisheries on young herring on the recruitment to the adult herring fisheries, a quantitative measure would be better. Such a measure has been estimated for the yellowfin tuna, and estimates for different assumed values of the important parameters are shown in Table 12C. The procedure was the reverse of that used for the herring, where we extrapolated backwards from known numbers in the adult stock to some guesses in the younger stock. For the tuna, the extrapolation was made forwards from the young stock and compared the catches in the young stock of a known number of fish (in the Table 3 million fish) which, if caught in the surface fishery, would have weighed 30,000 tons, with the expected catches in the longline fishery from the same batch of fish.

Table 12C. Catches of herring in the "small" and "fat" herring fisheries, estimated stock abundance at 3 years old (in billions of fish), and index of abundance of 0-group fish (from ICES, 1971).

Year Class	Catches: Small Herring	Catches: Fat Herring	Stock Abundance	0-Group Index
1959	31.08	3.25	24.0	326
1960	28.99	1.63	9.1	147
1961	11.45	1.32	2.9	38
1962	5.84	0.32	0.1	15
1963	8.27	5.10	4.1	54
1964	8.96	5.16	5.5	75
1965	3.00	0.01	0.7	9
1966	13.31	0.22	1.7	23
1967	0.66	0.08	0.2	4
1968	1.34	0.02	0.2	2
1969	1.81	0.38	0.5	5

The expected catch in the longline fishery is given by

$$C = Ne^{-\overline{M}T} \frac{F}{F+M} \overline{W} \qquad (12.3)$$

where T is the difference between the mean age in the surface fishery and age at recruitment to the longline fishery, $\overline{M}$ is the average natural mortality during this period, F and M are the mortalities in the longline fishery, and $\overline{W}$ is the average weight of fish caught in the longline fishery. Since the chief problem being considered when Table 12C was prepared was the possible effect of an expanding surface fishery, the table expresses the reduction in the longline catch and the net effect on the total catch of an extra 30,000 tons taken in the surface fishery. For the most likely combination of the parameters shown in the first column, an additional catch of 30,000 tons in the surface fishery would reduce the catches in the longline fishery by 18.2 thousand tons, but increase the total catch by 11.8 thousand tons. Since the parameters are not well known, a number of alternative values were considered, as shown in the table.

Virtually all the combinations of natural mortality, growth, etc., give the same qualitative conclusion that fishing on the younger fish will give an increase in total catch but decrease the longline catches. The most critical parameter is the natural mortality. If it is lower than 0.8, which was believed to be the most likely value, the loss to the longline fishery will be appreciably larger and may exceed the additional catch in the surface fishery. Other examples of stocks in which the balance between fisheries at different stages of life is important are the salmon in the Atlantic and in the North Pacific. There are many problems concerning salmon in both oceans, but the specific question relevant to the present discussion is whether or not we can increase the total weight by catching the salmon on the high seas, or by catching them just as they approach home streams. If the fisheries are being managed to maintain the optimum spawning stock, and the catch taken from the returning fish is the difference between this optimum spawning population and the total run, then fishing before the fish return to home waters will reduce the allowable fishing mortality in home waters. Equation 12.3 should be rewritten to give the loss to the adult fishery as the total reduction in the weight of fish in the run, among large salmon.

$$C = Ne^{-\bar{M}T}\bar{W} \qquad (12.4)$$

The case for fishing on the high seas is that these fish have a long way to migrate, which is a dangerous operation, and the mortality will be high. On the other hand, the mortality may be low. The problem is that while we can estimate the growth exactly merely by seeing how big they are when they are caught on the high seas, and how big they are when they reach the home streams, estimating mortality is more difficult.

There are also some possible snags in estimating the increase in weight. We must know whether we are comparing the same group of fish in the two fisheries. Returning Atlantic salmon fall into two size groups, the smaller fish, which spend one year less in the sea, being known as guilse. The mean weight of all mature fish going to spawn in the rivers is about the same as, or possibly less than, the mean weight of the fish at Greenland--where the main fishery away from the home streams occurs--because the Greenland fishery is based on fish that are going to spend an extra year at sea. So we have to compare the weight of fish at Greenland with the average weight of large salmon when returning to home waters.

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Chapter 13

GULF OF THAILAND TRAWL. MIXED SPECIES - COMBINED ANALYSIS

The Gulf of Thailand is part of the very large area of shallow water in Southeast Asia, and is shown in detail in Figure 13A. The 50-meter depth contour is shown in the figure, since it seems that the fish stocks are most abundant within this depth. We are extremely fortunate in being able to study what is happening in this fishery because there have been trawl surveys by research ships starting in 1963 and going on fairly regularly ever since. These have given us very good information about what fish are there, which are vulnerable to the trawl, and the changes in the abundance and species composition of these fish from year to year. For the purposes of analyzing the survey data, the area of the Gulf has been divided into zones, as shown in Figure 13A. This trawling survey was initially carried out by German experts as part of the German aid to Thailand for development of fisheries. (Tiews, 1965)

Another, and more important, element of this program was the introduction into the Thai fishery of single-boat otter trawling with fairly light gear, instead of the other types of gear that had been used, two-boat trawling, traps, etc. This light type of otter trawl proved to be extremely effective. There had been tests before of the more conventional type of otter trawl as used in the North Sea, with heavy gear, which was useful on rough ground, but in the rather soft muddy ground in this area and other parts of Southeast Asia, it dug into the bottom and filled the net with mud. This light trawl skimmed over the bottom and got good catches. As a result, soon after the technique was introduced into Thailand, everyone switched over to otter trawling, and there was rapid and extensive development in the Thai catches. The pattern of increasing catches is shown in Figure 13B. They increased from something like 100,000 tons in 1961, taken by a variety of mainly

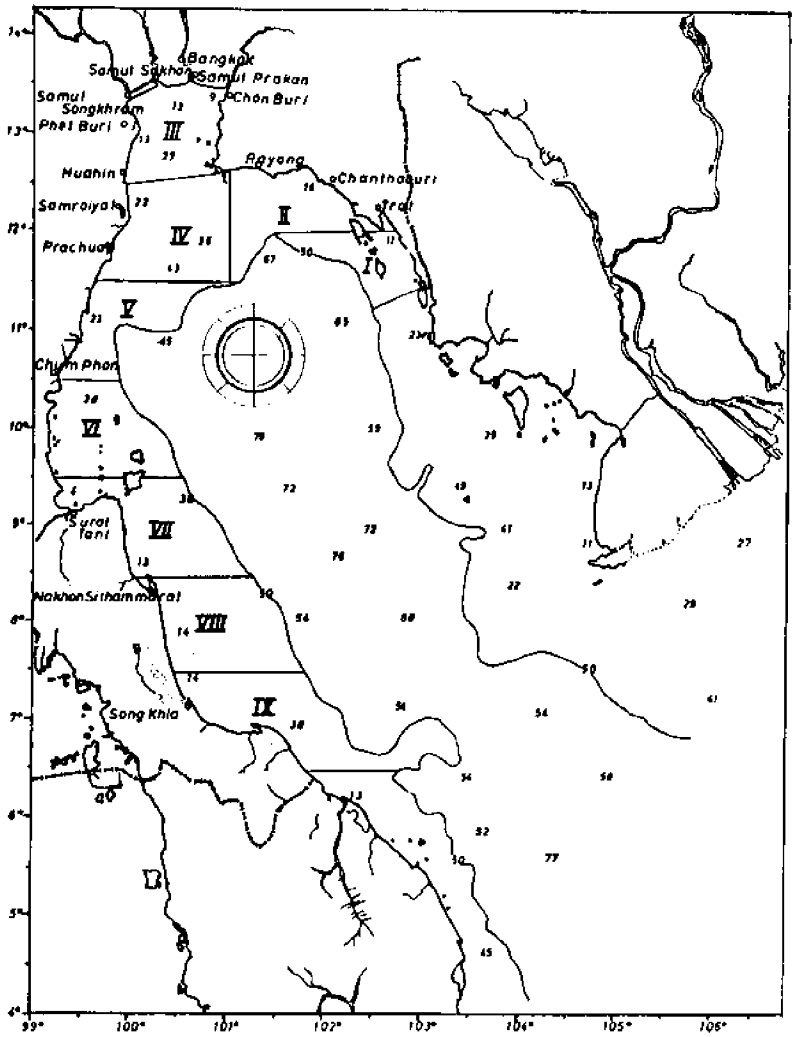

Figure 13A. Chart of the Gulf of Thailand, showing sampling areas.

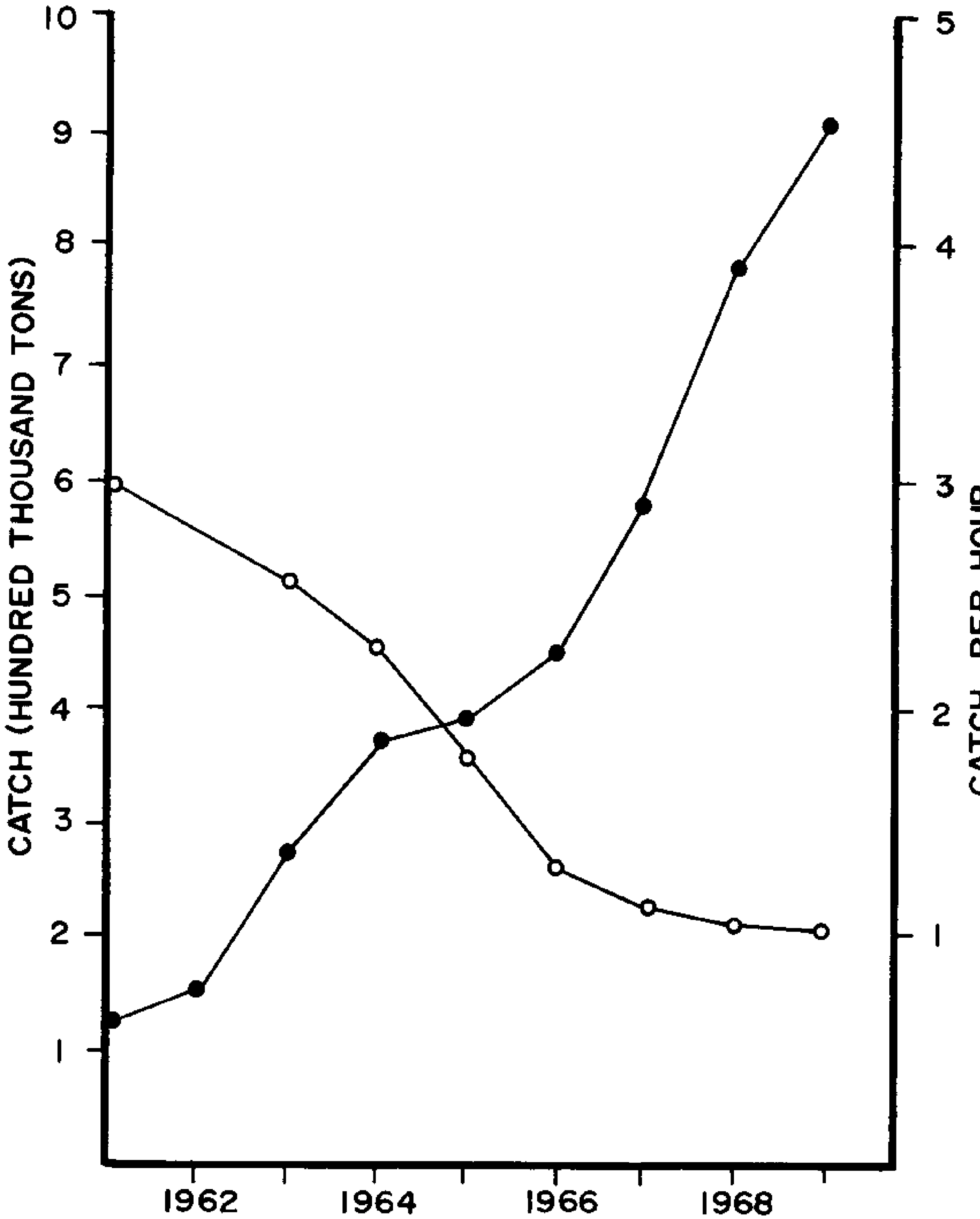

Figure 13B. Total catch of demersal fish (dark circles) and catch per unit effort of research vessels (light circles) in the Gulf of Thailand.

small inefficient boats and shot up to nearly 1 million tons by 1968.

During the same time, there was a continuous decline in the catch per hour by survey vessels (broken line). Since there is a very good measure of catch per unit effort obtained from standard gear, used the same way by the same people over this whole period, we have none of the complications and worries that we often do in trying to find a reliable measure of effort in the commercial operations. The practical trouble with estimating abundance from research ship surveys is that it is expensive and time-consuming. We need a good research boat working nearly full time to get this data, but in a big fishery like this with up to 1 million tons of fish, the use of a research vessel to get good survey data is well worthwhile.

By dividing this measure of catch per unit effort into the total commercial catch, an index of the total effort in the commercial fisheries can be obtained. This gives the usual quantities, of catch, catch per unit effort, and effort, which can be analyzed in the usual ways as shown in Figure 13C. This dotted line has been fitted by eye to the points for the earlier years and the lower values of effort. The increasing deviation from the line of the points at higher efforts, referring to the most recent years, shows one of the complications that we are running into in this area. When the fishery began to develop, all the fishing was done in the inner part of the Gulf, well within the survey area as shown in the earlier figure. Later, as the catch per unit effort dropped, and the fishermen became less successful in the traditional grounds, they moved further afield and there was a considerable spread from the local grounds to other areas outside the Gulf of Thailand. This means that we are no longer dealing with the same stock, but these catch and effort analyses should be restricted to the area covered by the surveys and include only the catches taken in that area—always assuming, as is probably

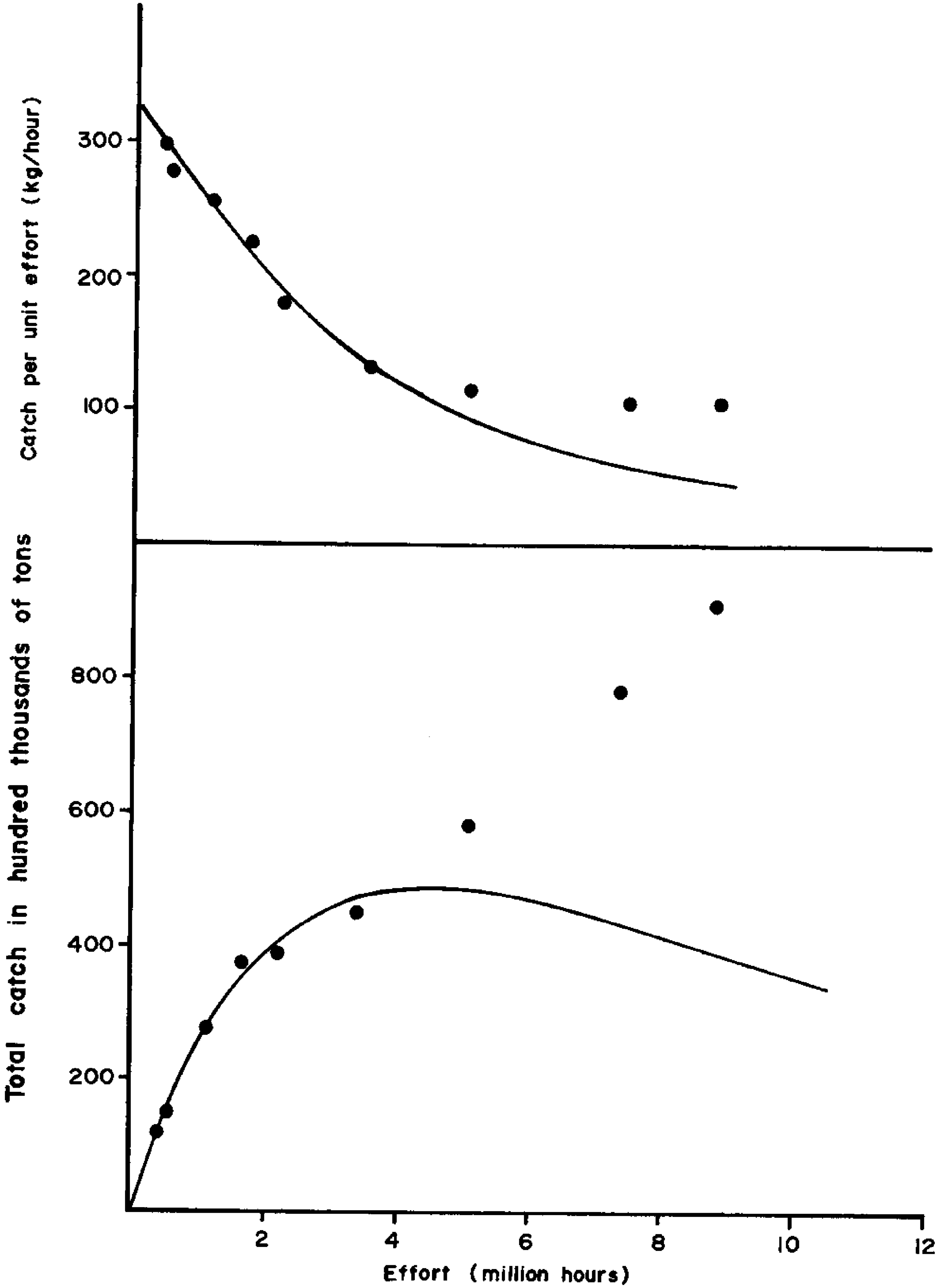

Figure 13C. Estimated relation between total effort and catch per unit effort (above), and total catch (below).

fairly reasonable, that most of the fish in the area are home-loving, and don't migrate too far. Then there will be little mixing between the survey area and outside, and we can treat the stocks in different areas as being independent. Probably a substantial and increasing proportion of the catches in the later years comes from outside the area. The difficulty in this fishery is that for political and other reasons it has been difficult to get accurate information on where the boats have been fishing.

