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SYMPOSIUM ON ENVIRONMENTAL CONDITIONS
IN THE
NORTHWEST ATLANTIC, 1960-1969

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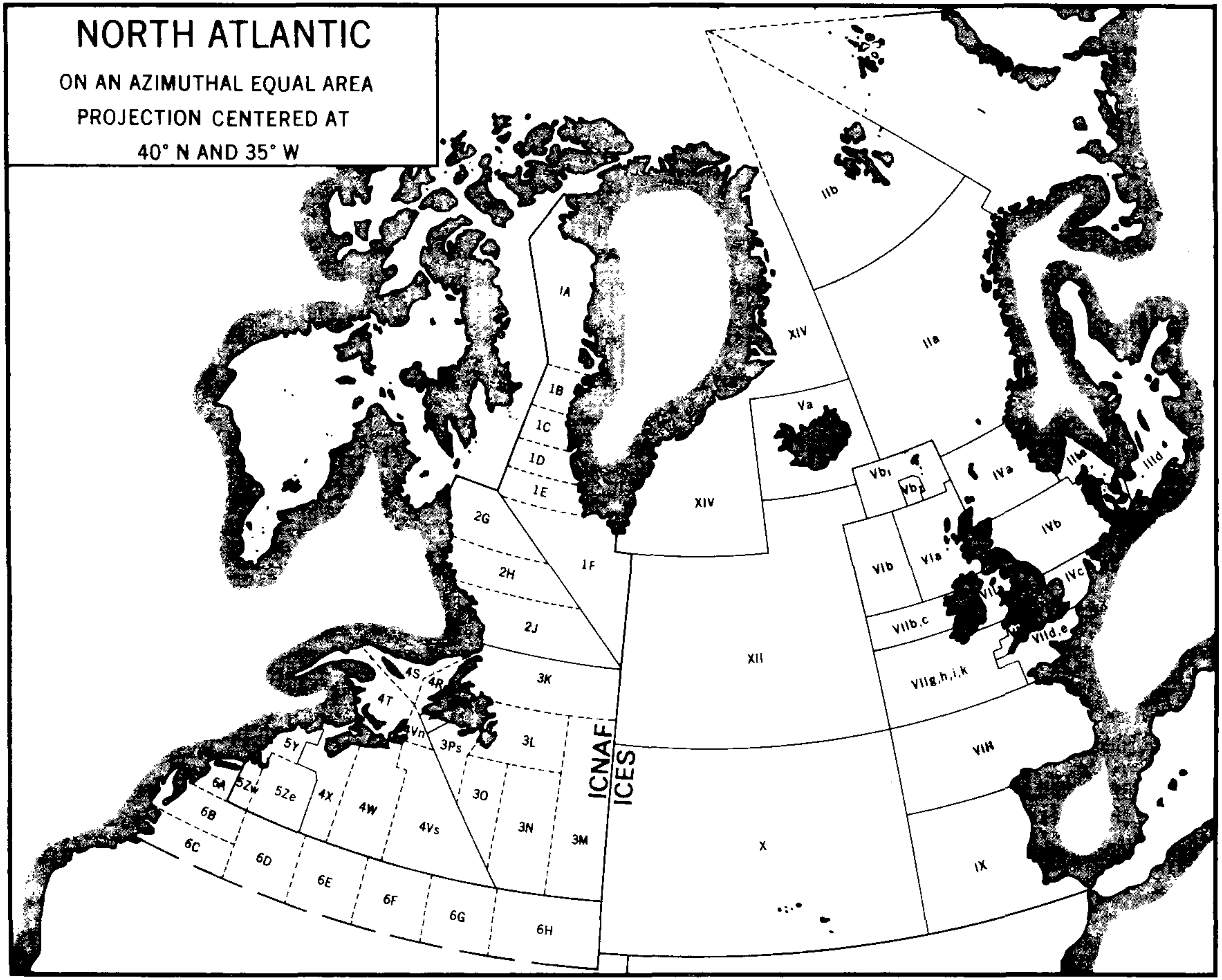
1972

NORTH ATLANTIC

ON AN AZIMUTHAL EQUAL AREA
PROJECTION CENTERED AT
40° N AND 35° W



Foreword

As part of a continuing general program of environmental studies adopted by the International Commission for the Northwest Atlantic Fisheries (ICNAF) in 1961, a decision was taken at the 1969 Annual Meeting held in Warsaw, Poland, to hold a Symposium in 1971 on the Environmental Conditions in the Northwest Atlantic. The organizing committee consisted of Dr H. W. Graham (USA) then Chairman of the ICNAF Subcommittee on Environmental Studies, Mr A. Lee (UK) and Dr A. Alekseev (USSR). The Symposium focused on the environmental aspects of oceanography, meteorology, ice, fisheries, plankton, geomorphology and their interrelationships and interdependence for the decade 1960-69.

This Symposium is a follow-up to an ICNAF-sponsored symposium held at FAO, Rome, in 1964, on the influence of the environment on the principal fish stocks in the North Atlantic ocean which dealt with the decade 1950-59 *(ICNAF Special Publication No. 6, 1965).* Other contributions to the ICNAF environmental program have included the first multi-ship environmental and biological survey of the waters around Greenland and south to northern Newfoundland in April-July 1963 *(ICNAF Special Publication No. 7, 1968)* and a special meeting in May 1967 on fluctuations in sea and air temperature in the ICNAF Convention Area since 1950 *(ICNAF Redbook 1967, Part IV, 1967).*

The Symposium was convened by Dr N. J. Campbell (Canada) the present Chairman of the ICNAF Environmental Subcommittee. It was held at the Bedford Institute of the Canadian Department of Fisheries and Forestry (now Department of the Environment), Dartmouth, Nova Scotia, Canada, from 18 to 19 May 1971 inclusive in conjunction with the 1971 Annual Meeting of ICNAF. Ten specialists were invited to present papers reviewing their fields for the decade 1960-69. An additional paper on water temperature by Mr V. Burmakin was selected from among the documents submitted to the ICNAF Annual Meeting bringing the total of review papers to 11. The papers were presented during morning and afternoon sessions on 18 May and a morning session on 19 May. Each paper was allotted 1 hr, 40 min for its presentation and 20 min for its discussion. The afternoon session featured an informal panel discussion, moderated by Mr A. J. Lee (UK), in which the invited speakers and symposium participants examined the effects of change in the environment over the last decade on the abundance of fish stocks in the North Atlantic in general and the ICNAF area in particular.

Early in the afternoon of 19 May two movie films were presented by the kindness of Mr Lee and Dr Dickson. The films, produced by the Fisheries Laboratory, Lowestoft, in collaboration with the British Admiralty Research Laboratory, featured the use of a high speed sector scan sonar device in examining the effects of offshore gravel dredging in UK and in examining fish behaviour in relation to bottom and pelagic trawls.

Much of the success of the Symposium was due to the invited speakers for their excellent review papers and to the meeting participants for contributing so willingly and well to the full and valuable discussion. Thanks are also due to the members of the organizing committee, Dr H. W. Graham, Mr A. Lee, and Dr A. Alekseev, for the thought and effort they put into planning for the Symposium and to Dr Wm. Ford, Director of the Bedford Institute, for providing excellent meeting facilities. Special thanks are due to Dr N. J. Campbell, Convener of the Symposium, and to Mr A. Lee, discussion leader, for their contributions. Finally, I wish to express the thanks of all to the members of the ICNAF Secretariat who were ably assisted by Mrs Marie Sweet of the Marine Ecology Laboratory, Miss Sheralyn Young of the Atlantic Oceanographic Laboratory, both from the Bedford Institute, and Mrs Roma Howse of the Marine Sciences Branch, Department of the Environment, Ottawa.

A. S. Bogdanov,
Chairman of the Standing
Committee on Research and
Statistics,
International Commission
for the Northwest Atlantic
Fisheries.

December 1971

Introduction

Dr N. J. Campbell: "Ladies and gentlemen, I would like to welcome you on behalf of the International Commission for the Northwest Atlantic Fisheries (ICNAF) to the second major Environmental Symposium, sponsored by this organization. Previous symposia were held in 1951 by International Council for the Exploration of the Sea (ICES) and later in 1964 by ICNAF in Rome. It is rather interesting to review some of the chairman's remarks of these sessions. Dr Cyril Lucas, the chairman of the 1964 symposium, outlined the task of the biologist to demonstrate the reactions of fish to different types of environment and that of the oceanographer to study the processes by which the environment is changed so that he is in a better position to forecast the hydrographic situations of fisheries significance. Within the short space of time, from the symposium in Rome until today, we have seen a great number of other significant environmental effects being recorded in the oceans, the atomsphere and on the land. We are faced today not only with the problem of studying the natural cause and effect relationships but now the man-made effects which are causing us so much concern in many of our countries. This symposium will cover some of the natural environmental phenomena and I think you will see that we have summarized many aspects in the invited papers. The task in the 1970's is not what the biologist or oceanographer should do in their respective disciplines but what teams of biologists and oceanographers including those specialists in chemical oceanography, geological oceanography can accomplish. The key question is the improvement of our ability to manage the ocean as a resource and as an environment. Basically, the problems transcend national jurisdiction and require collective knowledge and practices from all of us in order to perform these services for sensible husbandry and protection of the environment. As environmentalists, it is our responsibility to identify the significant scientific aspects and their inter-relationships in order that our colleagues and experts in inter-governmental affairs can lead into the next stage of development of international law and agreements.

"Some of these changes are being reflected in the reorganization of departments and ministries in Canada, the United States of America and in the United Kingdom to mention only a few. The problem is compounded by the fact that we are being asked to resolve and predict man-made effects on the total environment when we have not yet achieved sufficient understanding of the natural phenomena of the ocean. Time is of the essence and your presence here is indicative of the concern being expressed by scientists throughout the world.

"This Institute is involved in such problems and I would like to introduce one of our hosts, Dr W. L. Ford, the Director of the Atlantic Oceanographic Laboratory (AOL) of this Institute who, along with Dr L. M. Dickie, the Director of the Marine Ecology Laboratory (MEL), are by their courtesy and hospitality making this symposium possible."

Dr W. L. Ford: "Ladies and gentlemen, it is a pleasure to welcome you to the Bedford Institute of Oceanography. We of the Institute feel it is an honour to serve as your hosts during this ICNAF Environmental Symposium. To judge by the agenda and the broad international attendance, I am sure your deliberations will prove both informative and stimulating.

"I think it may be of interest to you to outline briefly something of the organization and function of the Institute. It may be viewed as a loose consortium of laboratories among which coordination and collaboration are effectively realized by virtue of our mutual interests in marine science and communal use of support facilities.

"Dr Campbell has already referred to MEL and the Director, Dr Lloyd Dickie, who is well known to most of you. However, I should mention that it is one of several laboratories operated by the Fisheries Research Board of Canada (FRB Canada), a part of the new Department of the Environment (DOE) now in the process of being organized by the Federal government. Closely associated with MEL is the newly established Marine Pollution Laboratory, a joint undertaking of the Fisheries Service and the Fisheries Research Board. Its pollution monitoring function is under the direction of Mr John Dalziel, while its pollution research is headed by Dr Donald Gordon. They occupy a suite of trailers attached to a fine new fish tank building.

"Then there is AOL which I head. It is one of the four main elements of the Marine Sciences Branch of the Water Management Service, formerly with the Department of Energy, Mines & Resources (DEMR), but now also to be a part of the Department of the Environment. Its main thrust is in chemical and physical oceanography and hydrographic charting.

"AOL encompassed research groups in Marine Geology and Marine Geophysics, but in the reorganization these have been retained in the Department of Energy, Mines & Resources since they have to do with the solid earth sciences rather than with the atmosphere, water (fresh and salt), and renewable resources which are the concern of DOE. Thus a fourth laboratory is taking shape in the Institute. Its proposed name is Atlantic Geosciences Centre and comprises, not only the geological and geophysical elements of AOL, but also certain other units of DEMR whose work is concerned with off-shore geoscience and mineral exploitation.

"A description of the Institute would not be complete without reference to the Secretariat of ICNAF and to its Executive Secretary, Lou Day, although he hardly needs introduction to this assembly. You will all appreciate, I am sure, the leading role he has played in the arrangements for this symposium.

"Again speaking for the Institute as a whole, we stand ready to do all we can to assist in making this a most successful symposium; our facilities are at your disposal. I hope you will have the opportunity, despite your heavy agenda, to visit around the Institute to see something of our activities and to meet with members of the staff other than those attending the symposium.

"In turning the proceedings back to Dr Campbell, may I wish you all success."

Dr Campbell: "Thank you, Dr Ford, for your kind words of welcome and your outline of the organization and function of this large and busy institute of ocean research."

Contributed Papers

Contribution Number 1

Contributors to the Discussion

Dr A. Walton,
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Temperature Conditions in the North and Northwest Atlantic During the Decade 1969-1970

By Martin Rodewald[1]

Abstract

World Weather Records, including decadal means, have been published for such decades as 1931-40 and 1951-60. Nearly 10 years ago, Rodewald (1963) reviewed sea temperatures of the North Atlantic during the 1951-60 decade. Thus, it may be interesting to review the temperature conditions of the North and Northwest Atlantic in the "meteorological" decade 1961-70 and to compare its decadal mean conditions with those of the preceding 1951-60 decade.

Addition of the year 1970 provides special interest. It shows that the cooling trend of sea temperature (ST) in the North Atlantic (figure 2, Rodewald, 1967) has now reached a new maximum value for the last 20 years, i.e. -0.09°C from the period 1965-69 to the period 1966-70 for all nine Ocean Weather Stations (OWS). Thus, the trend curve in the figure 2 referred to above may now be extended as follows:

1962-66	*1963-67*	*1964-68*	*1965-69*	*1966-70*
+11.74°	*+11.73°*	*+11.71°*	*+11.69°*	*+11.60°*

The coldest 5-year period of the last two decades was from 1966 to 1970. It was 0.43° colder than the warmest one (+12.03°) in 1951-55, and is not far (0.1°) from the normal for the approximate period 1900-40, as proposed by the author (table 2, Rodewald, 1952).

If ST monthly means for the 1961-70 and 1951-60 decades for all nine OWS are compared, it is clear that the recent North Atlantic cooling is mainly a summer phenomenon having its maximum of -0.51° in *the July-September quarter. Minimum cooling of -0.07° occurred in the December-February quarter. The result is emphasized by the distribution of positive and negative signs, i.e. for the 3 months July-September at the nine OWS, of a total of 27 ST differences [(1961-70) minus (1951-60)] 26 are negative and only 1 is slightly positive (+0.05°). For the winter months, the distribution of signs is equal, being 13 to 13.*

A tentative picture of the regional distribution of the ST summer (July-September) cooling shows a tongue of maximum decrease extending from the waters near Nova Scotia in the general direction of the oceanic Polar Front towards the sea area near 60°N, 10°W. The mean ST winter (December-March) changes between the decades 1951-60 and 1961-70 show a warming centered in the Irminger Sea (OWS A, +0.68°), extending with lesser values to the areas of OWS B, C, and M. If differences of these seasonal ST changes (winter minus summer) are formed, the Irminger Sea appears to be a "centre of action" which determines the pattern of isolines over the whole North Atlantic from the Norwegian Sea to Nova Scotia. It can be shown that this pattern of (at least relative) decadal winter warming of the sea is connected with a winter weakening of the Icelandic Low.

The winter rise in air pressure of up to more than +4 mbar that took place between the 1951-60 decade and the 1961-70 decade near Southeast Greenland was combined with a decrease in pressure over

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the Barents Sea and the Kara Sea down to more than -2 mbar. In this way a climatic change to much colder air conditions was initiated in the region between Eastern Greenland and the Kara Sea. This is illustrated by the fact that Jan Mayen – a station operated since 1922 – had 6 of its coldest 7 winters (Dec.-March) in the 1965-70 period. There are signs that this cooling trend is spreading more to the south and west. The top layer of Polar water off West Greenland showed a pronounced cooling during the last decade (a Norwegian station, in April, from +0.78° to -1.22° as a 3-year mean). OWS A (62°N, 33°W) now shows a sharp reversal of the warming trend that had prevailed in the Irminger Sea during the fifties and sixties.

The Arctic winter cooling and the Atlantic summer cooling may have their main connecting link in the ICNAF Area. Since the climatic outlook, based on oxygen isotope studies of the North Greenland ice sheet (Johnsen et al., 1970) is for a continued cooling trend for another one to two decades, future developments may include the danger of a new "Little Ice Age", and therefore all the questions of the inter-relationship of physical and biological effects are of increased interest.

Further details of ST conditions, especially in the Northwest Atlantic, in the 1961-70 decade are presented and discussed.

Introduction

World Weather Records, including decadal means, have been published for such decades as 1931-40, 1941-50, and 1951-60. Nearly 10 years ago Rodewald (1963) dealt with sea temperatures in the North Atlantic during the decade 1951-60. Thus, it may be interesting to review the temperature conditions of the North and Northwest Atlantic in the "meteorological" decade 1961-70 and to compare the decadal mean conditions 1961-70 with those of the preceding decade 1951-60.

Addition of the last year, 1970, is in this case of special interest. It reveals the fact that the North Atlantic cooling trend of sea temperature (ST) (Rodewald, 1967; figure 2, p. 13) has now reached a new maximum value for the last 20 years: -0.09°C from the 5-year period 1965-69 to the 5-year period 1966-70 for the whole of nine Ocean Weather Stations (OWS). The trend curve for all nine stations and for the 20-year period 1951-70 is shown in Fig. 1. With an average value +11.60°, 1966-70 was the coldest 5-year period of the last two decades. It was 0.43° colder than the warmest

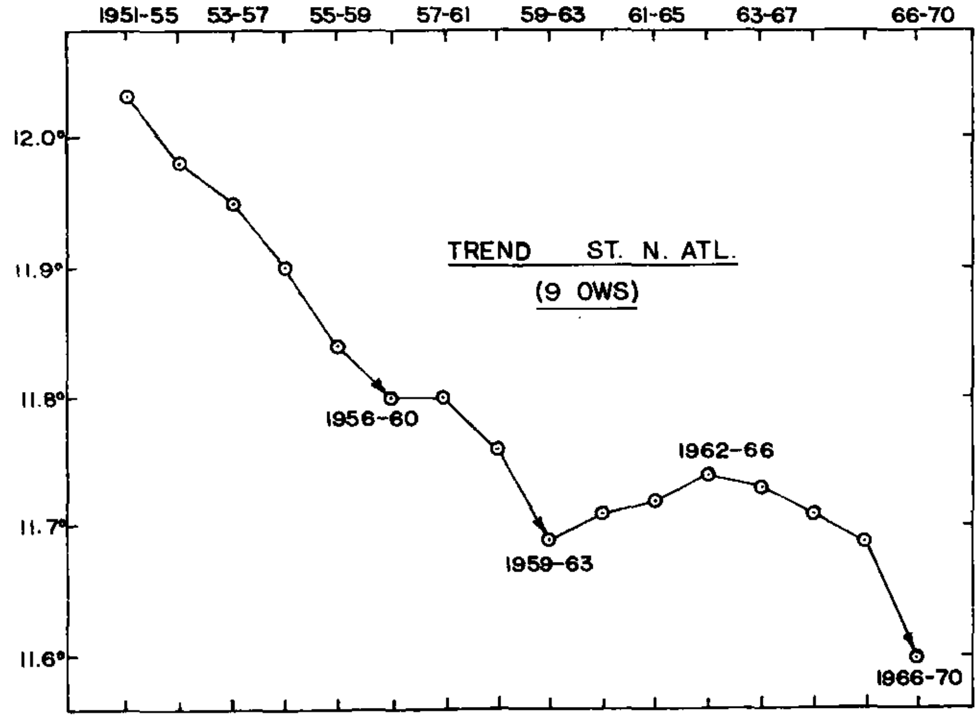

Fig. 1. Five-year running annual means of sea temperature (ST) for the whole of nine North Atlantic Ocean Weather Stations (OWS).

one in the early fifties (1951-55: +12.03°), and is not far (+0.1°C) from the normal (+11.51°) for all nine OWS, a normal which is very roughly valid for the period 1900-40 (Rodewald, 1952).

If differences between consecutive ST monthly means for 5-year periods are formed, the following approximate distribution of plus (+) and minus (-) signs may be expected for the case of *no* prevailing trend, and for 108 signs (9 OWS X 12 months): 50 plus, 50 minus, 8 zero.

Figure 2 shows the respective distribution of trend signs (zero excluded) for the last two decades. The special development between the pentades 1965-69 and 1966-70 is clearly shown: never before has there been such a high ratio of minus to plus signs, as 77:25.

General ST Comparison Between the Decades 1961-70 and 1951-60

Comparison of ST monthly means for the decades 1961-70 and 1951-60 and for all nine OWS shows that the recent North Atlantic cooling is mainly a summer phenomenon, with a maximum of -0.51°C in the quarter July-September (Fig. 3). Minimum cooling of -0.07°C occurred in the quarter December-February. The result is stressed by the distribution of signs: for the three months, July, August, September and the nine OWS, 26 out of a total of 27 ST differences "(1961-70) minus (1951-60)" are negative, and only one is very small in the positive (+0.05°). For the winter months, the sign distribution is equal at 13:13.

A tentative picture of the regional distribution of the ST summer (July-September) cooling shows a tongue of maximum decrease extending from near Nova Scotia seaward in the general direction of the oceanic Polar Front towards the area near 60°N, 10°W (Fig. 4, lower part).

The ST mean winter (December-March) changes from 1951-60 to 1961-70 show a warming centred in the Irminger Sea (OWS A, + 0.68°), extending with lesser values to the areas of OWS B, C, and M and surrounded by a southern belt of cooling (Fig. 5).

If differences of these seasonal ST changes (winter minus summer) are formed, the Irminger Sea emerges as a "centre of action" which determines the pattern of isolines over the whole northern ocean from the Norwegian Sea to the Newfoundland region (Fig. 4, upper part). The pattern of isallotherms resembles that of the isobars of the normal Icelandic Low which in this way appears as a centre of – at least relative – decadal winter warming of the sea.

It can be shown that a remarkable weakening of the Icelandic Low took place during the winter months of the last decade (Fig. 6, after Rodewald, 1971). There was a seasonal rise of the atmospheric pressure of 4 mbar between Iceland and Southeast Greenland from 1951-60 to 1961-70. The great effect on pressure gradients, averaged for the four winter months December-March, may be taken from the following comparisons:

OWS B ⟶ OWS A

Normal pressure difference	1005.0	–	1001.2	=	3.8 mbar (100%)
1951-60 pressure difference	1005.0	–	999.6	=	5.4 mbar (142%)
1961-70 pressure difference	1004.3	–	1003.7	=	0.6 mbar (16%)

OWS C ⟶ OWS A

Normal pressure difference	1005.2	–	1001.2	=	4.0 mbar (100%)
1951-60 pressure difference	1005.4	–	999.6	=	5.8 mbar (145%)
1961-70 pressure difference	1005.4	–	1003.7	=	1.7 mbar (42%)

Normal values have been computed from Scherhag's charts (Scherhag, 1969). The above table, with Fig. 6, reflects the decrease of cyclonic circulation conditions, or the increase of anticylonic circulations in those sea areas which are dominated normally by the rear flows of the Icelandic Low.

We can form similar mean pressure differences for the four winter months December-March and for the northeastern extension of the Icelandic Low between Akureyri, North Iceland (65.7°N, 18.1°W) and Dikson, Kara Sea (73.5°N, 80.2°E) which are shown below:

Dikson ⟶ Akureyri

Normal pressure difference	1014.0	–	1004.0	=	10.0 mbar (100%)
1951-60 pressure difference	1014.9	–	1004.6	=	10.3 mbar (103%)
1961-70 pressure difference	1012.7	–	1008.1	=	4.6 mbar (46%)

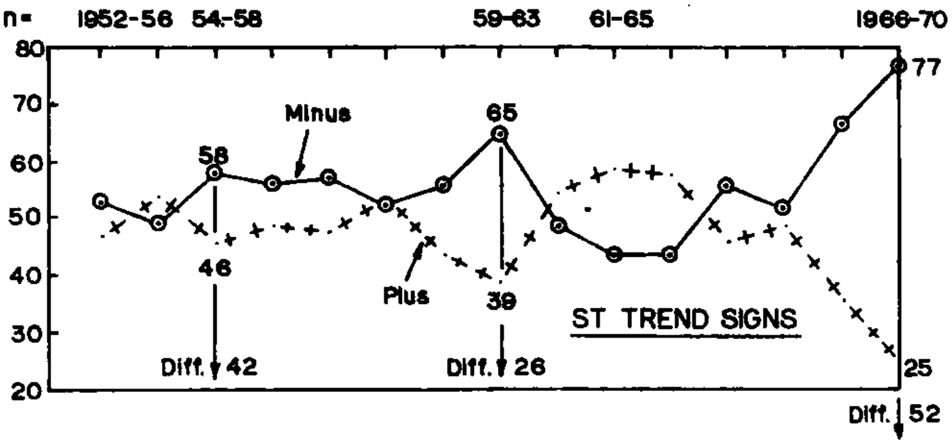

Fig. 2. Number of plus and minus signs in the difference between consecutive 5-year running monthly means of ST for nine OWS (= frequency of trend signs).

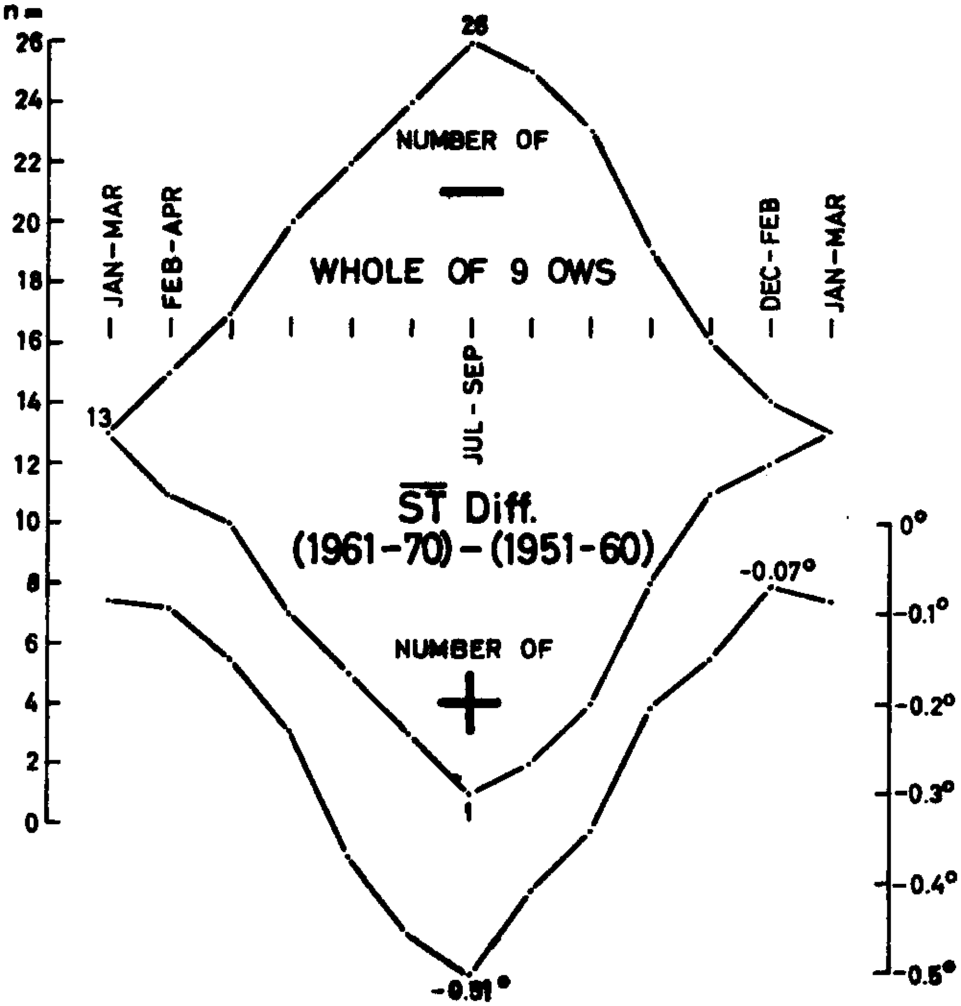

Fig. 3. Difference of decadal means of sea temperature, (1961-70) minus (1951-60), for all nine North Atlantic OWS and for running quarters. *Upper curves:* Number (n) of negative and positive differences of the decadal monthly means for the nine OWS and 3-month periods.

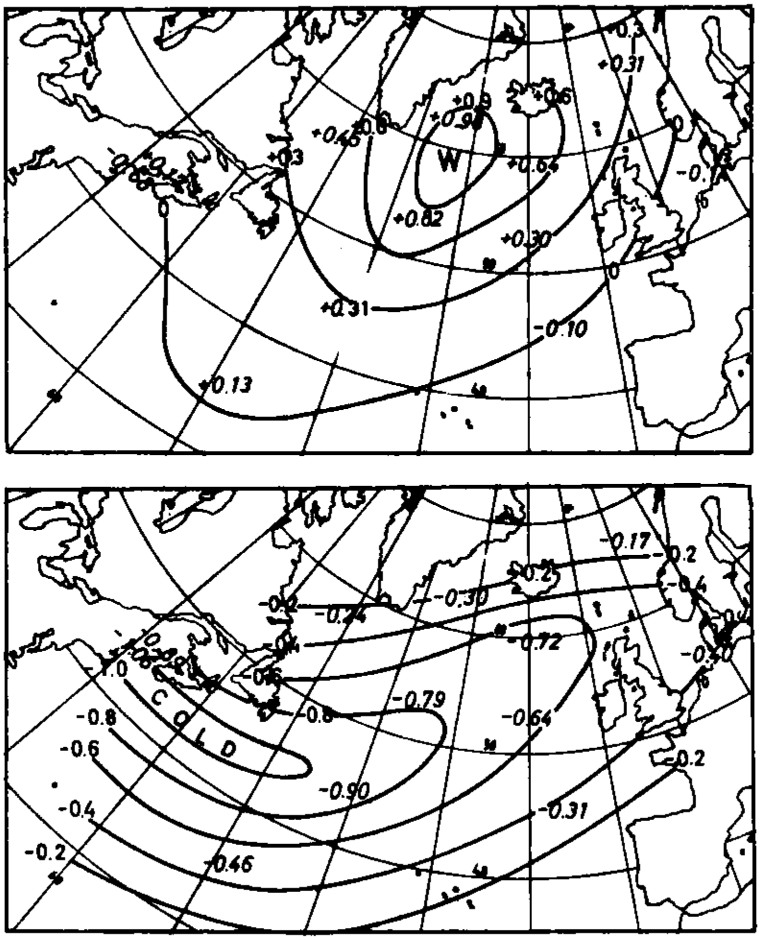

Fig. 4. (Above) Change of the decadal mean sea temperatures from 1951-60 to 1961-70 for the winter season *relative* to the change for the summer season.

(Below) Change of the decadal mean sea temperatures for the maritime summer (July-September) from 1951-60 to 1961-70.

Thus there was an effective weakening of the south wind component against the Arctic Front between Iceland and the Kara Sea during the last decade or, in other words, a much higher frequency of Arctic outflow from a northerly direction in the Eurasian sector of the Polar Sea. In this way almost a climatic drop to much colder air conditions was initiated in the region between Eastern Greenland and the Kara Sea. The centre of the cooling was near Franz Josef Land (Fig. 7, after Rodewald, 1971).

The outstanding strength of this climatic deterioration is illustrated by the fact that Jan Mayen – an island station in the East Greenland Sea operated since 1922 – had *six* of its seven coldest winters (December-March) in the period *1965-70.* The recent hydrographic changes in the waters off North Iceland have been treated by Sv. A. Malmberg (1969) and U. Stefánsson (1969), and indicate generally much colder conditions with less salinity in the upper layers, and with a highly increased frequency of drift ice.

Polar cooling (in the atmosphere) from 1951-60 to 1961-70 was mainly a winter phenomenon, (Fig. 8, upper part). For the two stations, Spitsbergen and Bear Island combined, February was *4.3°C* colder in the last decade, while August was 0.55°C colder. The annual variation in this Polar cooling is opposite to that of the North Atlantic cooling.

The west coast of Greenland, however, has a climatic winter warming, as shown for Egedesminde

(68.7°N, 52.9°W) (Fig. 8, lower part), and as can be explained by the change in the atmospheric circulation. It is remarkable that, in spite of this pronounced winter warming, the great majority of months at Egedesminde had cooler conditions in the last decade.

Returning to ST conditions, we may give another comparison between the decades 1951-60 and 1961-70 for the coastal surface water at Boothbay Harbor, Maine (Fig. 9). Compared with monthly normals for the 40-year period 1906-45, the decade 1951-60 was 1.8°C warmer than normal here, but the decade 1961-70 was only 0.4°C warmer than normal. During both decades the deviation from normal had a pronounced annual variation, with a winter maximum and a summer minimum:

1951-60: Δ ST December + 3.6°C; July -0.2°C
1961-70: Δ ST December + 2.0°C; July -1.4°C

Cooling of the coastal water was greatest in February (-2.0°C), least in August (-0.9°C). As Fig. 9, lower part, shows, the coastal water at St. Andrews, New Brunswick, had much less cooling from the decades 1951-60 to 1961-70, and the seasonal distribution of ST change was quite another one: -1.1°C in July, -0.5°C in December. At Boothbay Harbor, Maine, the ST difference between the warmest 5-year period, 1951-55, and the coldest recent period, mainly 1964-68, was from -2.0°C in August to -3.3°C in February (Fig. 10).

Conditions in the open Northwest Atlantic were not uniform in both decades, as Fig. 11 shows for OWS A, B, and D. In some respects, however, the deviations of decadal monthly means were similar at OWS D and B, with relatively warm conditions in the cold season and a relatively cold sea in early summer. The Irminger Sea (OWS A) had a different type of deviations.

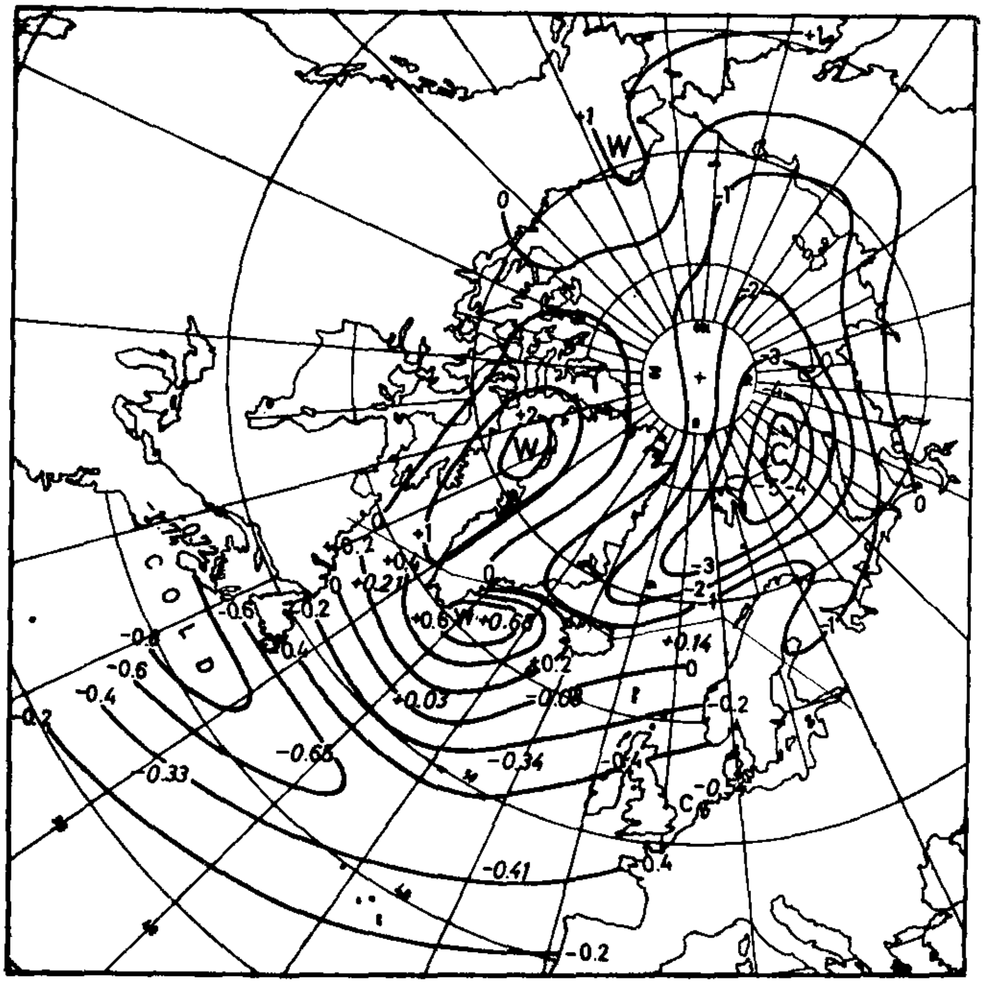

Fig. 5. Change of the decadal mean sea temperature for the maritime winter (December-March) from 1951-60 to 1961-70. *Polar part:* change of decadal mean *air* temperatures for the same winter months.

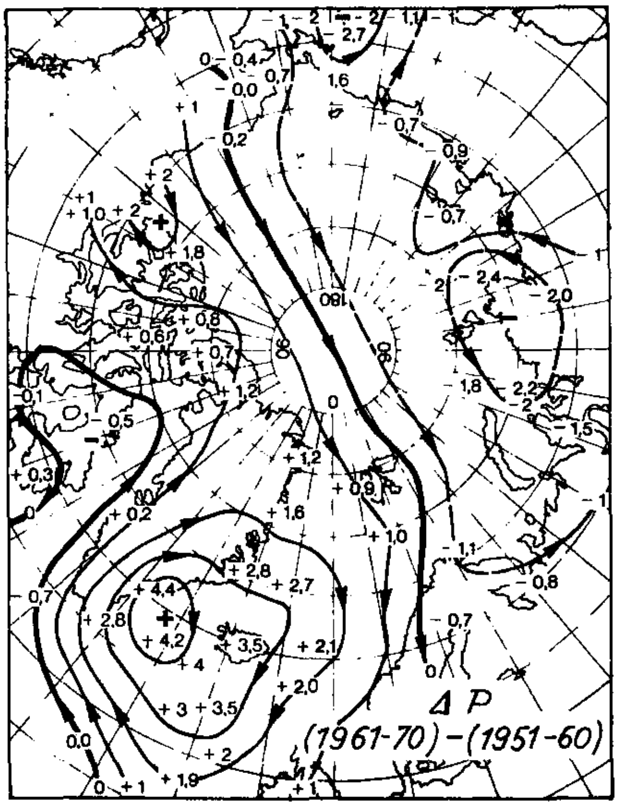

Fig. 6. Mean change of atmospheric pressure for the 4 months December, January, February and March, from the decade 1951-60 to the decade 1961-70 over the North Polar Sea and adjacent North Atlantic.

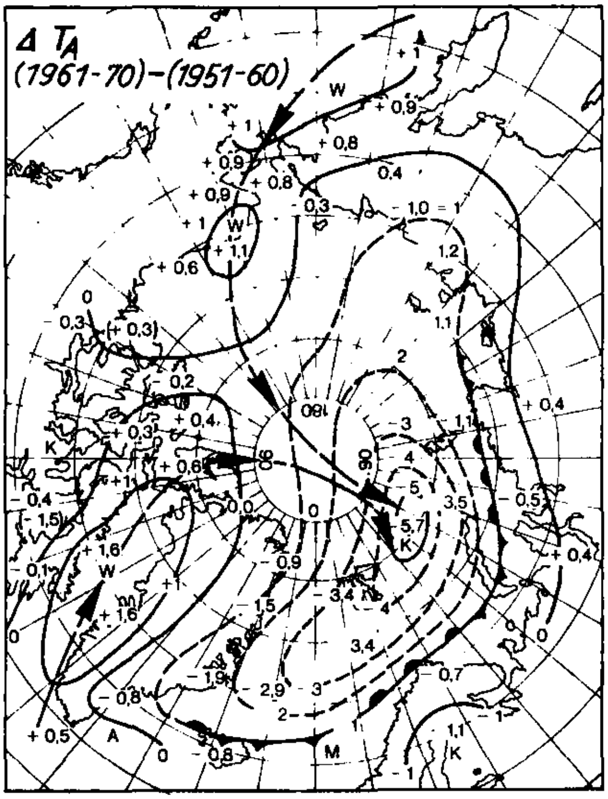

Fig. 7. Mean change of air temperature for the four months December, January, February, and March from 1951-60 to 1961-70 over the North Polar Sea.