It seems reasonable to believe that these points in Figure 13C deviate from the curves because large catches are coming from outside the survey area. At first there was a fairly smooth falling off of catch per unit effort with increased fishing, but in the last three years the catch per unit effort has not decreased much, while the total effort has apparently increased very greatly. Similarly, the curve of the total catch starts to bend over and to increase only slowly with increasing effort, but in the last three years the catches have gone up almost in proportion to the effort. It is likely that the curve describes more or less closely the fishery in the survey area, and that the extra catches are being taken from areas further afield. There is not much further we can do in analyzing these figures to see how right we are, until there is better information from the fishermen on the areas where they have been fishing.

Another major complication is the vast number of species in the fishery. We are a very long way indeed from dealing with a simple, one species, fishery. We are also a long way from the type of multispecies fisheries we have looked at so far, which are the tuna and the whales. In these there are only two or three very similar species with probably similar parameters of growth, mortality, etc. Essentially we have, in dealing with whales or tuna,

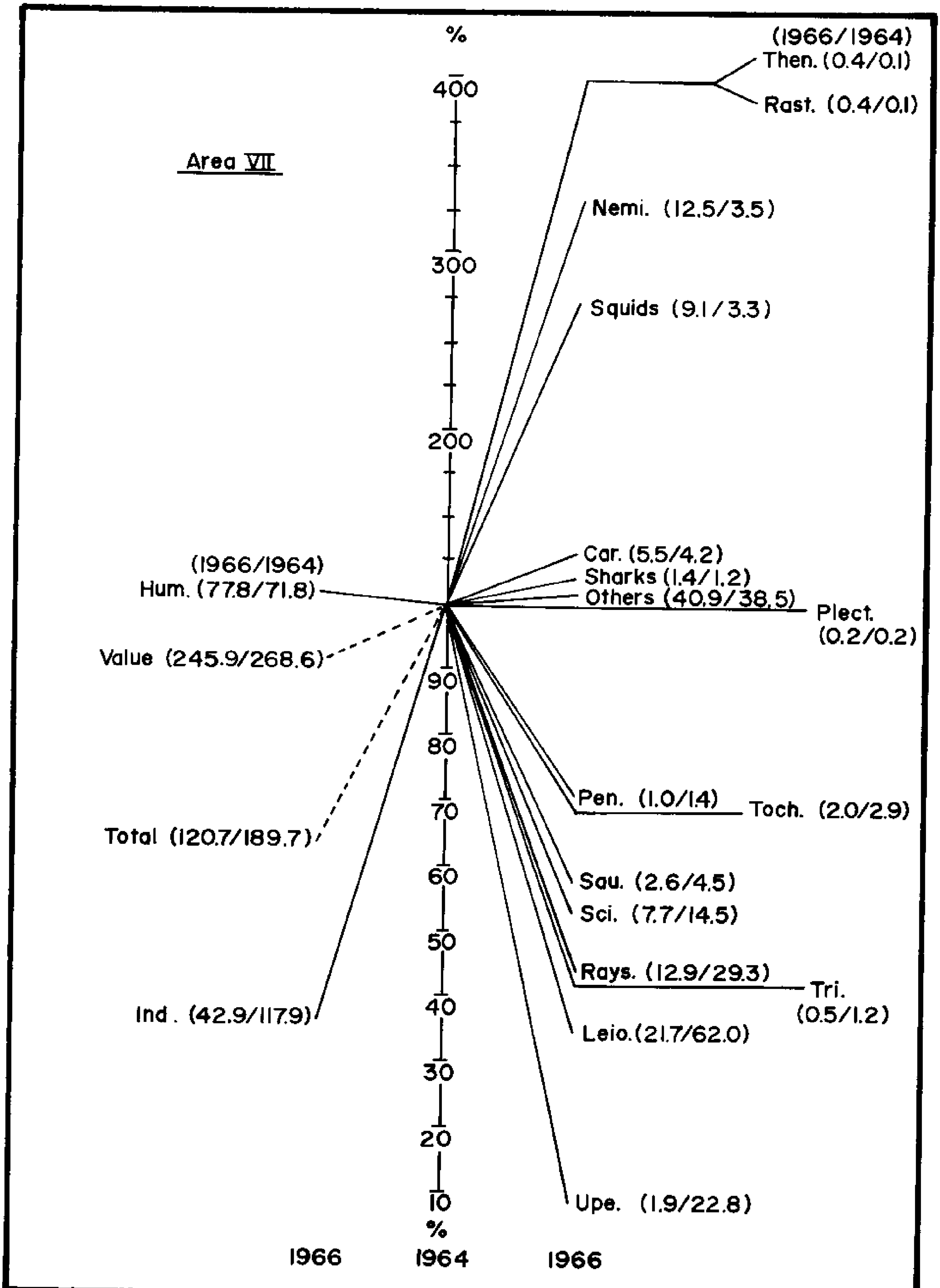

Figure 13D. Changes in catch rates of different species in two areas of the Gulf of Thailand (from Tiews et al. (1967).

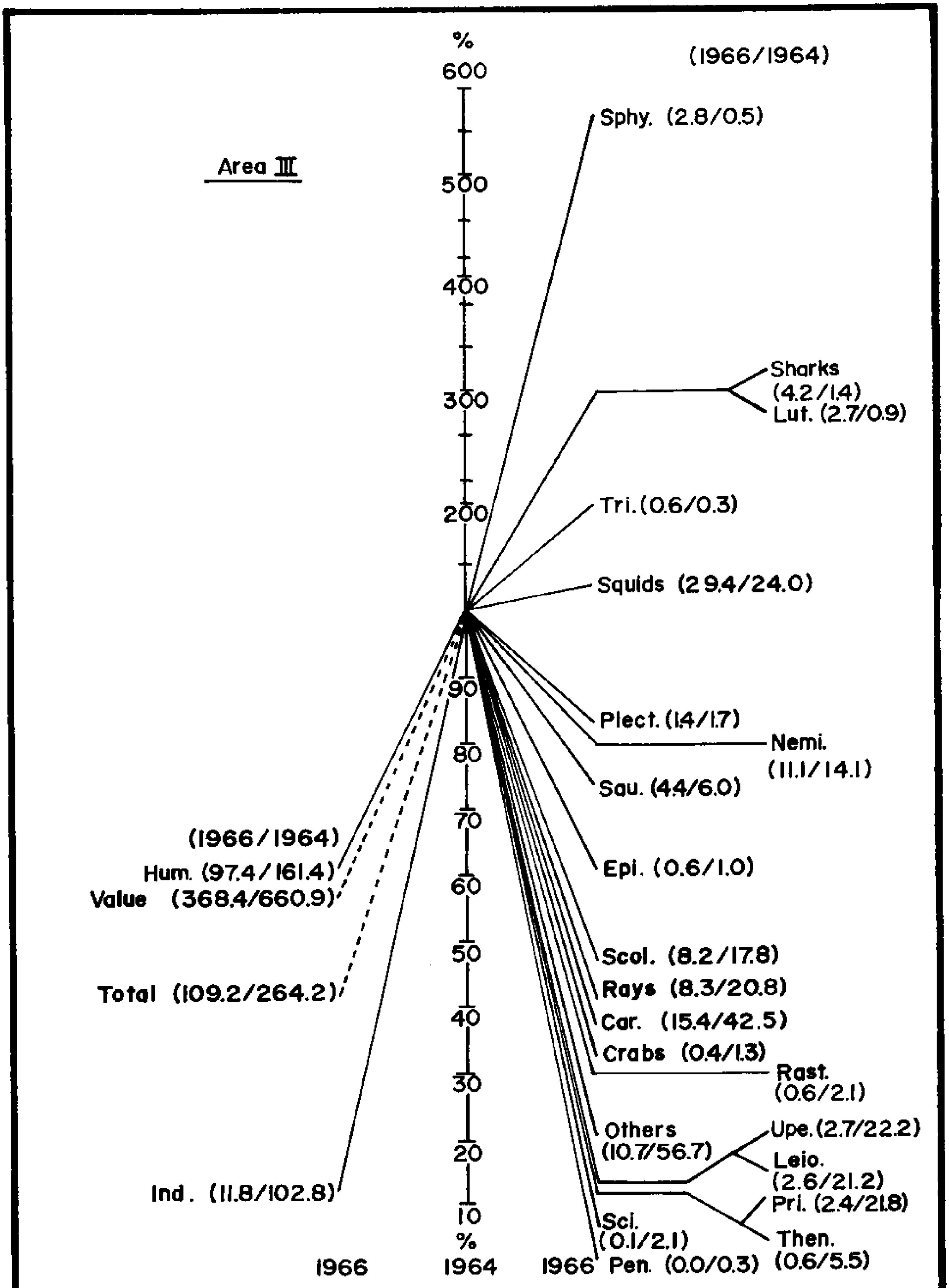

Figure 13D. (Cont'd)

reduced the problem to a single species situation. It is obvious that it is rather difficult to deal with this mixture as though it were a single species because the reactions to fishing are likely to be different for different species. Some are nice attractive fish to the fishermen, being large, probably fairly old, and fetching a good price. The fishermen will go where they are abundant, and also if the fish are old they will probably be more vulnerable to the effects of sustained heavy fishing. Other ones may be less attractive and will be avoided by the fishermen.

The resultant different trends in the abundance of the different species are shown in Figure 13D. This gives the rather complicated picture of the changes in catch per unit effort of different species in two different areas. Area III, on the left, is in the inner gulf, right at the northern end of the Gulf of Thailand and area VII, on the right is some way from the main center of fishing, towards the Malaysian boundary. All these figures are expressed as the percent changes in the catch per unit effort from 1963 to 1966. The changes in individual species are shown on the right of each diagram, and there are large and obvious differences between different species. Some have increased a great deal; e.g., Rastrelliger increased to 400% of the 1963 level in Area VII, others have decreased a great deal, and some haven't changed very much. The left-hand side of each pair of figures gives the changes in total catch. Since the prices for different species are very different, the more meaningful information for the fishermen is the changes in the total value of catch per unit effort, and this is shown in the figure as well as the change in total weight. The catch can also be divided between those species which are used for direct human consumption and the others which are small, bony, and otherwise less attractive fish used mainly for animal feed, particularly for duck food. The total catch per unit effort of those species used for human consumption is therefore also shown separately. Though there is a lot of scatter, on both diagrams the tendency between 1963 and 1966 is for decrease, which is bigger in area III, near the original center of fishing, than in the more distant area VII, down on the southwest coast which in 1966 wasn't probably as heavily exploited yet as the more local areas.

A clearer picture of the changes in some species is shown in Figure 13E which gives the trends from the first survey in 1963 up to 1970, of three groups of species. Typical of the changes in the more valuable species is the trend of catches of Nemipterus, the golden thread, which has decreased fairly steadily. Another group of species that have come down very greatly are the sharks and rays. This may be well related to the low fecundity of some of these species. In several parts of the world sharks or dogfish have appeared more susceptible to heavy fishing than other species, and probably their low fecundity means that increased survival of young fish cannot make up for reduced adult stock, which therefore results in reduced recruitment.

In contrast to these reductions, the combined catches of cephalopods (Sepia and Loligo) have steadily increased. Squids are often well off the bottom, and are therefore not particularly vulnerable to trawling. Also the fishermen have removed many of the predators on squid, and hence they have actually increased.

This shows the difficulties occurring in any complex multispecies fishery when the reactions of different species in the fishery are very different.

In theory this should be tackled by lengthy research work, looking at each species in turn and combining the analyses for each species, taking into account all the interactions between species that might be occurring. Alternatively it is possible merely to look at the total catch and relate this to the total

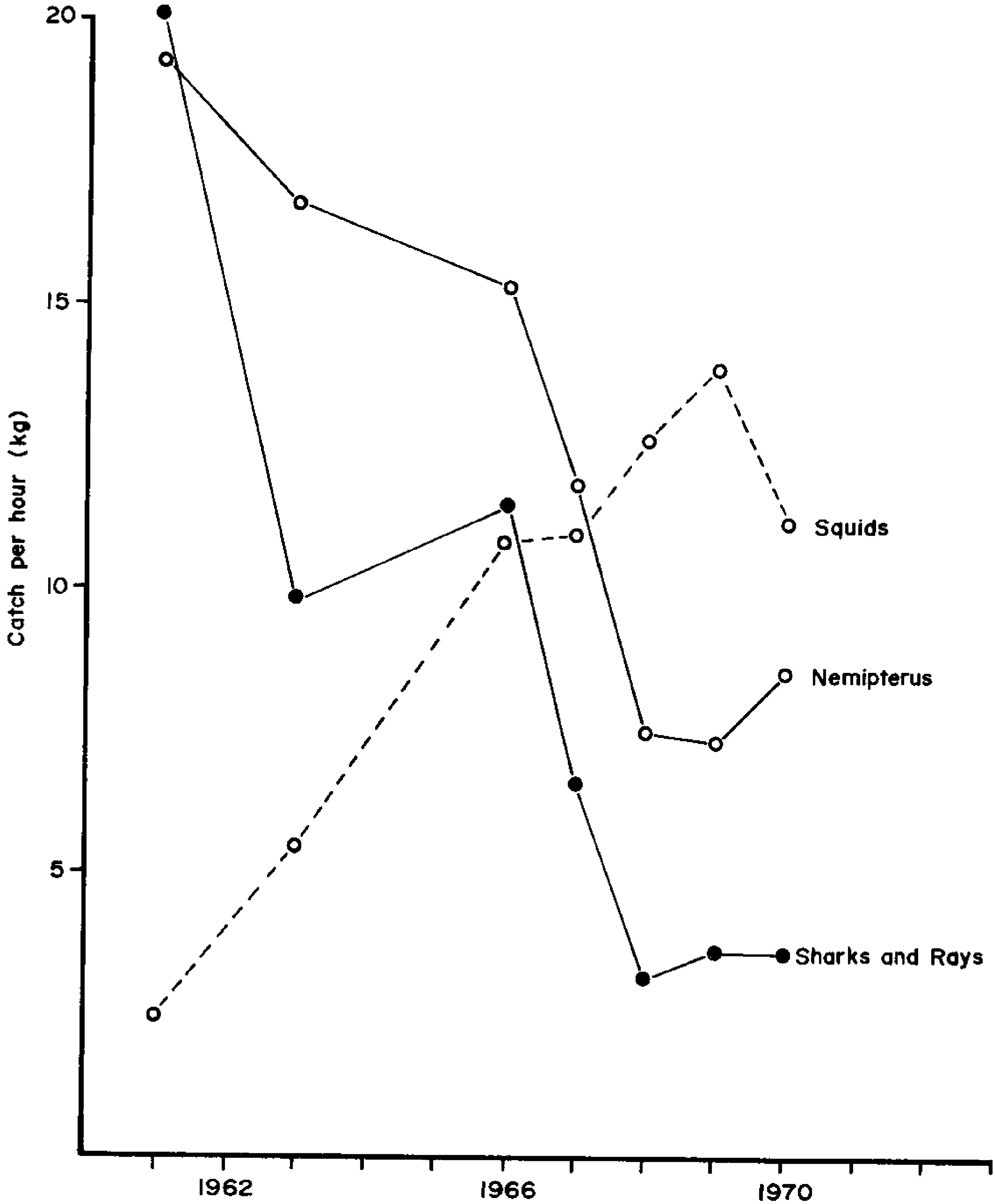

Figure 13E. Trends in catch rates of certain groups of species in the Gulf of Thailand (data from Ritragsa et al. 1971).

effort, and hope that this procedure will include the net effect of these interactions. The advantage of this second procedure is that it is simple, and can be done without waiting for the results of long research, but in the long run it is inevitable that we will have to start looking at individual species and include some assessment of various interactions between them.

The simplest analysis is to treat each species quite independently and analyze only the direct effect of fishing on each stock. This is what has been done most often for a fairly simple multispecies fishery such as the North Sea. Figure 13F, taken from Beverton and Holt (1957), shows the results of calculating combined yield curves expressed in terms of value in units of ten million shillings by adding together the individual catches of plaice, haddock, and cod in the North Sea, either as a function of the amount of fishing or as a function of mesh size. One of the complications that happens is that the curves of yield as a function of mesh size, which have been drawn for two values of the rate of fishing, have two distinct maxima. The optimum mesh size for plaice and cod is very large, about 200 mm, because the cod grow to a nice big size, and the plaice is a flatfish that does not pass easily through the meshes. The haddock doesn't grow so large as the cod and gets through meshes easier than the plaice, and so the optimum mesh size for haddock is small, in the nature of about 110 mm. As we increase mesh size from 110 mm, the catches of cod and plaice will go up but the catches of haddock will go down. To begin with, as we change upwards from 110 mm, we will lose more on haddock than we will gain on plaice and cod and the total value will drop, but with still larger meshes we get another maximum with very good catches of plaice and cod, though very little haddock.

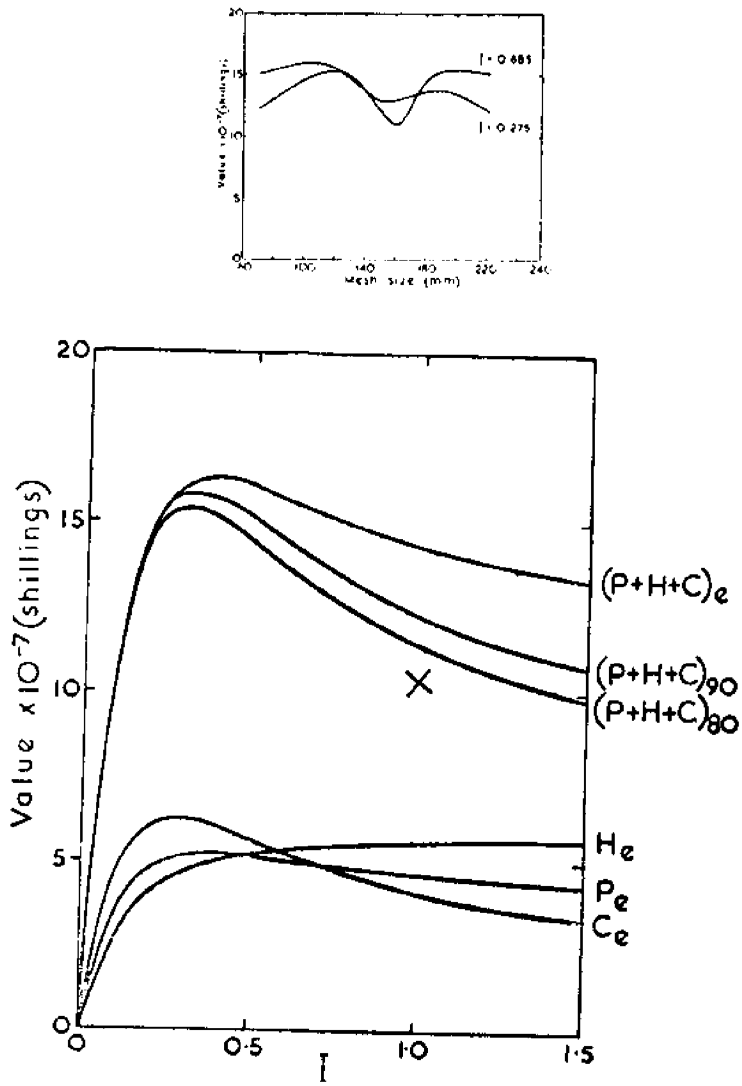

Figure 13F. Relation between the combined value of the yield of cod, haddock, and plaice, and mesh size (above), and amount of fishing (below) (from Beverton and Holt, 1957).

The two maxima give a choice here between a fishery based on three species more or less equally, or one based on the two bigger species. In this particular example the two peaks are about equal height, or if anything, the smaller mesh size gives a higher peak but it could well be that the biggest total catch will be taken with a large mesh with a lot of plaice and cod but little haddock. This would be fine for countries like England or Holland, which fish primarily for plaice and cod and are not particularly interested in haddock, but it wouldn't be so fine for Scotland because they are more interested in haddock. We have a very difficult choice between different species, which is one of the complications of a multispecies fishery.

In this North Sea fishery, the main interaction considered is that the fishing mortalities on haddock, plaice, and cod are not independent. If we want to fish harder on plaice and not so hard on haddock, it is difficult to do it, or if we want to use a large mesh on cod or plaice and a small mesh on haddock, we can't do it easily.

However, that isn't the only sort of interaction that can happen. Using the Beverton and Holt model, the yield from a particular species is a function of F, the fishing mortality, t_c, the age at first capture, which are the two factors that are directly affected by changes in the fishery, and the natural factors, R, the number of recruits, M, natural mortality and W_∞, K and t_o, the parameters relating to the growth. Our first assumption was that all these were constant. Then the various ways in which this assumption could be relaxed were examined. F could vary with age, as in the cod fisheries of the North Atlantic, as discussed in section 4 and 5. Also the natural (nonfishery) parameters could change. Recruitment is some function of the abundance of the parent stock, and the nature of the relation can be critical in determining what happens to the fishery. Also, natural mortality and the growth coefficients are functions of the stock abundance. Given some facility with mathematics, it is not too difficult to make some assumptions about the form of the functions, and to build these into the calculations of yield, (e.g., Beverton and Holt, 1957). We would expect that the natural mortality would increase with the increasing abundance, and that the growth rate would slow down with increasing abundance, and changes in these directions will not seriously affect the conclusions reached on the assumption of constant natural mortality and growth. Taking account of these changes will tend to flatten out slightly yield curves on the basis of constant parameters, but if the recruitment varies with population abundance, this will tend to exaggerate differences in yield taken with different fishing patterns. We may well find that the yield falls down to zero at not outstandingly high values of effort or that the benefit from decreasing the effort from some overfished state will be much more than expected.

Both of these are important things to know when advising on the state of the fishery. The changes in conclusions resulting in the incorporation of density-dependent mortality or growth rates are less drastic, and while interesting to know, don't worry us so much when we are trying to advise the fishing industry of what should be done. Hence we very often don't worry too much about these growth and mortality relationships, which is perhaps comforting in view of the difficulty of measuring changes in natural mortality from year to year. Natural mortality is usually estimated as the average, over a long period period; with only a single value for a long period, it is difficult to relate changes in abundance to changes in mortality. The growth can be analyzed more easily if we have a well-behaved fish for which we can tell the age, or even from the scales, the size the individual fish was at various times during its earlier life. We can then work out the amount of growth put on by individual fish in particular years and in fact draw up a growth curve and estimate growth parameters for individual years that can be related to the abundance in those years.

Nearly all these parameters, as well as varying with the abundance of the stock we are concerned with, may also vary with the abundance of the other fish in the same area. In addition, the fishing rate on one stock is likely to be some function of the fishing rate on other stocks. This is obvious in the sorts of trawl fisheries we have in the Gulf of Thailand or in the North Sea, where there isn't really any independent fishing rate on individual species. There is a total amount of trawl fishing that catches a number of species more or less indiscriminately. In the extreme case, fishing mortality on each will be equal because the fishery is randomly distributed.

But there are other cases of the fishing mortality on one stock being affected by fishing on another stock. An obvious example in the Northeast Pacific is that the fishing mortality on young halibut is some function of the amount of fishing by big trawlers on various demersal fish, other than halibut, such as ocean perch. While the only fishery specifically for halibut is the longline fishery, the trawl fishery unavoidably catches some quantities of small halibut. The study of what is happening to the halibut must take into account events in the other fisheries that incidentally catch small halibut. Similarly in the North Sea, trawlers fishing for herring with a small mesh catch small haddock, and if we are studying the haddock, the fishing mortality on small haddock is very closely related to the fishing mortality on herring, at least that part of the fishing mortality on herring which is due to the trawling for herring. In these situations, in which the incidental catches of haddock or halibut are of fish that are generally smaller than the fish caught in the main haddock or halibut fisheries, the effect may be considered as much an influence on t_c, the age at first capture, as on F.

In terms of general theory of population dynamics, some of the more deeper and troublesome scientific problems are the relationships of the natural parameters of recruitment growth and mortality in one stock to the abundance of other stocks. An example of the effect of one stock on the recruitment in another is provided by the Californian sardines. Though the Californian sardines supported one of the world's biggest fisheries thirty years ago, there are few Californian sardines now. This has been due to a continuing decline in the recruitment into the Californian stock, and the question is how much of this decline is due indirectly to fishing, which certainly reduced the adult stock. The relation between stock and recruitment in this fishery has been studied by Murphy (1966). He showed that in the period when sardine was relatively abundant, the plot of the survival of eggs to recruits of sardine, against the abundance of sardine, gave the expected picture of decreasing survival with increasing adult abundance. The data for year-classes up to about 1944 were adequately described by the same plot. About 1944 it was clear that the average recruitment was decreasing. In these later years, the moderate to very low adult stocks would be expected to give very reasonably good survival, but in fact resulted in only average to poor survival. The combination resulted in very poor recruitment. The survival-adult stock plot for these years was no longer adequately described by the same curve which held good in earlier years.

The other noticeable occurrence in the Californian current system is that there has been a very substantial increase in the abundance of anchovy. These changes have been followed well by extensive surveys in the area of the Californian current, of fish eggs and larvae. These surveys have concentrated on the eggs and larvae of sardines but also provide good data on the other species, and have shown clearly a big increase in the abundance of anchovy. One might suspect that the decrease of sardine is due to competition with anchovy. Competition is a nice word, but still leaves the need to explain what is the effect of anchovy on the sardine population. One fairly clear aspect of the competition is that the larval sardine and the larval anchovy occur in about the same area, at about the same time, and eat roughly the same sorts of things that is the smaller zooplankton. If the shape of the curve of decreasing survival of eggs and young fish with increasing abundance is because the larval fish are competing one with another for food, i.e., more larval fish, less food, therefore less survival, it would be meaningful, when analyzing the survival of sardines, not only to look at the abundance of sardines or sardine larvae but to consider the abundance of everything that is eating the same food. Then the survival of sardines from eggs to recruitment should be plotted against the abundance of sardines plus anchovy. What happens if we do this is that the early points don't change much, but the points corresponding to more recent year-classes, which are falling at present on quite a different curve, are moved well to the right because there are a lot of anchovy. As a result, they fall almost precisely on the same line as the earlier points, explaining that the survival of the recruitment of sardine is a function of the total biomass of sardines plus anchovy and that the decline in the recruitment has been due to the increase of anchovy.

To get a complete picture, we would like to have the other graph of survival from eggs to recruitment of anchovy plotted against the total abundance of sardines plus anchovy. What we might suspect is that in the middle period, when the sardines were going down but the anchovy were still not very abundant, there was increased survival, allowing anchovy to build up then to stabilize at a high level, with not such high survival but a high adult stock and therefore a high subsequent recruitment. The natural mortality of one species may also be a function of the abundance of another species, for example if the second species is a predator on the first. This possibility is particularly likely to be talked about when one group of fishermen are fishing the first species and another group of fishermen the second.

A typical example occurs in the waters around the British Isles. There are important fisheries for herring, and also some people fishing for dogfish--very good fish for fish and chips--which feeds on herring. The herring fishermen complain that the herring stocks have been damaged by dogfish, and therefore dogfish should be wiped out. On the other hand, the dogfish fishermen would like a healthy stock of dogfish to maintain their fishery into the future. The question is what quantitative advice the scientist can give in this situation. He can do some calculations that look fairly persuasive, at least on paper. Given a certain number of dogfish, they will eat a certain number of herring per day for a certain number of days per year. The total number of herring eaten by dogfish during the year can thus be estimated, and if divided by an estimate of the total stock of herring, can give us some expression for the mortality rate on the herring stocks due to the predation of dogfish. And assuming we have a good study of the fisheries on the two species, we will probably have a good measure of the stocks of dogfish, and herring. One can guess the consumption per day of predators by looking at their stomachs and get at least the consumption per meal and make some assumptions as to how many meals a day they have or how many days they go between meals to get some estimate of the total number of fish eaten by the predators. This is, of course, the same calculations that were done for birds in Peru. After their consumption had been calculated, the birds were treated as an additional fishing fleet working on the stock.

One of the strong theoretical objections to this procedure is that the fish eaten by the predator are probably not in any way a random sample of the total population. Though it is likely that fishing will tend to take a random sample from the population above a certain size (it will take the weak and the strong or the healthy and not so healthy, in the same proportion as they occur in the stock), it is very likely that the predator will take preferentially the slow, stupid, sick, or otherwise not such good fish, and the effect on the stocks of herring of the removals by the dogfish may be very different from that suggested by these calculations. It may be that most of those fish would have died anyway in the near future. It may be that by removing them, the dogfish improve the quality of the population and its genetic composition. Despite this, the calculations of the number of herring eaten can be quite helpful in mediating between groups of fishermen, one demanding fewer dogfish, the other wanting conservation of dogfish. Sometimes the calculations may show that in fact it is extremely unlikely that dogfish take more than 1% of the herring stock, which can probably by ignored. More explicit calculations of the effect of dogfish predation on the herring catches can be obtained by multiplying the number of herring eaten by dogfish by the proportion that will be caught, and by the mean weight of herring in the catches. This gives the immediate apparent reduction in the herring catches by the dogfish predation, which in turn can be multiplied by the price of herring per pound to give a total loss to the herring fishery in value. We can say that if herring had been eaten by dogfish, the dogfish would have grown, giving us a certain increase in total catch of dogfish meat. We can compare the two values and decide whether predation on herring by dogfish is or is not, from man's point of view, a good thing--again with the proviso that this is not really a random selection from the population. From the point of view of the dogfish stock, this means that the growth coefficients, K and W_∞, are likely to be some function of another stock--the prey of the stock being considered.

Questions of the interaction between species are becoming more and more important in more and more areas as the range of species being exploited increases. As a result there are an increasing number of real or apparent conflicts between fisheries in different species. For instance in the North Sea, fisheries are developing on a number of smaller fish, such as sand eels, for which a big fish meal fishery has been developed by Denmark. Many non-Danish fishermen object to this, claiming that it is ruining their fishery. Many of the fish in these fisheries, such as cod, eat sand eels, and now that sand eel fishery is taking place, the cod fishermen fear that there will be no cod left, that they will be thin, or that they won't go to the usual places because there are no sand eels there to eat. Many of these questions are difficult to resolve, but sometimes we can do calculations that show us the direction. For instance, though there are times and places where cod do eat vast quantities of sand eels, they are not the only item in their diet. Presumably the cod can maintain their food consumption near to their accustomed level by concentrating more on the items other than sand-eels.

Returning to the problems of the trawl fishery in the Gulf of Thailand, it is clear that with the information available, which in some ways is very good, we are in no position yet to understand precisely why, for instance, the squid has increased so much or why the shark has gone down. We would like to be able to do it. We would like to look at such things as the food consumption of the different species, just where one species is feeding on another, just where there is likely competition between one species and another, either in the adult and the commercial sizes, or even as in the case of sardine and anchovy, in the juvenile stages. For the present, however, we may hope that the simple-minded analyses with our eyes shut and everything out of focus, plotting total catch of all species or total valuable species against the effort, may provide reliable guidance on what is happening to the fishery. One necessity is that the analysis can be restricted to the fish in a known defined area. It seems that the answer obtained is the same as in many other areas, that in the inner part of the Gulf of Thailand, fishing intensity is now so high that the total catch has stopped going up and all that is happening is that the total catch is being divided among more and more boats. Again the solution by the fishermen is the same as it often has been, which is to build bigger boats and go further afield and find some new untouched ground. This is a solution which is acceptable to Thailand. Whether it is going to be as acceptable to the other countries in the area is the same sort of question as arises in the North Sea or off West Africa or elsewhere - how can effective management of an international high seas resource be achieved?

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Chapter 14

FISHERIES OFF WEST AFRICA: PROBLEMS OF FISHERIES ON SEVERAL SPECIES, WITH SEVERAL GEARS, BY MANY COUNTRIES

We should now attempt a comprehensive picture of all the fisheries of a single area, using those off West Africa as an example, and examine the problems, both scientific and nonscientific. Figure 14A shows the distribution of fish resources off West Africa, as known in 1960. Even though it is a fairly old diagram, it shows how well the general distribution of resources was known in this area, even ten years ago with the relatively small-scale fishing. The big fishery resources, like those fishing off most western continental coasts, are produced in upwelling areas where there is high primary production, because of the cool nutrient-rich water. This gives high production off Northwest Africa, about as far south as Dakar, and off southwest Africa, from Angola south almost to the Cape.