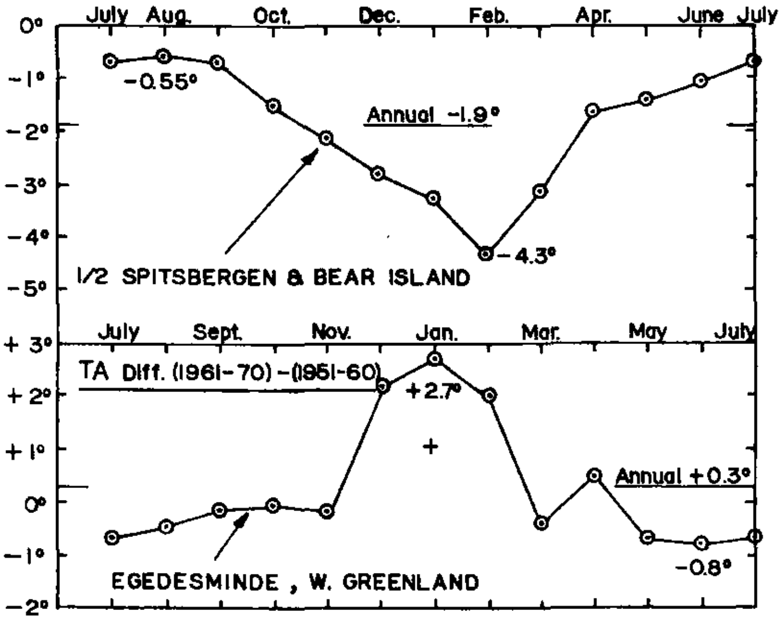

Fig. 8. Change of decadal monthly means of air temperature from 1951-60 to 1961-70 for the pair of stations Spitsbergen-Bear Island (above) and for Egedesminde, West Greenland (below).

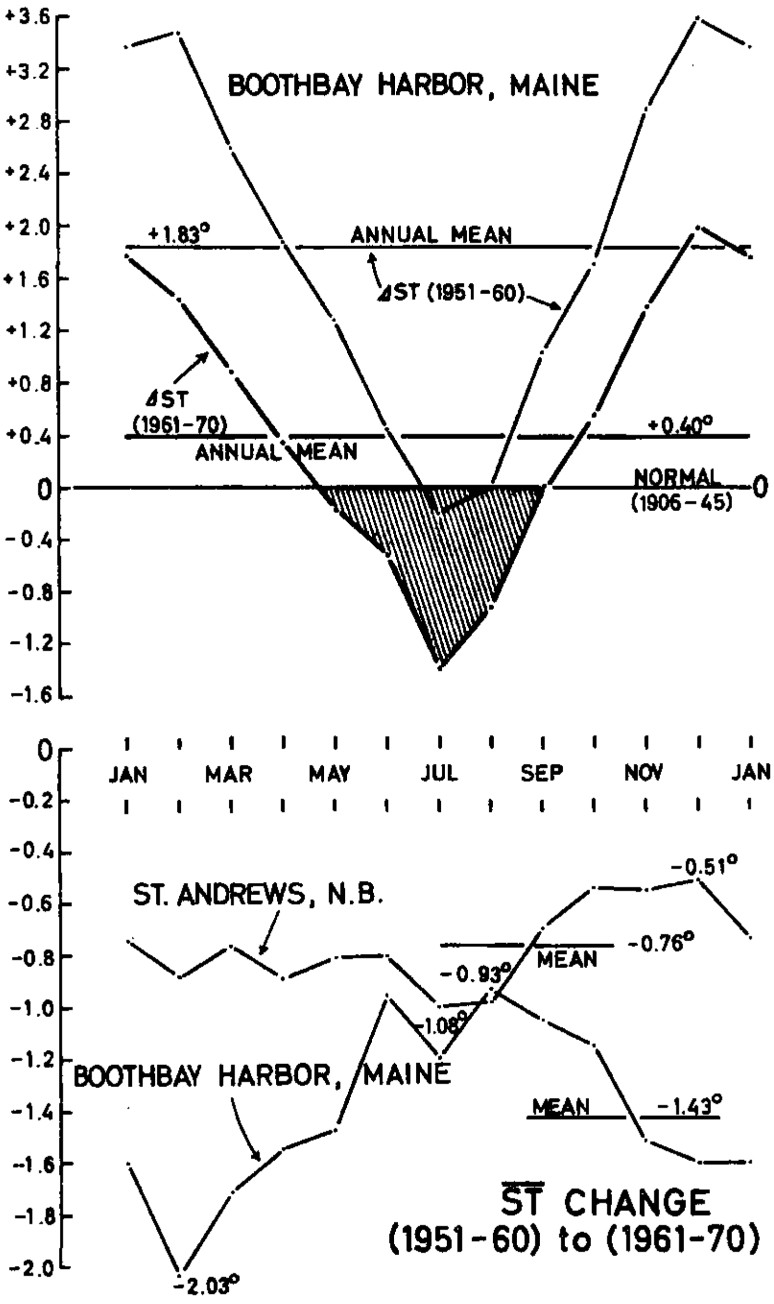

Fig. 9. (Above) Deviation of 1951-60 and 1961-70 decadal monthly and annual means of sea temperature from normal for Boothbay Harbor, Maine, USA.

(Below) Change of decadal monthly and annual means of sea temperature between the decades 1951-60 and 1961-70 for St. Andrews, New Brunswick and Boothbay Harbor, Maine, USA.

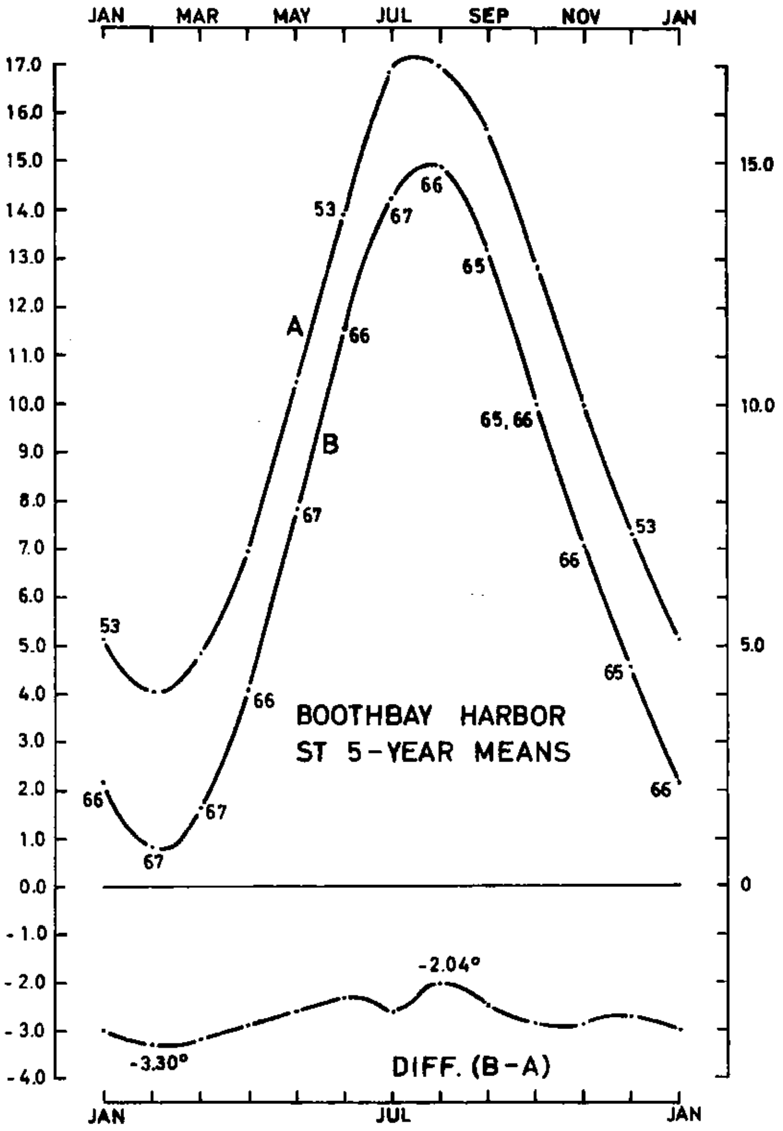

Fig. 10. Boothbay Harbor, Maine: 5-year monthly means of sea temperature for A = the *warmest* period, 1951-55, central year 1953, B = the *coldest* recent period, mostly 1964-68, central year 1966.

Change of ST deviations from 1951-60 to 1961-70 shows a more regular system, if OWS A, B, C, and D are compared (Fig. 12). On the whole, the order is A-B-C-D from higher to lower values, that means: warming tendencies more in the north, cooling tendencies more in the south. Secondly, all the four curves have their cool "valley" in summer, flanked by "mounts" of – at least, relative – warming in the colder seasons before and after. Figure 12 thus confirms the fact that the North Atlantic ST cooling trend so far was more a *southern* and a *summer* phenomenon.

ST Development During the Decade 1961-70

Annual means of sea temperature for the last decade and their trend curves are presented in Fig. 13 for the eastern OWS, and in Fig. 14 for the western OWS. The general tendency for the Northwest Atlantic, so far represented by OWS A, B, C, is a warming culminating in 1966, and followed by a cooling that makes the year 1970 the coldest one of the decade at OWS A and B.

It is interesting to note that the ST development in the coastal water at St. Andrews, N.B. and Boothbay Harbor, Maine, is in an opposite sense: cooling at first until 1967, and then warming sharply so that the last 2 years become the warmest years of the decade (Fig. 15). During the decade 1951-60, a distinct parallelism existed in the ST development between OWS D and St. Andrews, New Brunswick (Rodewald, 1963). However, this was lost in the last decade and 1967 was the warmest year of the decade at OWS D, and the coldest one in the coastal waters of Boothbay Harbor and St. Andrews.

The particularly cold conditions of 1968 in the sea area of OWS D, on the other hand, did not extend to the US coastal waters. A short treatment of this interesting case has been given in a former paper (Rodewald, in press), where the close connection between cyclonic activity and cold conditions could be shown.

Further details of the decadal ST development 1961-70 in the Northwest Atlantic may be taken from

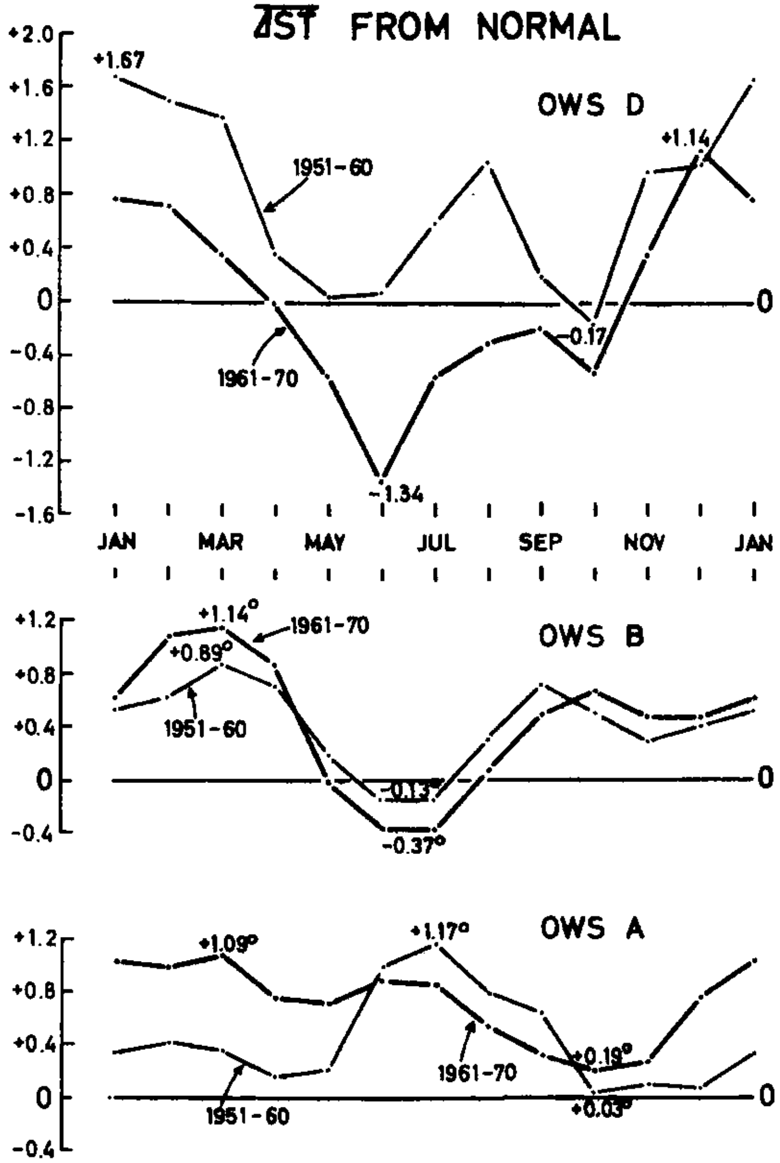

Fig. 11. Deviation of the 1951-60 and 1961-70 decadal monthly means of sea temperature from monthly normals for Ocean Weather Stations A, B, and D.

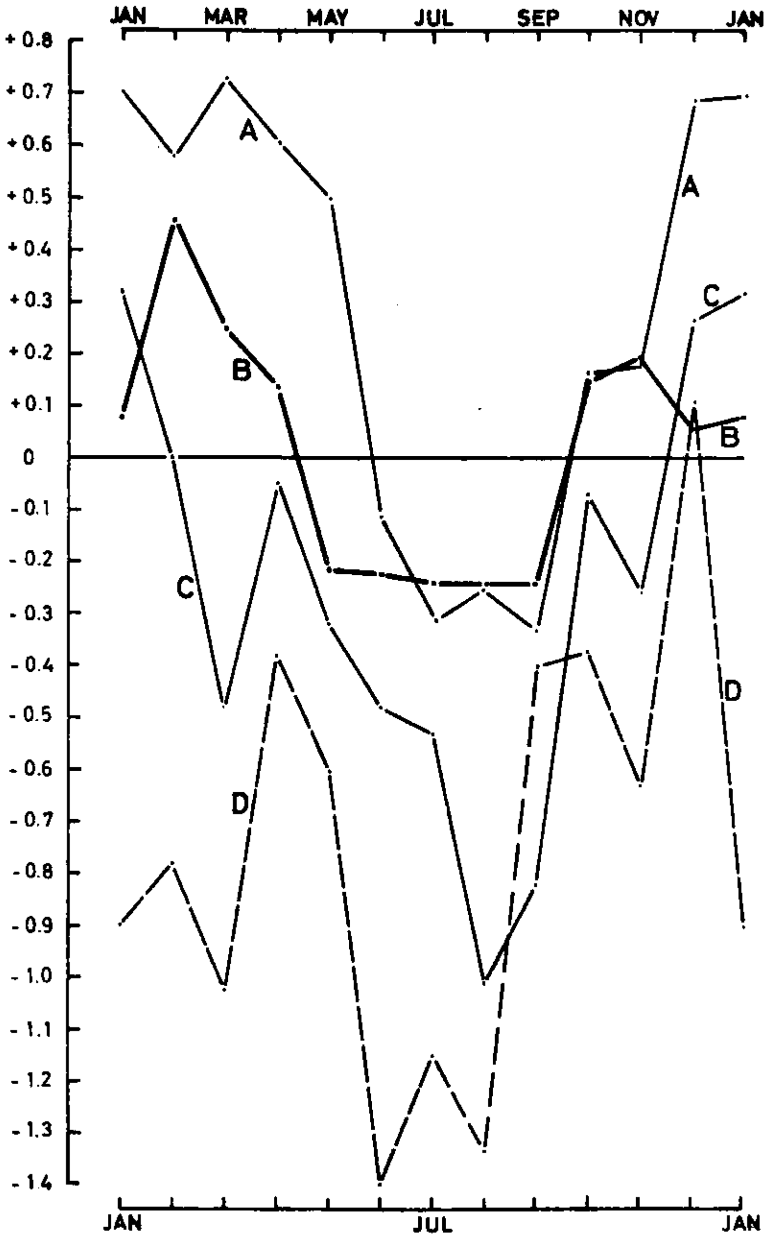

Fig. 12. Change of deviations from normal of decadal monthly means of sea temperature from 1951-60 to 1961-70 for Ocean Weather Stations A, B, C, and D.

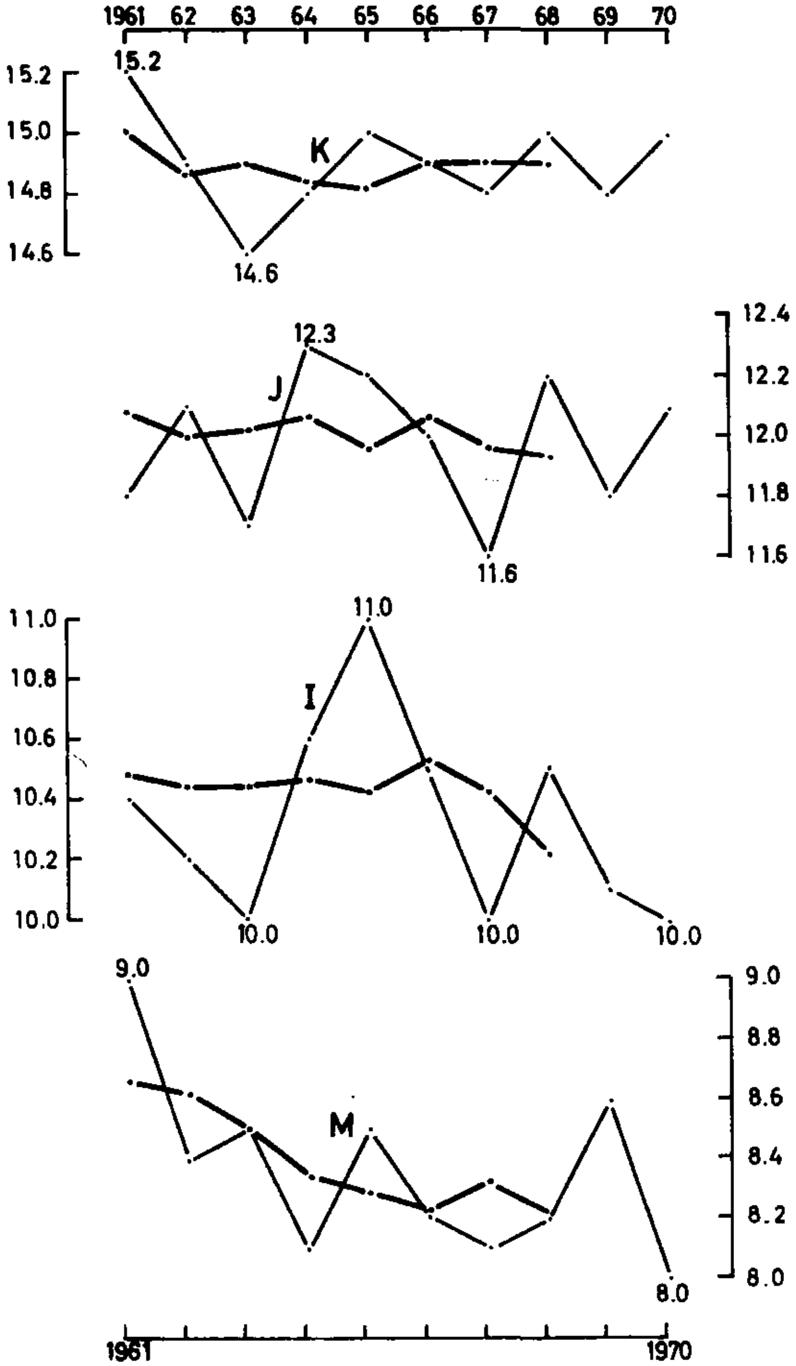

Fig. 13. Annual means of sea temperature for *eastern* Ocean Weather Stations from 1961 to 1970. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

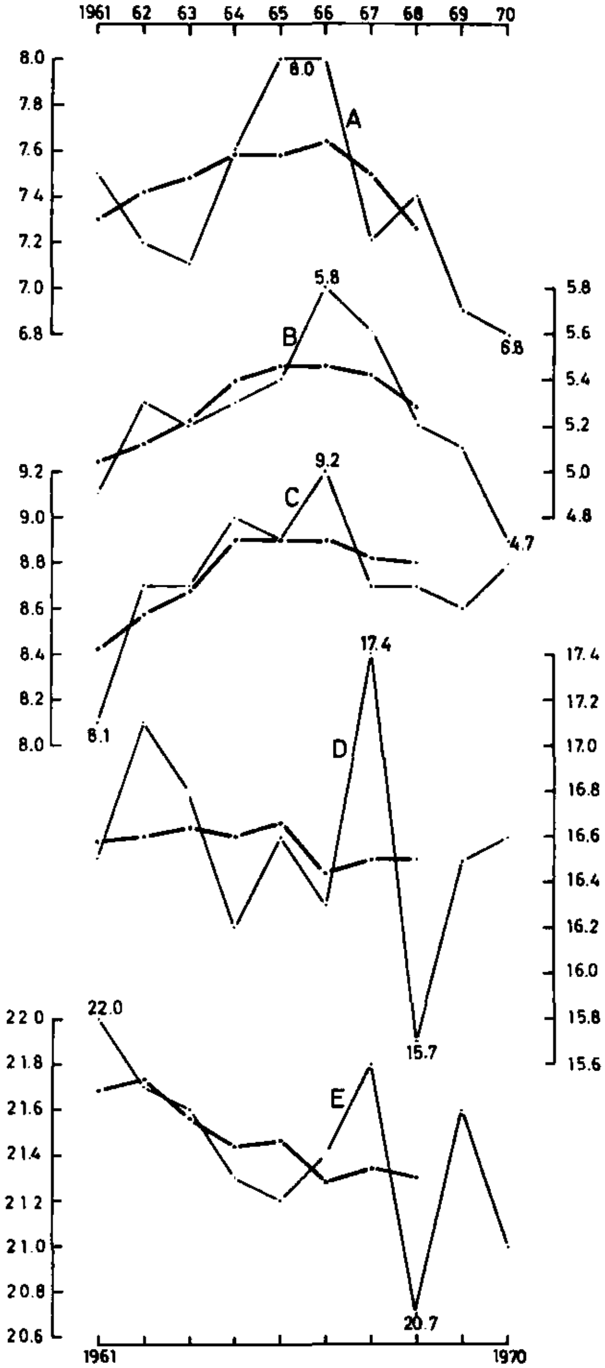

Fig. 14. Annual means of sea temperature for *western* Ocean Weather Stations from 1961 to 1970. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

the following figures:

OWS A (Figs. 16-19): The reversal of the warming trend into a cooling trend is the outstanding feature for every month of the year.

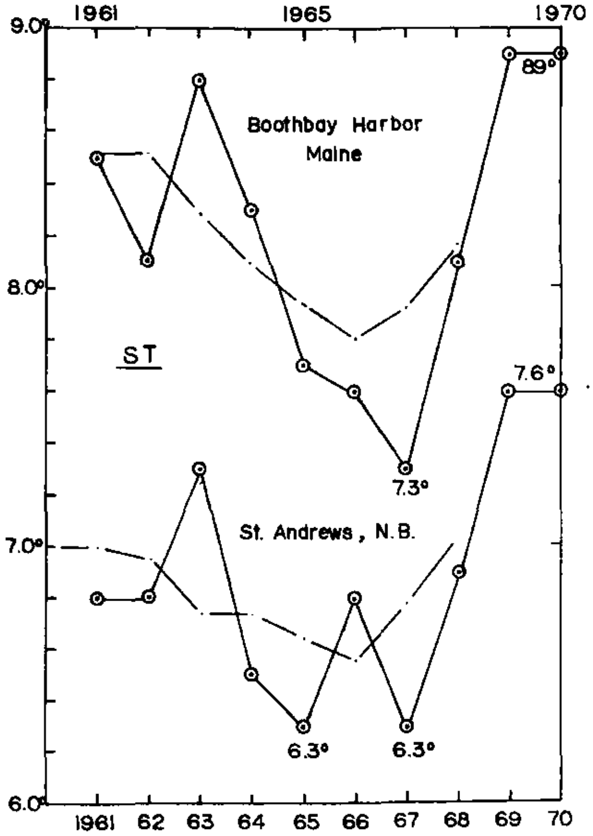

Fig. 15. Annual means of ST for Boothbay Harbor, Maine, and St. Andrews, N.B. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

OWS B (Figs. 20-22): Though the development is somewhat more irregular, it resembles greatly that at OWS A (exception July).

OWS C (Figs. 23-26): ST variation is rather different for the different months. The first and the last months of 1961 were extremely cold, but between was the warmest July of the decade. The majority of months show a warming trend, followed by a more irregular cooling.

OWS D (Figs. 27-29): Scale for ST in the figures has been reduced to half that of Figs. 16-26 because of the greater fluctuations which occur at OWS D. After the sharp cooling in the decade 1951-60 no remarkable and more uniform ST trend existed during the last decade.

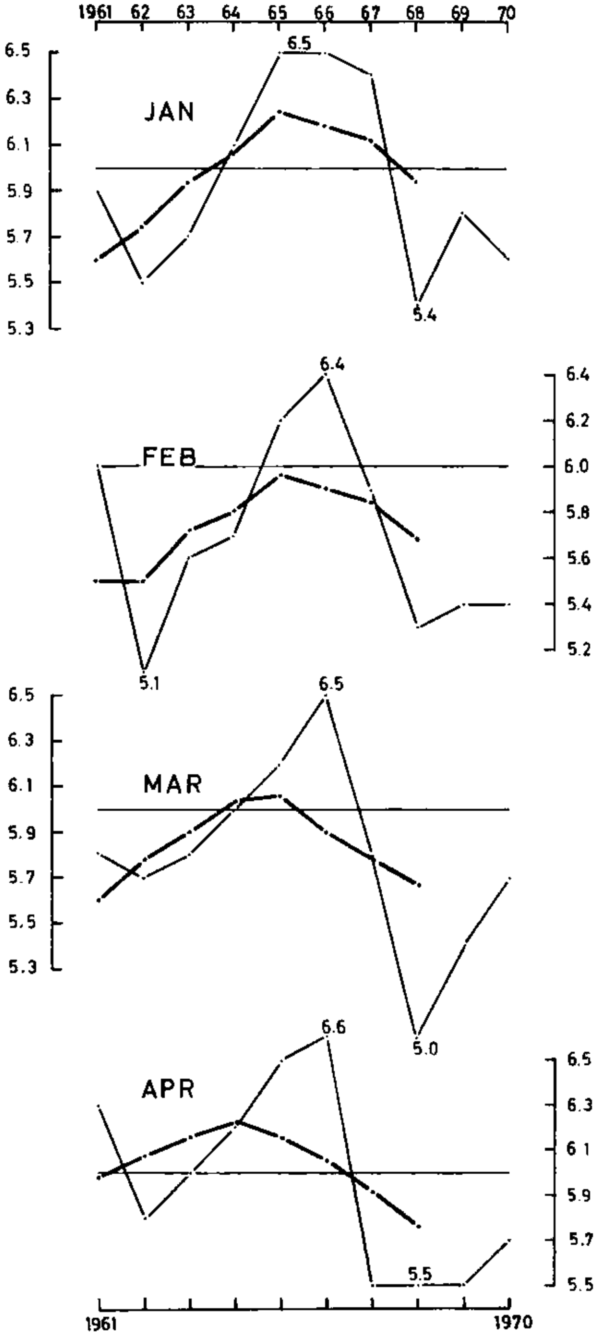

Fig. 16. OWS A: Monthly means of sea temperature 1961-70, January to April. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

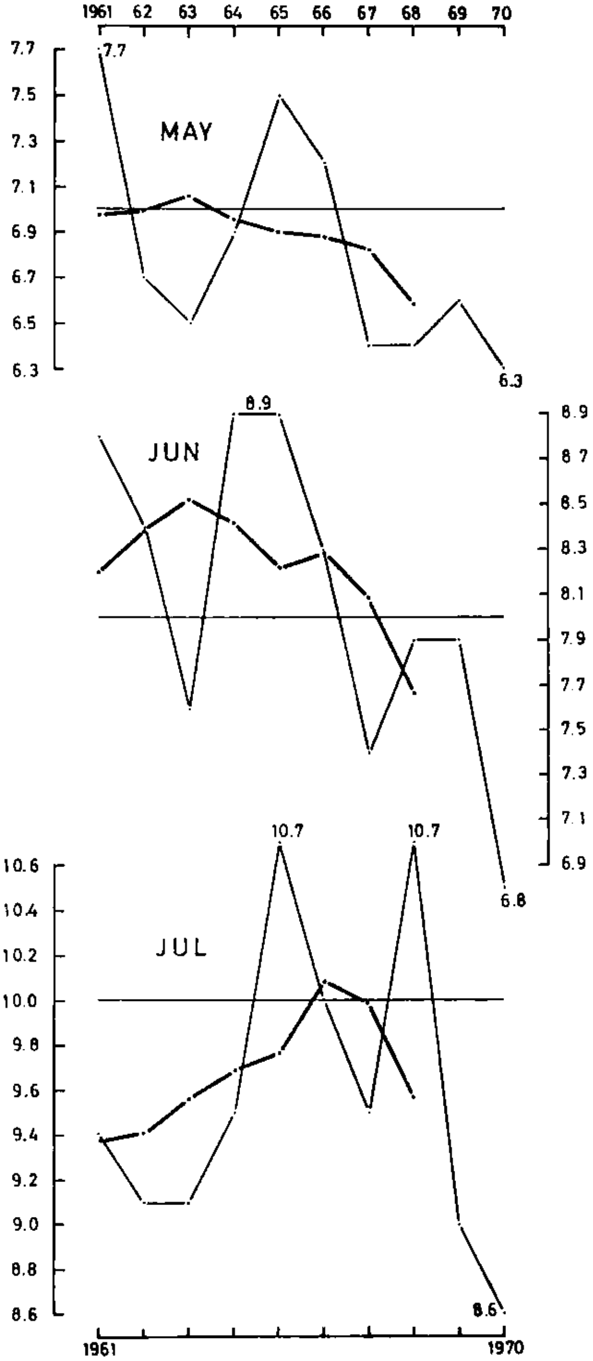

Fig. 17. OWS A: Monthly means of sea temperature 1961-70, May to July. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

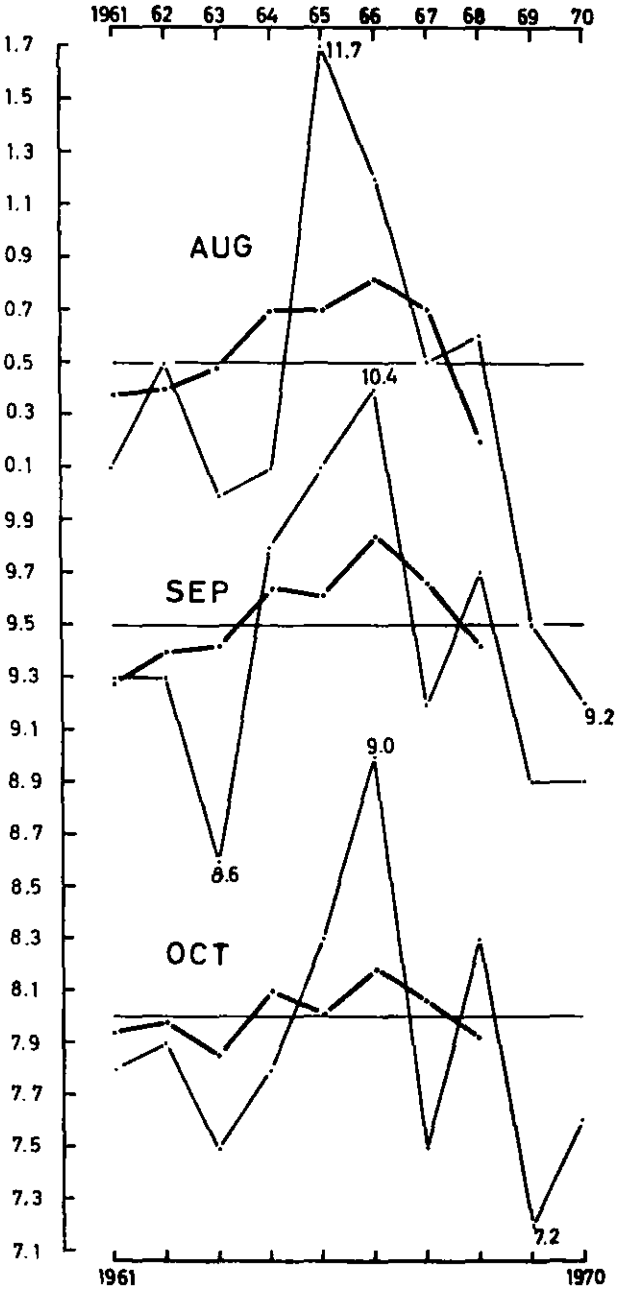

Fig. 18. OWS A: Monthly means of sea temperature 1961-70, August to October. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

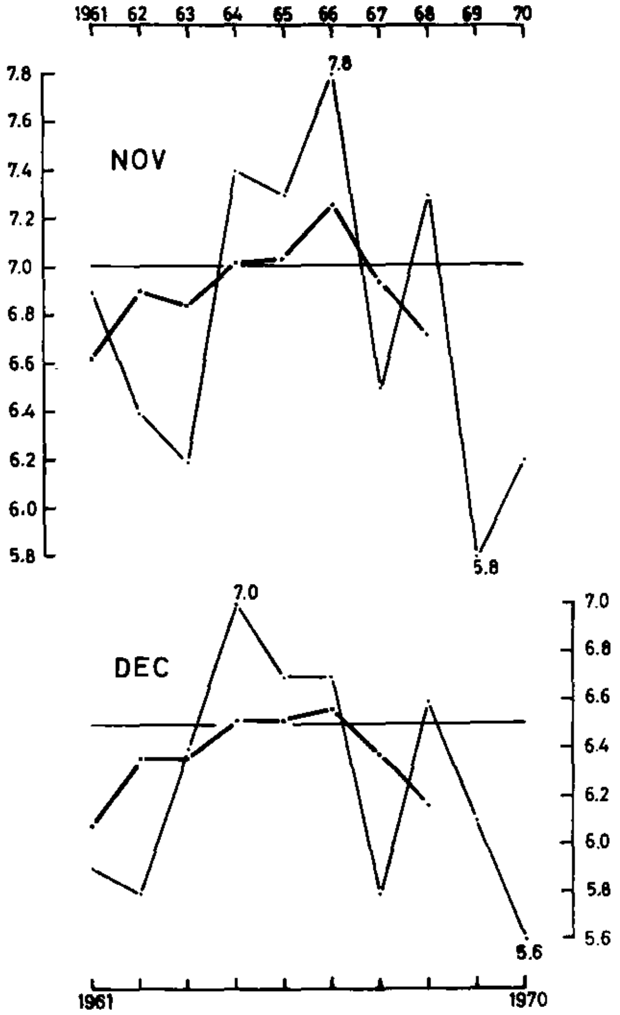

Fig. 19. OWS A: Monthly means of sea temperature 1961-70, November to December. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

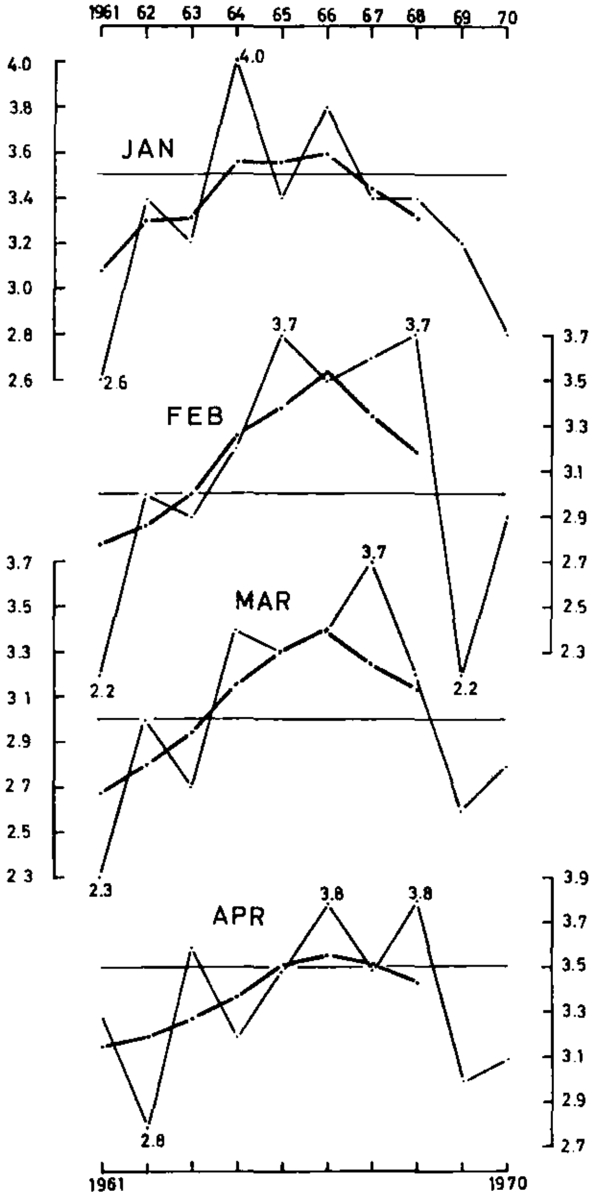

Fig. 20. OWS B: Monthly means of sea temperature 1961-70, January to April. Smoothed curves · 5-year running means from 1959-63 to 1966-70.