There are three distinct areas off West Africa--the two upwelling areas of reasonably cool water with temperate and subtemperate species and a high production, and between them a tropical zone with lower production and a quite different species composition. In the upwelling areas the dominant fish are the pelagic fish, particularly the sardines in the temperate areas and the Sardinella in the warmer water. There are also anchovy, though the stocks are not well known. Also in both areas there are slightly larger pelagic fish such as the mackerels, horse mackerels, etc. Among demersal fish, an important species in the cooler areas in the south is hake. In the north, there is an important fishery which is an extension of the hake fishery off Western Europe from Scotland southwards. The bottom fish community also changes as we move from the cooler water into the tropical areas. In the southern end of the northern upwelling area, there are important fisheries on a variety of species

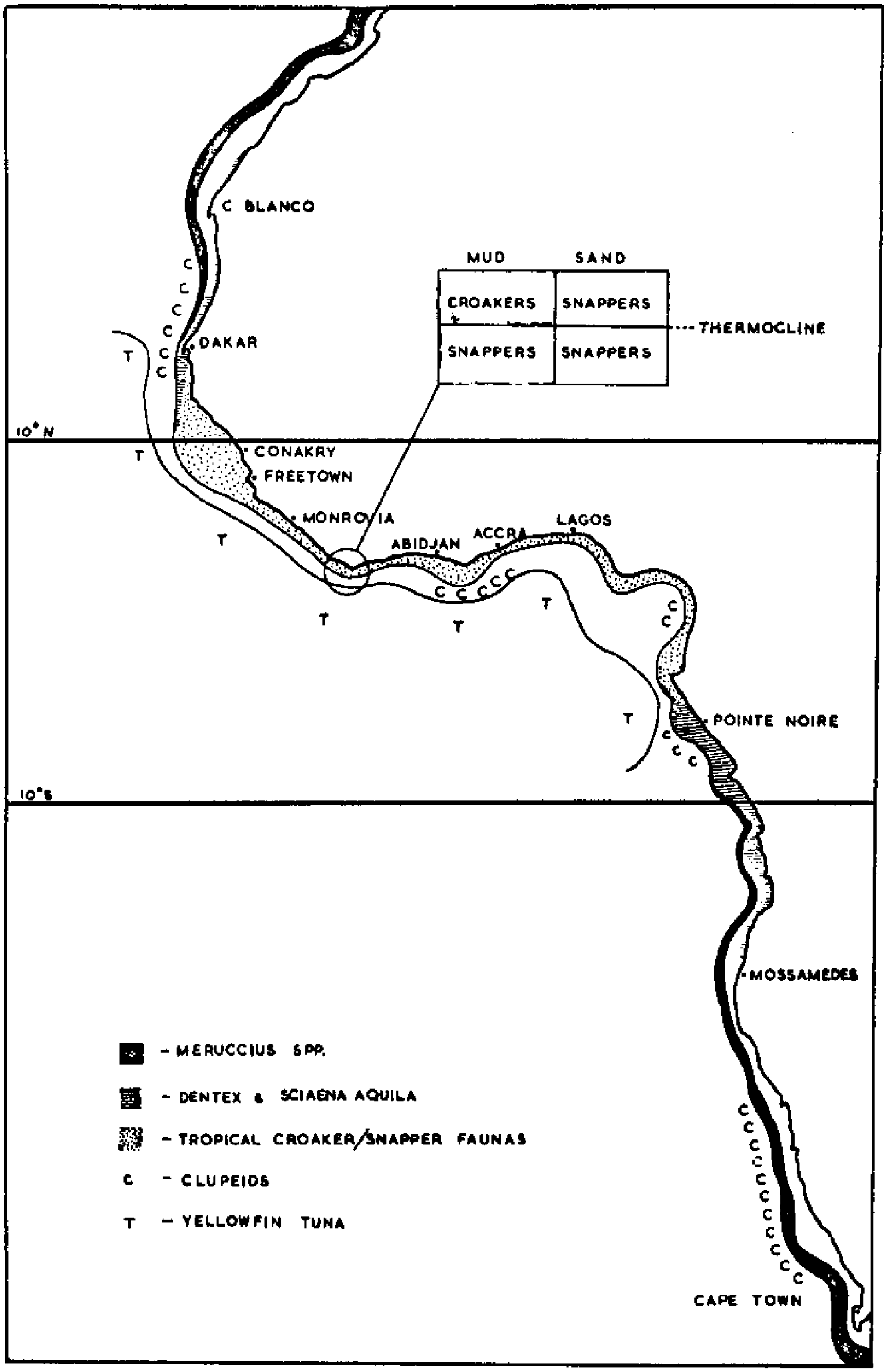

Figure 14A. Diagrammatic representation of the distribution of demersal and pelagic fisheries resources, as known in 1960, on the west coast of Africa (from Longhurst, 1963).

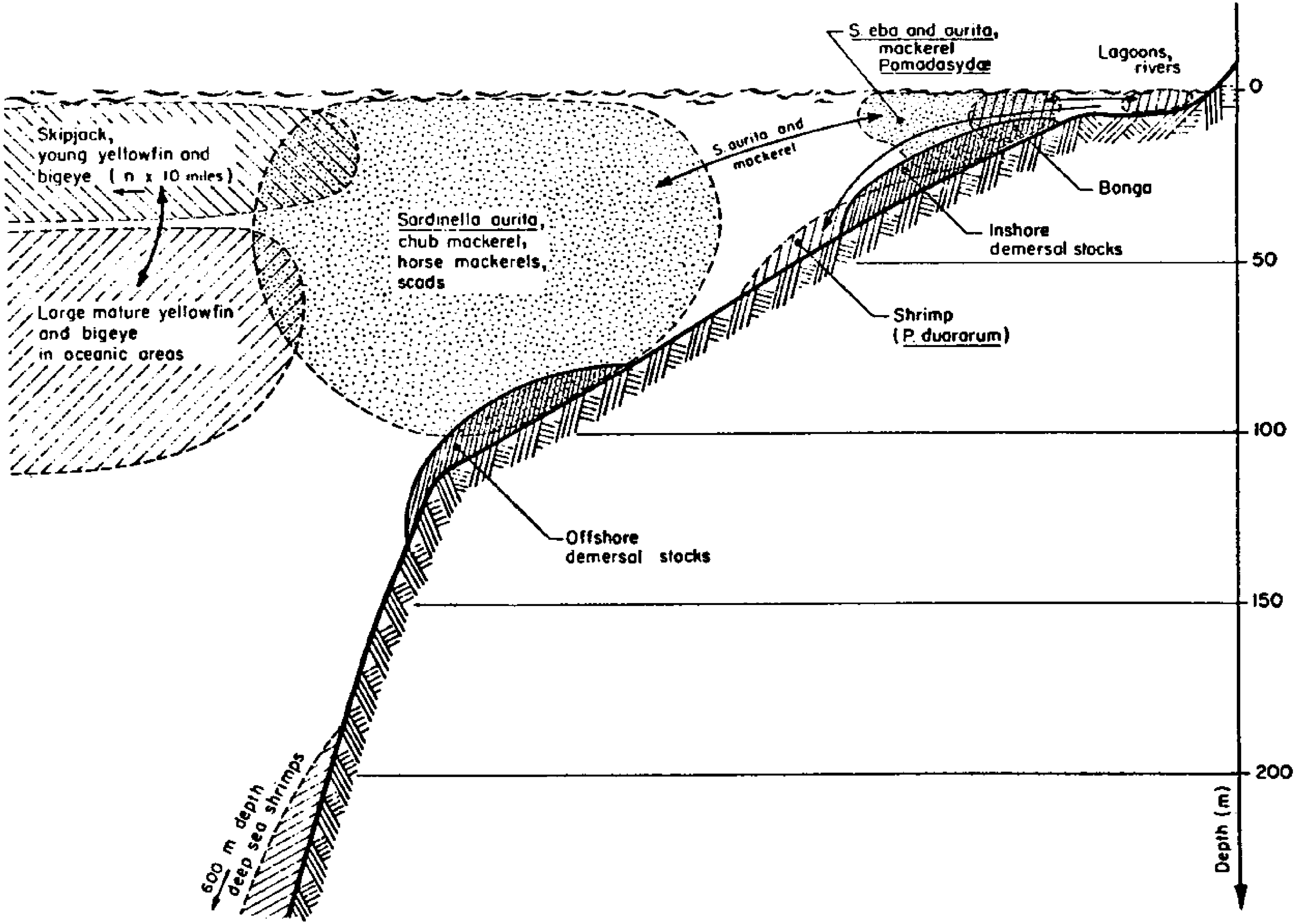

Figure 14B. Distribution by depth of species or species groups off the west coast of Africa between 20ºN and 15ºS (from FAO, 1971a).

particularly sea breams and also on various cephalopods.

This is a much more complex situation than in the temperate or northern areas such as the North Sea, where there is a range of only 4 or 5 important stocks, and where often the demersal stocks --the cod, haddock and plaice-- can be grouped together so that the analysis may perhaps in the first instance be restricted to two groups, the herring and the bottom fish. The trawl fishery in the Gulf of Thailand contains a bigger number of species, but there the situation could be simplified by considering all the species together since the fishermen fish to some extent indiscriminately on all the different bottom living fish. Off West Africa there are a large number of different stocks, which support more or less independent fisheries, or at least at any one time the fisherman is likely to be going after only one or two particular species, though he can rather easily switch attention from one group to another.

This is illustrated in Figure 14B, which shows in cross section the typical distribution of the resources, by depth and distance from the coast, in the tropical area. Starting from inshore, among the pelagic fish, we have the bonga (Ethmalosa) which is a clupeoid, rather like the menhaden, living mainly in lagoons and close inshore. Then a little bit further offshore from that, we have sardinella. There are two species of sardinella in this area, the S. eba and S. aurita, one of which stays inshore most of the time. The other one, S. aurita, moves offshore and thus can be fished both inshore by the local boats, and also further offshore.

Then as we move further offshore onto the open part of the shelf, we get the other large pelagic fish, such as mackerels and horse mackerels, and finally when we get out into the open ocean, we start getting the tunas. Among the crustaceans, inshore and in the lagoons, we have the young penaeid shrimps, which move when they are larger offshore onto the shelf --to a depth of perhaps 50 meters and are exploited there, and finally on the slope in very deep water, at a couple of hundred meters, we have a scattering of deep-water shrimp. All of these support different fisheries, as do the various demersal stocks. The other complication is the possible movements along the coast, which are very important in an area like this where there are a large number of countries, some with rather short coastlines. Some of these fish, particularly the pelagic fish such as the sardine, mackerels, and tunas, may make long migrations as shown in Figure 14C. In the course of these movements, they pass along the coasts of half a dozen countries. That is the picture regarding the resources: now let us consider the development of the fishery on these resources. For simplicity, we might think of these fisheries in three stages, starting off with the purely local fishery; then there was development of industrial-scale fisheries by vessels coming in from outside Africa, and the third phase, which is just beginning, the development by the African countries themselves of large-scale industrial fishing. The development of local fisheries has been hampered along much of this coast by a shortage of good natural harbors. There is also a quite strong surf, which makes getting out to fish in the sea rather difficult, and fishing in most of the African countries has been limited to small-scale canoe fisheries. An exception is Morocco where there has been for a long time a substantial sardine fishery, for canning and export, as well as a trawl fishery on a variety of species for local consumption.

Another feature discouraging the development of local African fisheries has been the mismatch between the distribution of fish and the distribution

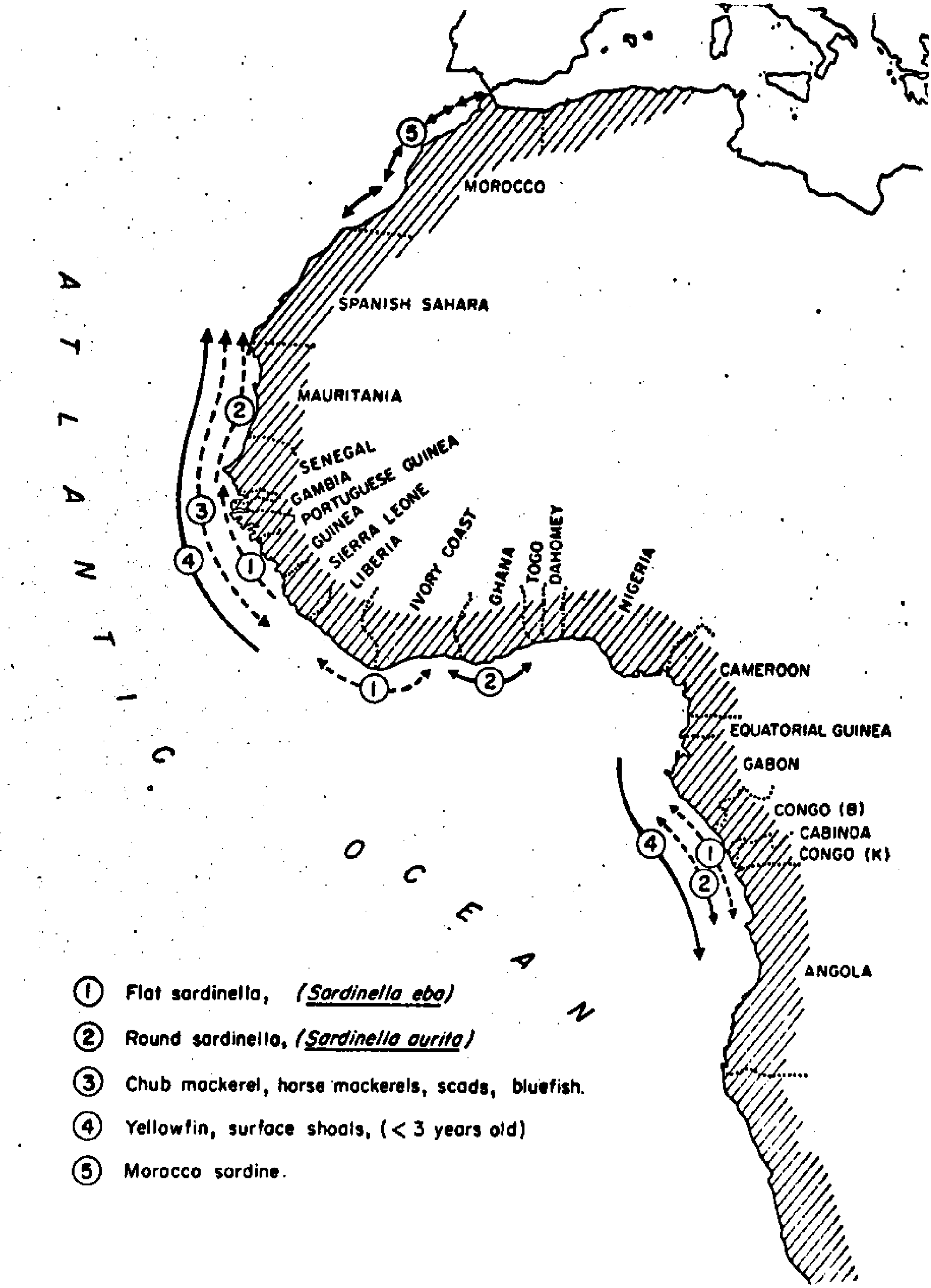

Figure 14C. Known (solid line) or assumed (broken line) migrations along the coast of some important pelagic species off West Africa (from FAO, 1971c).

of people. The same conditions really caused the two events. In the north and south the offshore winds associated with the upwelling and the high production in the sea also give the dry deserts of the Kalahari in the south and the Sahara in the north. Thus in these areas there are not many people, but there are big fish stocks in the sea. On the other hand, in the tropical zone along the Gulf of Guinea, there are countries such as Nigeria with very large populations, but without big fish resources very close to their shores, though many of them are big fish consumers. While the local fisheries have not, at least until very recently, developed very much, the fisheries from countries outside the region have built up very greatly. The long-distance nonlocal fisheries can be conveniently divided into four groups, the earliest being the movement of European fishermen down into the northern part of the area.

Among the first to come down were fishermen from England, coming down after hake, some 60 years ago, but until recently the majority have been from Portugal and Spain, who of course had not very far to come. They have fished principally in the areas along the Moroccan coast and this fishery was mainly a typical European fresh fish fishery with the catch preserved on ice. With the development of freezer trawlers, many of the boats that were fishing on ice for fresh fish were replaced by freezer trawlers, and the southern European fishery expanded to include freezer trawlers from Italy and Greece and other Mediterranean countries. The Mediterranean is an unproductive sea but the bordering countries have a good market for fish, and it is not surprising that many of these Mediterranean countries have built up a freezing fleet to work in the nearest area of high fish abundance along the northwest African coast and farther south.

The second group of non-African vessels are the much larger trawlers from the countries (particularly Russia and Japan) which have built up long-range trawling fleets capable of fishing anywhere in the world, wherever fish are abundant. These long-range fleets have moved into this area, as they have into similarly productive areas in most other parts of the world, each specializing in their favorite species. The Japanese have concentrated on sea breams and on cephalopods (octopus, squid, and cuttle fish), whereas the eastern European fleets have concentrated more on mackerels and horse mackerels. Though these species are strictly pelagic fish, they can be caught either by midwater trawls when they are concentrated in layers, or at certain times of the day when they go to the bottom, they can be caught with bottom trawls. The third important fishery that developed from outside the area is on tuna. Part of the tuna fleets are comparatively small, short-range European vessels working out of various African ports, and moving their base as the concentrations of tuna move up and down the coast. Another tuna fishery is the longline fishery by Japan, Korea, and Taiwan. This covers the whole Atlantic, but some of the areas not far from West Africa have been the most productive, particularly in the Gulf of Guinea.

The third tuna fishery, which is only about four years old, is the fleet of large purse seiners, mostly from California, which moves out of the Eastern Pacific in the second half of the year when the yellowfin quota has been filled and it is not possible to fish for yellowfin in that area. Good fishing for yellowfin has been found in the Eastern Atlantic particularly again in the Gulf of Guinea area. The most recent long-range fishery to develop is the fishery on pelagic fish, particularly on sardinella, by large factory ships supplied by fleets of purse-seiners for converting fish into meal and oil. These factory ships are usually converted whale factory ships. As the whale stocks declined and the quota was reduced, the number of factory ships that could find employment in the Antarctic fell. The companies looked for alternative employment and one of them was the production of meal and oil from fish instead of whales.

The first development took place in South Africa, partly as a way to get around the existing regulations where there were strict quota regulations on the catch. Economically these regulations were extremely successful, even though they were not based on any very precise assessment of the stocks. Only a limited number of factories could work, and those that were allowed to work made an extremely good living. All good businessmen faced with a situation like this, with restricted entry to a highly profitable operation, try to find some way of getting around the regulations. The solution was to use factory ships and work off the coast outside territorial waters and thus get a share of the catch without having to obey the regulations. Later their operations off South Africa were restricted and they were forced to find other employment for part of the year, which they did off Northwest Africa. They were joined by other factory ships, also converted whale factory ships, from Norway. These long-range fleets of all types at the present moment account for something like two-thirds of the total catch taken off West and Northwest Africa.

The third phase, which is just beginning, but which will probably be the logical conclusion, is for industrial-scale fishing to be taken up by the African countries themselves. At present they are naturally rather upset to see that most of the catches are being taken by non-African countries. A number of the countries in the Gulf of Guinea are now building up their industrial fleet. Some of them are working on pelagic fish with medium-size purse seiners, particularly Senegal, Ivory Coast, and Ghana. Ghana has also bought large freezer trawlers, which work as far north as Morocco, a considerable distance from Ghana, and Nigeria is planning to do the same.

In the case of Nigeria, there has been an interesting development. The first fishing by factory ships in this area was by these long-range boats; the Polish fleet in particular has been fishing in this area some time. There is a difference in market price for different species in the different countries and preferences vary from place to place. The Nigerians prefer one sort of fish, for example, and the Poles prefer another. Also the Poles are short of foreign exchange, and have been landing in Nigeria frozen fish of the species that are least popular in Poland to sell and use that money to buy equipment and food. This has encouraged one excellent distribution chain for fish in a hot country, which is to have a block of frozen fish unloaded from the factory ship, loaded on to a truck, and driven north into the interior. After about a day, the block of fish has just nicely thawed out and fresh fish has been delivered into the interior without any very complicated system of refrigerated trucks or other high capital equipment. There are problems, of course; if the truck breaks down halfway, for instance, then the fish have to be sold; but in general it has been an extremely efficient operation. These marine fish are now competing in the interior with the important freshwater fisheries in the lakes and rivers. Deliveries of good quality sea fish are spoiling the market for local freshwater fish. These sales of frozen fish by Polish and other trawlers have been so successful that a number of them have been chartered to work full-time supplying the Nigerian market. Soon it is expected that these will be replaced by a Nigerian-owned fleet.

The other likely development in local African fisheries, in countries like Morocco or Mauretania, will take advantage of the rich resources off the coasts and the potential demand for fish to develop more inter-African trade. Statistics of the recent fisheries in the area are given in the tables. The interesting feature shown in Table 14A is that there are some 35 countries fishing in this area now, from all parts of the world (not included in the table are Cuba and Argentina, which catch small quantities). The number is less when we consider catches of individual species, as shown in Tables 14B and C, but some 8 countries caught at least 20,000 tons of sardinella.

Clearly, we have an even more complicated situation than in the North Pacific or North Atlantic, where a smaller number of players exploit the same resource and can discuss on a basis of reasonable equality. The only concern in the international forum in those areas is for proper management of the resource. Off West Africa, however, the international community should also be concerned in adjusting the balance between the African countries, which need the fish and which also feel that geography gives them preferential right to the fish, and the richer countries chiefly responsible for development in this area.

FAO set up two international bodies to tackle the problems. (In addition, the International Council for the Exploration of the Sea, concerned with research in the Northeast Atlantic, has some interest in the northern part of the area, and the International Commission for the Conservation of Atlantic Tuna is responsible for the management of tuna in this area, as in the Atlantic generally.) One was set up some time ago, but because of political difficulties, it never came into effective operation. A new body, the Fisheries Committee of the Eastern Central Atlantic Fisheries (CECAF), was set up a couple of years ago. It had its first meeting in March 1969, and its second meeting in Casablanca in May of this year. It has become the focus for the various international activities in this area, the most immediately interesting of which are the research activities. These go back

Table 14A. Catches in the Eastern Central Atlantic (from FAO, 1971)

	1958	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970
	Thousand metric tons										
GRAND TOTAL	390.0	570.0	640.0	880.0	1 140.0	1 200.0	1 340.0	1 510.0	1 670.0	2 010.0	...
Angola (Cabinda)	1.0	1.0	1.0	1.0	1.0	1.1	0.9	1.0	1.0	1.0	...
Cameroon	2.8	3.7	5.3	6.6	7.0	8.5	9.0	10.9	12.6	15.5	...
Cape Verde Islands	1.7	1.6	1.5	2.0	2.5	3.5	4.0	5.9	4.9	4.0	...
China (Taiwan)	--	--	--	--	0.1	0.2	1.5	0.3	6.9	12.0	...
Congo, Dem. Rep. of	5.1	7.0	7.0	9.5	10.0	11.0	12.0	12.4	12.4	(12.0)	...
Congo, Pop. Rep. of	...	5.6	8.6	8.5	9.6	11.0	11.2	10.6	10.1	9.4	...
Dahomey	3.0	6.0	6.0	3.0	4.0	4.0	3.8	5.6	(5.0)	(5.0)	...
Equatorial Guinea	0.1	0.5	0.8	1.0	1.0	0.6	1.2	1.0	(1.0)	(1.0)	...
France	12.6	23.6	25.1	33.7	34.0	32.3	45.8	43.6	57.8	50.5	...
Gabon	...	1.2	...	...	1.0	1.1	2.6	2.6	(3.0)	3.5	...
Gambia	...	...	...	...	1.0	2.5	3.2	3.4	4.3	(4.2)	...
German Democratic Rep.	--	--	--	--	--	--	0.2	(0.5)	(0.5)	(0.5)	...
Ghana	30.9	34.5	42.7	56.8	73.1	66.5	74.5	103.1	94.1	140.1	...
Greece	--	14.5	17.0	18.6	21.0	27.1	30.1	31.6	36.8	33.3	...
Guinea	...	...	...	...	(4.9)	(4.9)	(4.9)	(4.9)	(5.0)	(5.0)	...
Israel	--	...	...	...	2.0	2.1	1.5	4.0	3.1	0.9	...
Italy	5.9	11.8	17.7	22.8	37.6	57.8	64.7	69.4	62.7	45.3	...
Ivory Coast	40.0	41.5	43.0	45.0	50.5	57.5	57.6	62.9	65.8	67.0	...

Table 14A. Catches in the Eastern Central Atlantic (from FAO, 1971 Cont'd)

	1958	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970
GRAND TOTAL (Cont'd)											
Japan	...	28.9	73.3	104.5	88.3	150.8	116.6	170.2	185.2	(163.5)	...
Korea, Rep. of	--	--	--	--	...	0.0	7.1	11.7	12.6	13.6	...
Liberia	1.4	2.3	2.6	4.0	7.9	11.5	11.8	13.5	15.6	18.5	...
Mauritania	...	...	...	...	(14.0)	(15.0)	(16.0)	17.7	(18.0)	(18.0)	...
Morocco	167.4	172.3	165.1	176.5	189.5	204.8	292.9	249.2	207.9	215.6	...
Nigeria	...	35.5	35.7	43.2	55.4	(58.0)	(60.0)	66.8	(67.0)	(67.0)	...
Norway	--	--	--	--	--	--	0.5	1.2	0.6	2.1	99.0
Poland	--	0.2	4.6	11.0	15.3	24.6	40.7	44.3	32.9	44.5	...
Portugal	43.8	52.8	48.3	47.6	47.4	45.8	41.4	39.8	40.0	36.5	...
Portuguese Guinea	0.5	0.7	0.6	0.6	0.8	0.9	0.7	0.7	1.3	1.7	...
Romania	--	--	--	--	3.0	4.9	7.1	8.8	5.5	(6.0)	...
São Tomé and Principe	0.6	0.7	0.6	0.5	0.8	0.9	0.8	0.9	0.8	0.8	...
Senegal	56.9	97.9	103.4	100.7	99.8	101.2	116.5	132.0	153.7	162.1	169.2
Sierra Leone	17.5	22.4	25.5	27.6	29.1	31.8	31.4	32.7	22.6	24.6	...
South Africa	--	--	--	--	--	--	--	--	--	48.0	350.0
Spain	...	...	...	...	155.0	167.1	181.4	179.4	178.3	178.4	...
Spanish Sahara	2.2	5.7	4.8	2.0	2.3	4.8	3.8	3.9	3.9	(4.0)	...
Togo	1.9	...	2.8	1.7	3.0	3.5	4.5	(5.0)	(5.0)	(5.0)	...
USSR	...	...	...	147.1	163.9	82.4	79.3	153.5	318.6	569.7	...
United Kingdom	--	--	--	--	--	--	--	--	--	--	36.0
United States	--	--	--	--	--	--	--	1.4	10.4	22.5	22.5

Table 14B. Catches of some bottom-living fish in the Eastern Central Atlantic (from FAO, 1971)

SEA BREAMS:

	1962	1963	1964	1965	1966	1967	1968	1969	1970
	....................Thousand metric tons (live weight).....								
TOTAL	125.0	138.6	152.1	141.5	137.0	143.3	133.5	149.7	...
German Dem. Rep.	--	--	--	--	0.0	0.0	0.0	(0.0)	...
Ghana	3.6	7.7	7.1	3.5	1.2	5.1	2.8	8.0	...
Greece	(8.5)	(9.3)	(10.5)	(13.5)	13.8	13.2	12.6	(13.0)	...
Israel	--	--	(1.0)	(1.0)	(0.7)	(2.0)	(1.5)	0.4	...
Italy b/	(4.2)	(4.8)	(6.8)	(9.8)	(9.1)	(10.4)	(9.4)	(9.4)	...
Ivory Coast	...	...	...	...	...	...	11.9	6.2	...
Japan	15.4	20.9	11.4	15.0	19.4	23.6	16.2	21.1	...
Korea, Rep. of	--	--	--	0.0	0.8	1.6	1.8	(1.8)	...
Liberia a/	(1.3)	(2.0)	(3.9)	(5.7)	(5.9)	(5.9)	(6.0)	(6.0)	...
Morocco	2.4	1.7	2.2	1.8	(2.0)	(2.0)	(0.0)	(0.0)	...
Nigeria c/	--	--	--	(20.0)	(20.0)	(20.0)	(7.0)	(7.0)	...
Poland d/	1.0	4.0	4.7	3.9	2.7	2.0	1.2	0.8	0.3
Portugal	11.6	10.0	8.5	7.8	6.0	5.9	5.9	(5.9)	...
Romania e/	--	--	(0.9)	(0.7)	(0.5)	(0.4)	(0.3)	(0.3)	...
Senegal f/	15.0	14.4	12.1	12.9	12.3	8.9	7.8	10.3	...
Spain g/	(37.0)	(36.0)	34.5	27.7	26.4	19.6	18.2	18.4	...
USSR h/	(25.0`	27.8	48.5	18.2	16.2	22.7	30.9	41.1	...

a/ 50% of total.
b/ "Sea breams" as percentage of total Italian catch assumed to be the same as that of Portugal.
c/ Includes catches by chartered vessels from USSR and Japan.
d/ For 1962 estimated as 25%.
e/ "Sea breams" as percentage of total Romanian catch, assumed to be the same as that of Poland.
f/ Offshore: estimate for 1962 refers to total catch.
g/ 1962-63, as given in the old estimate.
h/ 1962, rough estimate.

Table 14B. Catches of some bottom-living fish in the Eastern Central Atlantic (from FAO, 1971) (Cont'd)

CEPHALOPODS (CUTTLEFISHES, OCTOPUSES, SQUIDS):

	1962	1963	1964	1965	1966	1967	1968	1969	1970
	Metric tons (live weight)								
TOTAL	...	...	48 536	126 936	121 848	168 433	177 638	130 268	...
Cuttlefishes	...	...	24 051	46 782	36 998	45 572	42 474	35 211	...
Japan	9 900	19 800	14 451	31 082	22 098	31 672	29 374	24 054	...
Morocco	400	500	700	1 000	1 200	900	1 100	1 000	...
Portugal	2 300	2 300	1 500	1 900	1 200	1 700	(1 700)	1 057	...
Spain	...	...	6 600	8 200	7 700	7 800	7 700	6 500	...
Greece	...	...	400	800	900	700	900	900	...
Italy	...	...	400	3 800	3 900	2 800	1 700	1 700	...
Octopuses	...	...	16 506	69 579	74 393	103 437	122 114	81 954	...
Japan	...	...	6 706	26 479	25 693	53 837	72 914	46 154	...
Spain	...	...	8 500	35 900	39 100	41 600	41 700	29 300	...
Greece	...	...	1 000	1 500	1 800	1 700	2 500	2 500	...
Italy	...	...	300	5 700	7 800	6 300	5 000	4 000	...
Squids	...	...	7 979	10 575	10 457	19 424	13 050	13 103	...
Japan	...	...	2 679	5 775	3 957	11 524	6 650	5 617	...
Portugal	200	300	200	300	300	300	300	186	...
Spain	...	...	4 900	3 400	4 700	6 100	5 400	6 500	...
Greece	...	...	100	200	300	300	200	200	...
Italy	...	...	100	900	1 200	1 200	500	600	...

some time. Two important symposia dealing with the various scientific problems related to fish resources and conditions in the sea have been held, one in Abidjan in March 1966, and the other in Teneriffe, in the Canary Islands in 1968, one under joint auspices of FAO and UNESCO, and the second under joint auspices of FAO and the ICES, the International Council for the Exploration of the Sea, with headquarters in Copenhagen. These symposia dealt with a whole range of general scientific problems concerned with marine research, of which the fisheries were only a part. They did not attempt to give specific advice to governments about what was happening to the stocks and what needed to be done in relation to possible overexploitation. This has been undertaken by two international working groups. The first was again set up jointly by ICES and FAO and met directly after the Teneriffe symposium. When CECAF was set up, one of its first activities was to establish a scientific working group to examine the stocks. This group, which includes scientists from France, Ghana, Japan, Nigeria, Poland, Senegal, and Spain, met in Rome in March last year and again in March 1971 and presumably will meet again fairly early next year. The political difficulties which arose in the intergovernmental organization of getting all the countries to sit down together did not apply to the scientific groups. Scientists from different countries can sit down quite well together. In the Teneriffe working group, we had scientists from South Africa, Nigeria, Ivory Coast, East Germany, and West Germany and one or two others and no problem was encountered.

The first task at the early sessions of the working group was to arrange for the provision of data to work on, specifically comprehensive statistical data on catches and fishing effort. With 35 countries fishing in the area, all with their own systems of statistics, some of them rudimentary, the collection of statistics, on a uniform, or at least a compatible, system is far from easy. Statistics from national sources are likely to group species in different ways, use different subdivisions of the region, and otherwise be arranged in ways that make computation of international data difficult.

Following advice of the scientists, one of the first things done by CECAF was to arrange for improved statistics and to get countries to agree on a standard system of reporting statistics. Good statistics depend on adequate national systems of collecting, and while these are improving, they still are not uniformly good. As the footnotes to Table 14B show, the species composition of the catches of several countries, particularly some countries with long-range freezer trawlers, are not too well known. On the whole we are now getting reasonably good statistics, and this has enabled the most recent meetings of the working group to do the sorts of analysis of catch per unit effort and effort that have been described for other areas.