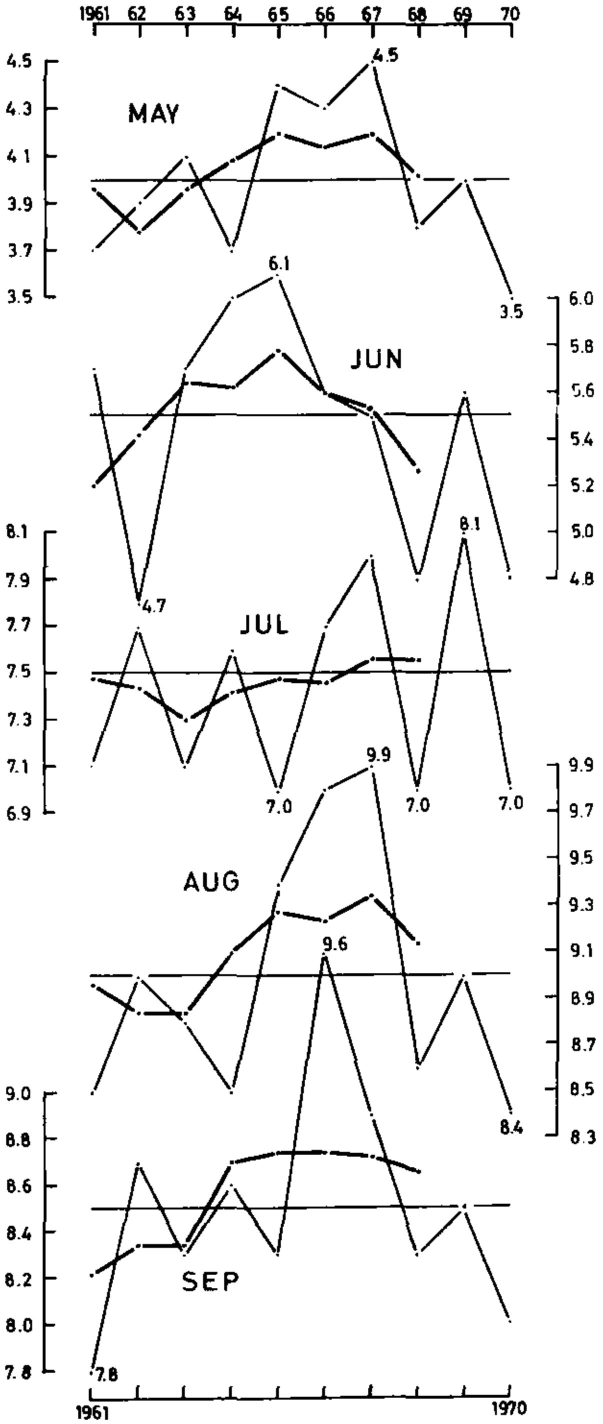

Fig. 21. OWS B: Monthly means of sea temperature 1961-70, May to September. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

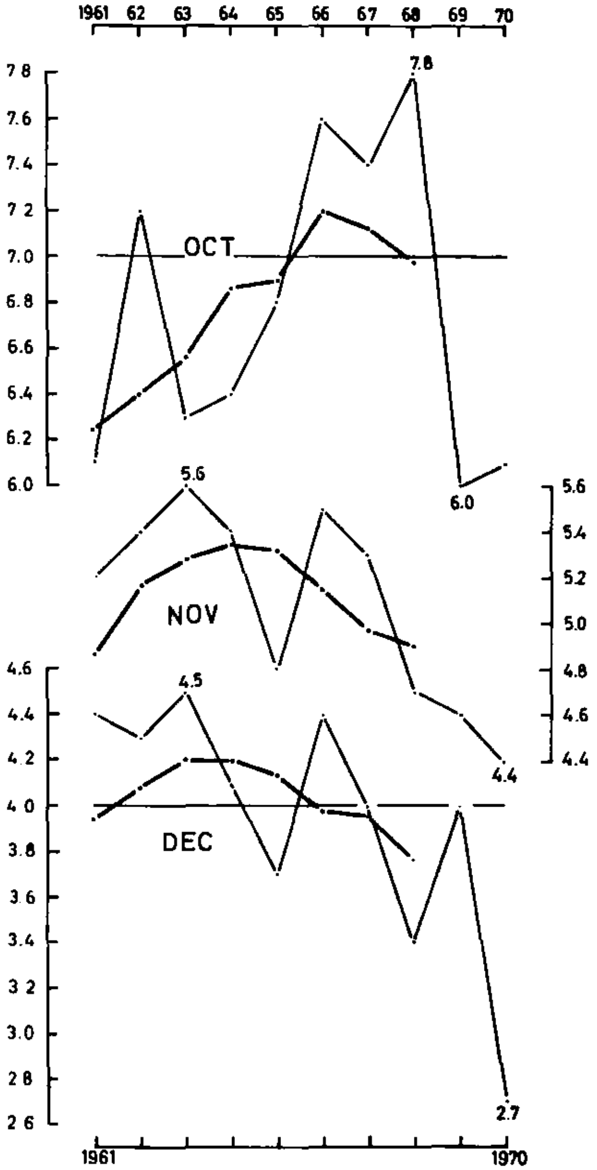

Fig. 22 OWS B: Monthly means of sea temperature 1961-70, October to December. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

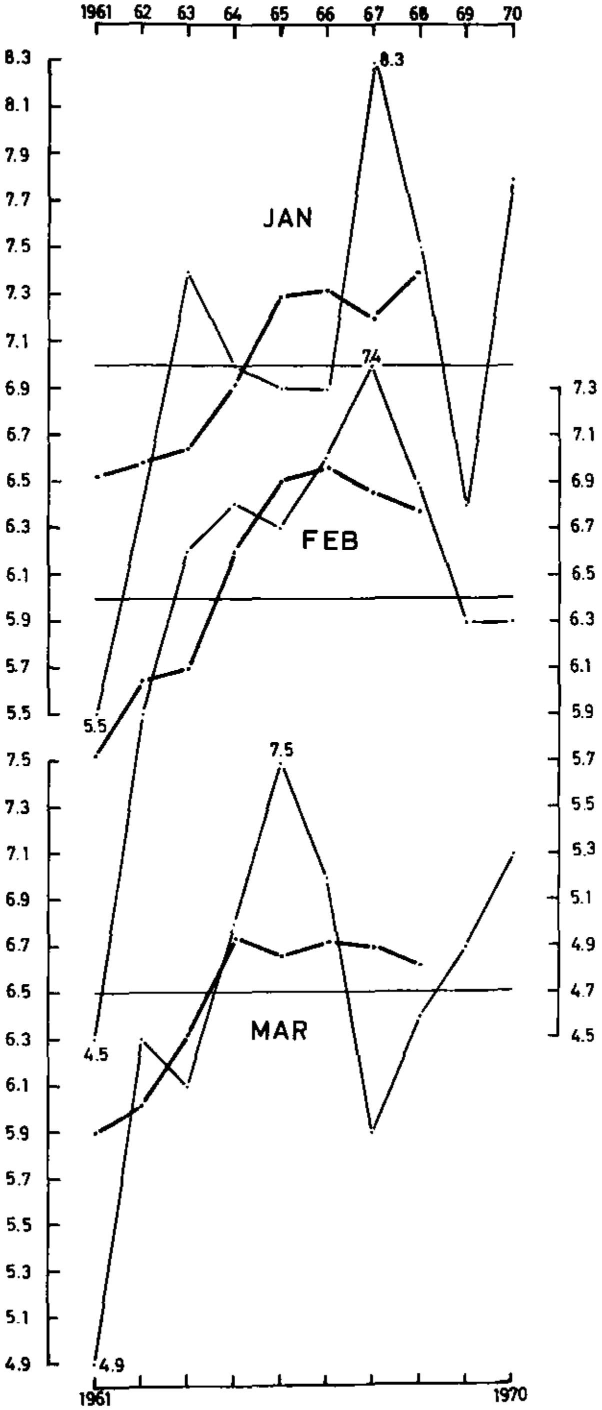

Fig. 23. OWS C: Monthly means of sea temperature 1961-70, January to March. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

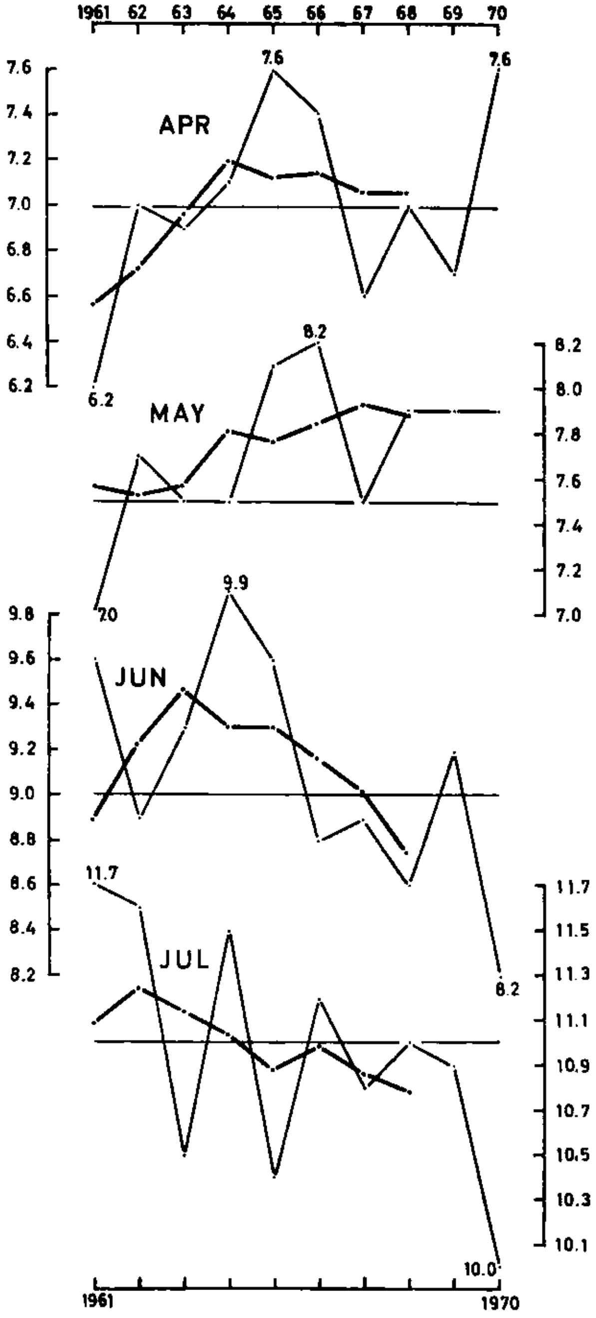

Fig. 24. OWS C: Monthly means of sea temperature 1961-70, April to July. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

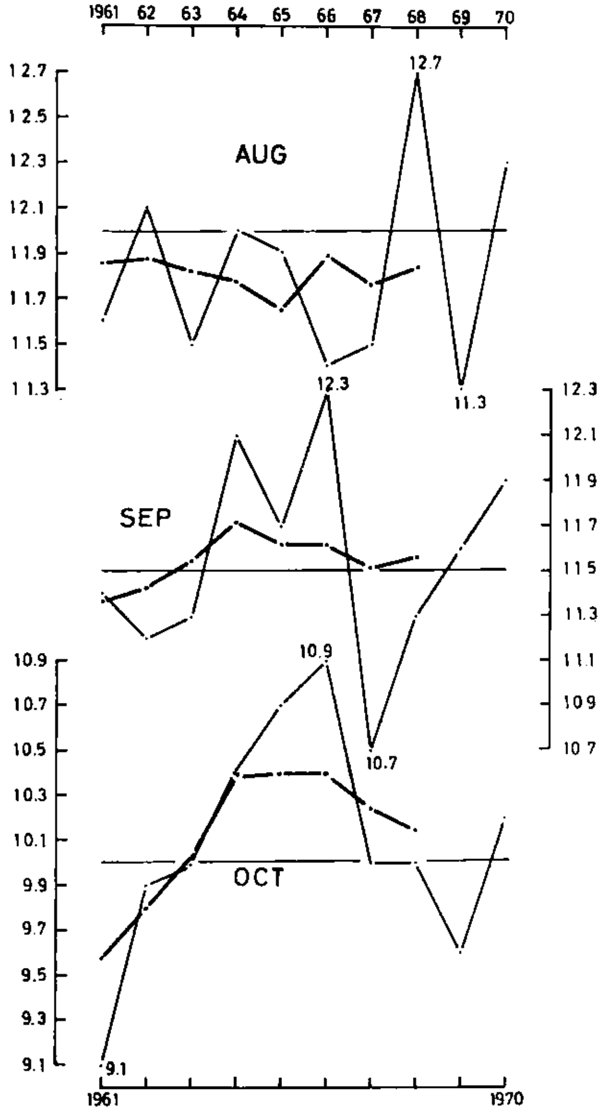

Fig. 25. OWS C: Monthly means of sea temperature 1961-70, August to October. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

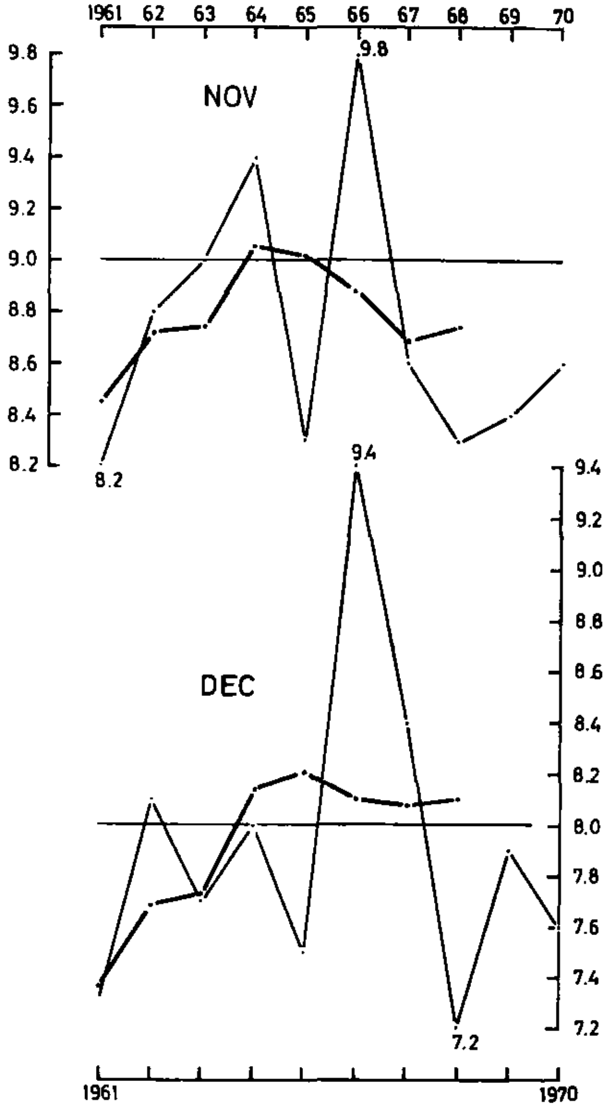

Fig. 26. OWS C: Monthly means of sea temperature 1961-70, November to December. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

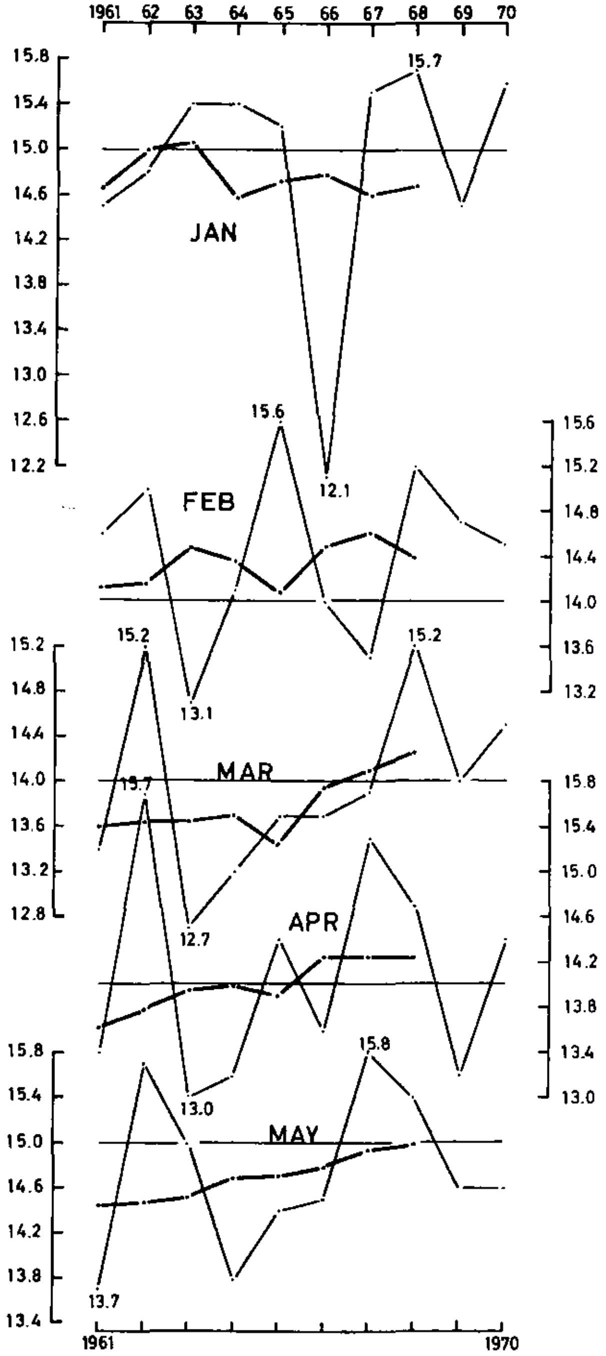

Fig. 27. OWS D: Monthly means of sea temperature 1961-70, January to May. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

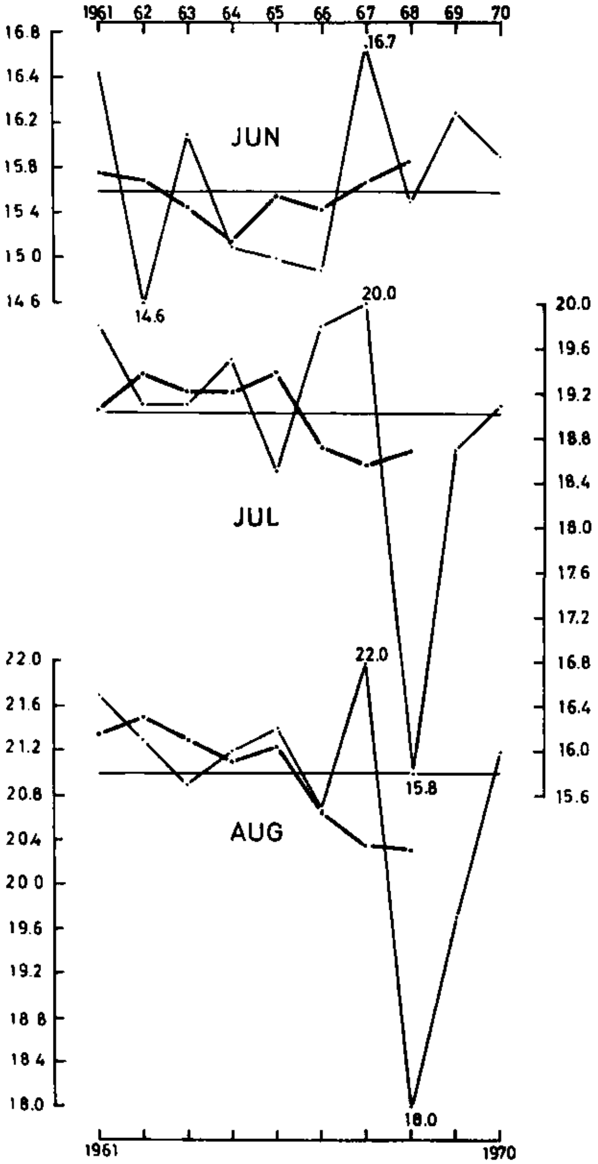

Fig. 28. OWS D Monthly means of sea temperature 1961-70. June to August. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

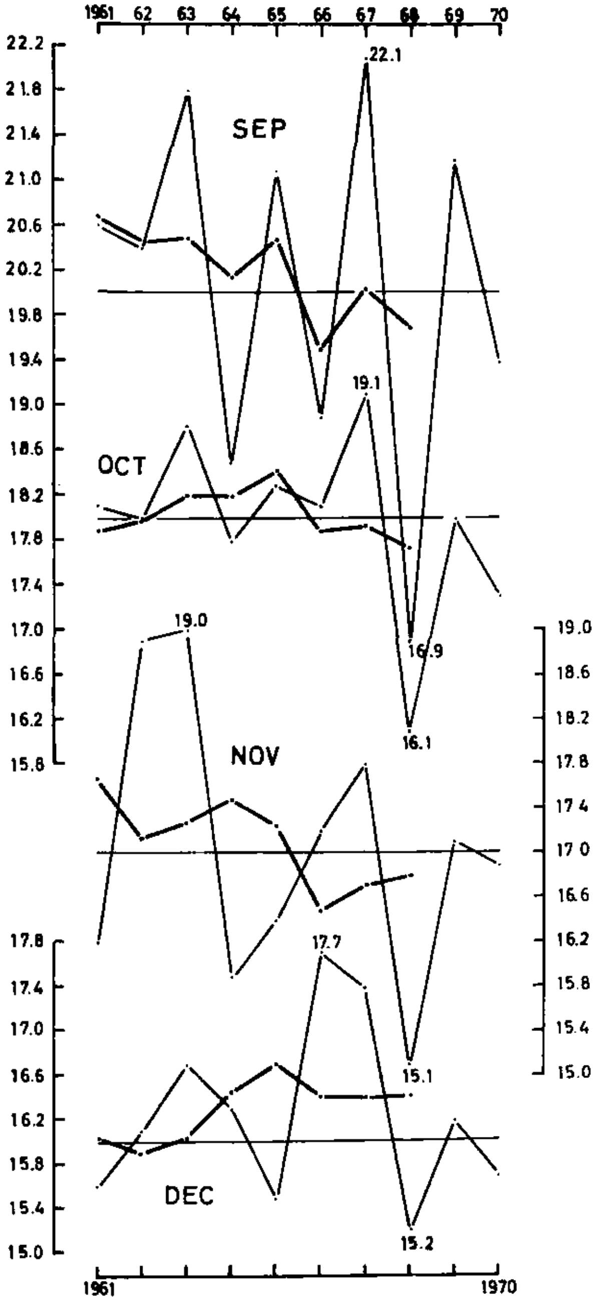

Fig. 29. OWS D: Monthly means of sea temperature 1961-70, September to December. Smoothed curves = 5-year running means from 1959-63 to 1966-70.

A very well developed cooling trend, however, can be shown for the Polar water of the West Greenland Current (Fig. 30), though only for April when a Norwegian hydrographic expedition operated there every year from 1959 to 1969 (Blindheim, 1967). If 3-year running mean temperatures for the upper layer (0-50 m) at the hydrographic station 63°57′N, 52°55′W are calculated, an uninterrupted cooling is apparent, amounting to 2.0°C from +0.78°C in the 3-year period 1959-61 to -1.22°C in the 3-year period 1967-69.

A similar cooling trend prevails at the other hydrographic stations off West Greenland, but with a preceding shorter warming at some of them.

It seems that the northern cooling was at first confined to the rather narrow Polar Current along the coast of South Greenland. But in the last years of the decade, cooling has spread over the Irminger Sea and the adjacent waters to the east and west. Figure 31 shows that the ST change from the 5-year period 1965-69 to the last period 1966-70 is characterized by this subpolar cooling and a warming of similar intensity of the coastal waters near Nova Scotia. Remembering that the change from 1951-60 to 1961-70 was toward warmer conditions in the north, we can say that the regional ST development has at last reversed, in a sort of seesaw motion.

The mean atmospheric circulation of the last (cold) 5-year period 1966-70 can be illustrated by the pressure deviation from normal (normal period 1900-39), presented by Fig. 32. There was a tendency for relative high pressure over South Greenland and for relative low pressure in subtropical latitudes. Northeasterly to easterly anomaly-winds extended over nearly the whole extratropical North Atlantic Ocean, and the picture is that of a below-normal general circulation (but with strengthened northeasterlies along the Southeast Greenland coast).

Some Correlations Between ST Changes and Other Statistical Values

When *average deviations* of ST monthly means for the period 1951-70 are worked out, the following lowest (second lowest) and highest (second highest) values (°C) result for the different OWS:

A	0.37	FEB	(0.40 JAN)	0.70	AUG	(0.63 JUL)
B	0.35	JAN	(0.35 APR)	0.70	OCT	(0.60 NOV)
C	0.45	DEC	(0.48 NOV)	0.70	SEP	(0.67 AUG/JUN)
D	0.70	OCT	(0.82 DEC)	1.11	SEP	(1.08 JUL)
E	0.52	FEB	(0.53 OCT)	0.74	DEC	(0.71 NOV)
K	0.36	JAN	(0.36 MAY)	0.71	SEP	(0.64 AUG)
J	0.30	MAR	(0.36 APR)	0.63	AUG	(0.52 MAY)
I	0.28	APR	(0.29 FEB)	0.67	JUL	(0.56 AUG)
M	0.27	MAR	(0.29 DEC)	0.64	JUL	(0.64 AUG)

The majority of stations show lowest average deviation in the cold season (December-April) and highest in the warm season (July-September). Some stations – as C, D, and E – are without a clear annual variation, but other stations have it so strongly that for the total group a clear annual variation emerges, when means for running quarters are formed (Fig. 33). This curve with maximum average deviation in maritime summer (July-September) is closely correlated with the seasonal distribution of ST change from 1951-60 to 1961-70 (Fig. 3). So it may perhaps be said: "Where and when variability increases, the possible power of a trend increases".

This annual variation of sea temperature variability (monthly means) is contrary to that of air temperature at land stations and Arctic stations. Kirch (1970) has computed standard deviations of monthly mean air temperatures for some Polar stations (period 1931-60). All clearly show a winter maximum and a summer minimum of variability. Now it has been demonstrated in Fig. 8 that the recent Polar cooling trend was by far greatest in winter. So we have also here the correlation between trend power and variability. Moreover, the strongest cooling trend developed in Franz Josef Land for which Kirch found the highest standard deviation of winter temperatures.

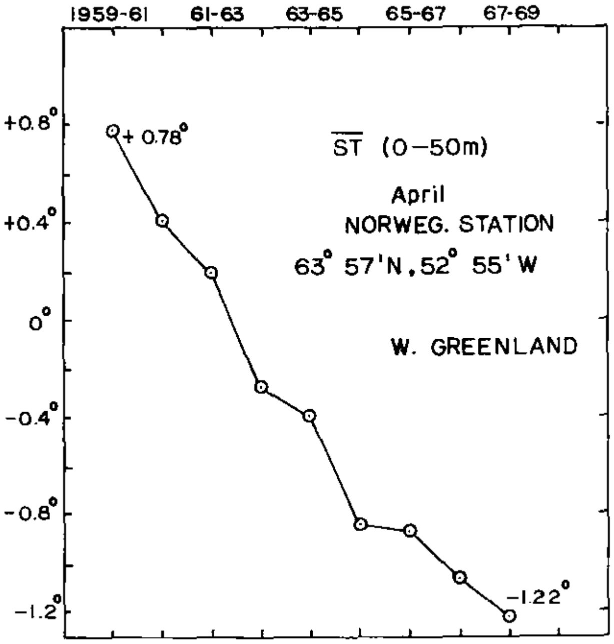

Fig. 30. Three-year running means of sea temperature in April for the 0- to 50-m layer at the Norwegian hydrographic station located at 63°57′N, 52°55′W off West Greenland.

From the appearance of its geographical distribution, the North Atlantic summer cooling (Fig. 4, lower part) might be due to a decrease in the incoming solar radiation, as a result of the air pollution from the industrial centres of eastern North America. For the coastal waters, however, where the cooling is greatest, a close correlation can be shown to exist between the ST change from 1951-60 to 1961-70 and the change of the offshore wind component from 1951-60 to 1961-70.

As a measure of this component, half the sum of the pressure differences between New York and Halifax (Shearwater), and Boston (Logan) and Sable Island has been used. For the sea temperature half the sum of the

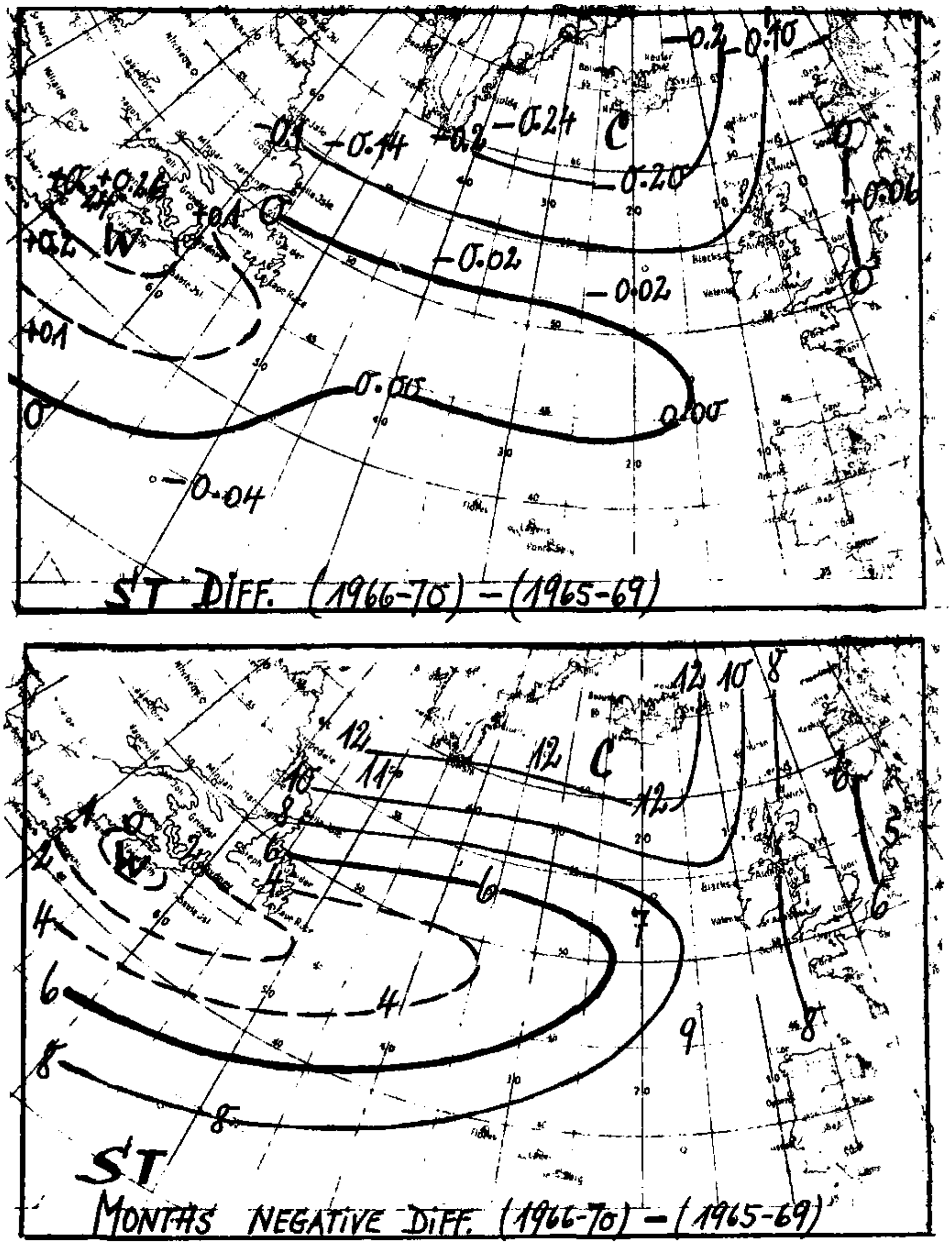

Fig. 31. Mean change of ST from the 5-year period 1965-69 to the 5-year period 1966-70 and number of months with ST decrease from 1965-69 to 1966-70.

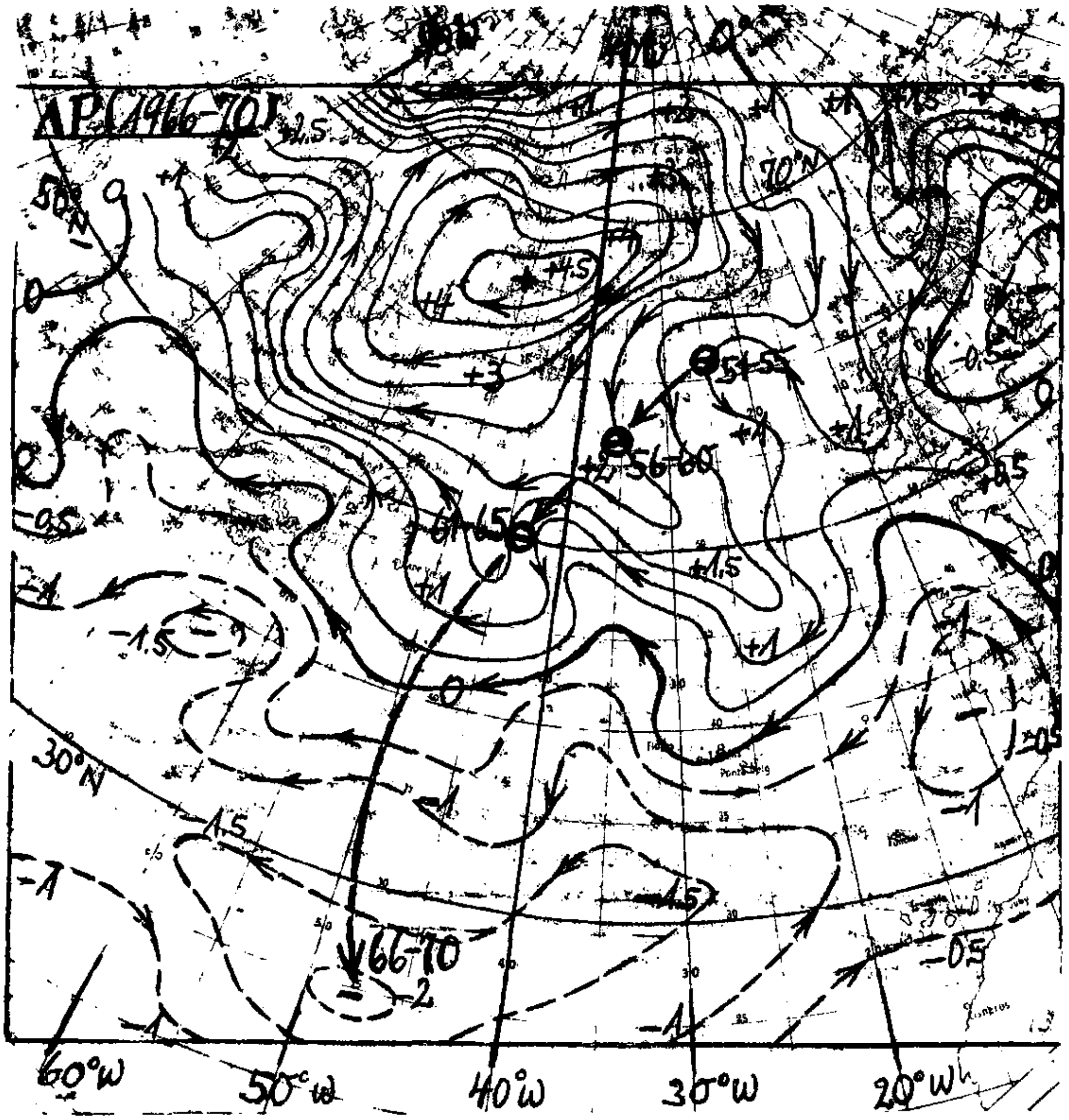

Fig. 32 Deviation of the 5-year mean atmospheric pressure for 1966-70 from normal (1900-39). Broken arrow line displacement of the centre of the main negative anomaly of the last 5-year periods.

ST differences at St. Andrews, New Brunswick and Boothbay Harbor, Maine, has been taken. Some further smoothing was done by forming mean values for running quarters. The result is shown in Fig. 34. In the same way as the sea temperature *decreased* throughout the seasons of the year, the pressure gradient - and so the offshore wind vector *increased* from 1951-60 to 1961 70, and a good parallelism exists between the two curves. The annual average change is an increase of the pressure difference by 1.26 mbar = *68%* of the normal mean pressure difference for both distances (1.85 mbar).

Now, as Fig. 31 demonstrated, a final warming of the coastal water occurred between the pentades 1965-69 and 1966-70. And it has been found in a counterproof that the combined pressure difference New York-Halifax/Boston-Sable Island *decreased* at the same time by 0.15 mbar (8% of normal).

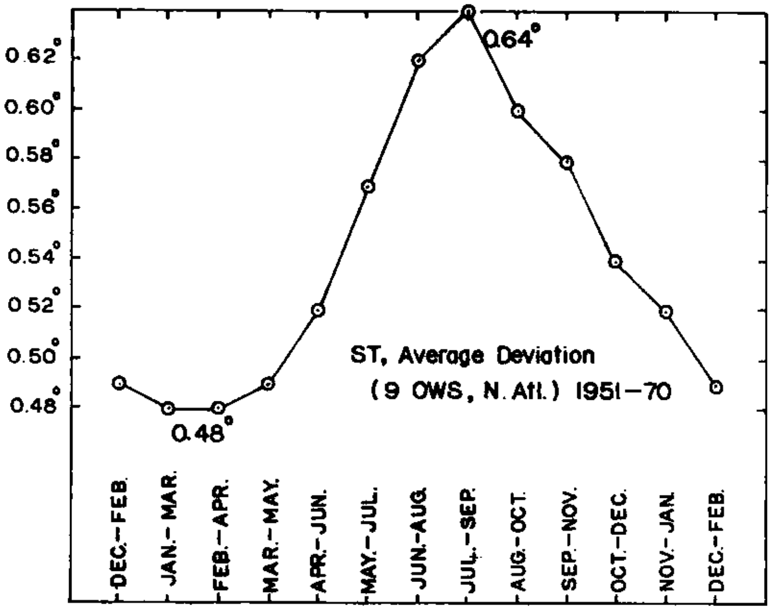

Fig. 33. Average deviation of ST monthly means for the nine North Atlantic OWS and for running quarters in the period 1951-70.

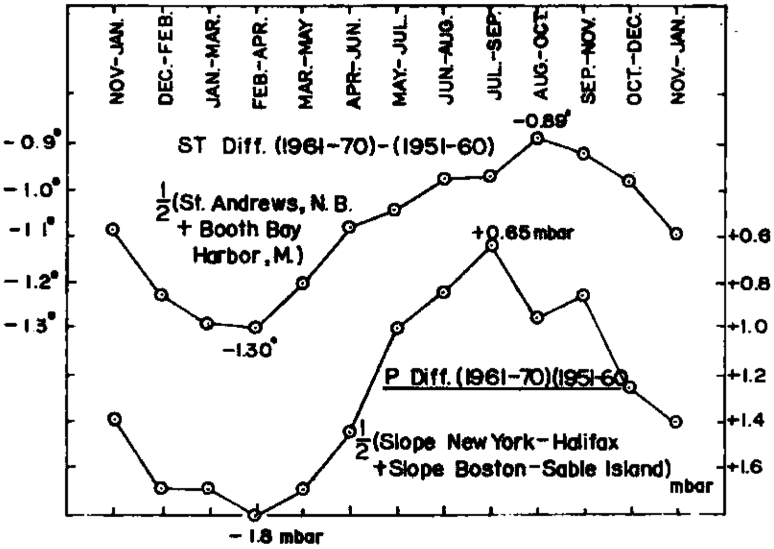

Fig. 34. Mean ST change between the decades 1951-60 and 1961-70 for the two stations St. Andrews, N.B. and Boothbay Harbor, Maine, and mean change of pressure differences for New York-Halifax (Shearwater) and Boston (Logan)-Sable Island between the decades 1951-60 and 1961-70.

Conclusions

Much sea temperature change seems to depend on the type and strength of atmospheric circulation – either directly on the winds or such accompanying factors, as air temperature, cloudiness etc. But conditions in the sea, horizontal as well as vertical gradients, are also important. In areas of crowded isotherms and during seasons of thermocline appearance, greater (long-term) variability may be expected, together with a tendency to stronger trends.

A direct connection between Atlantic cooling and Arctic cooling is not evident, but the common primary cause of both climatic phenomena may be seen in a recent decrease in the solar radiation input, if Budyko's (1968) finding is extrapolated for the last decade 1961-70. Such decrease in the solar energy received should influence the general atmospheric circulation in the direction of a southward expansion of the Polar vortex. This, indeed, is observed in the North Atlantic region: cyclonic activity has increased in lower (middle) latitudes and decreased in higher latitudes. This explains why the climatic cooling of the North Atlantic was, first, mainly in middle latitudes, whereas a warming took place in higher latitudes. In a second phase, the powerful Polar cooling now encroaches on the subpolar waters.

Since the further climatic outlook, based on oxygen isotope studies of the North Greenland ice sheet (Johnsen *et al.*, 1970), is for a continuation of the cooling trend for another one to two decades, future development may include the danger of a new "Little Ice Age". From the curve showing dominating periods of climatic development over the last 800 years, it would seem that 1970 is about 3 long periods of 181 years and 7 short periods of 78 years each away from 1425 which was the beginning of the "Little Ice Age" (about 1430-1850).