Figures 14D, E, and F show some of the results. Figure 14D shows the catch per unit effort of sea breams plotted against estimated total effort in the same year. A curve has been fitted by eye to these points, and the corresponding relation between effort and total catch is shown as a broken line. Figure 14E shows the same plot, but instead of relating catch per unit effort to effort in the year of observations, we have related it to the average effort in the year of observations and in the previous year. This takes into account some of the lag effect by which the results of increased fishing do not show up immediately in reduced catch per unit effort. This is a slightly different way of dealing with these lag effects from the calculations done by Schaefer, which take into account the changes in population size. In a case like the sea bream fishery

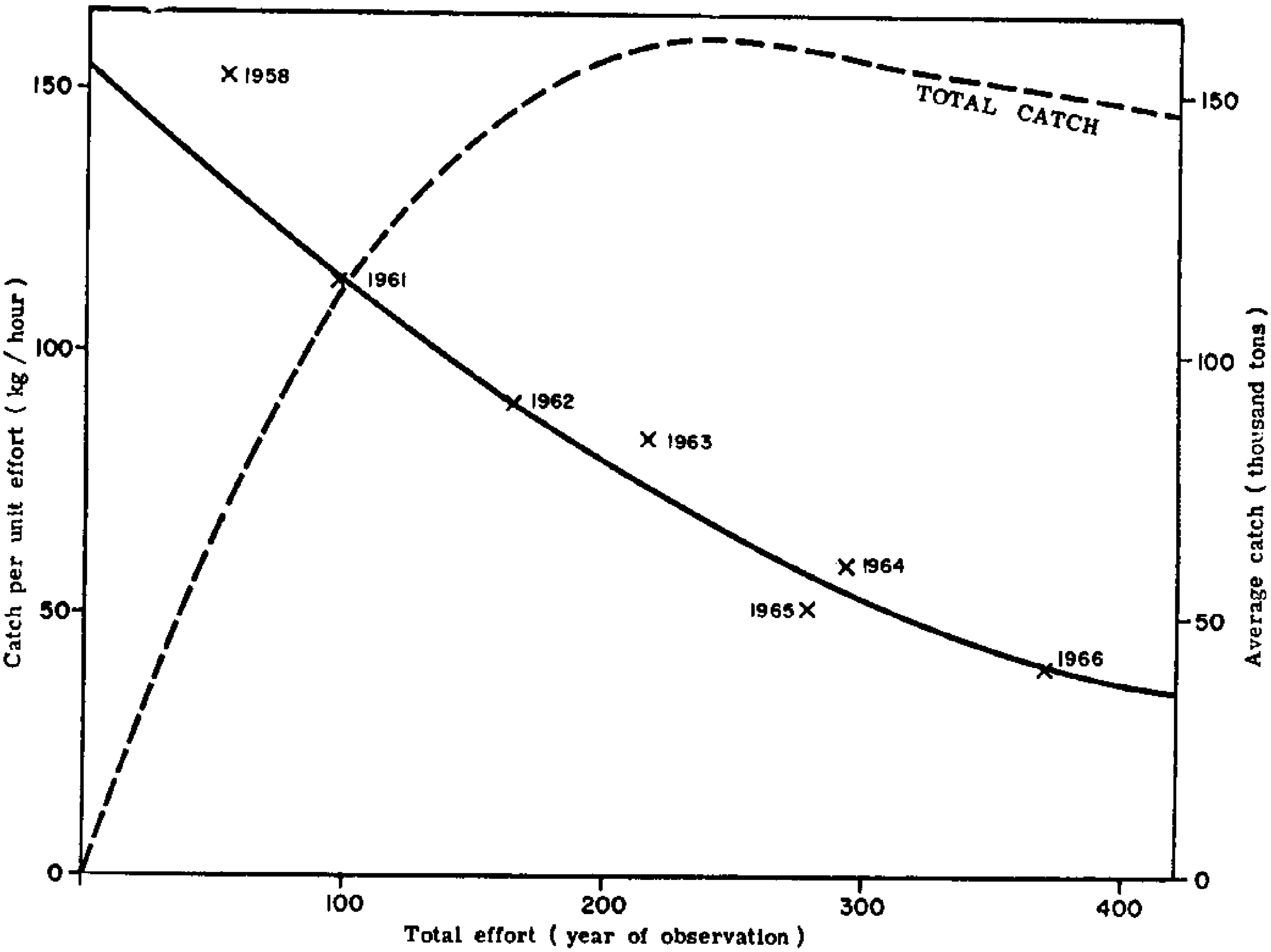

Figure 14D. Relation between fishing effort and catches per unit effort in the same year of sparids off North West Africa, and corresponding relation between total effort and average total catch (from FAO, 1968).

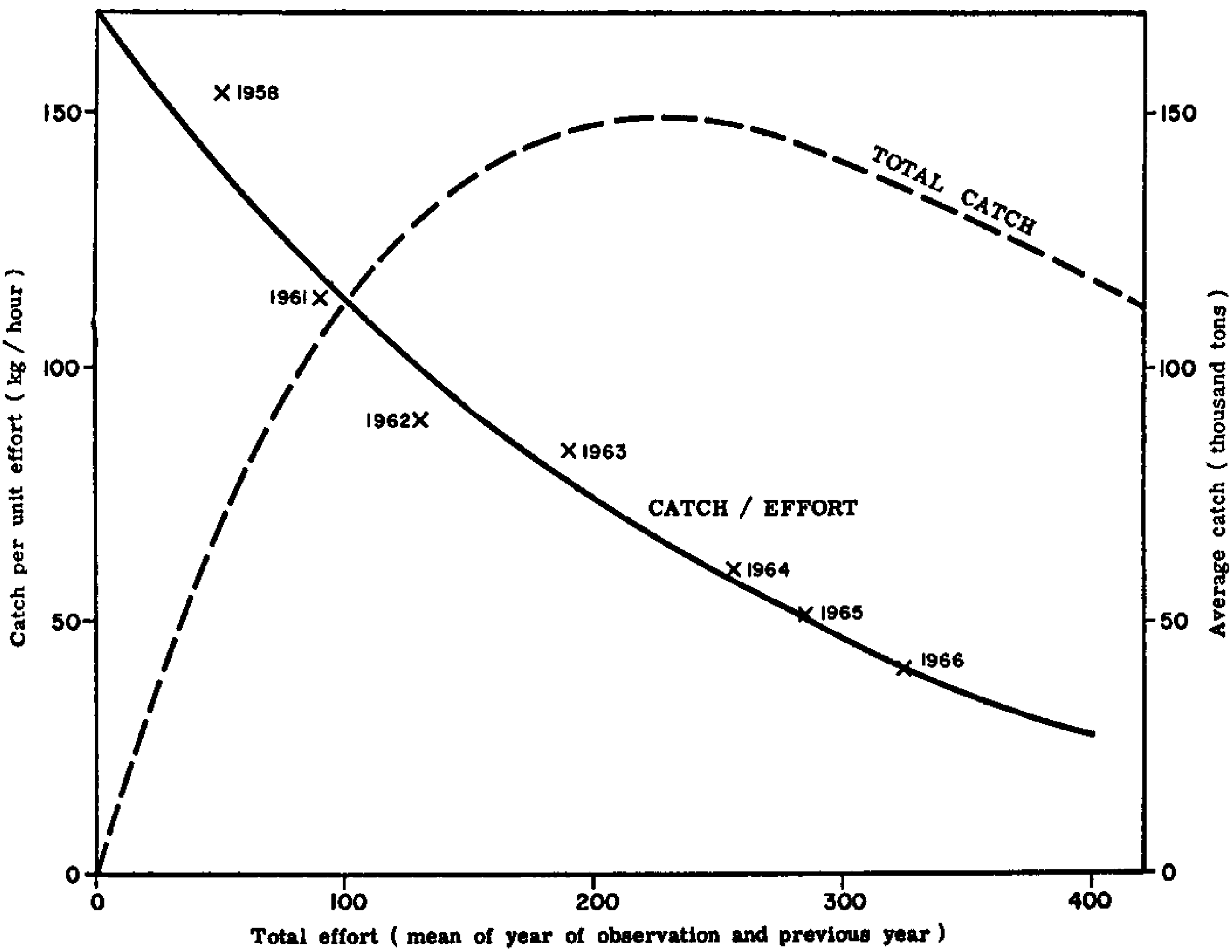

Figure 14E. As Figure 14D, but effort calculated as the mean effort during the year of observation and the previous year (from FAO, 1968).

off West Africa where the effort is increasing by taking account of the lag, we move the points to the left, giving a steeper curve. The curve relating total catch to effort reaches a maximum, if a maximum exists, at a lower effort and with a steeper decline to the right of the maximum. If we plot catch per unit effort against effort for the same year without taking into account the lag effects, we will underestimate the effects of fishing and think we are in a healthier situation than probably exists.

Figure 14F shows the results of similar analyses for the various cephalopods. The data have been adjusted to get them onto the same figure, expressing the effort as a percentage of the 1969-1970 effort. These analyses are based on detailed Japanese data of catch and effort data by small regions, taking into account the catches by the other countries fishing for these species, the biggest of which is Spain. We see that there is some difference between these. The fishery on squid seems to have reached a level where the catches are falling off with increased effort, and cuttlefish also seem to be heavily fished, but it looks as though the catches of octopus can be increased by an increase of effort.

One has to treat these conclusions with some degree of reservation. The numbers of years is not very large and there may be some other things happening. One of these is the indirect effect of the long-established fisheries on other species. Fishermen and others that don't like squid and cuttlefish so much as other fish tend to believe that as the other fish, the hake and the sea breams, have gone down, the cephalopods have increased. Now there is no doubt that in the actual catches by the commercial fishermen from these countries, the proportion of the cephalopods has increased, but this could well be a

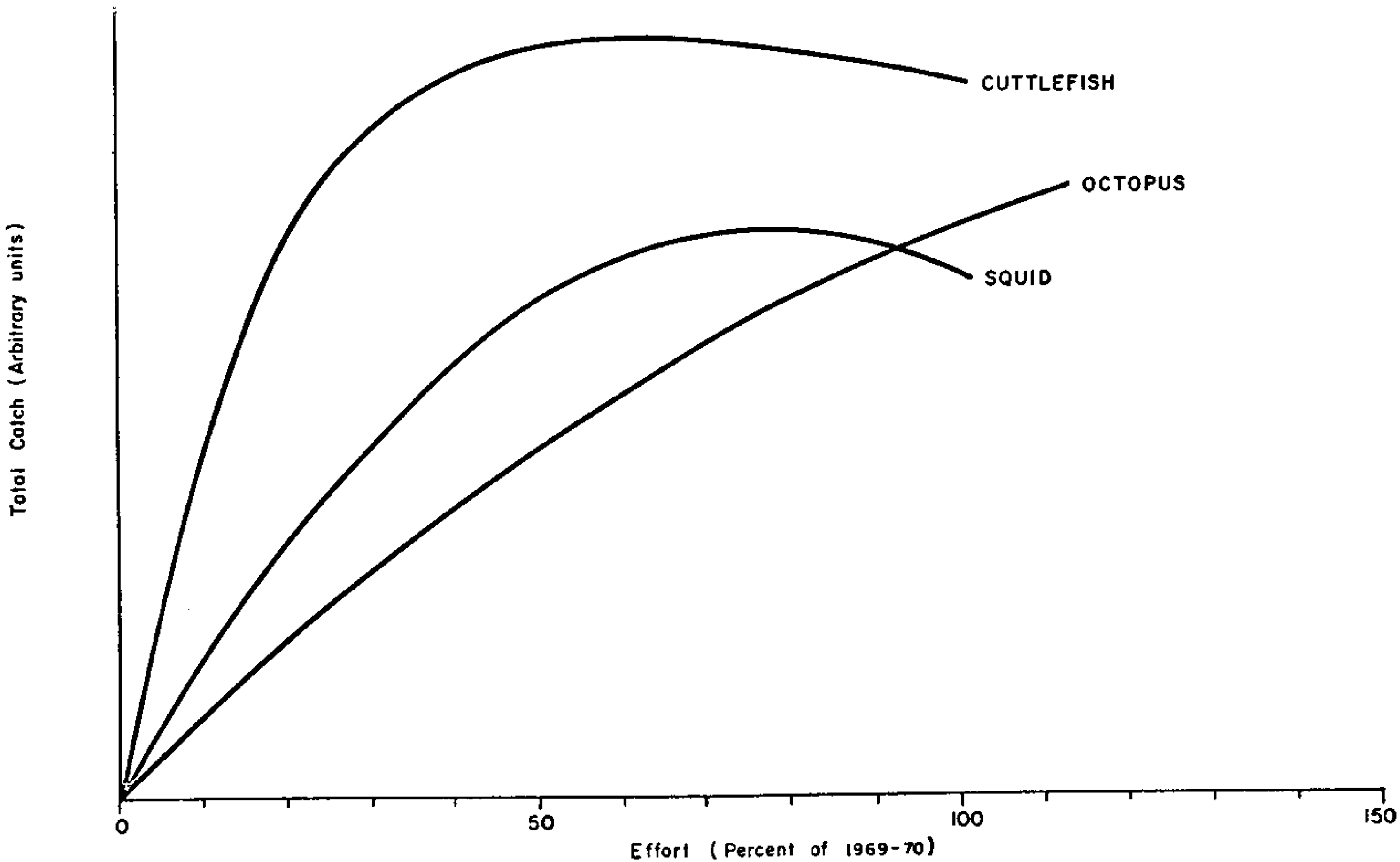

Figure 14F. Catches of cephalopods, as a function of the effort (from FAO, 1971b).

Table 14C. Catches of some pelagic fish in the Eastern Central Atlantic (from FAO, 1971)

CUPEOIDS:

	1962	1963	1964	1965	1966	1967	1968	1969	1970
	Thousand metric tons (live weight)								
TOTAL	189.6	194.8	293.9	291.3	392.3	414.8	369.6	524.9	...
Sardinella	53.2	56.9	130.4	103.3	103.1	158.1	144.5	290.6	...
Ghana	18.9	9.1	34.2	7.8	13.3	43.2	12.2	30.4	...
Israel	--	--	--	--	1.0	0.2	--	--	...
Ivory Coast	3.3	9.0	18.8	17.4	14.9	19.2	15.8	20.4	...
Nigeria	...	...	32.4	33.4	35.1	39.1	39.0	(39.0)	...
Norway	--	--	--	--	--	--	--	--	a/99.0
Senegal	28.5	36.3	42.5	41.7	26.7	45.3	34.7	40.7	...
Sierra Leone	(2.5)	(2.5)	2.5	3.0	4.0	4.2	2.0	2.0	...
South Africa	--	--	--	--	--	--	--	48.0	a/350.0
Spain	--	--	0.0	0.0	3.1	0.6	3.1	0.0	...
USSR	--	--	--	1.1	5.0	6.4	22.0	110.1	...
United Kingdom	--	--	--	--	--	--	--	--	a/36.0
Bonga	13.5	15.0	16.5	19.0	23.0	24.0	31.7	33.8	...
Sierra Leone	13.5	15.0	16.5	19.0	23.0	24.0	16.0	18.0	...
Senegal	...	...	...	...	...	...	15.7	15.8	...
European pilchard	122.9	122.9	147.0	169.0	266.2	232.7	193.4	200.5	...
France	...	...	6.2	5.5	4.4	11.6	8.3	10.4	...

Table 14C. Catches of some pelagic fish in the Eastern Central Atlantic (from FAO, 1971, Cont'd)

CUPEOIDS: (Cont'd)

	1962	1963	1964	1965	1966	1967	1968	1969	1970
Morocco	122.9	122.9	133.3	154.6	244.8	203.5	162.5	162.4	...
Romania	--	--	--	--	0.0	0.0	1.5	(1.5)	...
Spain	...	...	7.5	8.9	17.0	17.6	21.1	26.2	...
HORSE MACKEREL:	39 900	47 298	56 100	57 800	49 300	109 000	165 500	260 700	...
Ghana	...	...	...	5 400	8 855	6 783	2 500	9 327	...
Greece	...	...	...	...	600	100	300	(300)	...
Japan	...	...	2 579	4 098	3 364	4 020	4 216	9 965	...
Morocco	1 800	2 400	3 100	2 100	2 700	3 200	4 200	5 400	...
Nigeria	...	...	...	...	...	10 000	3 000	(3 000)	...
Poland	--	1 898	1 461	5 804	6 257	7 907	4 976	8 233	12 988
Portugal	1 400	2 000	1 200	800	700	300	300	...	...
Romania	--	--	--	2 000	3 800	4 800	2 700	(2 700)	...
Senegal	...	1 200	1 000	1 900	1 500	4 000	3 200	3 200	...
Spain	...	...	400	500	600	400	400	400	...
USSR	...	39 800	46 400	35 200	20 900	67 500	140 200	215 700	...
SPANISH CHUB MACKEREL:	56 600	48 500	75 400	38 000	30 600	72 400	124 300	171 800	...
German Dem. Rep.	--	--	--	--	200	(400)	(400)	(400)	...