Stykkisholmur, North Iceland, has one of the longest records of atmospheric pressure, beginning in 1845. But 10-year moving averages of mean winter pressure have never, since the beginning of observations, reached such high values as in the late sixties (v. Rudloff, 1967). This may be more than a random result, and points to the particular climatic situation of present years.

Acknowledgements

Data for this study have been supplied by the Seewetteramt, Hamburg. Further support, consisting of sea temperature data, came from Messrs. John H. Hull, Biological Station, St. Andrews, New Brunswick (Fisheries Research Board of Canada), Walter R. Welch, Biological Laboratory, West Boothbay Harbor, Maine (National Marine Fisheries Service, NOAA, US Department of Commerce), and from Johan Blindheim, Institute of Marine Research, Bergen, Norway, who kindly delivered hydrographic data from cruises off West Greenland. I herewith express my sincere thanks for all this helpful assistance.

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Discussion

Dr. Walton: *"Dr Rodewald, is there similar information on stratospheric changes during the 1951-60 and 1961-70 decades? Have there been any changes observed in the exchange patterns between the stratosphere and the troposphere during these time periods?"*

Dr Rodewald passed the question to Dr Dickson.

Dr. Dickson: *"I do not have too much to add except that the southward shift of the zonal windstreams and depression tracks which were both described in recent years, and which have tended to continue, have, I think, been followed by a southward shift of the upper winds as well. Indeed up to the 1930's the upper winds tended to move north with the surface winds and pressure belts. But this is the limit of my knowledge on upper winds and does not refer to the stratosphere."*

Dr Walton: *"I ask the question because there appears to be some strange behaviour with reference to carbon dioxide and particularly natural radio carbon concentrations in tropospheric carbon dioxide. Changes in these concentrations appear to be related directly to meteorological parameters rather than solar cycle behaviour. If one has variations occurring in the lower atmosphere, I would have expected related changes in atmospheric behaviour to occur in the upper atmosphere. Have you any information on these other portions of the atmosphere?"*

Dr Dickson: *"No, I do not. My colleague, Mr Lamb, would be more competent to answer this question."*

Mr A. J. Lee: *"Dr Rodewald has been giving these wonderful papers year after year based entirely on weather ship data. What is happening to all the other sea surface temperature data? It is impossible to get them out of the world data centers and meteorological offices. Now, Dr Campbell is the Chairman of the Working Committee for IGOSS (Integrated Global Ocean Station System) which is charged with producing and processing this information for the scientific community. My plea to him is – Can something, please, be done about making sea surface temperature data available for analysis instead of it accumulating in the meteorological offices of the world?"*

Dr Campbell: *"Your remarks are timely. It has been extremely difficult to obtain these data. The World Meteorological Organization (WMO) is planning to publish sea surface temperature charts. I have heard this repeated over many years but have yet to see such a chart. The problem is an indication of what we should be striving for in IGOSS in order to provide more timely information either in a, real time or non-real time mode. Too little attention has been paid to the availability and importance of the data by concerned scientists. Hopefully sessions such as this will attract more specialists into the environmental fields."*

Mr Bailey: *"A description of the sea surface temperature is prepared each day at my Halifax office. These descriptions are available to anyone on request. At present they are being distributed widely as far as Europe and the west coast. They are based on meteorological data from ships. Also, Ocean Weather Station DELTA, located where it is on the eastern part of the Grand Banks, is perhaps analogous to Dr. Løken's measurements of the tip of a glacier. It is located between the warm and cold waters so that you really do not know whether there has been an expansion of the cold water or a decrease of the warm water. In other words, the measurements may not be as meaningful as those from some other stations situated in the center of a water mass."*

Contribution Number 2

No Discussion

A Review of Recent Hydrometeorological Events in the North Atlantic Sector

By R. R. Dickson[1] and H. H. Lamb[2]

Abstract

This review seeks to describe the principal hydro-meteorological events in our sector during the present century, but with special emphasis on events of the last two decades.

The intensification of the westerlies to the mid-twenties and their subsequent weakening are discussed in terms of changes in the strength of the direct solar beam and of terrestrial volcanic activity (dust veil effect). This circulation intensification and the more prolonged northward migration of wind and pressure belts (to the mid-thirties) are briefly related to the observed hydrographic events of this period.

The accelerated weakening of the westerlies after the 1930's and the associated southward shift of windstreams and pressure belts are described. The increasingly meridional circulation of this period has been characterized by the development of an increased northerly airflow in two main zones – the European Arctic and Sub-arctic, and the Canadian Maritimes and New England – with consequent deterioration of both the atmospheric and marine climates since the early fifties. In the latter zone this development is most clearly reflected in the steep decline observed in air and sea temperatures. In the former zone, a general cooling set in over almost the entire Arctic cap after the early forties; the establishment of a strong pressure-anomaly ridge over Greenland in the early fifties and the later intensification of this cell brought about a progressive increase in the frequency of northerly outbreaks over the Norwegian-Greenland Sea, especially in the winter half-year, leading to a quasi-linear decline in mean winter air temperature across a broad band from the high Arctic to north Iceland. An increased southward transport of pack-ice and polar water from the Arctic Basin and (in the most recent years) an active ice-formation off North Iceland have resulted in a record extent of sea-ice and a south-eastward shift of the oceanic polar front. There is some evidence of increased bottom water formation in the Norwegian-Greenland Sea. Data from the continuous plankton recorder surveys (Oceanographic Laboratory, Edinburgh) show a delay in the initiation and development of the vernal phytoplankton outburst at the southern approaches to the Norwegian Sea, and other changes in the distribution and abundance of marine organisms can be recognized. By contrast an amelioration of the marine climate off West Greenland has until recently been taking place; the factor responsible appears to have been a narrow outlying ridge running southeastward from the main high pressure anomaly cell at Greenland, thus giving rise to an anomaly wind running in an anticlockwise sense over the eastern Atlantic to the Irminger Sea and boasting the transport of the Irminger Current. Since 1968 however, the factors responsible for this warming at Greenland appear to have ceased and there is now an active deterioration of the marine climate at Greenland.

Introduction

In the first half of this century, the most conspicuous climatic event in the northern hemisphere was the continuation and eventual culmination of the trend towards high-latitude warming (Ahlmann 1949*b*; Willett 1950). This long-sustained warming, which Mitchell (1963) shows to have been especially pronounced in the European arctic and subarctic regions, was linked by Defant (1924), Wagner (1939), and

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Scherhag (1950) with an increase in the strength of the general circulation of the atmosphere.

Initially, however, scientists were cautious in their acceptance even of such striking trends as the strengthening of the circulation and the warming of the global atmosphere. As late as 1948 the caution is obvious in the following remark from Ahlmann's paper delivered at the ICES Special Scientific Meeting on Climatic Change: "Prof. Keranen says that it is now no longer possible to deny that the climate is improving, even though this is contrary to the thesis of the old meteorological school concerning the stability of the present climate" (Ahlmann 1949*a*, p. 11).

In fact, the idea of an intensifying circulation did not become fully established until long after the period of maximum atmospheric vigour had passed, with the result that the "increase in the general circulation of the atmosphere" is invoked in the literature to explain a remarkable variety of events, covering a remarkable range of years.

It is partly for this reason, and partly to set recent events in their historic context, that we begin with a brief review of the major hydrometeorological events during the first half of this century.

In the North Atlantic sector the principal events were the striking inter-decadal changes in the strength and position of the prevailing westerly windstream. The surface and upper-air zonal circulation indices of Lamb (1965), Mironovitch (1960), Girs (1963), and Trenkle (1956 and *personal communication*) all indicate a steady strengthening of the westerly windstreams over the Atlantic sector of the northern hemisphere until the mid-1920's (Figs. 1-3) and the indications are (from the early evidence of cyclone and anticyclone tracks) that this increased zonality was accompanied by a contraction of the circumpolar vortex accompanied by a lengthening of wavelengths in the upper westerlies and by a northward shift of the upper-air zonal wind.

This increase in zonality to the mid-'twenties and the subsequent institution of a trend towards increasing meridionality was not merely a 'local' North Atlantic event. As Lamb (1964*b*, p. 497) observes: "We find that the average circulation (including all the main windstreams of the world – e.g. North Atlantic westerlies, trade winds, southern hemisphere westerlies, and the monsoon currents of south and east Asia) was intensifying, attaining a maximum strength about 1920 ± 15 years and since shows signs of decreasing. Along with this the strength and breadth of the subtropical anticyclone belt waxed and has begun to wane".

As regards the cause, Lamb (1964*a*, p. 8) notes that: "It is hard to avoid the suggestion that the world-wide access of energy to the general circulation from the 18th century (or earlier) up to this century, amounting to 1 or 2 per cent, implied an increase in the supply of solar energy effectively available". With only one observatory in the 1880's and only three or four before the 1900's, we lack direct observational evidence of this change, yet even if we assume that no change took place in the strength of the direct solar beam itself, we may assume that some such increase in available solar energy took place from the 1820's to the 1920's; the

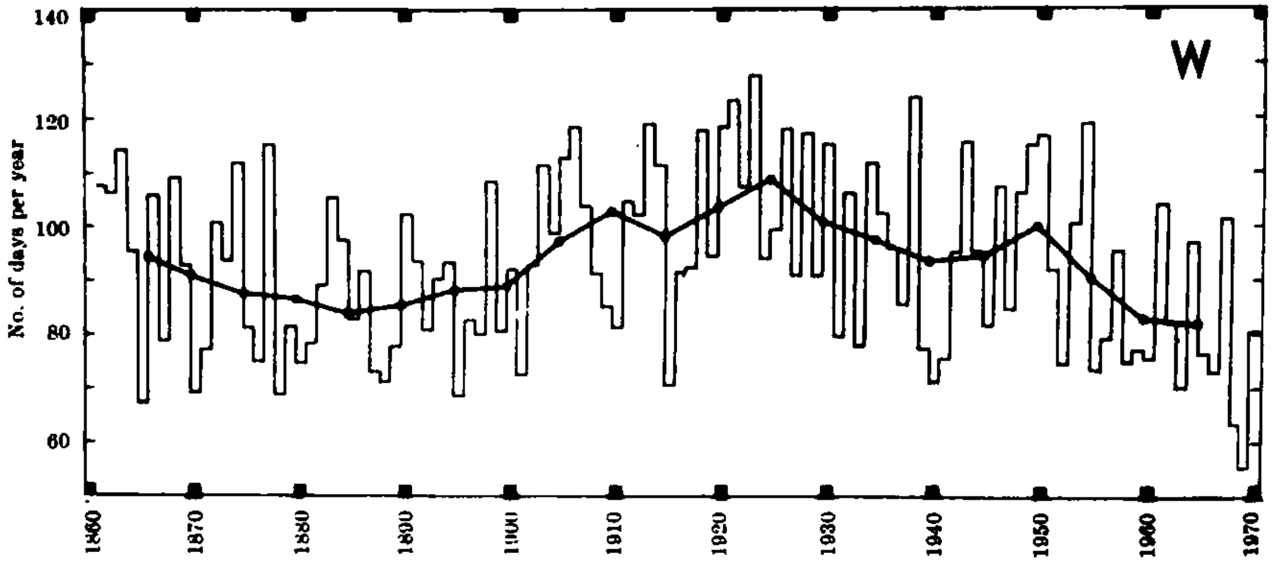

Fig 1 Number of days per year of general westerly type over the British Isles (1861 to 1970) • — •, 10- year means, plotted at 5-year intervals.

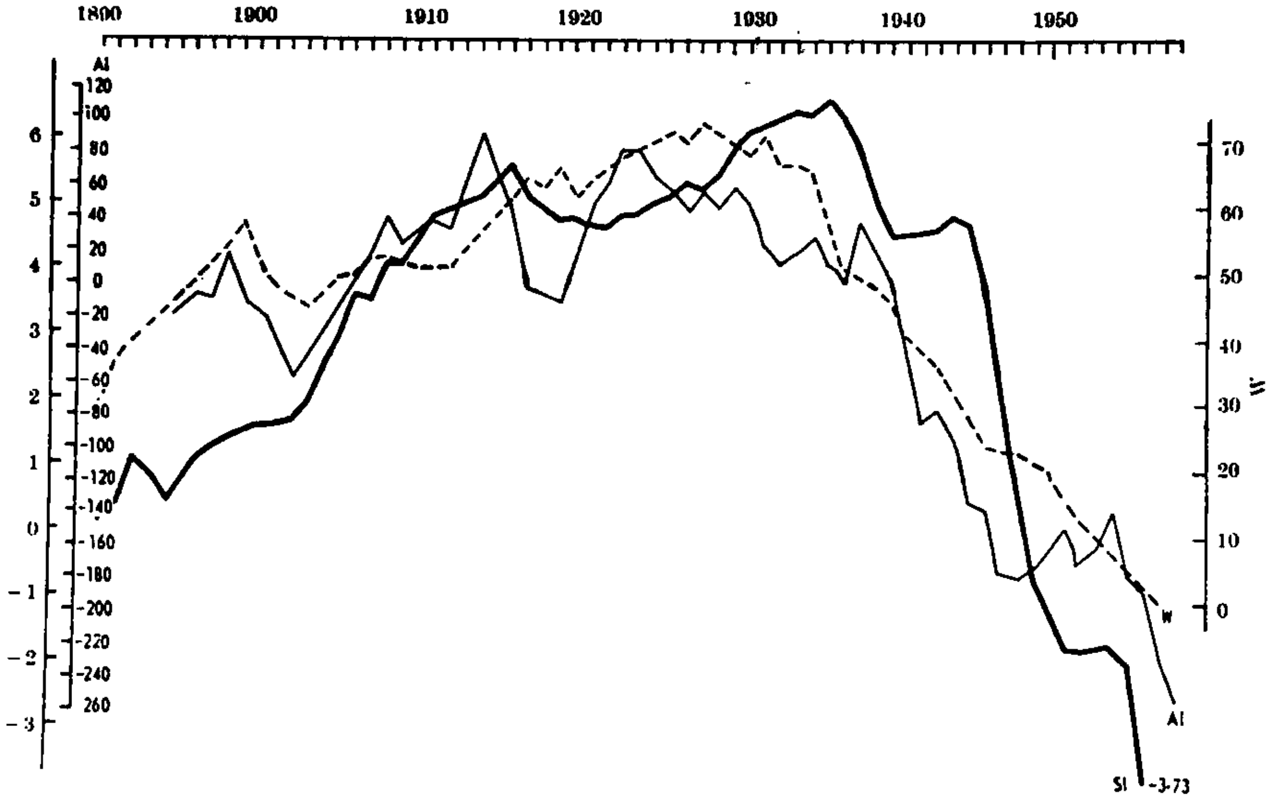

Fig. 2. Baur's solar index (SI) and prevalence of the middle latitudes westerlies (after Mironvitch). Baur's solar index (SI) expressed by $\frac{F}{F} : \frac{D}{D}$ where F is the area of faculae in millionth parts of that half of the sun's surface seen from the earth and F is its average value 1874-1952 D is the area of the sun's face dimmed by spots and $\bar{D}$ the overall average value of D. (The measurements of F are due to Baur from the daily sun photographs of the Royal Greenwich Observatory) ________, Atlantic zonal circulation index (AI): annual average values of the Azores Iceland pressure difference expressed in percentage of its overall average 1894-1952 (16.1 mb) ... ., Yearly frequency of the general westerly (W) zonal type over the northern hemisphere, defined in terms of 500 mb flow by Girs (for example, 1963).

former period was characterized by the greatest volcanic dust veils in the whole 400 year period covered by our dust veil estimates (Fig. 4a) while the volcanic activity of the 20th century has been negligible in comparison (Lamb 1970). The rather sensitive long-term relationship between dust veil index and polar ice extent (Fig. 4a and b) is at least partly explained by the fact that depletion of the incoming solar beam by volcanic dust (and hence surface cooling) is maximal at high latitudes; the stratosphere (characterized by slow vertical transfer and exchange of air) extends closer to the earth's surface, giving longer residence (fall out) times for stratospheric dust at high latitudes, the net poleward circulation in the lower stratosphere results in a concentration of the atmospheric volcanic dust-load near the poles, and the maximum obliquity of the sun's rays at high latitudes means not only that incoming radiation at the poles will pass through a maximum thickness of any dust load, but also that forward scattering (which greatly reduces the effectiveness of a reduction in the direct solar beam) is minimal (Lamb 1970, p. 441-442, 460-469).

As a converse to the above, one would assume that the post-1920's decline in atmospheric vigour was accompanied by a *reduction* in available solar energy; in this case we know that volcanic dust was not responsible for such a change, but we have direct observational evidence that such a change did take place. From actinometric measurements of the strength of the direct solar beam on clear days from the middle latitudes of the northern hemisphere (30°-60°N, observations from America, Europe, North Africa, Asia, and Japan), Budyko (1968) shows that a decline in incoming solar radiation did take place after the 1930's, and this decline is also discussed by Pivovarova (1968). These authors however are rather bold in assuming no change in the solar constant and in attributing the decline almost solely to an increase in atmospheric pollution. Some such increase in atmospheric turbidity due to man's industrial and agricultural activity has certainly occurred (documented in Glover, Robinson, and Colebrook 1970), but as Lamb (1969, p. 1232) has pointed out, although an increase in man-made dust can be presumed to cause some increase in the earth's albedo, and hence a loss of some solar energy received, "the case is unlike that of volcanic particles in that the dust is maintained in the lower atmosphere. Any radiation the dust intercepts can therefore heat the lower atmosphere much as if it had penetrated to the earth's surface, though not quite at the same level". Further, Mironovitch (1960) has undermined the initial assumption of Budyko and Pivovarova in showing (by means of Baur's Solar Index) that the waning of the westerlies was, in fact, accompanied by a progressive dimming of the solar disc and the effect of this latter event cannot be ignored. Confirmatory evidence supporting a 'natural' basic cause for the recent turnabout in westerly wind strength is given by Lamb (1969) in showing at least two precedents for the present situation (200 and 400 years ago) which quite obviously cannot be explained as the results of man's activity.

Turning briefly to the principal hydrographic events which accompanied this protracted change in the westerly circulation, Bjerknes (1963) has shown that between the 1890's and the 1920's the parallel intensification of the Iceland Low and the Azores High led to an enhanced north-south thermal gradient in the Atlantic (Fig. 5). Between 50° and 60°N, a general sea surface cooling of up to 1°C resulted from the increased loss of sensible and latent heat to the atmosphere under the intensifying westerly airstream: locally the cooling was most severe off South Greenland where an intense Iceland Low lying in a relatively northern position can be held responsible for boosting the East Greenland Current in the manner described by Lee (1962) and enhanced surface-water divergence and upwelling must also have augmented the cooling under the Iceland Low itself. To the south of 50°N, Bjerknes shows a zone of sea surface warming with core temperatures of over +2°C at 43°N.

Lamb and Ratcliffe (1969) point out that this steepening of the north-south thermal gradient up to the 1920's must itself have produced a strengthening and (equally important) a northward displacement of the upper and surface westerlies, and such a change is evident in Fig. 6. While the subtropical anticyclone showed no systematic latitudinal displacement with time, the subpolar minimum pressure belt (including the Iceland Low) tended in general to migrate northward as the westerlies intensified and to retract southwards as the westerlies weakened (data from Lamb and Johnson 1966).

By the pentade 1930-34, while the Azores anticyclone had maintained its latitudinal position, the subpolar low had advanced (on average) further north

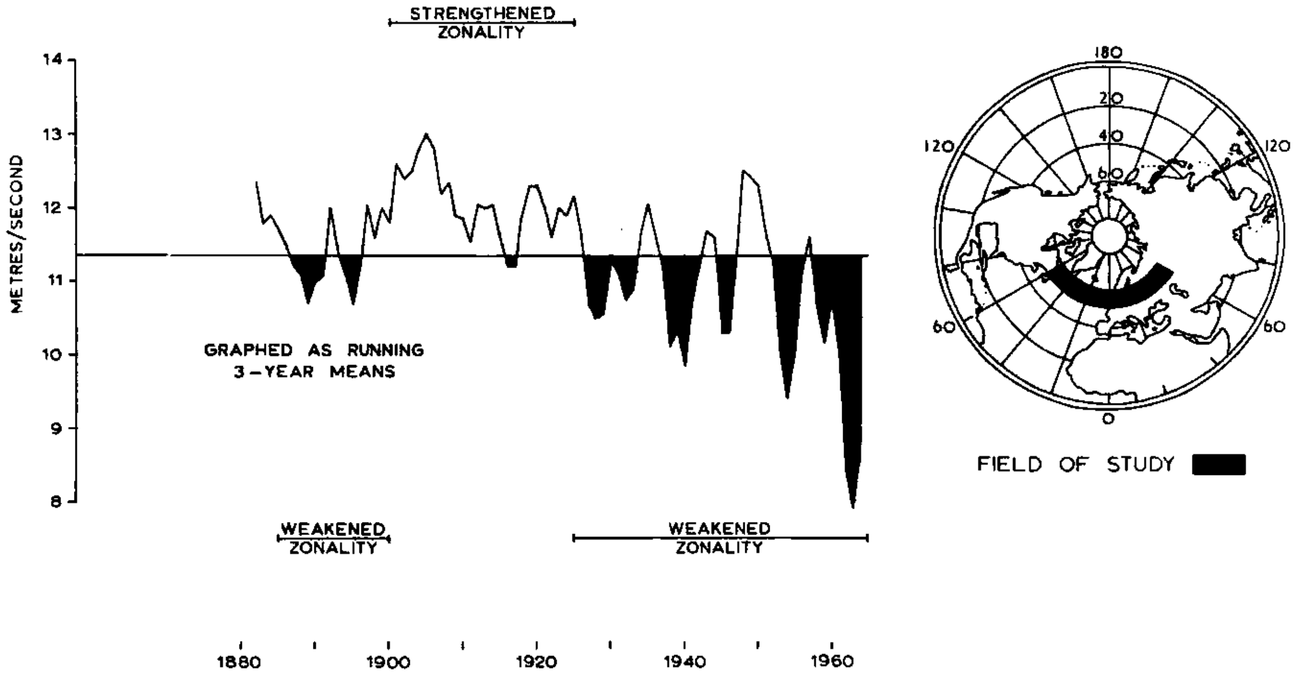

Fig. 3. Zonal component of the Geostrophic wind at 500 mb level (50°-60°N, 60°W-60°E) for each winter (Dec., Jan., Feb.) between 1881 and 1965 (after Trenkle).

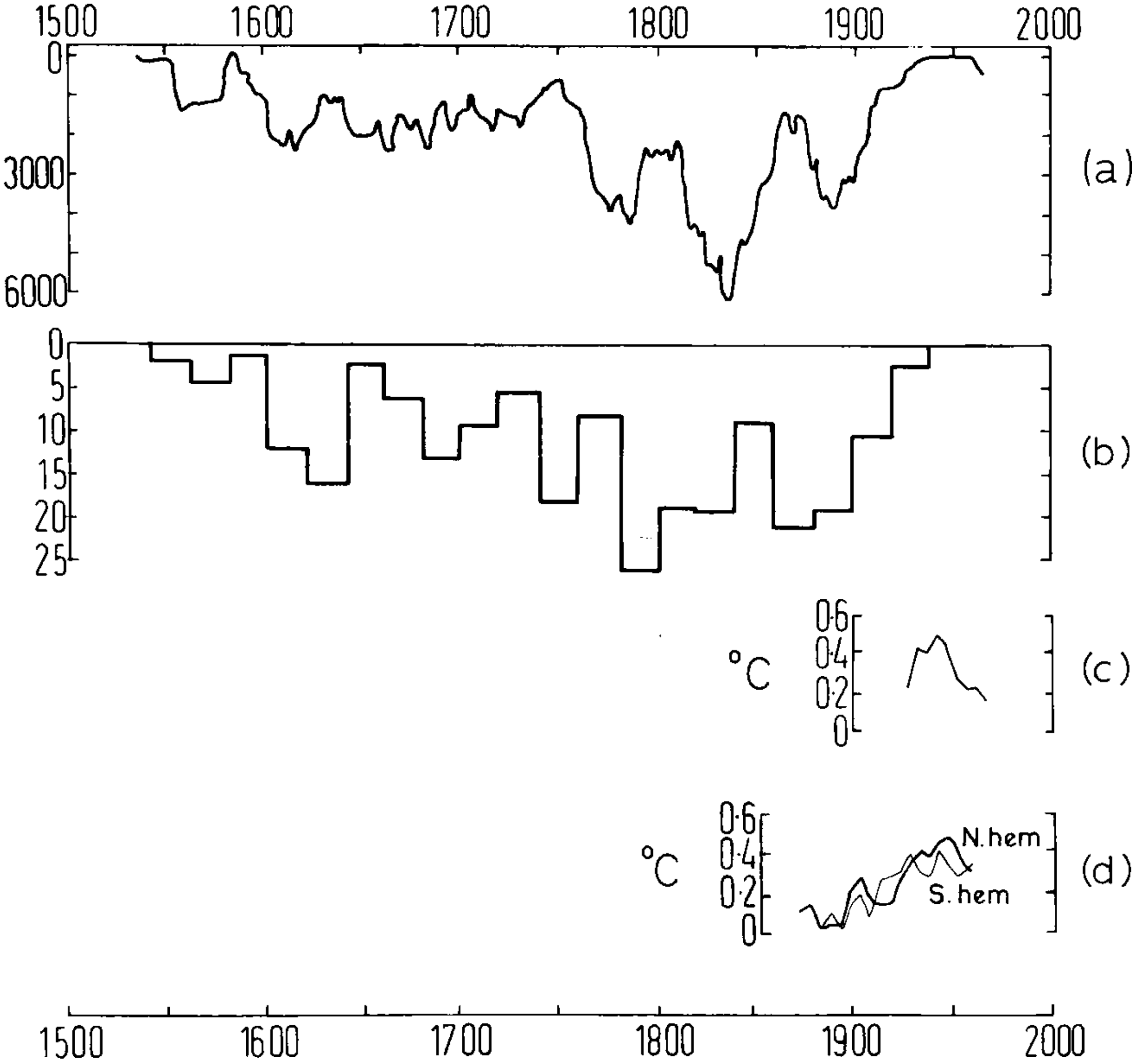

Fig. 4.(a) Twenty-five year cumulative values of dust veil index for the northern hemisphere (graph inverted). From Lamb 1970.

(b) Twenty year mean values of the number of weeks per year in which ice was reported at the Iceland coast (after Koch 1945).

(c) Global temperature trend. After Furukawa (in Anonymous 1970).

(d) Five year mean values of northern and southern hemisphere temperature (from Mitchell 1963).

than at any other time in this century, and the wind-spun ocean gyre can be assumed to have increased its diameter in response. Further, by the early thirties, a noticeable weakening of the surface westerlies had occurred which, by the converse of Iselin's (1940) theory, should have increased the northeastward mass discharge of warm water from this gyre. These two factors (principally the former) are held responsible for the invasion of the northeastern and northern Atlantic by a watermass of extreme southerliness of source area during the decade of the 1930's. At the Faroe-Shetland Channel this super-advection of southern water increased "from small beginnings in 1930-31, showed maximum concentration in 1933-34 and thereafter waned to extinction in 1938-39" (Tait 1955, p. 482). Salinities in this area reached unprecedented levels (in some cases exceeding 36.00°/oo), but high values were so consistently maintained that observations initially discarded as being too high were later re-included in the record. A scattering of contemporary observations from, for example, the North Sea (Dickson 1967, 1971), Norwegian Sea (Helland-Hansen 1934), Norwegian coast (Saelen 1967), and Baltic (e.g. Segerstrale 1965) show that the major and minor branches of the Atlantic current system in the eastern Atlantic were similarly characterized by high-salinity conditions. We have also some grounds for believing that this saline advective stream passed along the track of the Irminger Atlantic Current to the waters off Greenland. Referring to a hydrographic section running south-west from Cape Farewell, Harvey (1962, p. 17) draws our attention to the following interesting fact: "Apart from 1933 *when the salinity observations were thought to be in error* (our italics), the highest salinity recorded by the US Coastguard in any of their 22 observations along this section between 1928 and 1959 was a value of 35.07°/oo observed in both August 1931 and July 1934".

We must assume that the sharp onset of high-latitude cooling around 1940 is a reflection of the fact that the zonal windbelts and depression tracks had shifted southwards after the mid-thirties. Figure 4c and d, and Mitchell's (1963) figure 5c (not shown), illustrate both the mean trend and the geographical distribution of the cooling until the late fifties, while Table 1 illustrates the continuation and intensification of cooling over the Arctic cap in later years.

Although the cooling appears from the above examples to have been hemispheric or even global in

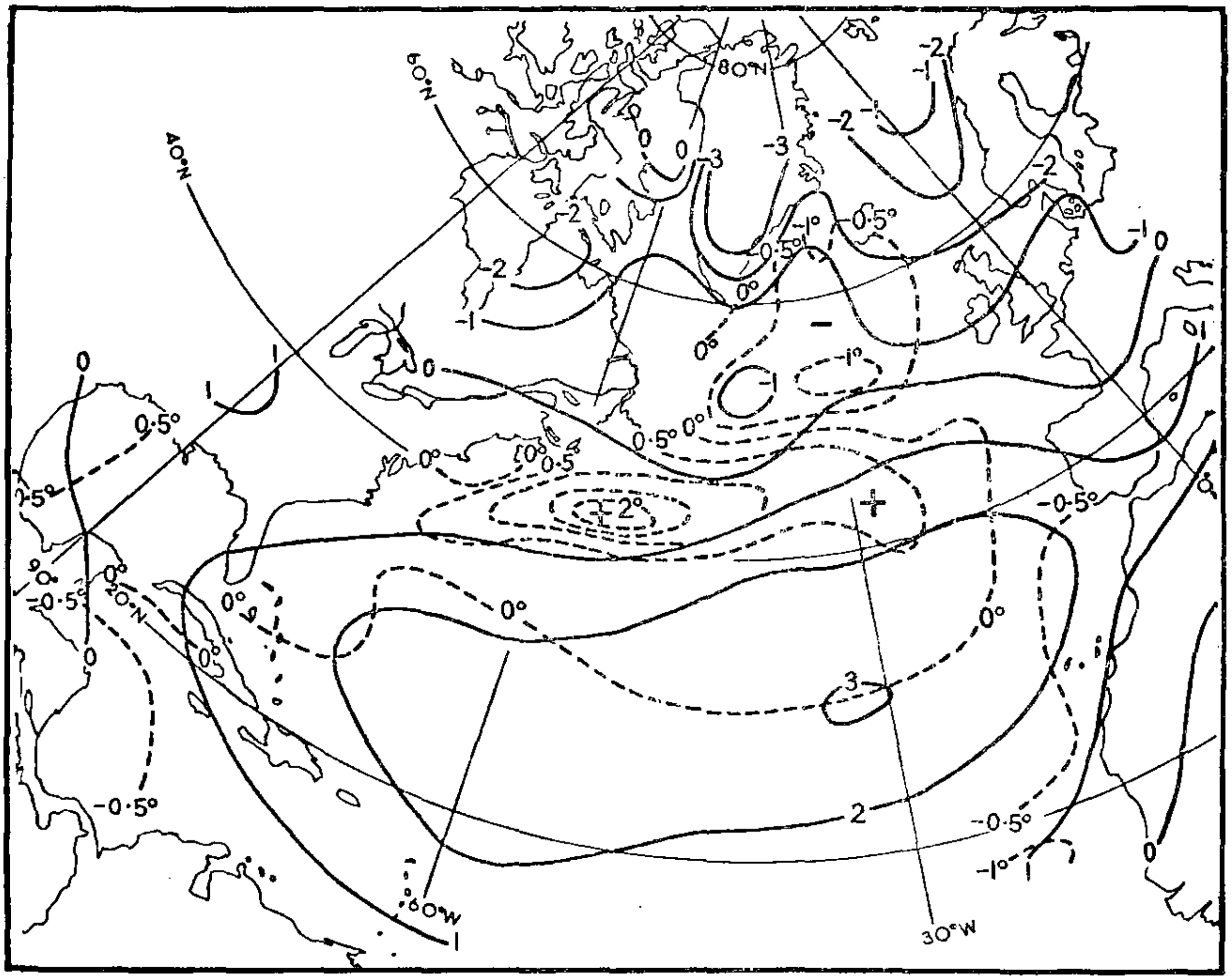

Fig. 5. Changes of annual mean barometric pressure in mb (solid lines) and of sea surface temperature in °C (broken lines) from 1894-1898 to 1920-1924 (after Bjerknes 1963).

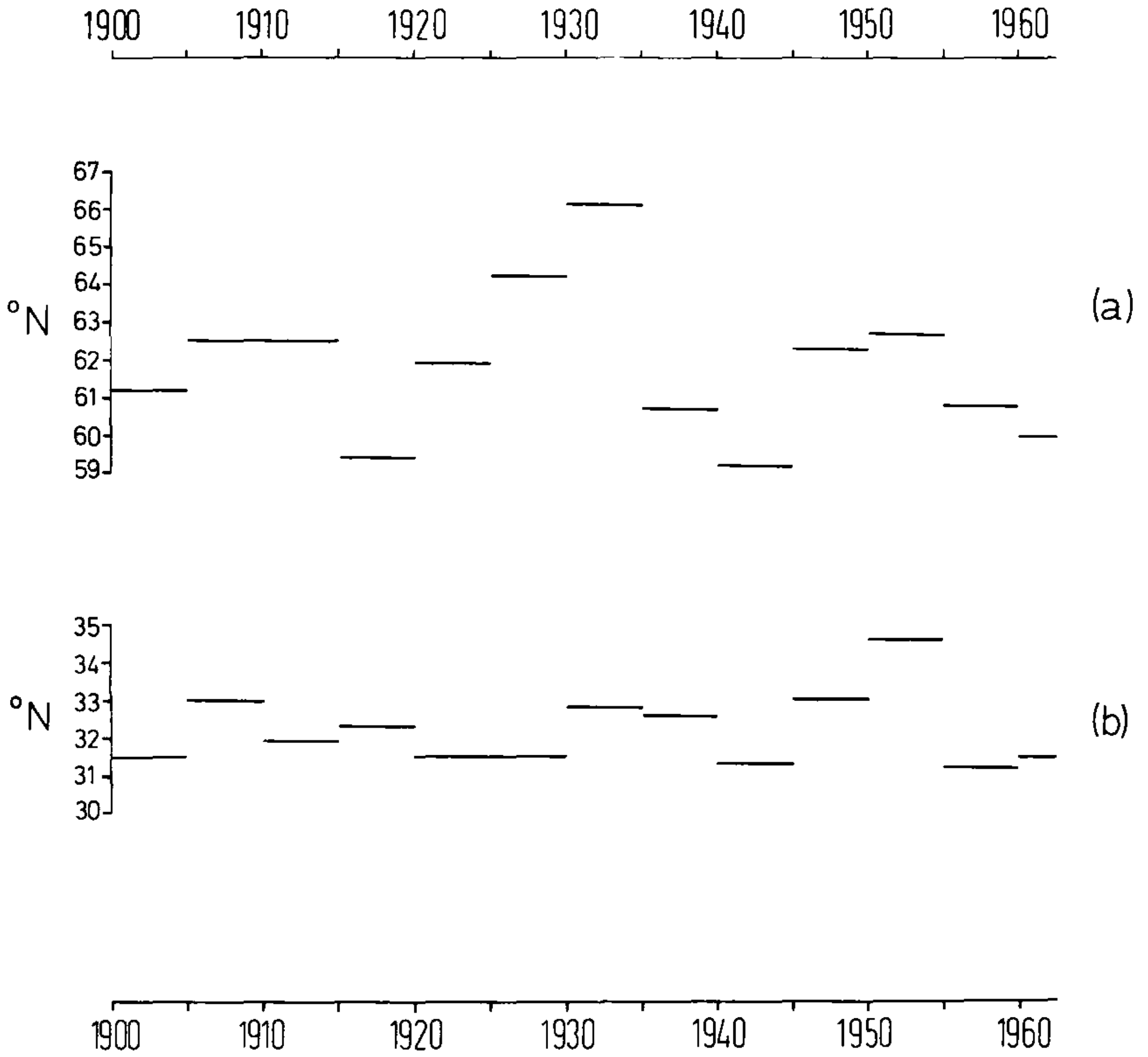

Fig. 6. The January-July average latitude of (a) the Subpolar Low at 40°, 30°, 20°, and 10°W; (b) the Subtropical High at 60°, 50°, 40°, 30°, and 20°W meaned over 5 year periods.

extent, this is to some extent an oversimplification since the cooling has in fact been concentrated along certain narrow zones or 'corridors'. This is, of course, to be expected since the cooling is not simply a result of a southward shift of the axes of zonal windstreams and depression tracks, but is also influenced by the increasingly direct latitudinal exchange of airmasses which the changeover from zonality to meridionality entailed.

Initially, between 1941 and 1945, this high-latitude cooling on a hemispheric scale was reflected in a sharp local decline in sea temperatures along the Norwegian and Murman coasts (Beverton and Lee 1965), but later, in the late forties, a sharp increase in southerly meridionality over the eastern Atlantic (Fig. 7, from unpublished data by Murray and Lewis) appears to have temporarily increased the northward heat transport of the Atlantic current in the Norwegian Sea (in the manner described by Hill and Lee 1958; Lee 1962), so that sea temperatures rose to a new peak and did not rejoin the general deteriorating trend of hemispheric air temperatures until after 1950. For the subsequent two decades the climatic trends of the entire North Atlantic sector are well summarized and explained by Rodewald's important map of surface-pressure change between the two periods 1900-39 and 1956-65 (Fig. 8, from Rodewald 1967). From this, as well as from other evidence, we can delimit two main areas of climatic deterioration and one of limited amelioration. In the remainder of this paper, it is intended to describe in detail the recent climatic events in these three areas, and to list their likely physical and biological implications; this listing is not intended to be comprehensive, but will serve merely to illustrate the various types of effect that have resulted. Secondly, it is important to note that since this review of events derives mainly from the published literature the most recent trends of change (after the late 1960's) will not be covered. The paper of Dr Rodewald (this volume) will, however, bring these events up to date and will show that some reversal of the trends we will describe has occurred in the most recent years.