Table 14C. Catches of some pelagic fish in the Eastern Central Atlantic (from FAO, 1971 Cont'd)

SPANISH CHUB MACKEREL: (Cont'd)

	1962	1963	1964	1965	1966	1967	1968	1969	1970
Ghana	...	...	0	0	0	16 800	18 500	6 400	...
Greece	...	...	...	...	300	300	600	(600)	...
Israel	--	--	--	--	--	--	500	--	...
Japan	...	...	1 693	608	623	891	455	447	...
Morocco	9 300	13 300	7 600	9 700	6 700	9 400	7 300	13 600	...
Poland	2 149	1 812	3 311	3 456	7 462	8 795	8 266	15 921	3 173
Portugal	200	200	200	200	200	200	200	300	...
Romania	--	--	700	1 500	1 600	3 000	1 200	(1 200)	...
Spain	...	1 400	1 300	300	600	400	800	800	...
USSR	...	31 700	60 600	22 000	12 900	32 200	85 800	141 100	...

shift in tactics. As one species goes down, something else has to be found and that something else is likely to appear to become more common. However, there is some evidence from research vessel data that indeed the abundance of cephalopods has increased in some areas. This would suggest that the observed falling off in catch per unit effort of cephalopods in the main fishing areas from Japan may underestimate the real effect of fishing. We might suppose, putting the early density of the cephalopods in the absence of any fishing as 100, that following heavy fishing on the other species, but with no fishing on cephalopods, there would have been an increase up to 150. If these were also the fishing on cephalopods, the stock density might be reduced to 75. The true effect of fishing would then be given by the ratio 150:75, i.e., 2:1, rather than the observed ration of 100:75, i.e., 1.3:1. The working group of scientists therefore, concluded that hake and sea breams were certainly heavily fished, that probably squid and cuttlefish were probably quite heavily fished, and that probably octopus was not as yet. The working group did not have such good data on pelagic fish, but was rather disturbed at the rapid increase that had taken place in the catches of pelagic fish, both of sardinella, as a result of the fish meal factory operation, and also of other species, as shown in Table 14C. For instance, the catches of horse mackerel went up fivefold between 1966 and 1969. It is really too early to see what is happening to these stocks, but it would be surprising if they are not now being heavily fished.

These were the conclusions given by the scientific working group to CECAF at its meeting in Casablanca this year. The question is where does the international community go from there, remembering the two objectives: one, the proper management of the resource, and the other, the need for the international community to assist the development of the local fisheries in the poorer countries. One action is to get better knowledge of the resource, obtaining improved and more up-to-date statistics, continuing the work of stock assessments by cooperative international working groups especially using further statistical data, which by 1972 might be expected to show the effect of the large increases in catches of mackerel and sardinella.

Another interesting part of international cooperation is the basic research on fundamental scientific questions, such as: How does this upwelling system work? What is the flow of production in the upwelling areas from nutrients through phytoplankton and zooplankton to fish? How are the general environmental conditions related to the fisheries? An interesting international program has been established, the Cooperative Investigations of Northern Part of the Eastern Central Atlantic, which is coordinating the international study of the general scientific problems in this area, though it is not directed purely at fisheries. An important aspect of getting a better study of this area is the improvement of the general scientific capacity of all countries concerned. Of the 35 countries fishing in the area, only a small minority have full programs of research, and scientific staff able to take a full part in the discussion of the problems. Obviously it is essential if we are going to ask, say Sierra Leone, to take action to restrict its catches, that the Sierra Leone government should understand the basis of the request--what is happening to the stocks and the validity of general scientific procedures on which this conclusion is based. This doesn't mean each country must have a highly skilled research group, but it is important that each country have some understanding of how these things are done.

Every country also needs to supply the information on its own fishery. At the very least, it needs to supply the basic statistics of how much fish are being caught, but it would be highly desirable if each country would also supply some of the other fairly routine information on detailed effort statistics, on the size composition of the fish, species composition, age composition, etc. that can be collected fairly easily. One of FAO's programs is, in fact, to assist countries in this basic work of understanding the problems, knowing what data to collect, and collecting it. Hopefully, these various actions will result in a much clearer picture of what is happening to the stocks.

The second set of actions is concerned with the use of this knowledge to manage the resource. There has been some discussion about what the role of FAO and of its subsidiary bodies like CECAF should be in this field. There is one school of thought which believes that management, in the sense of having to apply actual restrictions and rules and regulations on the fishery, should not be a matter for FAO, but for a separate treaty-based organization. It is felt that a treaty-based body would have more authority for its decisions and that such a body, by being restricted to those with specific interests in that fishery would be more effective. There is also a feeling that an FAO body has many other things to do and that a management body is so exposed to complaints that it would harm FAO's other activities if it got directly concerned in management. The other point of view is that management cannot be separated from all these other things that FAO and its subsidiary bodies certainly should be doing. Also, in the case of West Africa, it is much better to have an existing body, CECAF, deal with the problems of management, some of which are becoming urgent, rather than going through the lengthy procedure of setting up a new, treaty-based body.

For the present, off the West African area it is impracticable to recommend management measures to go into effect at once. Before doing this there is a need for very careful examination of the precise regulations required and the practical problems involved in putting these into effect. While scientists have established that certain stocks are being fished too hard, this has not yet been expressed in terms of explicit regulations which would reduce the amount of fishing nor have countries examined the difficulties that they might encounter in complying with such regulations.

Of the two sorts of management measures being examined, the simplest, which does not upset the fishermen too much, is control of the mesh size. For the hake and the sea bream, which are among the most heavily fished stocks, using a larger mesh in the trawls would allow the small ones to grow and would thus increase the long-term catch. There has been a strong proposal by the scientific working group that the mesh size in these fisheries should be increased. The problem is that the best mesh size is different for different species. For squid, which are caught by many of the same ships that fish for hake or sea bream, the best size is about 50-70 mm, for hake, around 70-80 mm, and for sea bream probably up to 90-100 mm. Since the same boats might wish to fish for one or the other of these groups at different times, the actual implementation of the regulations is a bit difficult. It is all too easy for a boat to use a small mesh, say a 60-mm mesh and say it is fishing for squid, but to go after hake or sea breams. There are other complications on mesh regulation, such as how precisely to measure the meshes, what sort of inspection procedure should exist, etc. All of these will have to be examined before an effective mesh regulation is introduced. CECAF has set up a subcommittee to examine these problems of introducing mesh regulation and also to examine the much harder problem of controlling the amount of fishing, by controlling number of boats, by catch quotas, or so on (FAO 1971a). Thus some appreciable international action is being taken to manage the resources.

The third group of possible actions is concerned with development. It is obvious that in this area, where there are resources moving up and down the coast and several countries are exploiting the same resource, it is impossible to plan development of the fisheries of one country without reference to what is happening in other countries. The usual method of helping development has been direct aid and assistance to individual countries, without much reference to events elsewhere. Therefore, at its last meeting CECAF recommended the setting up of a regional program of fisheries development financed by the United Nations Development Programme, to coordinate development programs in the individual countries. This should be an extremely interesting program. It will not, of course, replace direct assistance to countries either through UNDP or by individual developed countries, but will enable us to look at the region as a whole and make sure that plans for development of one country are compatible with those of other countries and do not, for example, attempt to catch between them more than the resource can stand. It will also look at some of the other problems that are appearing, particularly the impact of the long-range fleet on the local fisheries. At the moment, it is still in the planning stage, but hopefully it will move very quickly.

This, then, is the present situation in West Africa. We have an extremely complex fishery on a range of species by a large number of countries--rich countries, developed countries, backward countries--and the situation on the various stocks is different. Some stocks have been heavily fished for a long period. Other stocks are probably just beginning to be very heavily exploited such as the horse mackerel, or sardinella, and it may be that there are still some stocks such as anchovy which are relatively untouched. The fishery biologist has to give what advice he can on what has happened to the stocks, where fisheries should be developed, and where management should be considered.

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Chapter 15

GENERAL DISCUSSION: FUTURE OF WORLD FISHERIES AND NEW DEMANDS ON POPULATION DYNAMICS

Any discussion of the future of world fisheries should start with some review of the past. Figure 15A shows what has been happening to world catches over the last twenty years. This is plotted on a log scale and we see that the trend in world catches over this period has fallen very closely on a straight line on this log scale, i.e., they have increased at a constant exponential rate, doubling about every ten years. Also marked are some of the limits that might be set to this continuing expansion. A vital question is how long this expansion can go on, and the answer is, in some ways not very long.

The problem of determining how much further present expansion can continue or how much fish the oceans can produce depends on what we mean by fish. The minimum figure is of the traditional fish that are recognizable on the fish market and in the catches of the present commercial fleets. Two levels for the possible catch of these traditional fish are shown in Figure 15A. One is the total potential if all the stocks of these fish were exploited in the most rational possible way. In practice, in any area where there are a number of species together, such as the North Sea or Gulf of Thailand or West Africa, it is not going to be easy to manage every stock of fish at precisely the optimum level. Also some of these fish are so scattered that it is unlikely that they can, in fact, be harvested economically, so the practicable limit to the catch of these traditional fish is lower than the sum of the optimum catch of each species, as shown in Figure 15A. The potential catch of these traditional fish is about 100 million tons, while the present catches are about 50 million tons and the catches have been doubling every ten years. This shows that at the present rate of increase, within ten years the total world catches will be approaching the limit of the potential of these traditional fish.

In addition to these fish, there are a number of other types of fish and animals in the sea. There is a large quantity of squid in the open ocean and a large quantity of krill in the Antarctic. The most conservative estimate of how much krill could be harvested is on the order of 50 million tons per year. This is based on the fact that the large whale stocks which ate mostly krill must have in the past been eating well over 50 million tons per year. We can just replace whales by fishing boats and presumably harvest 50 million tons each year, without upsetting the ecosystem, though it is quite possible that the potential harvest could be much greater than that. Both these groups, the ocean squid and the krill, are quite large as suggested in the figure, and we are not too far off being able to harvest them on an economic scale.

There are other groups of smaller animals such as the small oceanic fish, the lantern fish, etc., many of which are the food of the tuna, that are spread all over the oceans. The problem is to harvest these not very valuable animals at a reasonable cost. If the technological and economic problems could be solved, the potential harvest could be very great. The sorts of figures we get for the potential harvest of these small fish and other animals are up to 500 million tons, or even up to 2,000 million tons compared with the present catches of 50 million tons, and the possible harvest of the traditional fish of about 100 million tons. It is quite clear from this that if we look at the very long view, the potential food harvest from the ocean is very large. On the other hand, it is unlikely that the technical problems of harvesting these animals will be solved in the immediate future, and if we look at the next ten years, a big crunch is going to come when the present catches approach the limits that the traditional resources can stand. In the past, the problems of the limited potential of one stock could be avoided by switching surplus effort from one area to another. As the North Sea became heavily fished, the

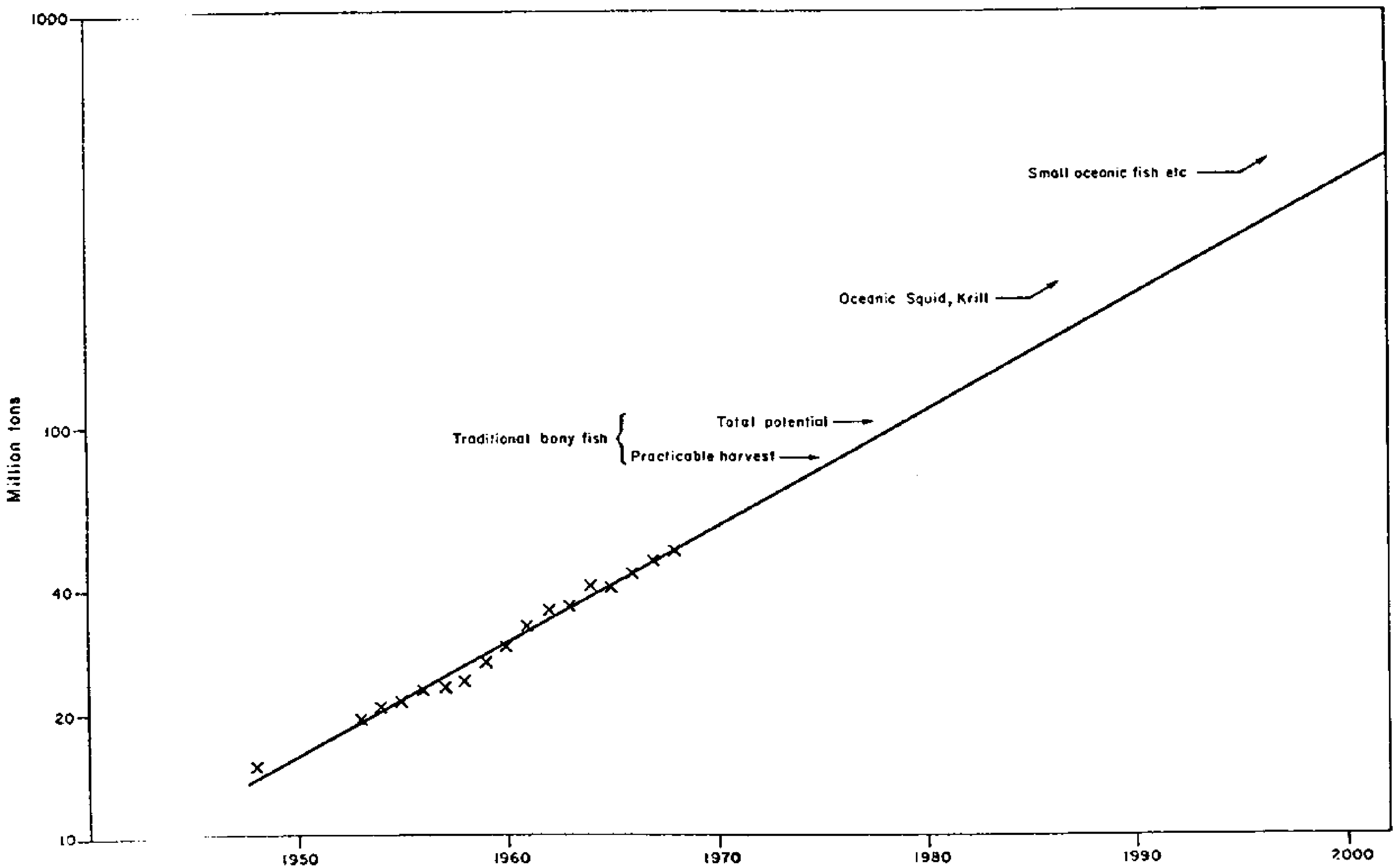

Figure 15A. Trends in world marine fish catch, in relation to the potential of various groups (from Gulland, 1970).

British fishermen went to Iceland, and when that became heavily fished, they went to the Arctic, and when that became heavily fished, across to Greenland. Now trawlers from Russia, Spain, and other countries are moving out of the North Atlantic, to West and South Africa and elsewhere. The same expansion has been taking place out of the North Pacific by fleets from Japan and USSR, but within the next few years there is not going to be anywhere left to go. This is going to make a much bigger demand, first on the scientists to give better advice as to what is happening and what needs to be done, and second, on the international community to set up reasonable arrangements for managing these resources.

But before looking at those problems, let us look at how we obtain the figures of global potential, which are shown in more detail in Table 15A. This sets out the estimated potential yield in millions of tons in various sea areas, according to groups of fish, starting at the top with the large pelagic fish such as tunas; then the larger demersal fish--flounders, cods, etc.--which support the big trawl fisheries of the world; and then the shoaling pelagic fish such as anchovies, herrings, sardines, mackerels, then the crustaceans, which are in terms of weight rather small, but in terms of value very substantial. There is no estimate for molluscs because for most of the molluscs such as oysters and mussels, the future production from the sea depends not so much on the natural production but on the degree to which these animals are produced by aquaculture for which they are highly suitable, because they lie early in the food chain. Raft culture of mussels in Spain gives extremely high production.

Further down the table, no estimates are given for cephalopods or lantern fish, because the data are not good enough. On the boundaries of the table, we have added these potentials together, and compared them with the present

Table 15A. Potential yields of various groups of marine animals and recent catches in million tons (from Gulland, 1970).

	Northern Temperate					Southern Temperate				Tropical						Antarctic	TOTAL	Catch		
	Atlantic		Pacific		Mediterranean	Atlantic		Pacific		Atlantic		Indian		Pacific						
	NW	NE	NW	NE		SW	SE	SW	SE	WC	EC	W	E	WC	EC			1966	1967	1968
A. Large Pelagic																				
Salmon	∅	∅	0.3	0.3													0.6	0.6	0.6	0.5
Large Tuna, Billfishes	Incl. in tropical areas									0.2		0.1		0.4			0.7	0.7	0.7	0.8
Skipjack, Bonito, etc.										0.9		0.6		1.5			3.0	0.7	0.7	0.7
TOTAL	∅	∅	0.3	0.3						1.1		0.7		1.9			4.3	2.0	2.0	2.0
B. Demersal																				
Flounders	0.4	0.8	0.1	0.5	∅	∅	∅										1.8	1.1	1.2	1.2
Cod, Haddock, etc.	1.8	2.9	0.1	0.1													4.9	4.2	4.3	5.0
Hakes	0.6	0.1	–	0.2	∅	1.5	0.6		0.2		∅				0.2		3.4	1.2	1.4	1.3
Small Gadoids	–	1.4	1.5	0.5		1.0	–										4.4	1.8	2.4	3.1
Rock Fishes	0.3	0.3	∅	0.3			∅								0.1		1.0	0.8	0.7	0.5
Croakers, Snappers, etc.	∅	0.1	∅	–			0.2	∅	∅	0.2	0.2			11.0	1.1					
Other Demersal	0.5	0.9		0.2	0.4	1.3	0.3	0.2	0.4	1.8	0.7	4.4	3.0				27.0	10.4	10.3	11.2
Shark, Rays	∅	0.1		∅																
TOTAL	3.6	6.6	1.7	1.8	0.4	3.8	1.1	0.2	0.6	2.0	0.9	4.4	3.0	11.0	1.4		42.5	19.5	21.0	22.4
C. Shoaling Pelagic																				
Anchovies		0.3	0.6	–	0.5	1.0	0.7	0.1	11.0	0.5	0.7	2.0	0.7	1.0	2.5		21.6	10.6	11.7	12.4
Herrings, Sardines	0.7	4.0	0.4	1.0	0.2	1.5	1.4	0.2	0.4	2.0	0.6	1.0	0.5	1.5	0.5		16.1	6.8	6.9	6.3
Mackerel, Jack Mackerel	0.1	1.1	1.4	0.1	0.1	0.5	1.0	0.1	0.2	–	0.4	1.0	0.8	1.5	0.6		8.9	3.2	3.5	4.1
Saury	∅	–	0.5	0.4					0.3						0.1		1.3	0.3	0.3	0.2
Capelin	1.0	0.8	–	0.5		–	–										2.3	0.5	0.5	0.6
Sand-eels	1.0	0.5	0.3	0.5		–	0.1										2.4	0.3	0.3	0.4
Various others						0.5	–			0.5							1.0	1.7	1.7	1.9
TOTAL	2.8	6.7	3.2	2.5	0.8	3.5	3.2	0.4	11.9	3.0	1.9	4.0	2.0	4.0	3.7		53.6	23.4	25.1	25.4
D. Crustaceans																				
Lobsters, Rock Lobsters	∅	0.1	∅		∅	∅	∅	∅	∅	∅	∅	∅	∅	∅	∅		0.2	0.1	0.1	0.1
Shrimps, Prawns	∅	0.1	0.1		∅	0.1	∅	∅	∅	0.2	0.1	0.1	0.1	0.5	0.1		1.4	0.8	0.8	0.8
Crabs	∅	∅	0.3		∅	∅	∅	∅	0.1	0.1	∅	∅	∅	0.2	∅		0.7	0.3	0.3	0.4
Various marine crustns.																		0.1	0.1	0.1
TOTAL	0.1	0.2.	0.4		∅	0.1	∅	∅	0.1	0.3	0.1	0.1	0.1	0.7	0.1		2.3	1.3	1.3	1.4
E. Molluscs	Production largely dependent on cultivation. Total potential may be many times that of naturally occurring stocks																			
Oysters																		0.8	0.8	0.8
Mussels																		0.3	0.3	0.3
Various others																		1.0	1.0	1.1
TOTAL (ex. cephalopods)																		2.1	2.1	2.2
F. Cephalopods																	10–100	0.8	0.9	1.2
G. Lanternfish etc.																	100+			∅
H. Euphausids																50+	?			
TOTAL A + B + C	6.4	13.3	5.2	4.6	1.2	7.3	4.3	0.6	12.5	5.5	3.4	8.8	5.3	16.0	6.0		100.4	–	–	–
Catch (including crustaceans and molluscs)																				
1966	4.0	10.2	13.2		1.0	0.6	2.2	0.4	10.1	1.3	1.3	1.3	0.8	3.2	0.6			50.2		
1967	4.0	10.2	13.8		1.0	1.3	2.6	0.4	11.2	1.3	1.4	1.2	0.9	3.4	0.7				53.4	
1968	4.7	10.3	14.5		1.1	0.8	3.3	0.4	11.9	1.4	1.5	1.2	1.0	3.8	0.7					56.6

Notes – ∅ denotes catches less than 100 tons
– information zero

catch. For the major groups in the upper half of the table, the present catches are half or better of the potential, but while in some regions the potential has been almost reached, e.g., in the Northwest Atlantic the potential is about 6 million tons and the present catches are approaching 5 million tons. In other areas, such as the Western Indian Ocean, which seems to be quite rich because of upwelling in the Arabian Sea, the potential is about 8 million tons, but the present catches are only a little over 1 million tons. Thus there is opportunity in certain areas of the world to increase catches.

Most of the figures, particularly the more reliable figures, have been derived by the traditional methods of stock assessment, using the techniques described earlier, based on the analysis of catches, catch per unit effort, age composition, mortality rates, etc. But there are many areas and stocks where at present there is no fishery developed, or the fishery has only recently developed, or for some reason the data from the fisheries aren't sufficient to carry out assessments using established population dynamics techniques and for which we have had to look for other methods. The most quantitative of these are various survey techniques which give the biomass present. It is possible to survey with a trawl, which is probably most useful because this also gives some idea of how a commercial fishery could operate. It is possible, however, to survey with other methods, by acoustic methods, which look very promising because we can survey a fishing ground with echo sounder or sonar much faster and cover a much bigger area than with a trawl.

Another method is to look at the distribution of fish eggs and larvae. The advantage of using this approach, particularly on fish eggs, is that fish eggs don't dodge the net very well, whereas all sizes of fish are only too capable of dodging the nets. Therefore we can usually get a quantitative measure of the abundance of fish eggs and use that to estimate the abundance of the parent stock. The disadvantage of this is that it is as lengthy as carrying out a fishing survey, and doesn't give much information about where we can fish or what the catch rate (e.g., catch per vessel per day) is likely to be. On the other hand, it is quite often that some other marine scientists have collected plankton samples for other purposes and these existing samples can be examined by fishery people to get an idea of the biomass of spawning fish without much expense. The biomass doesn't immediately tell the amount of fish that can be harvested each year. Obviously this will be proportional to the biomass; the more fish there are, the bigger the potential catch. If we have a very long-lived fish, such as the ocean perch in the Bering Sea, the biomass present when we start fishing may be composed of 10, 15, or even 20 year-classes, and we can remove those, but after we have removed this large standing stock, the annual production will be very small. On the other hand, we may have a fishery on tropical shrimp where there is effectively only one year-class present at a time and we can remove virtually all that are present one year, and then next year hopefully there will be the same quantity again. Thus the catches we can take will be proportional also to the rate of turnover of the population, which is perhaps best measured by the natural mortality. The catch is therefore equal to some constant times the biomass times the natural mortality i.e. $C = a M B_o$. Playing around with either the Schaefer model or the Beverton-Holt model and seeing on the basis of those models what this ratio is likely to be, we find this comes out to about 0.5. This gives us some method of estimating roughly what the potential catches will be if we know the unexploited biomass and the natural mortality. In this case, estimation of natural mortality is not difficult because we are dealing with an unexploited stock where there is no fishing mortality, so the total mortality which we can get from an age composition will be equal to the natural mortality. This gives us some method of using figures of biomass obtained from surveys to provide estimates of potential annual yield. An even cruder method of estimating potential yield is by extrapolation. For bottom fish the potential yield per unit area doesn't vary greatly but does so in a sensible manner--that is, where it is known from physical conditions or plankton investigations that the area is highly productive, the yield per unit area of bottom fish is higher than the general average. In other areas where the primary production is low, we get a low yield. If we have no quantitative information on the fish stocks in an area at all, but some measure of the extent of the bottom, we can get some rough idea of the yield of bottom living fish from the average yield per unit area (around 30 kg/ha), which can be improved by taking into account information on the primary production. From one source or another, we now have for all oceans of the world some rough estimates of how much fish can be taken from each area. One thing that needs to be done in the future is to improve these estimates. Another important action is to use our knowledge of the resources and the present fisheries to try and insure that this knowledge is used for the best advantage of mankind in some rational utilization of these resources. On the one hand, we need to be sure that we don't overfish the stocks--reduce them as we did for the whales, for the blue whales particularly down to a level where nothing much can be taken. On the other hand, we do need to use the resources and not leave them neglected.

So far as improving our knowledge is concerned, there are a number of things we can do. One of the most important is to get better data, particularly on catches and catch per unit effort of the type already shown for many of the stocks. The most useful analysis that can be shown to fishermen and administrators is the analysis of what is happening to the fishery. But these analyses need good information on catches and good information on effort or catch per unit effort. In most of the fisheries described here, this information is good, but there are still many stocks for which the information is not too good, particularly in areas where a number of countries are fishing and it isn't possible always to put all the data together.

FAO is busily engaged in promoting the collection of information on catches and effort, and in putting it together so that scientists anywhere can work on it. But even with the best data, we still need to analyze it. While the analyses of catch and effort data will often give useful results, it is very difficult to fit additional knowledge, e.g. on variations of year-class strength, into the model. The Beverton and Holt model which takes into account the characteristics of the fishery--the fishing mortality and the age at first capture--and the natural characteristics of the population--the natural mortality, and growth, and the number of recruits--allows additional information to be incorporated into the model much more easily. Thus initially the natural parameters can be taken as constant, but with more data we can consider how they vary with the abundance of other stocks, the indirect effects of fishing, and also changes in the environment.

Now that it is possible to get better regular information on the environment, particularly on the physical characteristics of the surface water, such as temperature, it should be possible to fit these into the model, and thus get rid of some of the scatter on the points, and hence achieve a better understanding of what is likely to happen in the immediate future. While computer models are becoming a very important tool in population dynamics, they are only one aspect of the situation. There is a need for a balance between the sophisticated computer model and drawing a line through a set of curves by eye. Given a set of points relating catch per unit effort to effort, there are a variety of ways by which a curve can be drawn through them. If we have a curve such as the catch per unit effort against effort line, the simplest statistical fit is a straight line, but we can think up various kinds of mathematical models that can be derived to fit other types of curves. However, it is most unlikely that the population of fish concerned will obey precisely a straight line relation, or any other relatively simple mathematical relation. An advantage of fitting a curve by eye is to know that we are not kidding ourselves that we have done anything very exact. Also, experience with other stocks, or a knowledge of the possible errors in certain data points, can be used, admittedly in a subjective way, to assist in choosing the curve to draw.

Besides determining the best-fitting curve, it is important to know about range of possible values. Confidence limits can be calculated in the usual way, but they probably only give a minimum estimate of the possible range, because they fail to take into account the degree to which the real population may depart from the model. The subjective estimate, obtained by drawing curves by eye, may be equally useful when taking into account other information on the likely accuracy of difficult points.

All the various models, whether derived from sophisticated computer techniques or otherwise, will have implications in terms of how the natural parameters of growth, mortality, and recruitment are reacting to events in the sea. A very steep decline in catch at high levels of effort means that the recruitment is probably falling off as the adult stock goes down, so that we really ought to be looking at what is happening in the recruitment process, and understand how the survival of eggs and larval fish is related to the abundance of adults and possible competition between the adults and young or between the young themselves. This means that the population biologist, or stock assessment expert who is giving advice to the fishing industry or to the government about what is happening will have to take into account a very wide range of knowledge of what is happening in the sea. He should not just look at the catch and effort data, however sophisticated his mathematical analysis of the data might be, but he has to look at the results of other research going on in the sea, particularly studies of other stages in the life history of the fish. Also he has to know a lot about what is happening to the fishery and how the fishermen themselves are operating. The biggest question in all these analyses of catch and effort data, and indeed most other fishery data is not how to draw the line through the points, but whether in fact those points are where we think they are. It is only too likely, for example, that the nominal unit of fishing effort has become more effective, so that, in a plot, say, of catch against effort, the points corresponding to the more recent years should be moved progressively to the right, which makes a big difference to the line that we draw. Detection of such changes is not too easy from analysis of the data, but a fair idea of the existence of the change, and its likely magnitude may be obtained from talking to the fishermen. The demands on population dynamics scientists are therefore perhaps twofold. One is to do a lot more, to collect more data, and to use the existing analyses in a more effective way. The other is to look at the wider events that are going on and take into account a wider range of information.

The other important action required in the future is the use by the world fishing community of these analyses to achieve a better utilization of the resource. This is becoming more urgent since the opportunity to avoid the impact of heavy fishing in one area by going elsewhere or by exploiting another species is now no longer so easily available. The speed of development has also increased very much. The old traditional way for scientists and stock assessment experts to advise on the problems of a fishery is well illustrated by events in the North Atlantic, where fishing developed rather slowly. There had been a long history of international collaboration and a long series of good statistical data. The scientists had steadily built up their understanding, but in relation to any particular problem the arrangements for providing advice by interchange between the scientists and the administrators were rather slow. If the fishery approached a critical level in 1967, the data for the 1967 fishery was examined by the scientists in 1968 or in 1969. This was reported to the commission in 1970 who might decide to take action which at the earliest would not be implemented until 1971. There is at least a four-year gap between the fishery reaching what might be critical stage, and any action being taken. If in four years the fishery doesn't change very much, probably this is not too bad. If, on the other hand, the fishery is changing very rapidly, a four-year delay can be very critical. In some fisheries the time between reaching a peak and disappearing is considerably less than four years, and this means that the scientists have been in a position to give nothing more useful than some precise post mortem on some fisheries.

One implication of this is that because fisheries are developing so fast, we can't rely on the classical analyses of either catch and effort data, or mortality data, because these analyses by their very nature can't give early advice. Because there is always some scatter in catch per unit effort, the effect of increasing fishing will not be detectable until the fishery has reduced the catch per unit effort quite substantially. Similarly, if we are looking at other estimates of the effect of fishing such as increases in mortality rate, decreases in the mean size, mean age of the population, again this won't show up until fishing has had some considerable effect. If we then have to wait for the normal mechanics of analysis, reporting to the administrators and taking action, again we have a post mortem but not much else. This means that the scientists will have to rely on some of these other methods, such as using survey data to give an early warning to the fishery of what the limit to the potential catches might be, and when the fishery might be running into trouble.

Another aspect of the provision of scientific advice is that scientists are not accustomed to publishing their conclusions until they are fairly confident of their validity. And as scientists, this is very right and reasonable. On the other hand, a scientist giving advice to the fisheries usually can't afford to wait for conclusive evidence, because the advice by then will not be of practical value. The useful practical advice is early and rough and dirty advice, and scientists must be prepared to give such advice, and to be wrong a proportion of the time. What has to be remembered is that even if no scientific advice is given, action will be taken anyway. Such action may be wrong 50 per cent of the time; and if the scientist is right 51 per cent of the time, he will be of some use. The other thing is that the administrators and the fishermen themselves must become accustomed to using early, rough, and dirty scientific advice. The scientists themselves have encouraged the fishermen and the administrators to look on scientists as all-knowing gods and their advice, when finally delivered, as being certainly correct. The fishermen and administrators will have to expect to get answers from the scientist that may be wrong 49 per cent of the time and take action on that basis. An important aspect of the advice is concerned with the range of likely values, rather than the particular estimate that best fits the observations. For instance, on the most optimistic estimate an increase in effect, say by 50%, could result in an appreciable increase in total catch, though some decrease in catch per unit effort. On the other hand, the most pessimistic estimate, which would still not be inconsistent with the observed data, might predict a sharp fall in total catch, with of course a very big drop in catch per unit effort. We can say it may be that they are going to lose a lot of catch and extremely heavily on the catch per unit. In economic terms, the extra investment, far from giving any more fish, would on the second hypothesis lose fish, though the extra investment might give more fish, at the cost of a drop in catch per unit effort. This is a considerable range of uncertainty, but the information might be enough to aid in deciding on future action.

If the government, or the industry, is keen on getting more fish and believes in taking a gamble, it might hope that the optimistic analysis is right, and increase their effort. On the other hand, it might think that both these alternatives are not very attractive, and at least the chance of getting more fish at the cost of reduced catch per unit effort doesn't balance the disaster of getting even less fish for the extra investment. Then they might allow only a small expansion, which even in the most pessimistic analysis would not cause much in the way of disaster, and hope that following this, there would be sufficient information to allow us to establish the curve more precisely.

The biologist also has to link up very closely with the economists and others working on the shore side of the fisheries to enable the practical and economic consequences of the various possibilities to be determined, including particularly the consequence of acting on one analysis when in fact it is wrong. We have already stressed that the fish population dynamicists has to link up very closely with the other scientists working on the sea side of the business--with fishery biologists looking at food consumption, recruitment pattern, the distribution and behavior of young larval fish; and also with people looking at the primary production and how this is finally taken up by the fish. The population biologist has to maintain contact with them, and also with the people ashore, with the economists who can put the results of the yield-effort analysis into economic terms, of how much there is to lose, or gain under various possibilities. He also has to talk very closely with the fishing industry and with the administrators so that they understand just what the problems of the fishery biologist are and how reliable his answers are likely to be. He also needs to maintain contact with the industry so that he can interpret changes in say catch per unit effort due to some subtle method of adjusting the gear to provide bigger catches from a given abundance of fish.

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