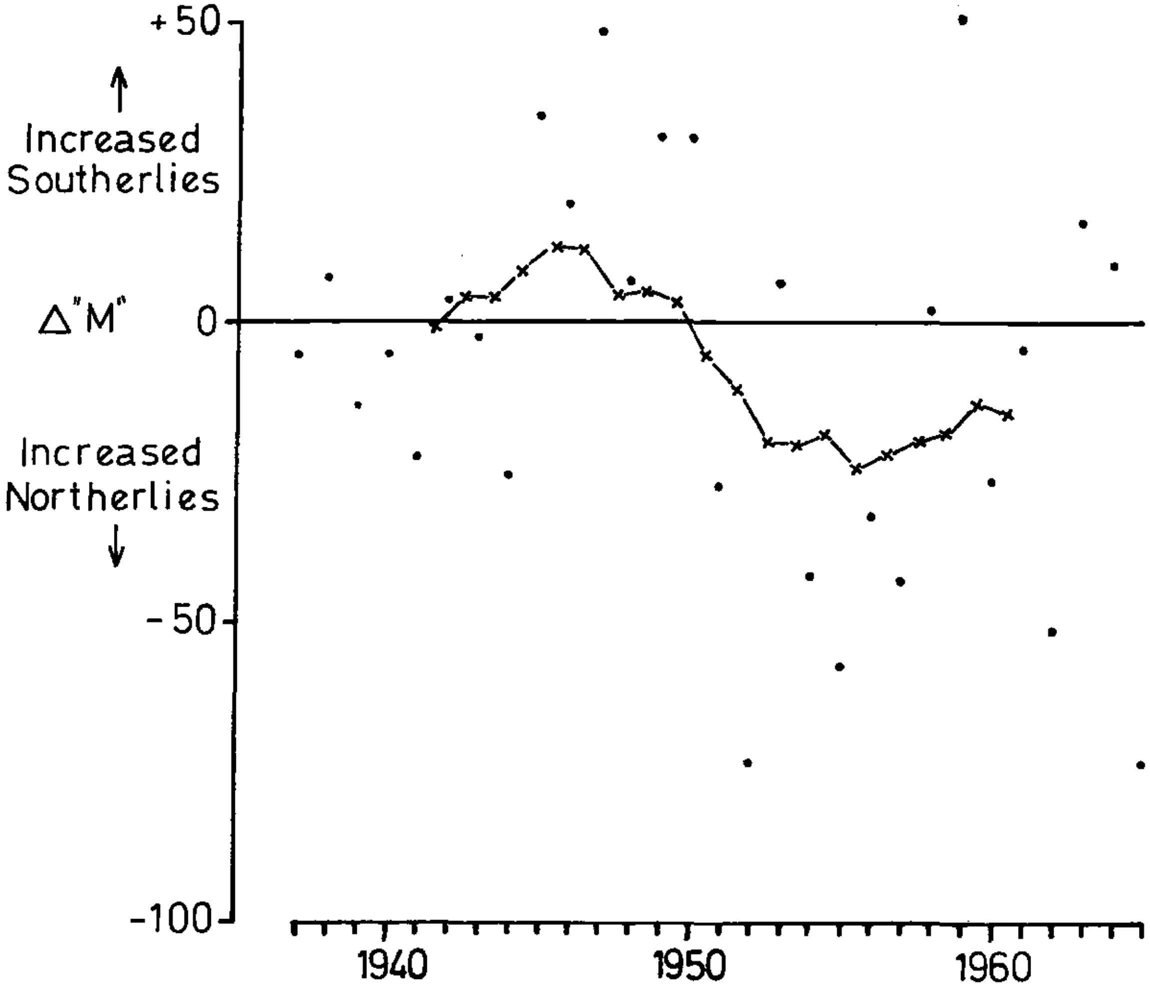

Fig. 7. Anomalies of the annual algebraic sum of meridional index for the British Isles in each year 1937-65, together with 10 year running means. Unpublished data kindly supplied by Murray and Lewis, Meteorological Office, Bracknell. ('Normal' period 1900-65)

Daily meridional index scores were calculated from the daily weather type over Britain as follows:

Synoptic weather type	Daily 'M' score
Northerly	- 2
Northwesterly, northeasterly	- 1
Non-directional, westerly, easterly	0
Southwesterly, southeasterly	1
Southerly	2

Annual values were obtained by summing the daily 'M' scores.

TABLE 1. Months with positive (+) or negative (–) air temperature anomaly over most of the Arctic north of 70°N (cf. normal 1931-60 where possible). Gaps in the table represent months when the chart coverage was inadequate to supply an estimate; o/+ (or o/-) indicates that only a part of the area showed a positive (or negative) anomaly of temperature, the remainder showing no change.

	Months												Number per year	
Year	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	+	–
1955				+						–	–	–		
1956	+	o	+	+	–	o	o	o	o	–		+	4	2
1957	+	+	o	+			o	+	–	o	–	–	4	3
1958	+	o	–	–	–	–	o	o	o	–	–	–	1	6 or 7
1959	+	+	–	–	–	o	+	+	o	+	+	+	7	3
1960	+	–	o	–	+	+	o	–	+	–	–	o	4	5
1961	o	–	–	–	–	o	–	–	–	o	+	–	1	8
1962	o	–	o	o	–	o	–	o	–	–	–	o	0	6
1963	·	–	–	o	o	–	–	o/-	–	–	–	o/-	0	9
1964	–	–	o/-	–	–	–		o/+	–	–	–	–	0	11
1965	–	–	o/+	o	–	–	o/-	o	–	–	+	–	1 or 2	7 or 8
1966	–	–	–	–	o/-	–	–	o/-	–	–	o/-	+	1	8 to 11
1967	–	–	o	o/+	+	–	–	–	o/-	o/-	–	–	1 or 2	7 to 9
1968	–	–	o/-	–	–	o/-	–	–	–	–	–	–	0	10 to 12
1969	o	–	–	–	o	–	–	o	o	+	–	+	2	6
1970	o	–	o/+	–	o/-	–	o	–	–	–	–	–	0 to 1	8 to 9
1971	–	o/-	·											

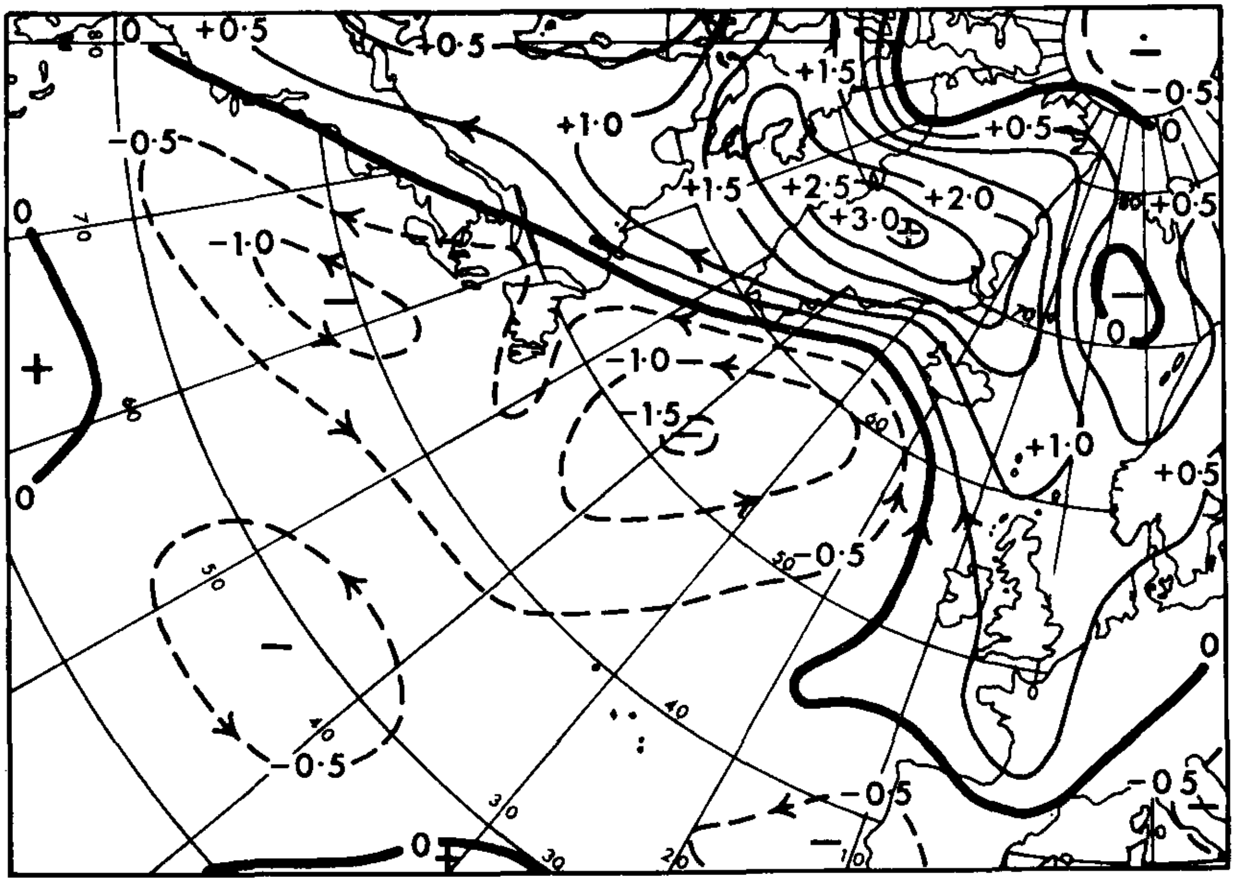

Fig. 8. Mean pressure deviation (mb) from normal for the period 1956-65 (normal period 1930-39). Arrows indicate the sense of anomaly circulation (from Rodewald 1967).

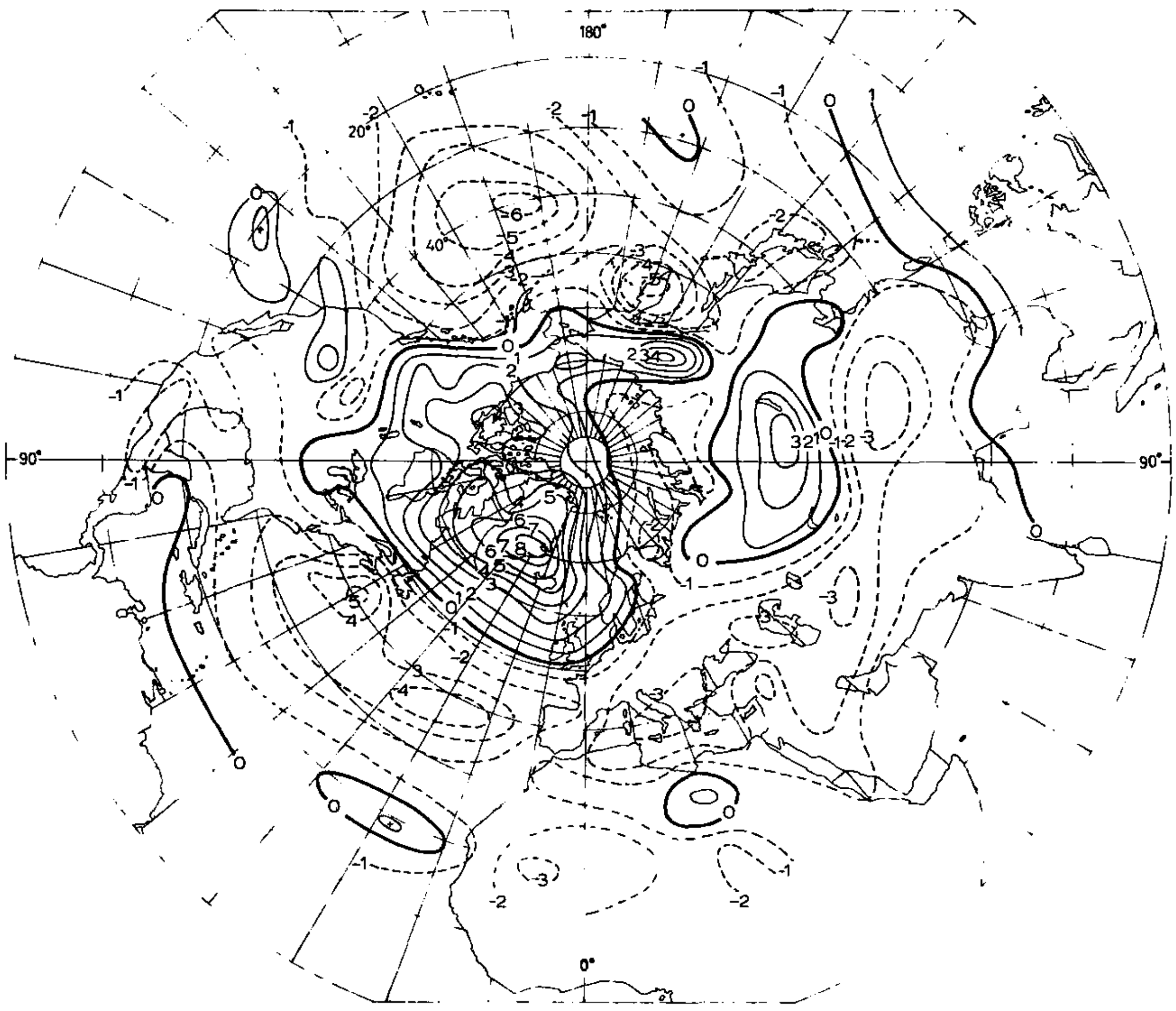

Fig. 9. Change of surface pressure (mb) between 1900-39 and 1951-66 (January).

Climatic Deterioration Over the Norwegian-Greenland Sea

Since the late 1940's, direct northerly outbreaks of increasing frequency have swept the Norwegian-Greenland Sea, adding almost every year to the severity of climate in areas as far south as the British Isles. The meridionality index of Murray and Lewis, for example, shows that by the mid-1950's the occurrence of this weather type in Britain was more frequent than at any other time in this century (Fig. 7; see also the tables of weather-type frequency in Lamb 1965). Lamb (1965, p. 10) ascribed this increasing northerliness to "a remarkable increase in the average difference of pressure between Greenland and the eastern Norwegian Sea: this has increased since 1950 in every month of the year, and the annual mean value of the difference between 40° and 0°W at 70°N for the 1950's decade was 4.2 mb compared with 1.2 mb for 1900-39". To some extent these mean figures mask the full extent of the change towards increased northerlies in this sector since we have evidence not only that this west-east pressure gradient over the Norwegian-Greenland Sea has continued to steepen during the 1960's, but also that the greatest steepening has occurred during the winter months, the anomaly circulations for other seasons being rather weak. Monthly charts of hemispheric pressure change between the two periods 1900-39 and 1951-66 have recently been prepared by the Climatology Division of the Meteorological Office, Bracknell, and these show the pressure difference between Greenland and Scandinavia to have been greatest from November to March with a general maximum in January (Fig. 9). These charts, and Rodewald's chart (Fig. 8), make it clear that the establishment of this pressure gradient has been almost solely due to the build-up of a strong anomaly ridge over Greenland, the negative pressure-anomaly cell over the eastern Norwegian Sea being a much weaker feature.

The physical repercussions of the increase in northerlies induced by these two cells have been widespread. Firstly, the disproportionately great boosting of northerlies during the winter months is reflected in a steep decline in mean winter air temperature throughout this sector (data from Anonymous 1951-69), especially in those areas lying to the north and west of the atmospheric and oceanic polar fronts (Fig. 10). The total winter[3] cooling, as described by the regression lines in Fig. 10, amounts to 5.54°C at Isfjord (Spitsbergen), 5.60°C at Bear Island and 4.06°C at Jan Mayen from the winter of 1949-50 to that of 1967-68. In contrast to this quasi-linear decline of mean winter air temperature, mean air temperature for the spring (March, April, May) and autumn (September, October, November) at these three stations have shown no clear linearity of trend since 1950, although some cooling has certainly occurred since around 1960.

An important implication of this extensive winter cooling lies in the fact that air temperature (acting in conjunction with windspeed, and to a lesser extent, with sea temperature) constitutes an important factor in determining the incidence and severity of the 'icing' hazard to fishing vessels. For this reason, a programme for the computer-filtering of original meteorological data to distinguish 'icing' situations of specified length and severity is at present in the course of preparation at the Lowestoft laboratory. This approach should permit the quantitative estimation of any recent change in the 'icing' hazard in northern seas.

To the south of Jan Mayen, the decline in mean winter air temperature during this period becomes rapidly less marked, and amounts to only 0.5°C at north-east Iceland (0.46°C at Grimsey Island; 0.66°C at Raufarhöfn, Fig. 10). The reason for this appears to concern the outlying synoptic ridge running south-eastwards from the main high pressure-anomaly cell at Greenland and covering the sea areas immediately to the north of Iceland (Fig. 8). The effect of this ridge appears to have been to deflect the northerlies to the south and east of Iceland, thus cushioning the Icelandic north coast against the direct impact of climatic deterioration. (Jan Mayen lies in the main path of the northerlies well to the north of this ridge and is consequently unprotected.)

In recent years, the strengthening of the northerly airstream in this sector has also caused a great scouring of ice from the Arctic Basin and has been responsible for scattering heavy polar pack-ice further and further to the south of its 'normal' limits. The progressive build-up of drift-ice at Iceland between 1950 and 1968 is strikingly illustrated by Sigtryggsson (1969) and Malmberg and Stefansson (Fig. 11). By the spring of 1968 ". . . . the eastward extent of the drift-ice north of Iceland was . . . . greater than in any year since 1918. Furthermore, the drift-ice extended farther south along the east coast of Iceland than previously observed in this century" (Malmberg and Vilhjalmsson 1968, p. 2). On a wider scale, Lamb (1963, p. 205) has observed that in February 1963, "the position of the limit 150-200 miles south-east of the island of Jan Mayen and more generally between 70° and 75°N in the Norwegian and Barents Seas exceeds the extreme of the present century by up to five times the previous range of variation and approaches the extreme end-of-winter positions ever known". Even more extreme records for ice extent were successively established in 1965 and 1968. Figure 12, kindly supplied by the Climatology Division of the Meteorological Office, Bracknell, compares the extreme ice extent of early May 1968 with the 1911-50 'normal'.

Recent Icelandic observations have shown that the vast build-up of sea-ice at Iceland is not simply the result of an increase in the southward transport of drift-ice

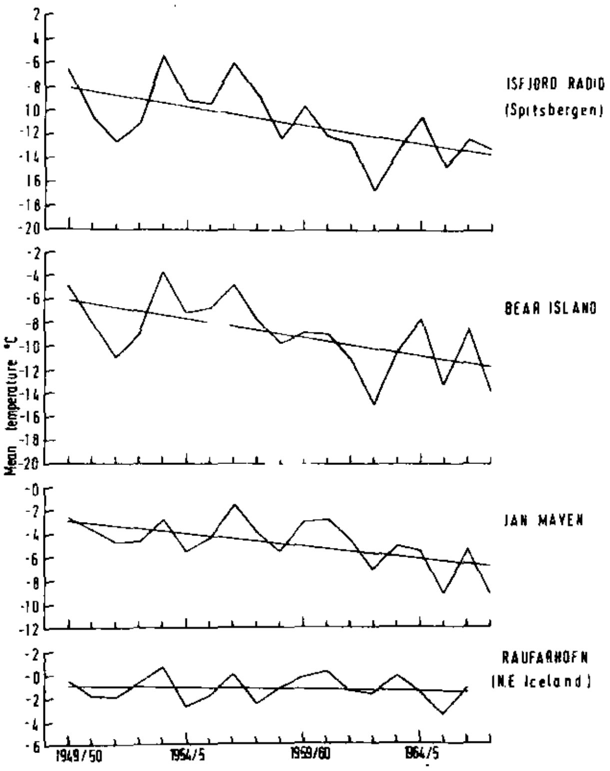

Fig. 10. Mean air temperatures and cooling trend at four Arctic stations from December to February.

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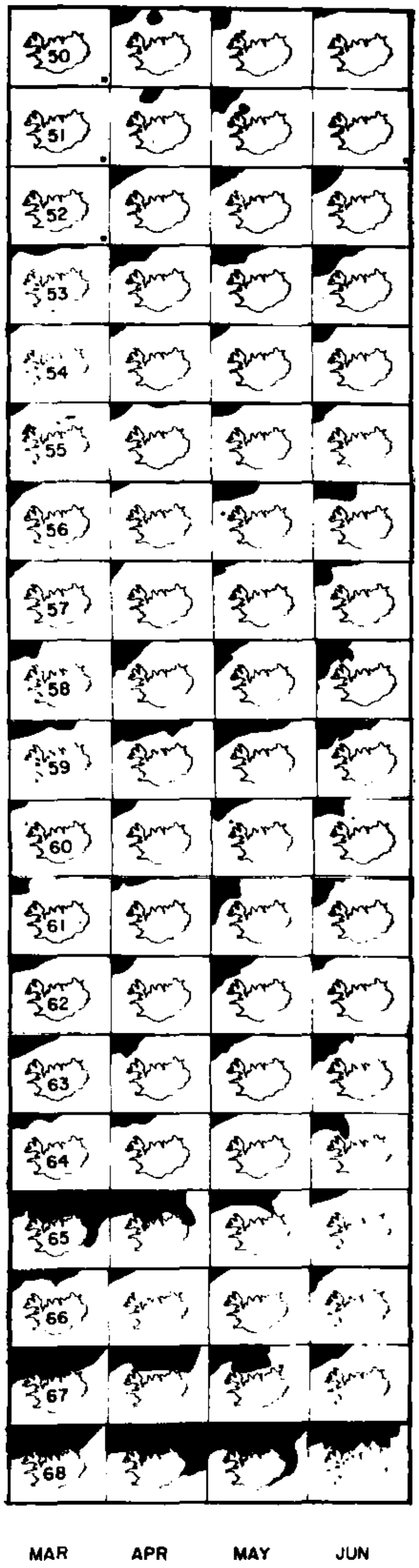
[3] Winter is defined as the months December, January and February.

(a)



(b)

(c)

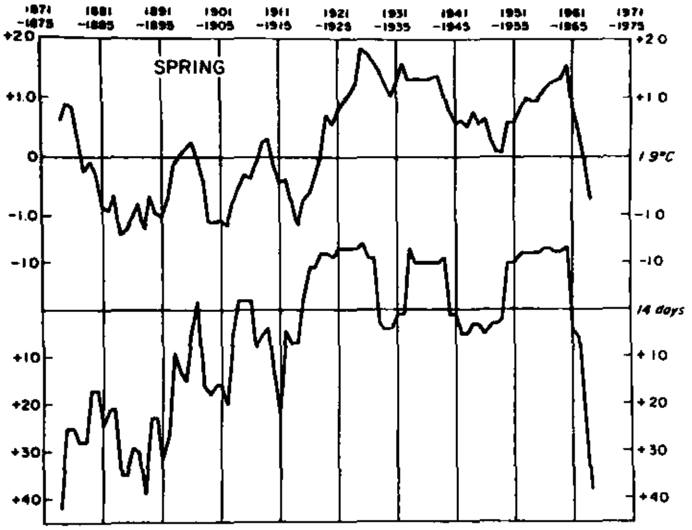


Fig. 11.(a) Distribution of drift-ice at Iceland in the months March-June 1950-68 (from Sigtryggsson 1969).

(b) Five year running means of surface temperature at Grimsey in March-May (from Stefánsson 1969).

(c) Five year running means of the frequency of drift ice in the Icelandic coastal area in March-May (from Stefánsson 1969).

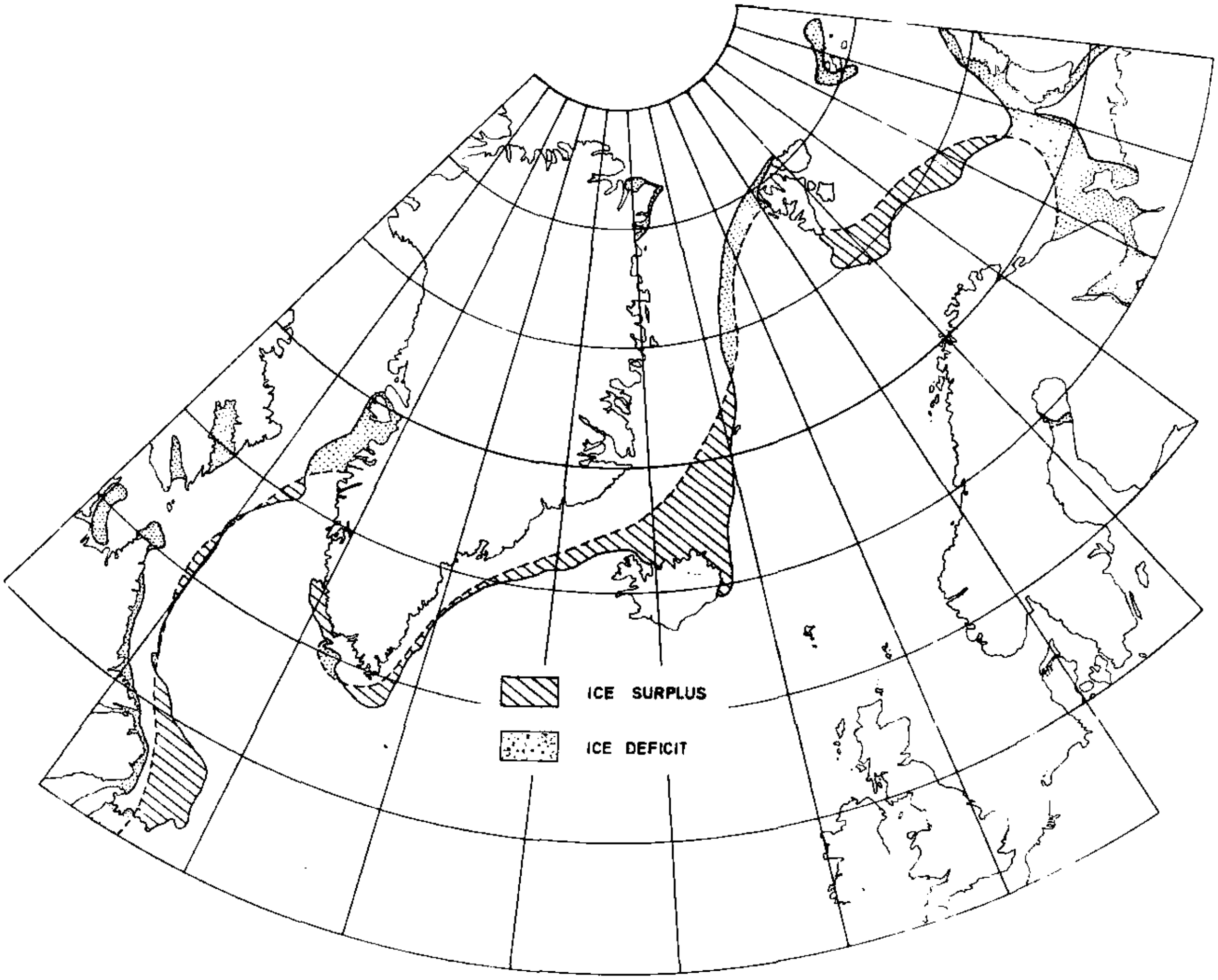


Fig. 12. Ice extent at 8 May 1968 (solid line) compared with normal (broken line). The normal is a composite one based on an American average concentration of ice 1911-50.

from the Polar Basin. The fact that the polar current in the Greenland Sea has also accelerated has meant that the hydrographic character of the East Icelandic Current has changed, becoming cooler and less saline as the proportion of polar water increased (Malmberg 1967*a* and *b;* 1968*a* and *b;* 1969*a* and *b;* Malmberg and Stefansson 1969; see also Penin and Solonitsina 1968). For this reason the East Icelandic Current changed from being an ice-free arctic current in 1948-58 to a polar current in 1964-68, transporting drift-ice and preserving it. With the continued intensification of this polar influence in the most recent years (1965, 1967, 1968) an additional factor has come into play, in that the polar water component has been so great that temperatures have approached the freezing point (-1.8°C) while salinities have dropped below 34.7‰. This salinity level is critical off North Iceland since below this value the surface water will not reach a sufficiently high density to start deep convective mixing, even at the freezing point. For this reason, Malmberg concludes that the *formation* of sea-ice in the deep water northeast of Iceland has contributed to the extension of sea-ice in this region in recent years.

There is some evidence that the gross changes in high-latitude temperature over the present century have been followed by sympathetic changes in the temperature of the deep water in the Norwegian-Greenland Sea, presumably through changes in the efficiency of bottom water formation. The bulk formation of bottom water in this sector is thought to occur within the cyclonic gyral of the Greenland Basin (e.g. Eggvin 1961; Aagaard 1968), where deep water of subzero temperature is domed upwards to within a few metres of the surface

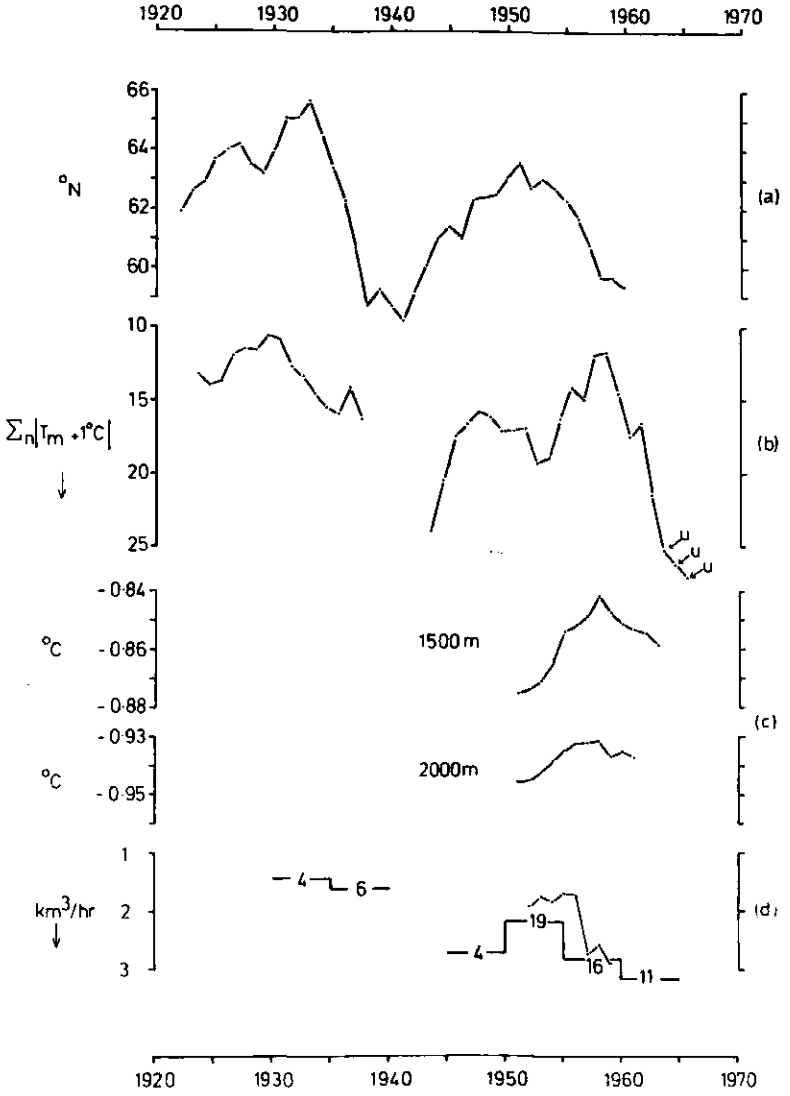


Fig. 13.(a) Five year running means of the January-July average latitude of the subpolar low (Atlantic sector) from 1920-62.

(b) Five year running means of the annual Jan Mayen surface cooling index (Σ_m | T_m + 1°C |) from winter 1921-22 to winter 1967-68 'U' indicates underestimates of the index due to lack of data in the following winter months:

1966 April
1967 April
1968 March, April and May

(c) Five year running means of the average 1,500m and 2,000m temperature at OWS METRO in the months January to June. 1949-65.

(d) Five year means of southwestward bottom water transport in the Faroe-Shetland channel from 1930-1934 to 1960-1964, together with the number of estimates in each pentade. The running 5 yearly means are also shown for the period with the greatest uninterrupted data cover (1951-61). Data from Martin 1966.

(Gladfelder 1964). Features of the bottom water formation process have been described by Metcalf (1955) and Mosby (1959, 1962). Despite the fact that deep temperature observations in the Greenland Sea are confined to the two periods from 1901 to 1919 and from 1952 to the present, it is clear that long-period variations have occurred. Aagaard (1968) shows that some time between the first and sixth decades of the century, there was a 'considerable warming' of the deep water in the Greenland Sea and that conditions as cold as those of the first decade have not been observed since. He uses air temperature data from Jan Mayen to construct a 'surface cooling index' and shows – in keeping with the earlier findings of Mosby (1962) – that over the last two decades there is good agreement between the trend of this type of index and the trend of deep water temperatures. Specifically, Aagaard reports a temperature sub-maximum in the deep water of the Greenland Basin in the late fifties, with a subsequent decline in both air and deep sea temperatures until the present (implying a recently renewed intensification of bottom water formation).

Using the relation between the Jan Mayen 'surface cooling index' and deep water temperature we may set these recent trends in the context of longer-term change. The surface cooling index is merely the sum (for each winter) of the quantity $|T_m + 1°C|$, where T_m is the mean monthly air temperature at Jan Mayen during months when T_m was less than -1°C. In Fig. 13a and b, overlapping 5-year means of this index for the period 1921-68 are compared with the 5-year means of the January-July average latitude of the subpolar low. The good general agreement between the two curves is not unexpected; Lamb (1964*b*, p. 497) has already suggested the well-known retreat of Arctic sea-ice in the twenties and thirties to be due to the more frequent penetration of the Arctic region by depressions as the Iceland low moved north, and for the same reason a general link is expected between the latitude of the subpolar low and Arctic air temperatures. Figure 13 does suggest however that peak warming of the Greenland Sea deep water (following the conditions of extreme cold in 1901-10) was attained in the early thirties, and that by comparison the warming of the deep water in the late fifties which Aagaard describes was less protracted and somewhat less intense.

Using published data (Mosby 1963, 1964) supplemented with more recent observations kindly supplied by the Geophysical Institute, Bergen, running 5-yearly means of 1,500 and 2,000 m temperatures at OWS METRO have been calculated for the period 1949-65 (Fig. 13c). Due to the lack of observations for the months July-December in certain years, the mean temperature from January to June was taken to represent the annual mean in each year. (On average 21 observations at 1,500 m and 17 observations at 2,000 m are available for the calculation of each of these 6-month means.) Despite the fact that the amplitude of the fluctuation in temperature at these depths amounts to only a few hundredths of a degree Centigrade, the interannual changes are in the expected sense showing a temperature submaximum in the late fifties and a subsequent cooling.

Finally, Fig. 13d (graph inverted) shows 5-year mean values of the south-westward transport of 'bottom water' through the Faroe-Shetland Channel, together with the number of estimates available for the calculation of each mean. (Data from Martin 1966.) According to Martin this 'bottom water' is not all Norwegian Sea Bottom Water, but incorporates Arctic Surface and Arctic Intermediate Water of similar characteristics, deriving from the East Icelandic Current. Bearing in mind the facts that the number of estimates of bottom transport is uncomfortably low in certain pentades, while the overall variance of transport is high, it is clear that any apparent long-term change in transport must be treated with caution. However (as shown above) the general southward shift of the subpolar low after the thirties, and the changeover from the highly zonal circulation of the thirties to a circulation of increasing northerly meridionality over the Norwegian-Greenland Sea has led not only to a strong cooling of the European Arctic and Subarctic (with an implied increase in the formation of Norwegian Sea Bottom Water), but also to an increase in the vigour of the East Icelandic Arctic Current and a south-eastward extension of the oceanic polar front. Under these circumstances the trend towards an increase in the 'bottom water' transport of the Faroe-Shetland Channel from the early thirties to the sixties (Figure 13d) is a sensible one.

It is relevant at this stage to refer very briefly to certain biological events which appear at present to be connected with the recent deterioration in the atmospheric and marine climates of this sector.

Off Iceland, the chief event has been the progressive eastward shift in the migration route of Atlanto-Scandian herring (Jakobosson and Vilhjalmsson 1967; Jakobsson 1969) resulting in their withdrawal from north and east Icelandic waters as the oceanic polar front spread towards the south and east (Vilhjalmsson and Stefansson 1967; Malmberg and Vilhjalmsson 1968; Vilhjalmsson and Malmberg 1968; Malmberg 1969b).

Following their March spawning off the Norwegian coast, this stock normally migrated north-westward to feed in the mixed Atlantic and Arctic waters of the oceanic polar front in the western Norwegian Sea and off north and east Iceland. They reached the latter area in June and were joined by Icelandic spring spawners

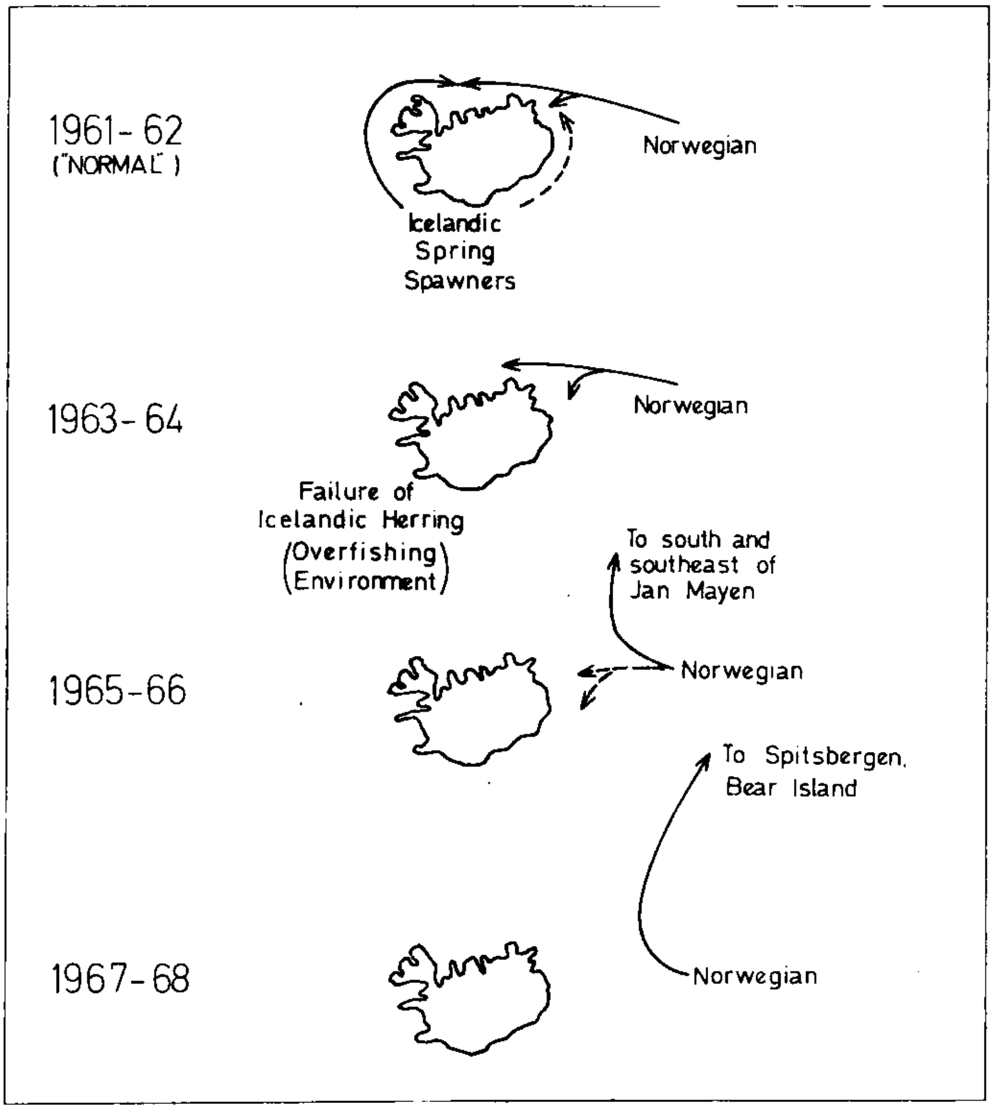


Fig. 14. Summary of recent changes in the feeding migration of herring at Iceland. Data from Jakobsson, 1969.

which had travelled mainly around the Icelandic west coast.

Jakobsson (1969) shows that since 1961-62 drastic changes have occurred in the migration-routes and these changes are crudely summarized in Fig. 14. The years 1961 and 1962 showed a 'normal' migration pattern. In 1963 the migration of the Icelandic stock failed for the first time, due partly to a reduction in the stock through fishing, but also to a combination of poor feeding conditions and low temperatures off the North Icelandic coast. The latter conditions also resulted in a less extensive penetration of the north coast by the Norwegian stock in 1963-64, and since 1963 no Atlanto-Scandian herring have been observed west of Langanes. In 1965-66, with the further cooling and deterioration of feeding conditions off North Iceland, the Norwegian stock no longer moved against the current to penetrate the north coast, but turned to run rapidly north with the current along the eastern boundary of the polar front to richer feeding grounds south and east of Jan Mayen. With the extreme south-eastward extension of the oceanic polar front in 1967-68 this revolutionary change in migration behaviour was maintained; almost the entire adult stock turned northwards well to the east of Iceland, and travelled far to the north along the boundary of the polar front to feed off Spitsbergen and Bear Island.

A second event of importance in this sector has been the change in the timing and duration of oceanic production in recent years. Our evidence for this derives largely from the Continuous Plankton Recorder Surveys conducted by the Oceanographic Laboratory, Edinburgh. An analysis of the timing of primary production by Robinson (1969) has shown that at the southern approaches to the Norwegian Sea the peak of the spring phytoplankton outburst has been progressively delayed by a total of almost 1 month over the period 1948-68.

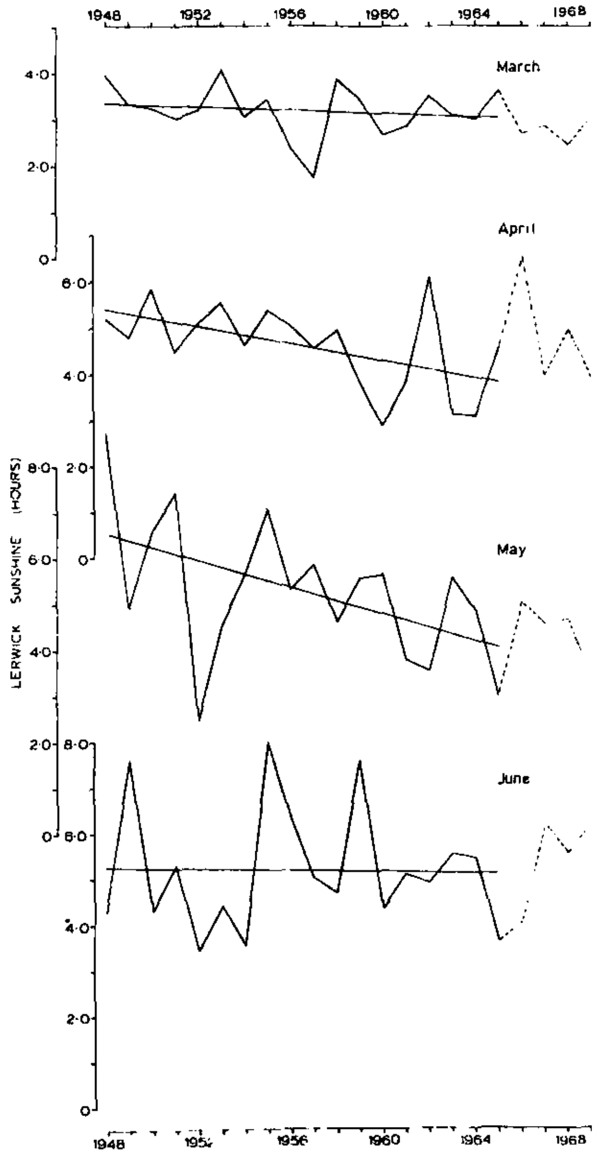


Fig. 15. Mean daily "hours of bright sunshine" at Lerwick for March, April, May, and June 1948-69. Data from the Daily Weather Reports of the Meteorological Office, Bracknell. The regression lines apply to the period 1948-65.

Later, using different criteria to describe the timing of production, Glover, Robinson, and Colebrook (1970) were able to show a similar delay for the North Sea which was not evident using the technique of Robinson's earlier analysis. No significant delay was detected to the west of Britain.

The two main environmental factors governing the onset and development of vernal algal production in temperate waters are the rate of change of solar radiation and the rate of change of vertical stability in the water column. Assuming no interannual change in windspeed, a long-term change in solar radiation will induce a progressive change not only in the timing of the initiation of production, through a change in the rate of descent of critical depth (Gran and Braarud 1935; Sverdrup 1953), but also in the subsequent development of production, through a change in the ratio of compensation depth to depth of mixing. Further, such a change in insolation will also affect the rate of energy input at the sea surface, leading to a change in the time of establishment of the spring thermocline (i.e. a change in stability).

As mentioned earlier, a progressive decline in incoming solar radiation did take place after the 1930's, and this has been associated with radical changes in the global atmospheric circulation. It is likely however that this global change of a few per cent in incoming radiation would be insignificant compared with the *local* effect of a change in cloud cover in certain months resulting from a local change in the atmospheric circulation. Thus although Budyko (1968) shows a 4% weakening of the direct solar beam since the forties, Fig. 15 shows a 29 and a 38% decline in the mean daily hours of bright sunshine[4] at Lerwick in April and May respectively over the period 1948-65; later figures show that insolation has tended to remain generally low at Lerwick over the period 1966-69. No change is evident for the months of March or June (or indeed for the whole year at Lerwick – see Glover, Robinson, and Colebrook 1970). Yet with Lerwick lying central to the area for which Robinson shows a progressive delay in the timing of the spring phytoplankton bloom, it is possible that the progressively diminished insolation in the critical spring months of April and May has contributed to the delay which Robinson describes.

Thirdly we have some evidence that the extension of climatic deterioration from the Norwegian Sea to the area of the North Sea and English Channel has had an effect on the distribution and abundance of various marine organisms (e.g. Southward 1967), including some fish of economic importance. Our evidence for this derives mainly from events in the English Channel where the distribution limits for a number of species appear to coincide rather closely and where, as a result, the effects

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[4] These data are used here since measurements of total solar radiation on a horizontal surface are only available at Lerwick after 1954. The post-1954 trends of the two types of data are essentially identical.

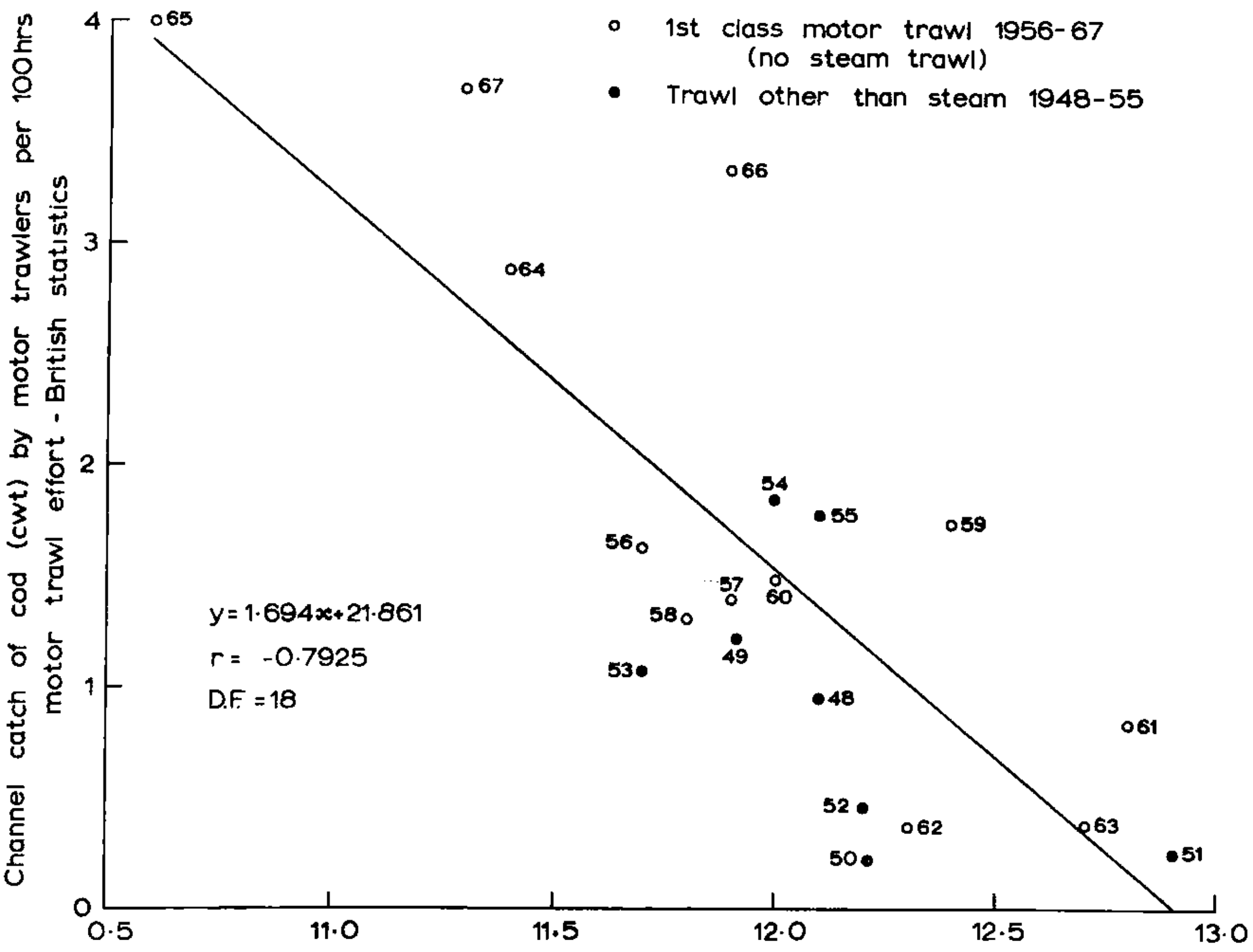

Fig. 16. Catch per effort of Channel cod (1948-67) vs. mean annual sea temperature, Plymouth Sound, 2 years previously.

of a changing environment are reflected most closely in species abundance.

As might be expected the abundance of cod (all things being equal) appears to show a positive relation to temperature at its poleward limits of range, and a negative relation at its equatorward range limits (although the relation may not be a direct one but a second or higher order effect). This has been shown at West Greenland (Hermann 1953; Hermann, Hansen, and Horsted 1965), at West Greenland and Labrador (Elizarov 1965), in the Norwegian and Barents Seas (Kislyakov 1961), and off Nova Scotia and New England (Martin and Kohler 1965). In keeping with this generalization, data from the Sea Fisheries Statistical Tables (Anonymous 1949-68) show interannual changes in the British catch per effort of Channel cod during the post-war period which are unrelated to changes in trawl effort, but which are closely correlated with mean annual sea temperature in the Channel 2 years previously. The temperature versus catch per effort relationship for the period 1948-67 is shown in Fig. 16. [in this, only motor trawl catch and motor trawl effort are shown since the steam effort was relatively unimportant after 1948 and disappeared completely after 1955. It was also found necessary to distinguish between the motor trawl catch and effort data of the two periods 1948-55 and 1956-67, since the listed statistics for the earlier period refer to all vessels other than steam (including inshore boats), and only in the later period are the statistics for first-class motor trawlers listed separately. This discrepancy is thought to have only a minor effect on the comparability of the two sets of data, however, since the catch of cod by inshore vessels was small in relation to the catch by first-class trawlers in the earliest years after 1956 when statistics for the two categories of fishing vessel were first separately listed. Mean annual temperatures at Plymouth Sound (Southward 1967) were taken to represent the post-war pattern of sea temperature change for the English Channel as a whole, since at this station observations are available from even the earliest post-war years and since the temperature trends

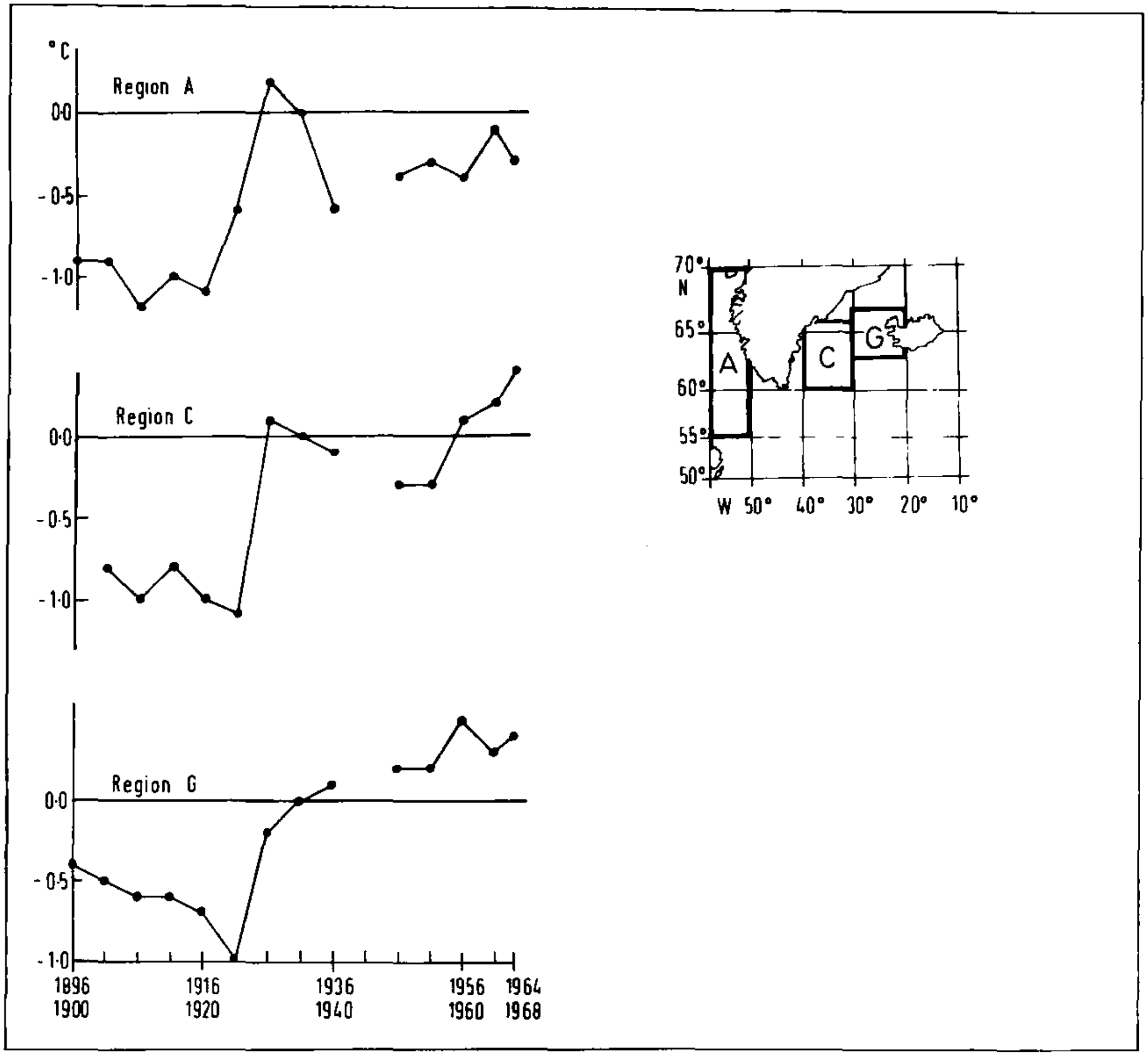

Fig. 17. Pentade means of sea-surface temperature anomaly in Regions A, C, and G (Smed 1947-69), regarding 1931-35 as "normal". After Lee (in preparation).

observed at Plymouth Sound are essentially identical to those shown in the shorter post-war records of Channel Lightvessels. Since the abundance of Channel cod is largely determined in any year by the abundance of 2- to 3-year-old fish (Garrod *personal communication*) and is therefore a crude measure of the relative numbers of fish spawned 2-3 years previously, the abundance statistics in terms of catch per effort by British trawlers are plotted in Fig. 16 against the sea temperatures 2 years previously.]

The significant negative correlation shown in Fig. 16 must be interpreted with caution. Quite apart from our lack of reliable estimates of the catch and effort of foreign fleets in the area, there are a number of factors other than temperature which may have influenced the apparent abundance of the stock. In particular, changes can be generated by the fishing effort, although in this case there is no evidence of a decline in fishing which might have led to an improvement in the stock. More important, the estimates of abundance may be biassed by changes in fishing strategy or in the fishing power of the vessels and in this context it will be seen that the absolute level of the catches is very low, and hence sensitive to such changes. However, it is thought unlikely that these sources of variance could account for the

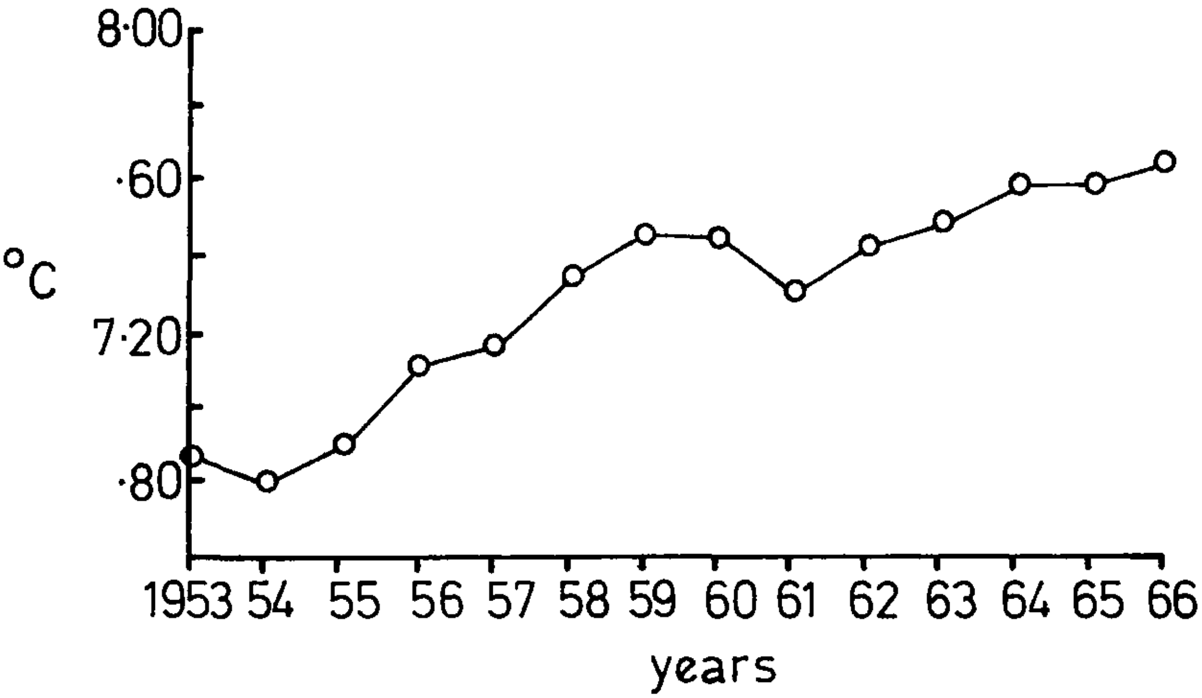

Fig. 18. Five year running annual means of sea temperature for OWS 'A' (from Rodewald 1969).

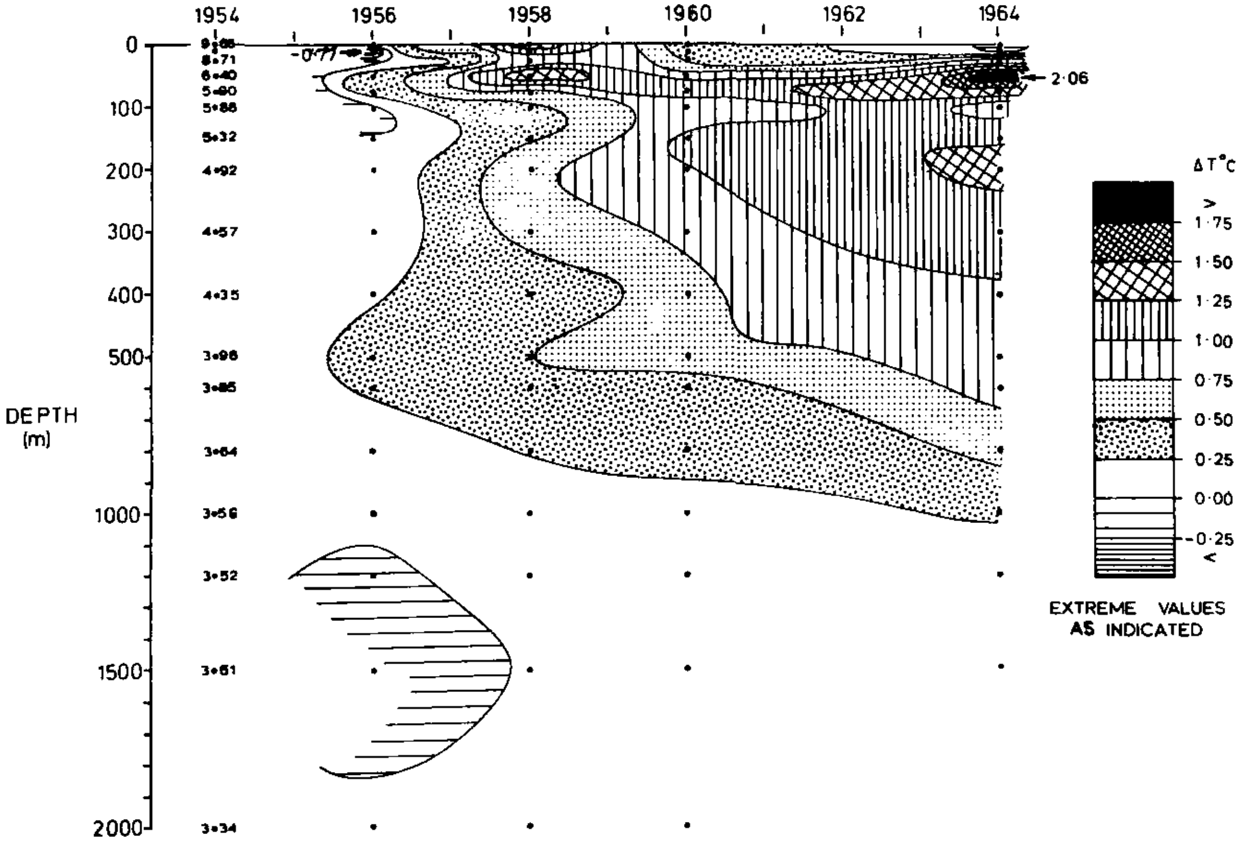

Fig. 19. Change of July temperature (°C) at OWS ALFA, 0-2,000 m, relative to July 1964. From deep hydrocast data tabulated in Mosby, 1965. (The mean temperatures for each standard depth in July 1954 are given.)

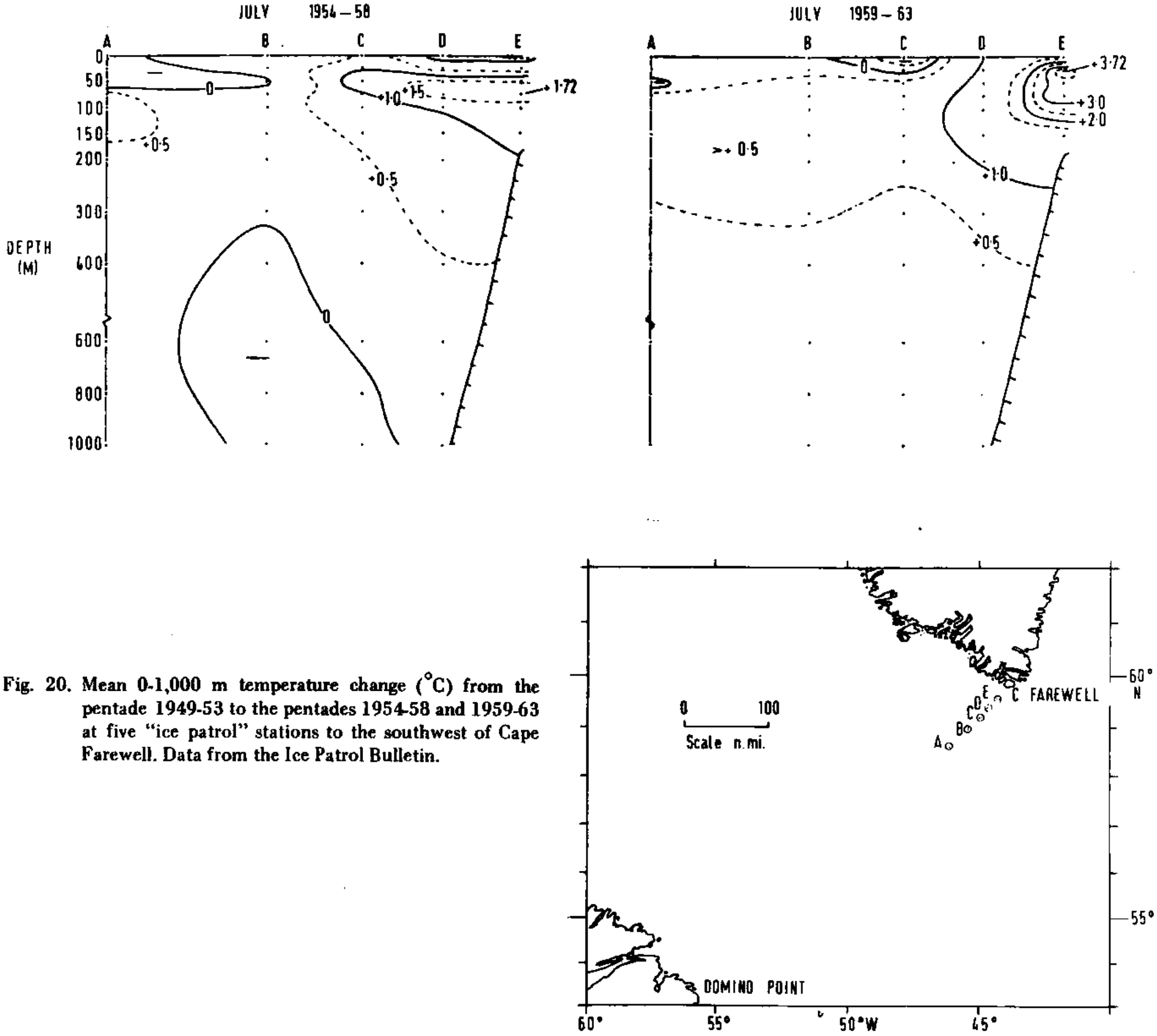

Fig. 20. Mean 0-1,000 m temperature change (°C) from the pentade 1949-53 to the pentades 1954-58 and 1959-63 at five "ice patrol" stations to the southwest of Cape Farewell. Data from the Ice Patrol Bulletin.

observed three- to fourfold change in catch per effort since the early fifties, especially since (from the Annual Reports of the Southwestern and Southeastern Fisheries Inspectorates) there appear to have been no *similar* changes in fishing grounds or composition of the fishing fleet. One source of variance which is of importance, however, is the fact that individually strong or weak year-classes may influence a number of consecutive yearly estimates of abundance, and it will be some years before enough data are available to separate long-term trends from the secular effects of, for example, the strong 1963 year-class.

The relatively high cod abundance in the Channel from 1964 to 1967 (reflecting relatively abundant year-classes after 1961 or 1962) is supported by the work of M. J. Holden of the Lowestoft Laboratory. He has compared the abundance of 2-year-old fish from the trawl and seine cod landings at Grimsby and North Shields since 1956, and shows that "the average year-class strengths in the period 1954 to 1959 were much lower than in the years 1960 to 1967, except in the North Shields trawl fishery" (Holden in press). In both areas the increased abundance in the 1960's is correlated with decreased sea temperatures.

TABLE 2. Pentade means and standard deviations of position and date of five Ice Patrol stations southwest of Cape Farewell.

Station		1949-53		1954-58		1959-63	
A	Mean position	58°36.6'N	46°06.6'W	58°40.4'N	46°08.6'W	58°36.2'N	46°03.2'W
	S.D. (')	3.18	8.73	0.73	4.80	1.91	2.14
	Mean date	21 July		26 July		20 July	
	S.D. (days)	8.58		18.23		10.20	
B	Mean position	58°56.8'N	45°23.4'W	59°01.0'N	45°21.8'W	58°58.8'N	45°20.4'W
	S.D. (')	2.98	7.63	2.24	4.79	1.94	2.24
	Mean date	21 July		26 July		20 July	
	S.D. (days)	8.28		18.54		10.46	
C	Mean position	59°11.3'N	44°55.0'W	59°12.5'N	44°58.4'W	59°12.3'N	44°50.0'W
	S.D. (')	2.82	5.02	2.28	3.83	1.36	3.29
	Mean date	21 July		26 July		20 July	
	S.D. (days)	8.28		18.54		10.38	
D	Mean position	59°23.4'N	44°36.0'W	59°23.8'N	44°37.4'W	59°23.1'N	44°30.2'W
	S.D. (')	4.49	7.87	2.04	12.04	4.04	3.65
	Mean date	21 July		26 July		20 July	
	S.D. (days)	8.28		18.32		10.39	
E	Mean position	59°33.3'N	44°17.0'W	59°32.1'N	44°18.6'W	59°31.5'N	44°09.2'W
	S.D. (')	2.69	10.20	1.43	10.38	2.76	6.83
	Mean date	22 July		26 July		22 July	
	S.D. (days)	8.65		18.25		10.86	

Climatic Amelioration in the Irminger Sea and off South and West Greenland

In the Irminger Sea, and in waters off South and West Greenland the marine 'climatic optimum' of the early thirties was followed by a progressive decline of sea temperature until the late forties or early fifties. Then, when it appeared from catch data that a southward retraction of the West Greenland cod stocks had set in (Hansen and Hermann 1965) the temperatures in these sea areas began to rise once again and the warming trend has continued until at least the late sixties (Hermann 1967; Lee in preparation).

Figure 17 from Lee (in preparation) shows that the rise in surface temperature has been most conspicuous in the Irminger Sea (Smed's areas C and G), and Rodewald's (1969) pentade means of annual surface temperature at OWS ALFA show a similar trend (Fig. 18). Thus, while surface temperatures in the Irminger Sea have recently exceeded the high values of the early thirties by around 0.4°C, the surface temperatures off West Greenland (Smed's area A) have remained below the 1930's level.

A slightly different picture is shown by the subsurface observations. These rather limited data appear to show that the recent general warming has been more intensive at depth than at the sea surface; they also show that in recent years the warming of the subsurface layers off South and West Greenland has been almost as pronounced as that observed in the Irminger Sea. Figure 19 shows the change of 0-2,000 m temperature in July at OWS ALFA from 1954 to 1964, regarding conditions in July 1954 as normal. This figure must clearly be interpreted with caution; the temperature means for each standard depth were derived from the deep hydrocasts only (station type 'A' – Mosby 1965) and are available for only 5 of the 11 years under consideration. Nevertheless the warming trend shown is a consistent and progressive one. By 1964, a temperature anomaly of > +2°C was observed within the 'core' of the warming (at 50-150 m depth) while the wave of warmth had spread to a considerable depth. Figure 20 represents an attempt to estimate the recent July temperature trends off South Greenland by comparing conditions at five stations on the standard Ice Patrol section running southwest from Cape Farewell. These data are far from ideal as regards comparability, since

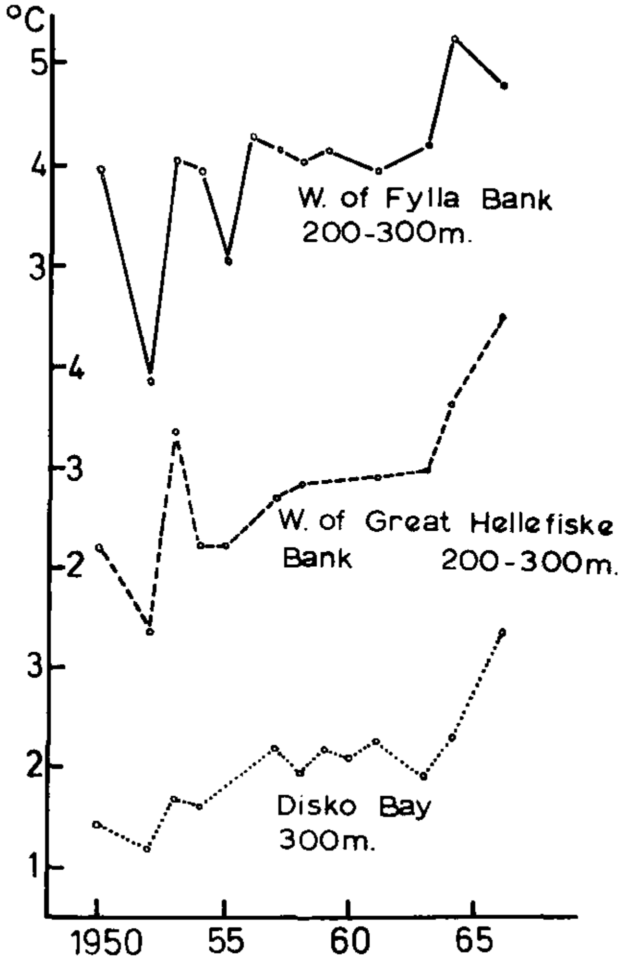

Fig. 21. Deep temperature on the West Greenland Banks in July, 1950-66 (from Hermann, 1967).

the position and date of each station varied slightly from year to year. To smooth out some of this variability the temperatures for each standard depth at each station were averaged over the three pentades 1949-53, 1954-58, and 1959-63. The means and standard deviations of the date and position of each station are listed below for each pentade (Table 2) and the pentade mean positions are shown (circled) in Fig. 20. Regarding conditions in the pentade 1949-53 as 'normal', the anomalies of temperature in the 0-1,000 m layer were calculated for 1954-58 and 1959-63 (Fig. 20). Even though the data do not permit too close a comparison between years – especially as regards station E, since this station was not worked in 1960 – it is fairly evident that a considerable and progressive warming has taken place throughout the period considered, being most pronounced towards the edge of the Continental Shelf. We may compare these conditions in the Irminger Sea and off South Greenland with the subsurface temperature trends for the West Greenland banks reported by Hermann (1967). He shows that at 200-300 m depth, the temperatures in July rose by 2° to 3°C from the early fifties to the mid-sixties (Fig. 21). In the period 1954-64, however, the warming off West Greenland appears to have been slightly less than at OWS ALFA, amounting to + 1.4°C at Fyllas Bank and Store Hellefiske Bank, and + 0.7°C off Disko Bay.

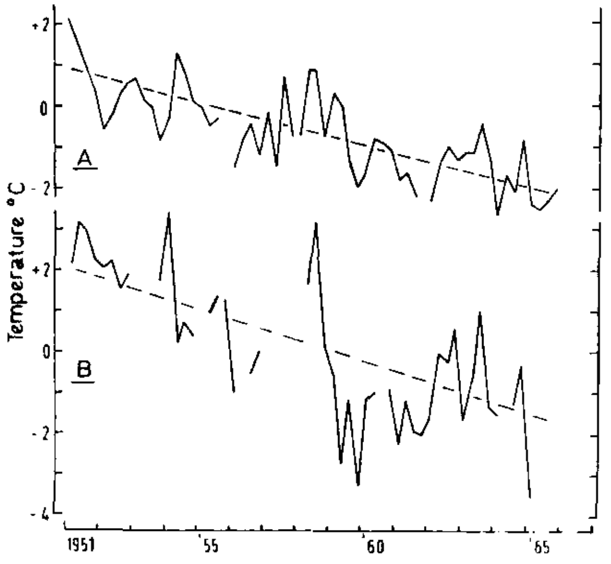

Fig. 22. Quarterly deviations of water temperatures: (A) Sambro LV, bottom temperatures, (average 1949-59); (B) Emerald Bank, bottom temperatures, (average 1950-64). The broken lines represent temperature trends (from Lauzier 1967).

In certain instances we can explain a part of this warming as being due to local factors. For example, Lee (in preparation) has pointed out that a part of the recent deep warming off West Greenland may be due to an increasing frequency of offshore winds driving a layer of light coastal water over the Irminger component of the West Greenland Current, thus inhibiting deep convectional overturn in the cooling season. However, the widespread nature of the recent warming from the Irminger Sea to West Greenland demands that other less local factors be considered in addition.

A general warming of the seas around Greenland might at first appear surprising in view of the fact that a steepening west-east pressure gradient across the Norwegian-Greenland Sea has for some time been giving rise to conditions that are apparently ideal for a strengthening of the East Greenland Arctic Current. However, Fig. 8 shows that the rather narrow outlying ridge running south-eastwards from the main high-pressure anomaly cell at Greenland has recently been giving rise to an anomaly wind running in an anti-clockwise sense over the eastern Atlantic to the Irminger Sea. This wind explains the amelioration of conditions off South Greenland at a time when neighbouring sea areas were experiencing a deterioration of climate, since it can probably be held responsible for boosting the flow of the relatively warm Atlantic current branch running through the eastern Atlantic to the Irminger Sea and South Greenland. Certainly the years 1964-66, which are shown by Hermann (1967) to be the years of peak warming at depth off Fyllas Bank, Store Hellefiske Bank, and Disko Bay, are also the years when the Irminger Current was extremely well developed (Blindheim 1967). If this interpretation of events is correct, then it must be suggested that there is still some cause for anxiety regarding the future welfare of both the major West Greenland cod stocks since, with only a slight change in the average atmospheric circulation, the apparently robust warming process could rapidly be replaced by an equally strong (or even stronger)

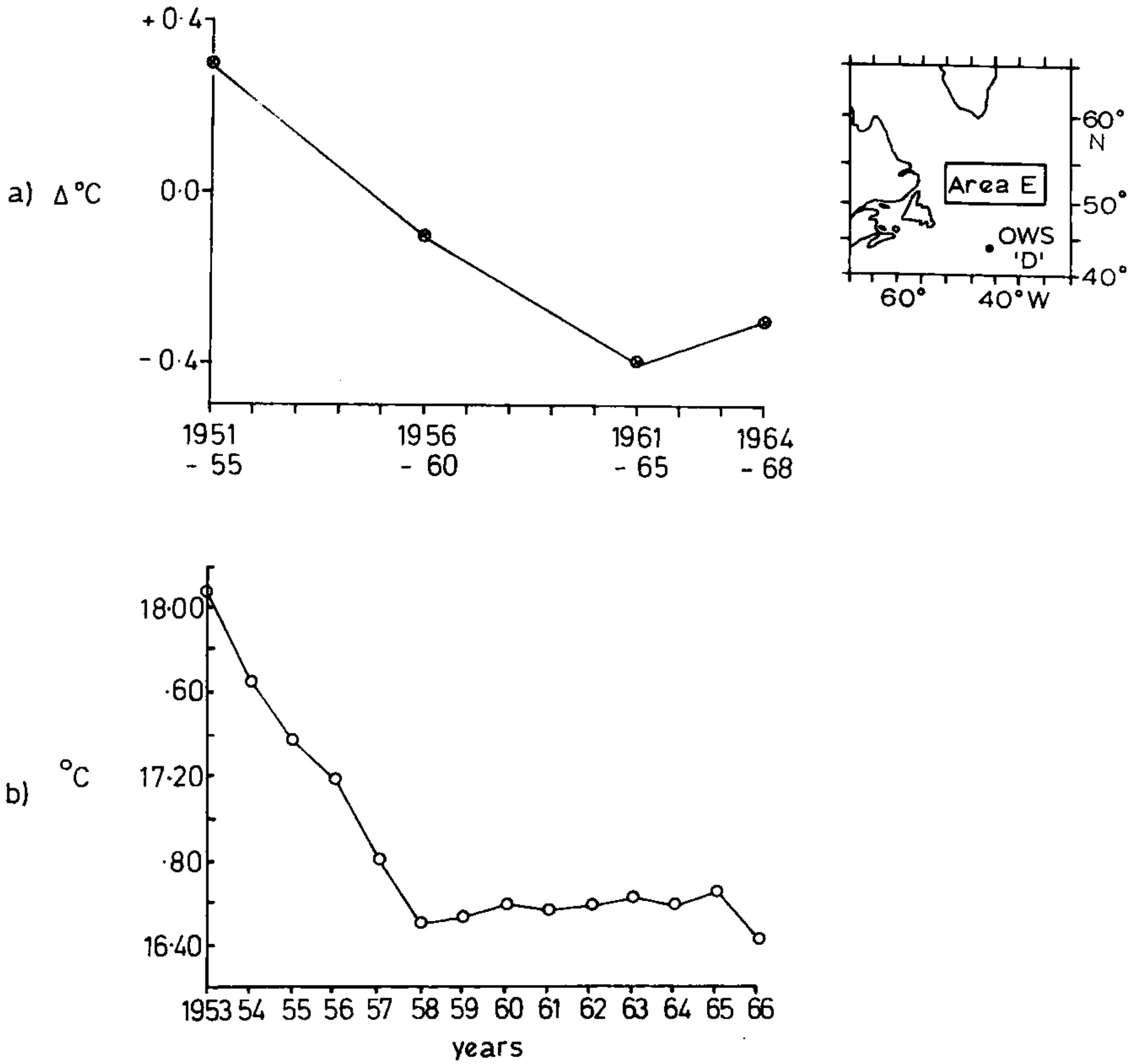

Fig. 23.(a) Pentade means of mean annual surface temperature anomaly for Smeds' area E, regarding 1931-35 as "normal". Adapted from Lee (in press).

(b) Five year running annual means of sea temperature for OWS 'D' (from Rodewald 1969).

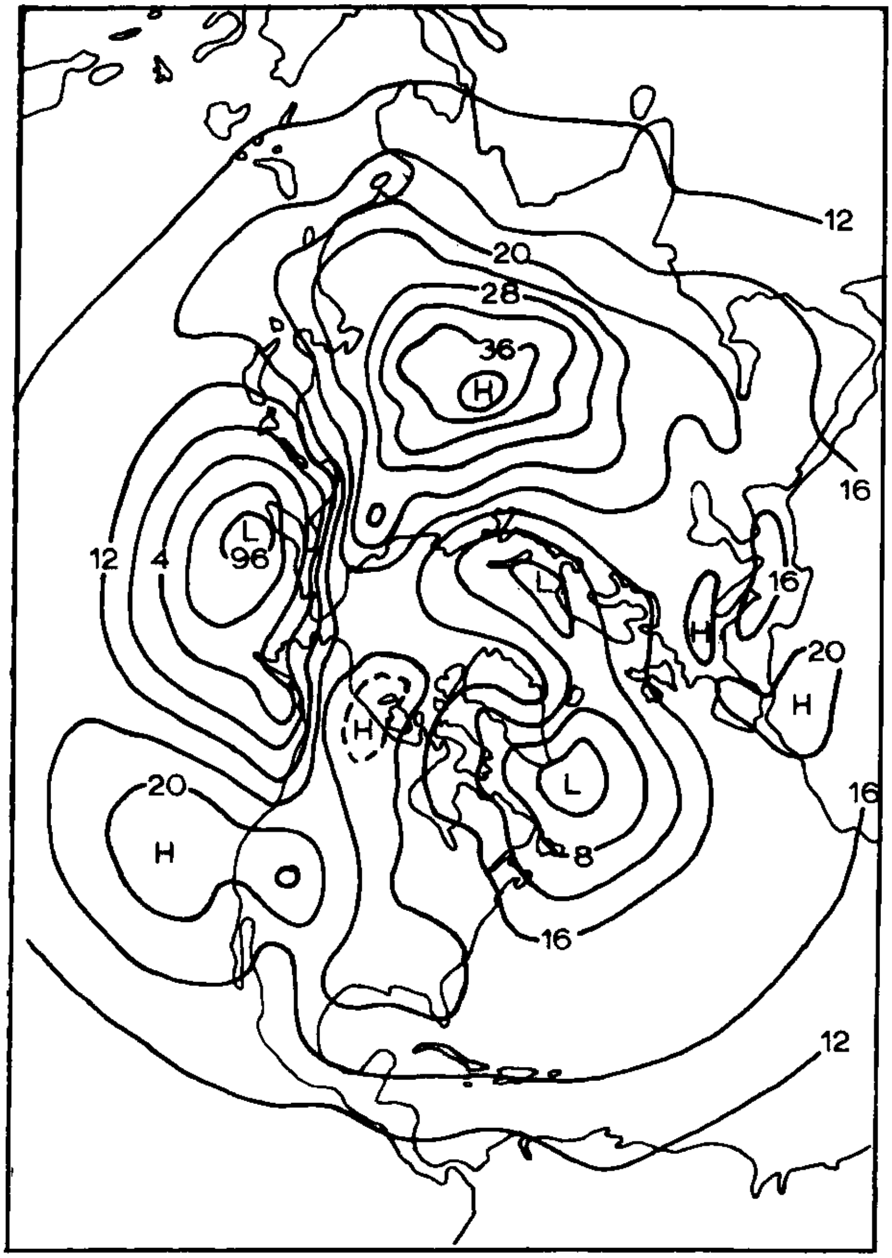

Fig. 24. Mean surface pressure (mb) in January, 1963-68 (from Hesse and Stevenson 1968).

tendency towards cooling. The reasons for adopting this view are clear; at present the south-easterly anomaly wind which appears to be incrementing the flow of warm water to West Greenland owes its orientation to the narrow ridge running south-eastward from the main high-pressure anomaly cell at Greenland. This ridge is also preventing any acceleration of the East Greenland Arctic Current by the northerlies of the Norwegian-Greenland Sea. With any retraction of this ridge the south-easterly anomaly wind would weaken and northerlies running along the East Greenland coast to the Denmark Strait would be in a position to accelerate the cold Arctic Current. There would thus be a dual change towards a deterioration of the marine climate at Greenland.[5]

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[5] Rodewald's paper (this volume) shows that in the most recent years this changeover of hydrographic trends has in fact occurred. In Part 2 of the Danish Research Report for 1970, Hermann (1971) shows a general and "rather alarming" cooling and freshening of the waters overlying the West Greenland Banks in 1968-70 suggesting an increased strength of polar water advection from the East Greenland Arctic Current.

Climatic Deterioration off the Canadian Maritime Provinces and New England

At the special meeting of the ICNAF Environmental Subcommittee in May 1967, it was made clear in a number of papers that along the north-eastern seaboard of North America, both sea and air temperatures have shown an almost linear downward trend since the early fifties. Lauzier (1967) shows a decline of 0.2° to 0.25°C per year both for sea surface temperatures at St. Andrews and the southwestern Gulf of St. Lawrence, and for bottom temperatures in the Bay of Fundy, Gulf of Maine, and Scotian Shelf (Fig. 22). A progressive cooling tendency was also evident in the warm deep layer in the Cabot Strait since at least the late fifties (possibly earlier), although the annual rate of cooling was somewhat less in this layer than in the bottom layers on the Scotian Shelf. In general, Lauzier shows the rate of cooling to have been greatest in the summer and autumn within the Bay of Fundy, but in winter on the Scotian Shelf. Welch (1967) shows a general decline in surface temperature at Boothbay Harbour, Maine, since 1953 with maximum cooling in the winter period, and both Colton (1967) and Chase (1967) show a similar pattern offshore in the shelf waters between Nova Scotia and Long Island at both surface and subsurface depths. From a consideration of surface temperature conditions on the Grand Banks and Georges Bank in the months February-July, Lee, Corkum, and Laevastu (1967) show that compared with the period 1946-65, surface temperatures were above average in 1963 but well below average (up to -4°C) in the period 1964-66. In the open waters of the Northwest Atlantic, Lee (in preparation) shows a cooling in the 5-year means of surface temperature in Smed's area E from the pentade 1951-55 to that of 1964-68 (Fig. 23a) and at OWS DELTA Rodewald (1969) shows a precipitous decline in surface temperature from 1953 to 1958 followed by a general maintenance of cold conditions until at least 1966 (Figure 23b).

The general cooling of the shelf and oceanic waters of the Northwest Atlantic during the last two decades [paralleled by a decline in air temperature – see Lauzier (1967) and Chase (1967)] is once again explained by Rodewald's (1967) chart of pressure change from 1900-1939 to 1956-1965 (Fig. 8). This draws attention to the establishment of a direct north-easterly *anomaly*-wind running along the northeastern American seaboard from South Greenland. We may expect the loss of sensible and latent heat to the atmosphere to have been greatest in the winter months when the anomaly airflow was at its most intense (see p. 38 and Fig. 9). Hesse and Stevenson (1968) have computed the monthly surface pressure means for the northern hemisphere over the period 1963-68 and from their charts it is clear that the *actual* wind affecting the Northeast American seaboard has had a more northerly or north-westerly orientation in this later period. Again, this airflow has been most intense in the winter months December-March (typified by the mean pressure pattern for January, Fig. 24). There are indications that events in the Pacific sector have helped to bring about the intensification of north-westerly airflow that has characterized the eastern part of the North American continent in recent winters. In a recent informal report (Anonymous 1971) Namias is reported as suggesting that anomalously warm sea surface temperatures over much of the North Pacific have led to a persistent shift in the winter jet stream during the 1960's so that the jet stream path was deflected to the north over the western American seaboard, and to the south over the eastern part of the continent, strengthening the storm systems there and leading to a more frequent airflow from the Canadian Arctic to the eastern coast.

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Contribution Number 3

No Discussion

Seasonal and Year to Year Variations in Water Temperature in the Labrador and Newfoundland Areas

By V. V. Burmakin[1]

Abstract

Seasonal and year-to-year variability of water temperature in the 0- to 200-m layer in the Labrador and Newfoundland areas is considered in the paper. During a year two maxima and two minima are observed in the waters of the cold and warm components of the Labrador Current. One maximum and one minimum is registered in the Coastal jet of the Labrador Current.

Year to year variability is considered for the period from 1936 to 1970. Three and four year cycles in the fluctuation of water temperature are revealed. According to the cycles determined it is suggested that a next period will finish in 1971 and hence the year will be "cold".

An anti-phase in the fluctuation of water temperature in the Labrador and Newfoundland areas and in the Barents Sea, that was earlier noted by Elizarov (1962) and Izhevsky (1964), is confirmed.

The relationship between the sun activity and mean annual temperature in the surface layer at station 27 is observed 3-4 years later.

Periods of cooling (1963-1964) and heating (1965-1967), that were responsible for the thermal regime of waters during the past 11 years, are shown on diagrams of isopleths of the mean temperature in the 0- to 200-m layer on the section across the Labrador Shelf and Grand Bank in the years 1960-70.

Introduction

The present work is aimed at following the variation of water temperature in the Labrador and Newfoundland areas for the years 1936, 1938-41, 1948-70. Seasonal variation of the mean water temperature in the 0- to 200-m layer has been analysed from data collected in standard hydrological sections across the Grand Bank, the South Labrador Shelf and at coastal station 27, off Cape Spear, Newfoundland.

Temperature variations in the waters of the Northwest Atlantic and the Barents Sea are compared.

Materials and Methods

Mean water temperature in standard hydrological sections in the areas of Labrador and Newfoundland (Fig. 1) has been analysed. Mean temperature of the 0- to 200-m layer was calculated for the stations which are bracketed in Fig. 1. On sections 8-A and 7-A, the stations are situated in the coastal and main branches and in the Irminger component of the Labrador Current; on sections 6-A, 4-A, and 3-A, in the frontal zone of the Labrador and North Atlantic Currents; on sections 2-A, 1-A, and 44-A, in the transformed waters of the Labrador Current and bank waters where they meet the

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Gulf Stream waters.

Data were collected from these sections by Soviet research vessels, mainly from PINRO, and the vessels of the International Ice Patrol.

Wolf's digits are used to characterize sun activity.

Results of Analysis

Seasonal variability of water temperature

No regular monthly observations were made on standard sections off Labrador and Newfoundland. An attempt was made to obtain a mean curve of the annual variation of water temperature in the 0- to 200-m layer for particular portions of each of the sections: 8-A (B), 7-A, 6-A (G), 4-A, 3-A, 2-A, 1-A, and 44-A, using data collected over different years and months.

Figure 2 gives curves of the annual variations of water temperature for sections 8-A, 7-A, and 6-A, which cross the main branch of the Labrador Current. Four extrema are distinctly apparent for section 8-A: two minima and two maxima. For sections 7-A and 6-A the secondary minimum is less pronounced.

The spring minimum is due to winter cooling; the summer maximum to heating. The July-August minimum is associated with a strengthening of the Labrador Current due to the increased afflux of Arctic waters from melting ice. Also considerable stratification of the waters prevents penetration of heat into the deeper layers by means of vertical circulation. The September-October maximum is evidently due to the intrusion of the warm

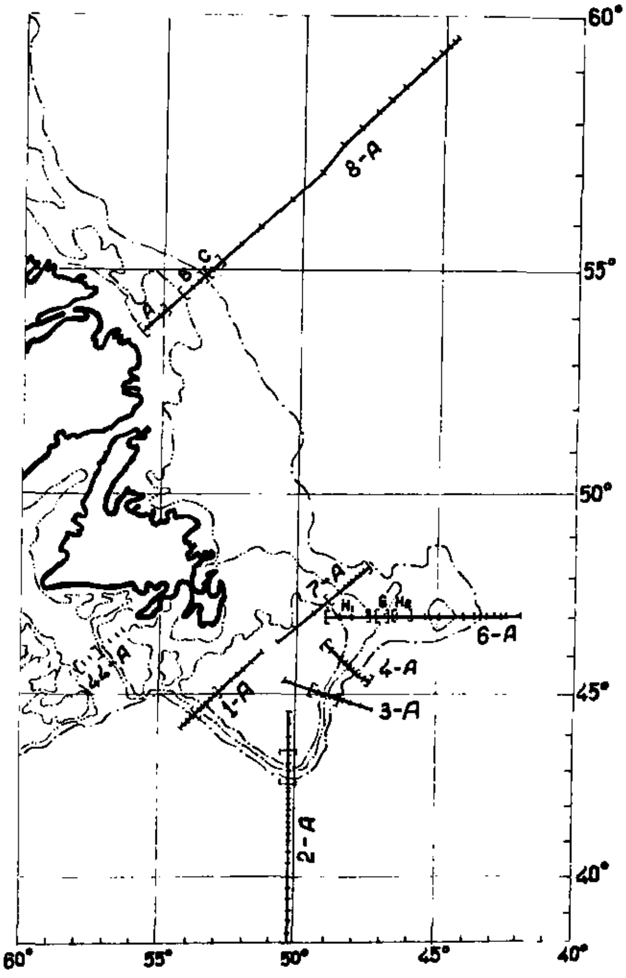


Fig. 1. The position of the standard hydrological sections in the areas of Labrador and Newfoundland (the stations from which the mean temperatures were calculated are given in brackets).

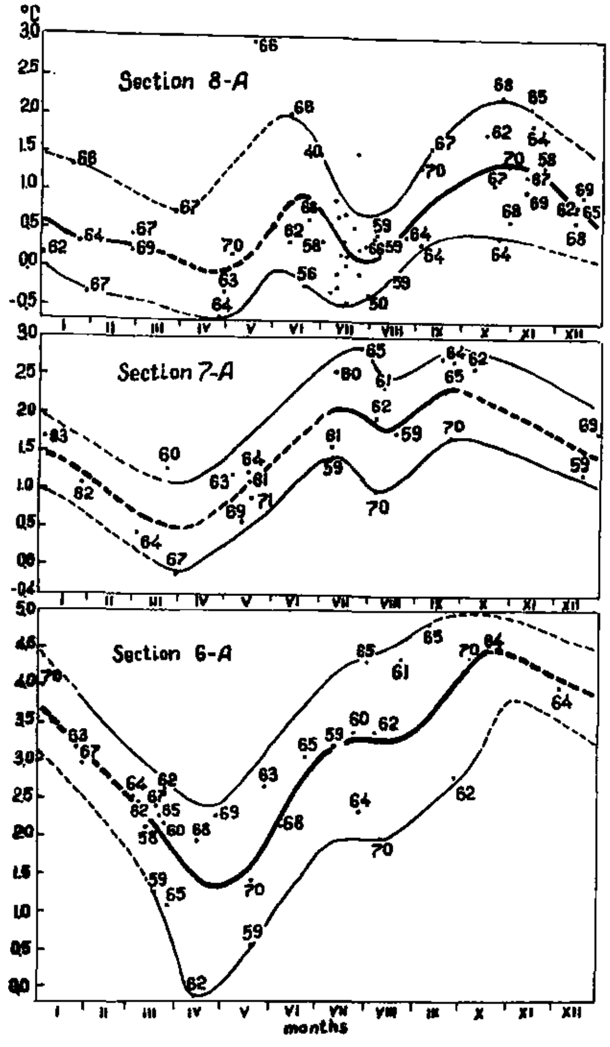


Fig. 2. Seasonal variation of temperature in the 0- to 200-m layer on sections 8-A(B), 7-A, 6-A (G) (numbers mark the years of observations).

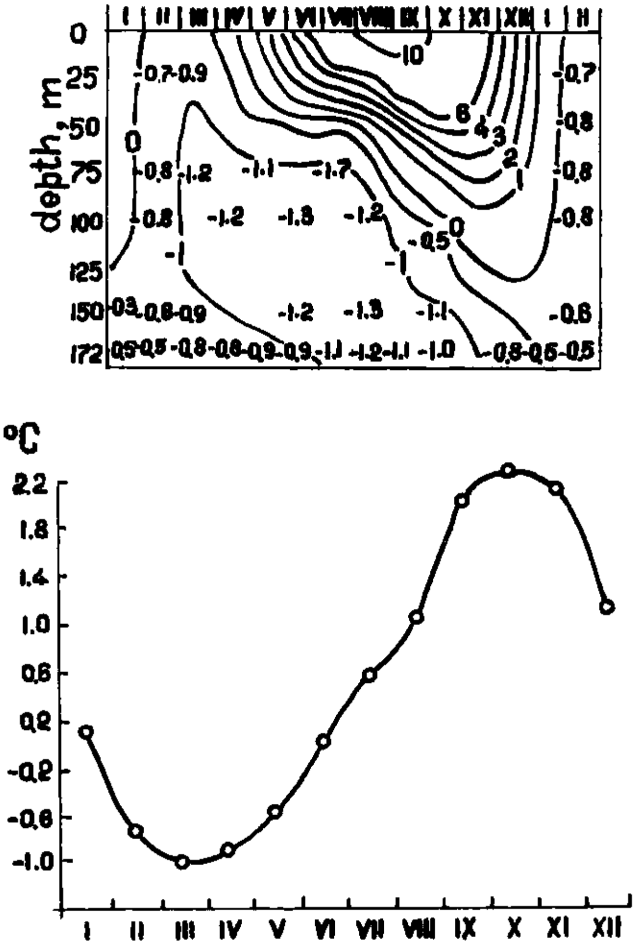


Fig. 3. Isopleths of temperature for 1950-62 at station 27 off Cape Spear near the city of St. John's, Newfoundland (upper figure) and seasonal variation of the mean temperature of the 0- to 172-m layer for the same period (lower figure).

Irminger component onto the slope.

From autumn to spring the waters in the 0- to 200-m layer of the Labrador Current are cooling until minimum temperatures (-0.7°) are reached in April. An insignificant increase in temperature is observed in February in some years, caused by a short-term intrusion of the warm Irminger component onto the slope under the cold waters of Arctic origin.

The marked maxima and minima of the Labrador Current temperature are also observed in the West Greenland Current (Adrov, 1962; Külerich, 1943).

Off Newfoundland, monthly water temperature observations have been made since 1950 by Canadian scientists at station 27 (depth 172 m) at Cape Spear near the city of St. John's. This station is situated at the western edge of the coastal jet of the Labrador Current. Templeman (1965), using data from this station, constructed monthly mean isopleths for the period 1950-62 (Fig. 3, upper part). The figure clearly shows seasonal temperature variations in all the layers from the surface to the bottom. Maximum warming is in

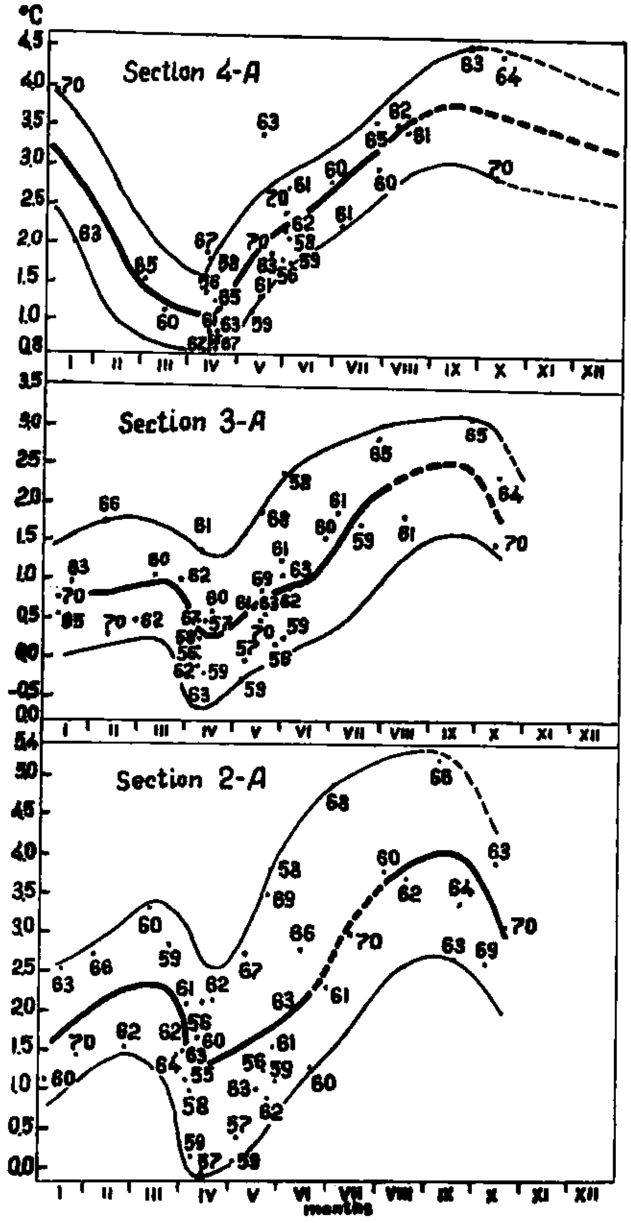


Fig. 4. Seasonal variation of temperature of the 0- to 200-m layer on sections 4-A, 3-A, 2-A (numbers mark the years of observations).

September, followed by uniform cooling to the minimum in March. Warm waters penetrate to about 100 m in November. In April, an increased volume of Arctic waters of below -1° temperature is observed along with initial surface warming. In July-August the Arctic waters reach the bottom.

Figure 3 (upper part) shows the monthly variation of mean temperature in the 0- to 172-m layer at station 27 for the period 1950-62. The curve has a minimum in March (down to -1°) and a maximum in October (up to 2.2°). Results available from a series of observations confirm this curve as typical of the temperature variation within a year's period in the coastal jet of the Labrador Current.

On sections across Grand Bank and Cabot Strait (1-A, 2-A, 3-A, 4-A, and 44-A) the most complete series of observations were made in the spring months (March-June).

In Fig. 4 curves of the yearly variation of averaged temperature of the 0- to 200-m layer are presented. They are constructed from data collected on sections 4-A, 3-A, and 2-A which cross the south-eastern and southern slopes of the Grand Bank. The curve for the section 4-A has an April minimum and an August maximum. On this section, stations from which the mean temperature for the 0- to 200-m layer was calculated (Fig. 1), were established in the waters where the influence of water mass transport is not so great as on sections 3-A and 2-A. On the southern sections (3-A and 2-A) the influence of the North Atlantic Current is considerably greater and causes the appearance of a second maximum (in March). On section 8-A, across the Labrador Current and on sections 3-A and 2-A across Labrador – North Atlantic Current, the second maxima and minima are also observed, caused by dynamic reasons.

The next group of curves (Fig. 5) is constructed from data collected from sections across the south-western slope of the Grand Bank (1-A) and the Cabot Strait (44-A) and has the minimum temperature in March-April and the maximum in August.

Comparison of the curves of the yearly variation of mean temperature on the sections in the Labrador and Newfoundland areas and at station 27, shows that two types of curves can be singled out, (1) the curves having two maxima and two minima (sections 8-A, 7-A, 6-A, 3-A, 2-A), and (2) the curves having one maximum and one minimum (station 27 and sections 1-A, 4-A, and 44-A) (Table 1).

Variability of temperature from year to year

Generalizations on water temperatures over standard sections in the area of Labrador-Newfoundland for 1936-41, 1949-59 were made by Elizarov and Zotov (1960) and Elizarov (1962) mainly from observations of the International Ice Patrol. These studies are continued in this paper.

Concerning the Labrador area (section 8-A), data on temperature of the 0- to 200-m layer of the cold component of the Labrador Current (AB) were adjusted to the 15 July and plotted for the years 1936, 1938-41, 1948-64, and 1969, (Fig. 6, curve 1) and to 1 November and plotted for the years 1958, 1962, 1964-70. (Fig. 6, curve 2). From 1950 to 1968, one can easily single out three periods, of 4 years each, and two periods, 3 years

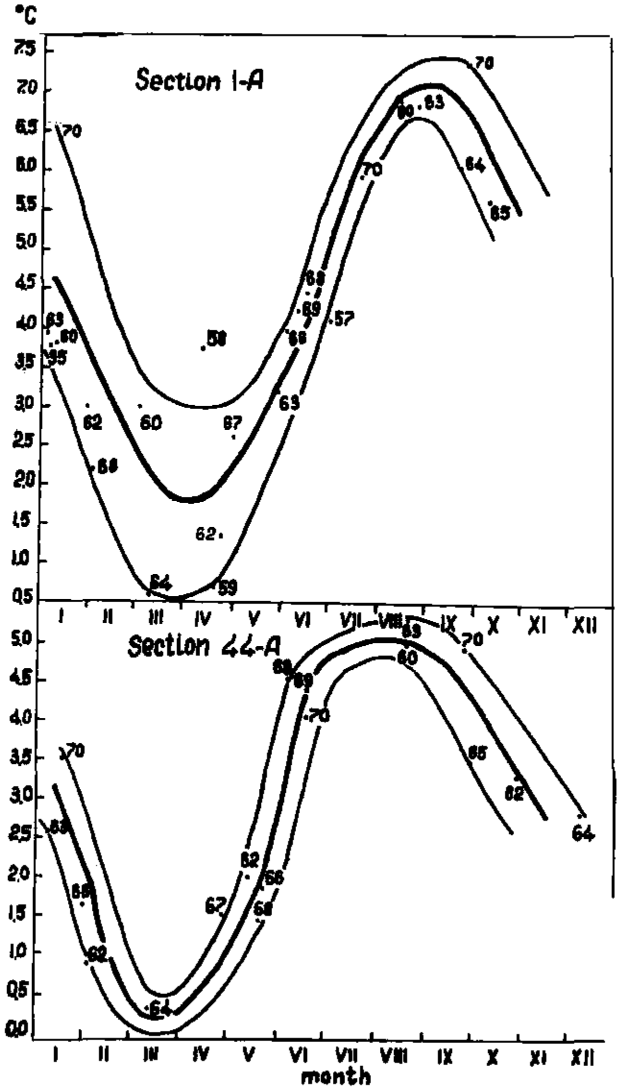


Fig. 5. Seasonal variation of temperature of the 0- to 200-m layer on sections 1-A and 44-A (numbers mark the years of observations).

TABLE 1. Periods of extrema in the annual variation of mean water temperature in hydrographic sections in the areas of Labrador, Newfoundland, and at station 27 (see Fig. 1 for location of sections).

Sections	Climatic		Dynamic	
	Minimum	Maximum	Minimum	Maximum
8-A, 7-A, 6-A	March-May	June-July	July-August	September-November
Station 27	March	October	–	–
3-A, 2-A	April	August-September	?	March
1-A, 4-A, 44-A	March-April	August	–	–

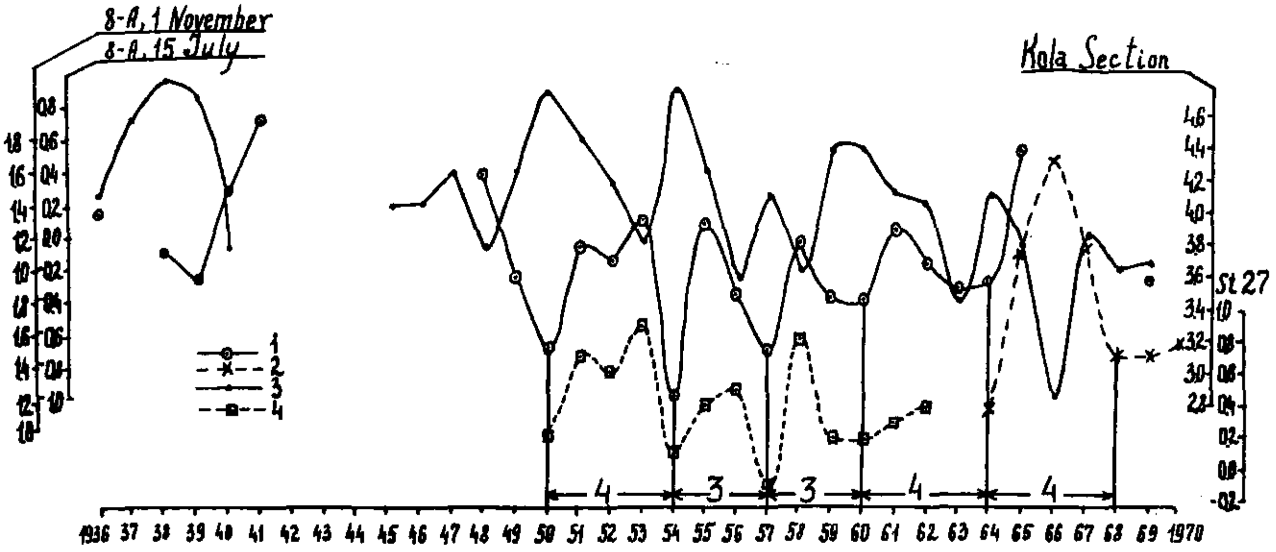


Fig. 6. Year to year fluctuations of temperature of the 0- to 200-m layer:

1) of the cold component of the Labrador Current on section 8-A, adjusted to 15 July for 1936, 1938-41, 1948-64, 1969;
2) adjusted to 1 November for 1958, 1962, 1964-70;
3) the mean annual temperature on section "The Kola Meridian" in the Barents Sea for 1936-40, 1945-69;
4) the mean annual temperature on station 27 and off Newfoundland for 1950-62.

each. As a rule, in each period, one can single out two "cold" years (1950, 1954, 1957, 1960, 1964, and 1968) and one "warm" year (1953, 1955, 1958, 1961, and 1966). From this one can assume that the last period, which began after 1968, will finish in 1971. Temperature variations for the period of 3-4 years are recorded in other parts of the ocean as well (Bochkov, Sarukhanyan, and Smirnov, 1968).

Izhevskii (1964) showed that temperature fluctuations in the Northwest Atlantic and in the Barents Sea occur in anti-phase. Elizarov (1962) showed this for the years 1936, 1938-41, 1948-59, and obtained a coefficient of correlation higher than 0.7. During the analysis of the data for this paper, it was found that the same anti-phase phenomenon is characteristic of the fluctuations of the annual mean temperature of the 0- to

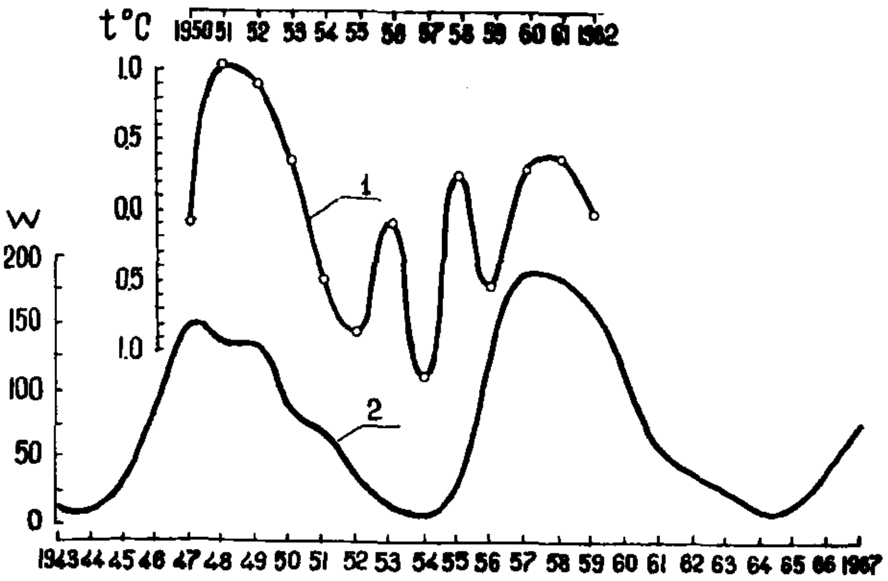


Fig. 7. Mean annual variations:
1) of surface temperature at station 27 in 1950-62;
2) of sun activity for 1943-67, as expressed by Wolf's digits.

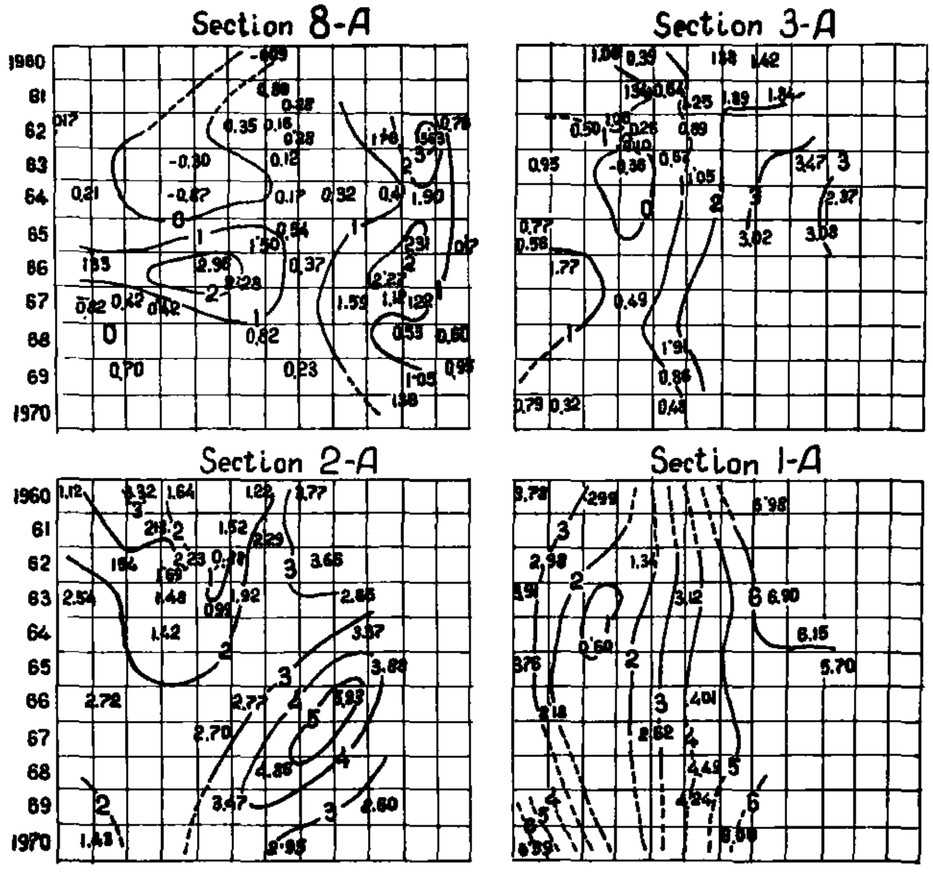


Fig. 8. Graphs of isopleths of the mean temperature of the 0- to 200-m layer for particular parts of sections 8-A, 3-A, 2-A, and 1-A, for 1960-70.

200-m layer on the Kola section of the Murmansk Current when compared with the temperature fluctuations on section 8-A in the cold component of the Labrador Current (AB), which was adjusted to 15 July for the years 1936, 1938-41, 1948-65, and to 1 November for the years 1964-70 (Fig. 6, curves 1, 2, and 3). It should be noted that the reverse relation is especially marked in the abnormally warm and cold years which are difficult to predict using the methods available (Bochkov *et al.*, 1968).

Then, year-to-year variations of the yearly mean temperature of the 0- to 172-m layer at the station 27 in the coastal jet of the Labrador Current were analysed (Fig. 6, curve 4). Data analysed were taken from Templeman (1965) and covered the period 1950-62. Figure 6 shows that temperatures fluctuated at station 27 in phase with the temperature of the 0- to 200-m layer in the cold component of the Labrador Current on section 8-A in July. The 4- and 3-year cycles for July found for section 8-A are clearly apparent in the yearly mean variations of temperature at the coastal station 27.

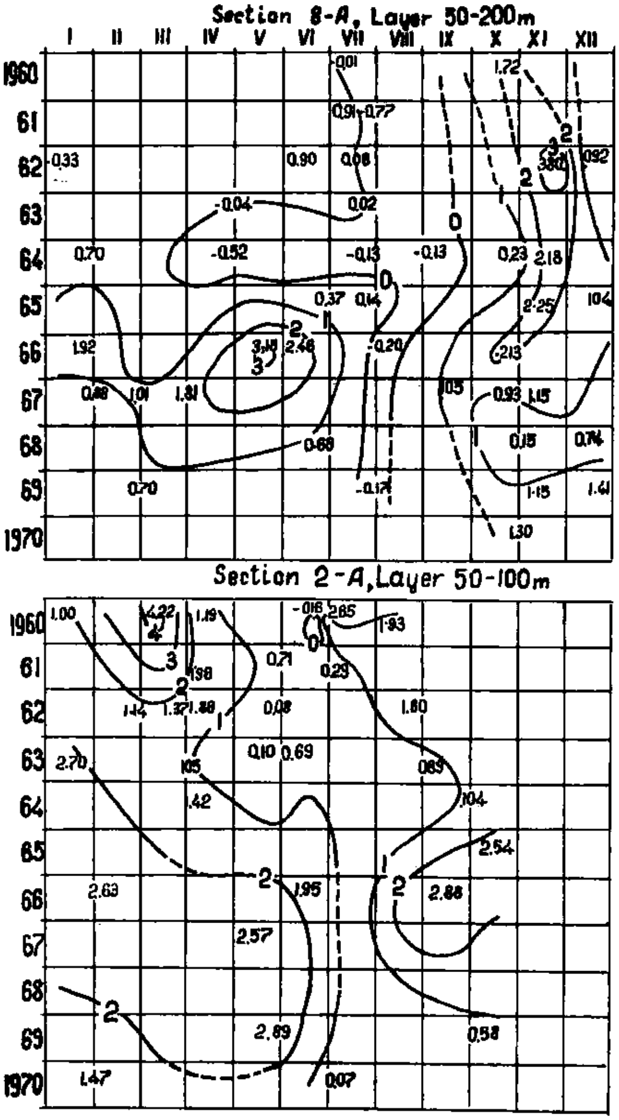
This marked synchronism of fluctuations in the temperature of the 0- to 200-m layer on section 8-A of the cold component of the Labrador Current and in the yearly mean temperature of the 0- to 172-m layer at station 27 suggests it is due mainly to the influence of the intensity of the Labrador Current.

The anti-phase relationship is observed between variations of temperature at station 27 and on the section "The Kola Meridian". The relationship was correlated for the period 1950-62. The correlation coefficient obtained is 0.523, the stability of the relation being less than 3. Thus, the presence of a reverse relation is proved, but is unstable, perhaps, due to the fact, that the relationship is curvilinear.

Identification of the periodicity of year-to-year variations of water temperature, related to the sun activity, was carried out at the station 27. The curve of the sun activity, expressed in Wolf's digits for the period 1943-67, is presented in Fig. 7 (curve 2). The relationship between the sun activity and the mean yearly temperature in the surface layer at station 27 can be shown as 3-4 years out of phase (Fig. 7, curve 1). Such a phase shift was registered also in the Barents Sea and is described by Bochkov *et al.*, 1968.

The most complete picture of temperature variations by months for 1960-70 can be obtained from the graphs of isopleths for each year (Fig. 8). These isopleths were compiled from data on the mean temperature of the 0- to 200-m layer in sections 8-A, 3-A, 2-A, and 1-A, off Labrador and the Grand Bank. The graphs show a cold wave in the first half of 1963 and 1964, while warm waves were registered in 1965-67. They were especially well marked in the second half of the year (sections 2-A and 8-A). The numbers of observations being large enough, the graphs of such isopleths represent the year-to-year variations. It is interesting that a considerable cooling or heating simultaneously affects the whole area from Labrador to the south of the Grand Bank, though, according to Izhevsky (1964), they are situated in areas which have different phases of the thermal regime.

This synchronism is clearly demonstrated by the waves of warmth (1965-67) and cold (1963-64) recorded on section 8-A in the 50- to 200-m layer in the Labrador


Section 8-A, Layer 50-200m

Section 2-A, Layer 50-100m

Fig. 9. Graphs of isopleths of the mean temperature of the 50- to 200-m layer on section 8-A(B) and of the 50- to 100-m layer on section 2-A, in 1960-70.

Current at Labrador and on section 2-A in the 50- to 100-m layer in the extreme south of the Grand Bank (Fig. 9).

Conclusions

Four types of curves of seasonal variation of temperature have been identified: (1) for the area of Labrador, with two minima – in April and in July, and with two maxima – in June and November; (2) for the coastal Newfoundland waters, for the eastern and northeastern slopes of the Grand Newfoundland Bank, with a minimum in March-April and a maximum in October; (3) for the southwestern slope of the Grand Newfoundland Bank, a minimum in March and a maximum in August; (4) for the southern and southwestern slopes of the Grand Newfoundland Bank, a minimum in April and a maximum in September, a small rise of temperature being recorded from January (February) to March.

Analysis of year-to-year temperature variation for section 8-A revealed 3- and 4-year cycles, which are in phases with the mean yearly temperature variations at station 27 in the coastal waters of Newfoundland.

Temperature fluctuations in section 8-A and at station 27 are in counter-phase with fluctuations of mean yearly temperature on section "The Kola Meridian" in the Barents Sea.

Waves of cold (1963-64) and warmth (1965-67) are revealed in graphs of isopleths of monthly variations of temperature. These waves have determined the thermal regime of waters for the last 11 years.

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Contribution Number 4

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Growth and Decay of Glaciers as an Indicator of Long-Term Environmental Changes

By O. H. Løken[1]

Abstract

Glaciers have for a long time been considered as sensitive indicators of climatic fluctuation but the nature of the glacier climate relationship has not been well understood. This is still the situation although great advances have been made in the last 10 to 15 years. The progress has come from two sources: (1) vastly improved understanding of the dynamic behaviour of glaciers and of their response to changes in the mass balance, and (2) greatly improved field data from several parts of the world due to scientific cooperation under the auspices of the International Geophysical Year (IGY) and the still ongoing International Hydrological Decade (IHD).

The theoretical studies, mainly contributed by J. F. Nye (e.g. Nye, 1960, and subsequent papers), show that fluctuations in the position of the ice margin is not a direct indicator of the "health" of the glacier. The longest established method of measuring ice front fluctuations is thus not a reliable method for studying climatic fluctuations. Surging glaciers complicate the problem further as they behave anomalously and their frontal fluctuations are governed by unknown factors. These glaciers are much more common than previously assumed.

The IGY and particularly the IHD lead to a sharp increase in glaciological studies. Comparable accumulation, abalation and net mass balance data are now available from several glaciers on an annual basis. In the North Atlantic area especially long and detailed records are available from two glaciers: Storbreen (Norway) and Storglaciären (Sweden). The records from these glaciers from the late 1940's to the present are considered in detail and supplemented with data from other glaciers. From the beginning of this period the glaciers suffered substantial loss of mass until the early 1960's when years with positive balance occurred. However, at the end of the decade, years with great loss have occurred again. Predictions for future trends are barely more than conjectured.

The recovery of cores from the deep hole drilled through the Greenland ice cap at Camp Century, has spurred further interest in studies of the late Pleistocene climatic record. The problem of extrapolating results from one spot to a larger area remain important.

The connection between oceanographic conditions and the climate of the adjacent land areas as these are recorded in glacier variations is poorly known, and further research may yield significant results.

Introduction

That glaciers fluctuate in size from year to year and also from decade to decade can easily be ascertained by comparing photographs of glacierized areas taken a few years apart. On a slightly longer time scale, the same phenomenon is documented in the historic records from the valleys in the Alps, in Norway and from other areas where advancing glaciers in the past have encroached upon farms and substantially influenced the lives of the

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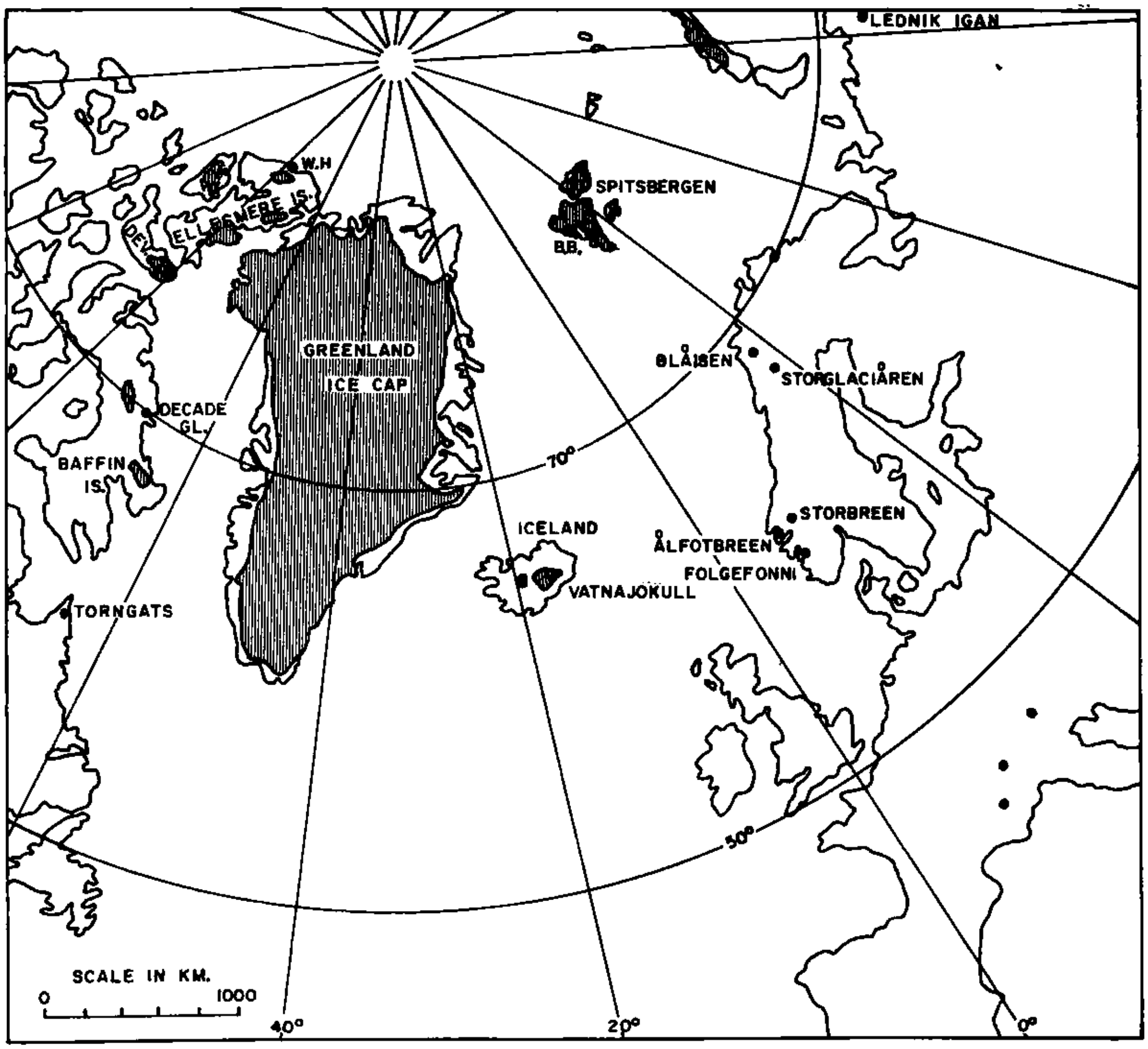


Fig. 1. The North Atlantic region and its glaciers (shaded areas). Many glaciers are too small to be shown, but their location and name are indicated.

farmers. On a still longer time-scale we know that most of Canada and Scandinavia and adjacent areas were ice covered some 10,000 years ago. These glacier fluctuations show a general correlation with variations in climatic parameters and hence with environmental changes, but the details of this relationship are not known.

This paper examines the above mentioned relationship and can be broadly divided in two sections: the first deals with the nature of the glaciers, their reaction to climatic changes and the nature and characteristics of the methods used to determine glacier fluctuations; the second section reviews the fluctuations of the glaciers in the North Atlantic region (Fig. 1) during the 1960's. Some reference is made to older records but these older fluctuations have been more fully treated by Ahlman (1953) and Ahlman et al (1970). Comments about the glacier climate relationship are made in the final part.

The Nature of Glaciers

Before turning to the question of glacier fluctuations, it will be useful to describe briefly what a glacier is and some of its main characteristics in the context of environmental change. An excellent review of what is known about glaciers and their behaviour can be found in Paterson (1969*b*).

A glacier is a moving body of ice which exists for many years. It may take the form of a small glacier, with a surface area of less than 1 km^2 nested in a depression in the mountain, or it may be a large ice sheet as we have on Greenland or in the Antarctic which both cover thousands of sq km and which completely mask the terrain underneath, except along the periphery. Between these two extremes we have a tremendous variety in shapes and sizes, but for the purpose of this paper it may be useful to envisage a valley glacier which originates in a high mountain massif and descends down into a valley (Fig. 2). Most of the following can be extrapolated to any type of glacier, although Weertman (1961) has discussed several interesting aspects of the stability of large ice caps or ice sheets, such as those of Antarctic and Greenland and this analysis is, of course, limited to such ice sheets. However, in the present context they are not particularly relevant.

Each year a glacier gains in volume from winter snowfall and loses volume due to melting in the summer period. While winter snowfall is the main source of "income", it may also gain by condensation of atmospheric moisture onto the glacier surface, by deposition of wind-blown snow, by snow avalanches from adjacent areas and, finally, it may gain by the freezing of rain which falls onto the glacier. The role of freezing rain is of particular importance in arctic (or generally in high latitude) areas where the glaciers have below freezing temperatures throughout the year. Summer snowfalls also occur frequently in arctic areas. The sum of all these gains is called the *accumulation.*

The growth of the glacier is checked by extensive melting during the summer period, particularly in the lower areas where the temperature is highest. However, the glacier may also lose volume by calving or by snow being blown off the glacier. It may also lose by evaporation and sublimation, but these processes are usually not very significant. The sum of all losses is called the *ablation.* The difference between the accumulation and the ablation gives the *mass balance* which is normally calculated on an annual basis. If the mass balance is positive, the glacier has gained in volume; if it is negative, it has lost.

It may be inferred from the above that winter precipitation and summer temperature are the key factors in determining glacier variations. This is actually true, but there is no simple relationship between these parameters (which can easily be obtained from standard meteorological observation) and variations in the glacier mass balance.

This is partly due to the fact that few meteorological stations are located near the glaciers, but it is even more important that summer melting is not determined by air temperature but by the amount of energy absorbed at the glacier surface. The amount of absorbed energy is in turn determined by (1) the amount of incident radiation, (2) the albedo of the glacier surface, and (3) the turbulent structure of the lowest part of the atmosphere. We have no standard meteorological observations by which we can easily determine these elements. Finally, it must be emphasized that a

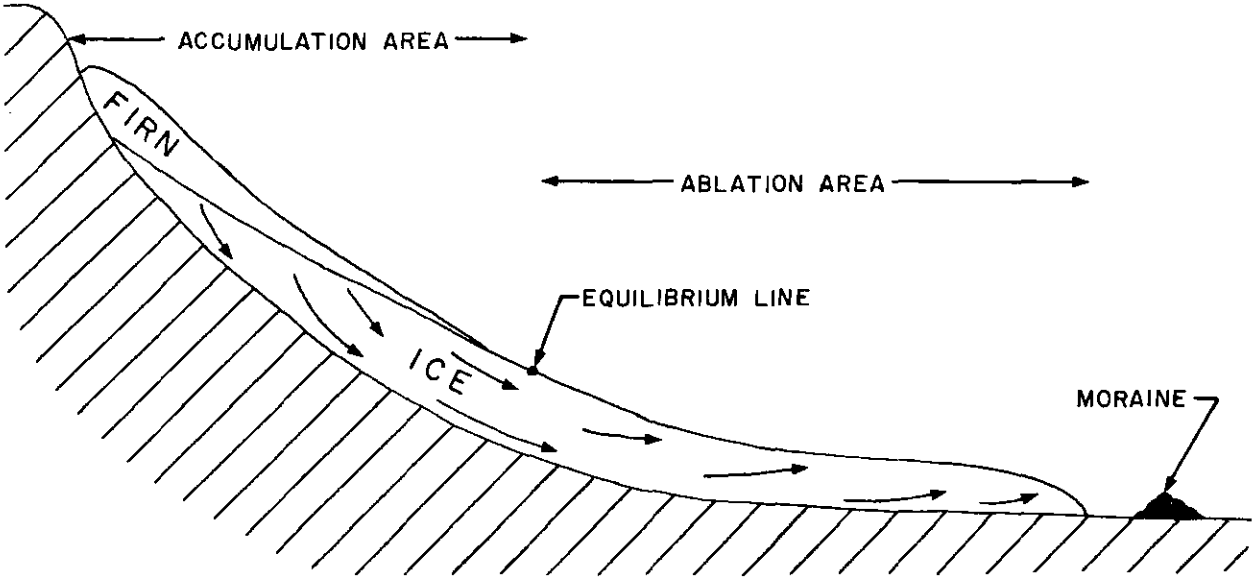


Fig. 2. Schematic profile of a small glacier showing the ice in the lower part and the firn (old snow) in the highest part. The accumulation and the ablation areas which respectively gain and lose mass due to precipitation and melting are separated by the equilibrium line.

certain mass balance can be caused by an infinite number of combinations of accumulation and ablation.

Mass balance on the lowest part of the glacier is normally negative and in the highest part it is positive or at least less negative. In order to retain a constant glacier thickness it is necessary that the loss in the lowest part is compensated for by influx of ice from the highest part. The rate of this flow depends on several factors, such as, the steepness of the glacier, temperature of the ice, variation in mass balance along the glacier, and also the variation of the mass balance from year to year. The way in which the glacier responds to these variations is referred to as its dynamic response characteristics and it has a major influence on the fluctuations of the glacier snout. Nye (1969) has given a brief review of this relationship, largely based on his very extensive theoretical studies of the problem (Nye, 1960, 1965 *a* and *b*).

The relationship between climatological conditions can be represented schematically as shown in Fig. 3 (modified from Meier, 1965). The illustration is simplified and several secondary influences are not shown, e.g., the local climate influence, the temperature of the glacier which in turn influences the dynamic response and the very existence of the glacier has an important influence on the local climate.

Evidence of Glacier Fluctuations

Moraine deposits and any other glacial landforms situated far from present-day glaciers are evidence of glacier fluctuations on a geological time scale. Other types of evidence were referred to in the introduction. The evidence is, however, of qualitative rather than quantitative nature. This section examines more closely the main types of evidence of the decay and growth of glaciers which lend themselves for quantitative studies on an annual basis. Three types of evidence are considered: (1) frontal fluctuations, (2) mass balance measurements, and (3) data from deep core studies.

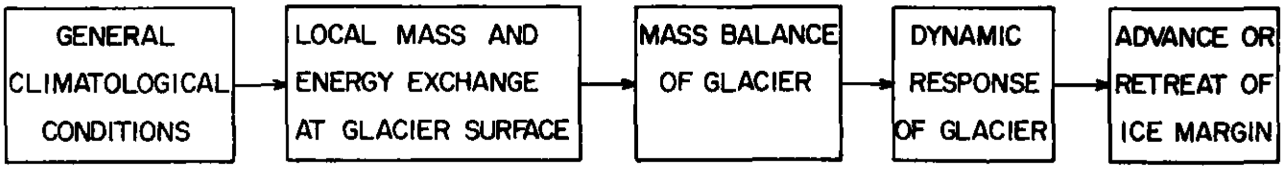


Fig. 3. Schematic diagram showing connection between meteorological conditions and glacier snout fluctuation (modified after Meier, 1965).

Frontal fluctuations

The position of the glacier snout in relationship to fixed points in the adjacent terrain has been recorded for more than a century in the Swiss and Austrian Alps and these data represent the longest quantitative records of glacier fluctuations in existence. Similar observations over shorter periods have been, and are still being collected from almost all the glacierized parts of the world. The measurements are normally taken at right angle to the front at the lowest part of the glacier, where it is most sensitive to changes in the climatic parametres. Over the last century the fronts of most glaciers have receded or retreated considerably, a fact which is often paraphrased as "the retreat of the glaciers". Such a statement may imply that the ice is actually moving back, but this is completely erroneous as the ice in *any* part of the glacier, including near the front, is continuously moving down valley.

The rationale behind the survey of frontal fluctuation is that climatically induced changes in the glacier volume must be reflected in the position of the snout. This is true, but recent theoretical studies particularly by Nye (1960) have shown that this relationship is very complex due to the formation of kinematic waves caused by variations in the mass balance. The analysis shows that rather small mass balance variations can create major fluctuations of the fronts, and that these frontal fluctuations are delayed by many years (order of 100 years) relative to the climatic fluctuation that caused them. The glacier's response depends on a complex set of factors such as the shape, slope, size of the glacier as well as its temperature, and on the amplitude and period of the mass balance fluctuation itself.

An initial examination of the theoretical analysis may lead one to regard all measurements of frontal fluctuations as useless, but it is possible to determine past mass balance fluctuations from knowledge of frontal fluctuations. This requires, however, much data and sufficient information is available only from a few glaciers (Nye, 1965*b*). It is worth noting that studies of the dynamic response give changes in net mass balance only and not in the two main components, accumulation and ablation.

Recently much interest has been focused on surging glaciers (Post, 1969). These glaciers show anomalous behaviour by sudden and rapid advances with ice velocities one to two orders of magnitude greater than normal, and several of them are known from the North Atlantic region, e.g. on Iceland (Thorarinson, 1969). Records of snout fluctuations from such glaciers cannot be treated in the same manner as those from normal glaciers, and must be evaluated with utmost caution.

Mass balance measurements

Modern studies of glacier fluctuation stress the need to investigate the changes in *volume* or thickness of the glacier, rather than in its *area* extent which was, although it may not be the essence of ice front measurements. These studies are referred to as mass balance measurements, and it is normally determined on an annual basis. The mass balance year on the Northern Hemisphere usually extends from September to September and does not coincide with the calendar year. Mass balance parametres are given in units of mass per unit area and are usually averaged over the whole area of the glacier.

With the exception of calving (from glaciers that terminate in water) and of snow deposits caused by snow drift or snow avalanches on to the glacier, the mass balance parametres are intimately related to climatic parametres. They therefore provide much better bases for the evaluation of the glacier-climate relationship than the data on frontal fluctuations, as the latter are greatly influenced by the dynamic response of the glacier. Furthermore, the net mass balance studies provide separate information on the accumulation and ablation rather than the mass balance only, which enters into the dynamic analysis. This is an important point because an indefinite combination of accumulation and ablation values can give the same mass balance, yet if one desires to study the details of the glacier-climate relationship the separate components must be investigated.

Mass balance measurements require that fieldwork be carried out *on* the glacier, therefore the ice surface must be reasonably free from crevasses. But many glaciers, particularly in steep-mountain areas such as the Alps, are unsuitable and even impossible, for such studies. The mass balance studies are much more laborious than the measurement of snout variations and the cost is accordingly greater. The cost-benefit ratio of the two types of studies must be considered, in view of the purpose of the measurements.

Core studies

In the accumulation area of a glacier new layers of firn are added to the glacier each year. The stratigraphy of a core taken there provides a record of the mass balance changes. Such core studies are of limited value in temperate glaciers where summer melting at the surface may lead to extensive infiltration of melt water into the snow and firn, thus destroying the stratigraphy. Core studies are therefore most useful in high Arctic and Antarctic regions. Extensive studies of such cores from Greenland (Dansgaard *et al.*, 1970) and Antarctica (Gow, 1970) have recently been made and others are in progress. Somewhat similar studies have been made earlier in deep snowpits (Hollin and Cameron, 1961) and from shallow cores (Schytt, 1955). Core studies, however, have only recently developed into a major tool in studying the history of glaciers and some characteristic features about the cores should be mentioned.

The stratigraphy of shallow cores provide a picture of the mass balance changes at a point in the top part of the glacier. At greater depths, say below 50 m, the stratigraphy is blurred and it can no longer be discerned. The depth to which the stratigraphy can be discerned varies from glacier to glacier; it is deepest in cold glaciers, shallowest in warmer glaciers.

Variations in the $0^{16}/0^{18}$ isotope ratio along an ice core can be very distinct (Dansgaard *et al.*, 1970) and they can be used to infer past temperature variations. Dust from the Krakatao explosion and the onset of above-ground testing of nuclear weapons provide distinct horizons to date relatively recent events, but in order to study older variations it is necessary to have data on the mass balance at the location one considers. As mentioned above, the lack of stratigraphy in ice cores from depths greater than about 50 m makes it impossible to determine mass balance fluctuations. Consequently it has been necessary to *assume* the past mass balance in order to date the temperature variations shown by the isotope ratios. Furthermore, due to glacier movement the core material is not from the same spot on the glacier, since the 'corepoint' is at a different point today than it was when the snow was deposited, e.g. 1,000 years ago. Not only has the 1,000 years old layer moved, but it has also been deformed in the process. Thus the dating of the various layers in the core involves assumptions about glacier flow as well.

Looking at the deep ice cores from the viewpoint of an environmentalist, rather than from the viewpoint of the strict glaciologist/climatologist, the cores are tremendously valuable. They actually conserve the old precipitation and hence its chemical and physical composition, although some correction must be made due to diffusion and other physical and chemical processes that may have occurred in situ. Precipitation in this context comprises snow and rain as well as volcanic dust and extra-terrestrial particles (Hamilton, 1970) and thus the cores are records of the past environmental quality.

Modern core analysis employs a series of methods such as grain-size studies, chemical analysis, spectrophotometry, neutron activation, etc. (Langway, 1970). No doubt glaciology, as well as other fields of science, will benefit greatly from such studies in the future.

International cooperation

This section would be incomplete without reference to the international cooperation on glacier fluctuation which has been established recently. It started during the International Geophysical Year (IGY) in 1957-58, but got a major impetus with the advent of the International Hydrological Decade (IHD) in 1965. At that time international agreement was reached on glaciological terminology and on the development of standard methods of recording, so that compatible results could be obtained from all areas. A central data depository was established in Switzerland to collect data through a series of national correspondents and to publish the results (Kasser, 1967).

An important part of the IHD glaciology program was the establishment of a network of glaciers so that a reasonable coverage of all glacierized areas could be obtained. Three global chains of glaciers, where detailed mass balance measurements are made, form the core of this network; two chains run East-West at latitudes of about 50° and about 70°N respectively, the third chain runs North-South along the west coast of the Americas.

The Pattern of Recent Glacier Variations

The preceding section shows that the record of mass balance observations provides the most valuable data for the study of recent glacier variations, because these are the only data that give information about the change of the whole glacier. Admittedly, the available data is thus severely restricted as there are very few glaciers with long records of mass balance observations, but it is felt that the gain in quality outweighs the loss in quantity.

This section deals with the recent mass balance changes in the North Atlantic region. They will be treated on a regional basis, starting in Scandinavia, where we have the most detailed records, and then going westwards in a counterclockwise direction.

Scandinavia

Storglaciären in northern Sweden and Storbreen in Jotunheimen, Norway, have the most detailed and longest mass balance records in the area under consideration (Liestøl, 1967, 1969, 1970; Schytt, 1962, 1964, 1965, 1966, and later personal communications). The records extend from 1945 (Storglaciären) and 1948 (Storbreen) to the present and we thus have records for 25 and 22 years, respectively (Fig. 4). Both glaciers have suffered substantial losses, e.g., on Storglaciären only 4 of the 25 years show a positive mass balance. On Storbreen, 8 of the 22 years have shown a positive balance. Over the period of record a point on Storglaciären has had a total loss of 1,990 g/cm^2 or an ice layer of about 22 m; the average loss was 79.5 g/cm^2 per year. On Storbreen the total loss has been 895 g/cm^2 or an average of 40.5 g/cm^2 per year. (Mass balance figures are always given as the water equivalent of the ice and/or snow which was gained or lost.)

The mass balance parametres show great variation from year to year and it is difficult to discern any trends, but the following is of particular interest:

1) The early part of 1950's had consistently, but predominantly small, negative mass balances on Storglaciären when Storbreen had 2 years with small positive balances;

2) A period of excessive losses followed in the late 1950's and early 1960's. The largest negative balance ever observed on Storbreen was recorded in 1958-59. On Storglaciären the year 1959-60 was the most negative with the exception of 1946-47, i.e., the memorable year with the extremely warm and dry summer;

3) A series of years with predominantly small negative or positive mass balances followed from about 1962 to 1968. Storbreen, for example, had a net gain over the period 1963-68;

4) An abrupt change occurred in 1968 when the period of 'near-blance' of the mid 1960's was followed by 2 years of very negative balance.

The Norwegian Water Resources Board (Østrem and Pytte, 1968) has carried out mass balance studies on a great number of glaciers in Norway, but their records

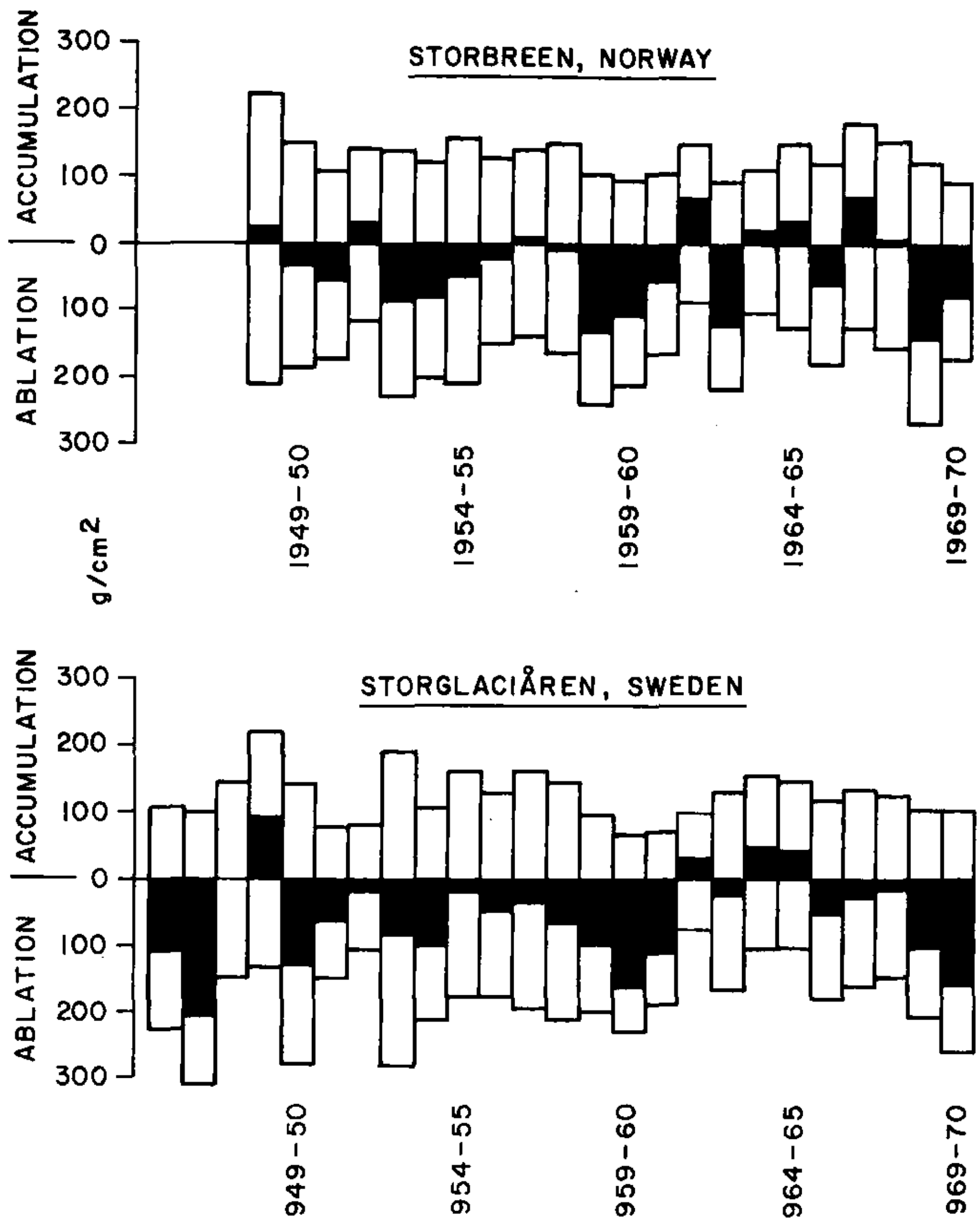
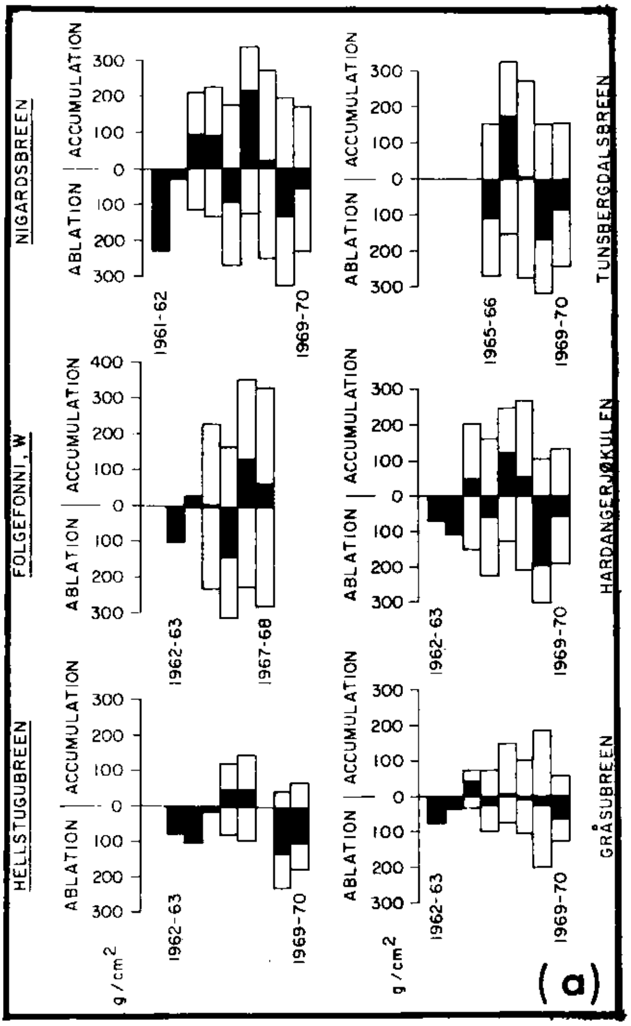
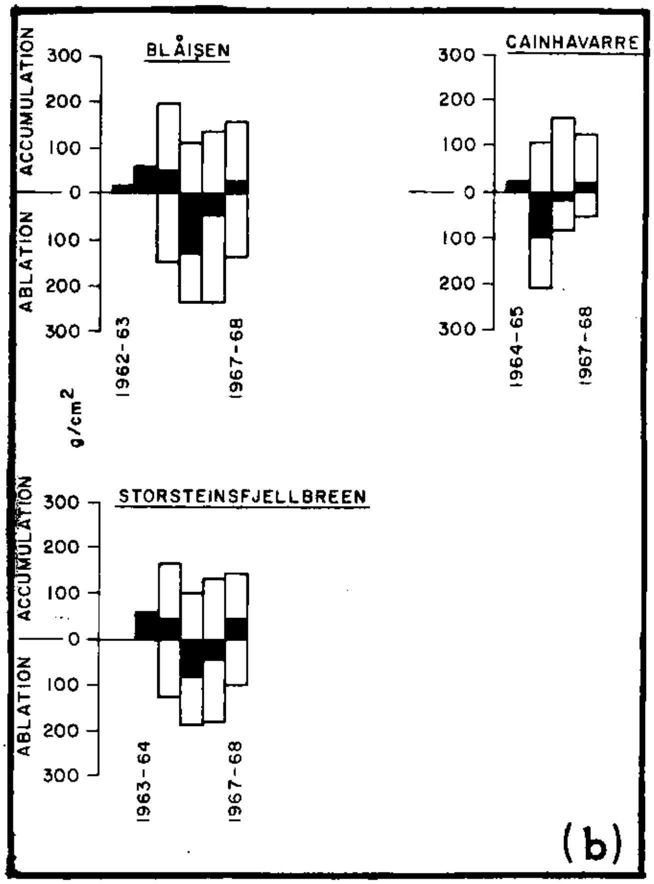


Fig. 4. Mass balance records from Storbreen, Jotunheimen, Norway (after Liestøl 1967, 1969, 1970, and personal communications) and from Storglaciären, Northern Sweden (after Schytt 1960, 1962, 1963, 1965, 1966, and personal communications).

are much shorter (Fig. 5a, b and, c). In Southern and Northern Norway the glaciers followed closely the pattern outlined above for Storbreen and Storglaciären: The negative years at the beginning of the 1960's were followed by generally positive years in the middle part of the decade and then by an abrupt change to negative mass balances for the last 2 years (1968-69 and 1969-70). The year 1965-66 had a noticeably negative balance, when compared with the adjacent years. The negative balance can be seen at all glaciers except on Hellstugubreen.

It is of interest to note the differences in the accumulation and ablation values between the various





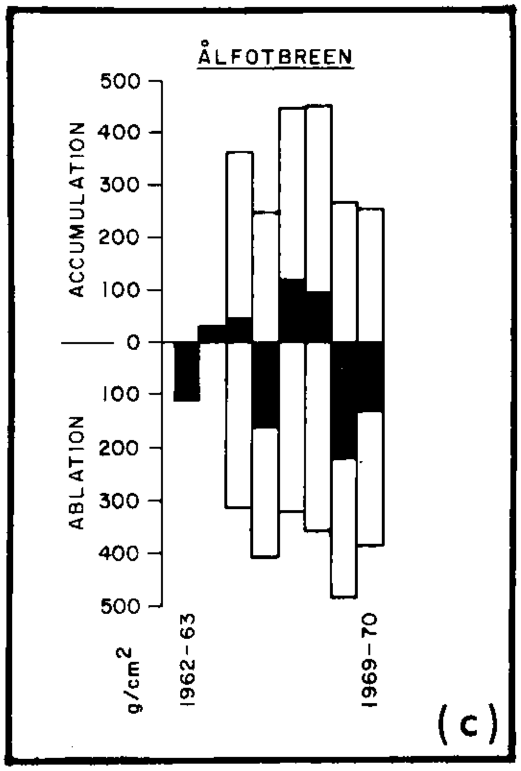


Fig. 5. Mass balance records from glaciers in (a) Southern Norway, (b) Northern Norway, and (c) from Alfotbreen, Western Norway (data from Østrem and Karlen [1962], Østrem and Liestøl [1964], Østrem and Pytte 1968, Pytte [1969, 1970], Pytte and Østrem, [1965]).

glaciers. The contrast is most distinct between Ålfotbreen (Fig. 5c), and Hellstugubreen and Gråsubreen glaciers. Ålfotbreen has typical accumulation and ablation figures between 300 and 400 g/cm^2 while the corresponding values from the others never exceed 200 g/cm^2. Ålfotbreen lies very close to the west coast of Norway while the others are some 200 km inland in the precipitation shadow on the lee (eastern) side of the highest mountains.

Polar Urals

Mass balance studies started on selected glaciers in the Polar Urals during the IGY and have been continued since (Grosswald and Kotlyakov, 1967). The Lednik IGAN glacier had a consistently negative mass balance from 1957 to 1967, the major losses occurring in 1962-64. In the following 3 years there has been a trend to more positive (less negative) balance (Fig. 6.).

The record from the Lednik Obrucheva glacier, started 2 years later, confirms the trend at the previous glacier; 1962-64 had very negative balances, while subsequent years had more positive balances. In fact, the two last years on record (1965-67) were both positive. The following year (1967-68) gave a positive mass balance for both glaciers (personal communication, V. Kotlyakov, 1971).

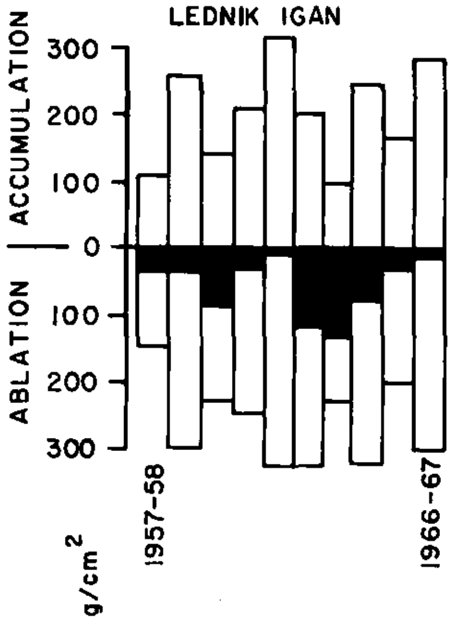
The overall trend seems to be different from that observed in Scandinavia although individual years show strong similarity, e.g., 1961-62 had the most positive mass balance between 1957 and 1965 and 1962-63 was strongly negative. The latter year was, however, followed by an even more negative year in the Urals while a positive balance occurred in 1963-64 on both the Scandinavian glaciers. The distinctively negative balance which was observed in Scandinavia in 1965-66 had no parallel in the Polar Urals.

Spitsbergen

Norsk Polar Institut has carried out mass balance studies on three glaciers (Liestøl, 1969) but the published record is too short for interpretation. They show, however, the trend towards increasing negative balances after 1968, and thus seem to react in the same way as the Skandinavian glaciers.

Greenland

Mass balance studies of the type discussed above have not been done on Greenland because the ice cap is too large and it is not possible to integrate spot values over the whole area. However, spot surveys have been made at certain locations and the net balance for a series of years has been determined. Only net values are obtained in this way and some have been presented by



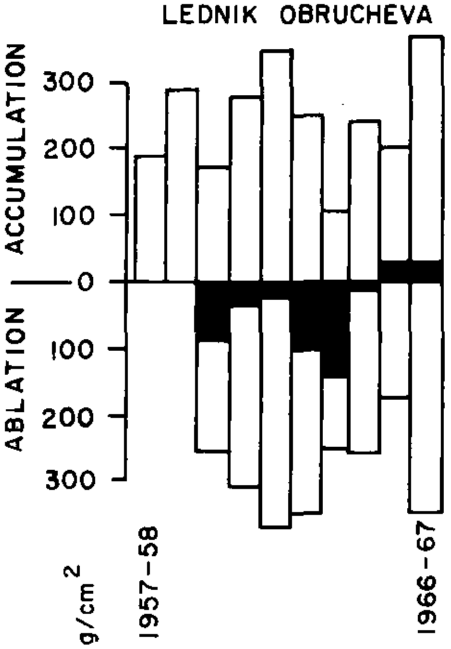


Fig. 6. Mass balance records from glaciers in the Polar Urals (from Grosswald and Kotlyakov 1969).

Möck (1968), but they cannot be used to determine the overall 'health' of the ice cap.

Of greater regional interest are the results obtained by relevelling of the EGIG (Expedition Glaciologique International au Groenland) profile across Greenland. Under the auspices of this expedition a profile was in 1959 carefully levelled across the ice cap in an east-west direction from Disko Bugt on the west coast. The western half of this profile was resurveyed in 1968 (Seckel, 1968) and a comparison shows that the ice cap surface had risen over all but about 25 km of the 400 km long profile. Only near the ice margin was the surface lower; in other places it had risen as much as 140 cm over the 9 year period. The smoothed change was much less and it averaged about 70 cm over a distance of 250 km. The results show that a considerable buildup of the ice cap is taking place in this area. How representative this is to the whole ice cap is not known.

Ellesmere Island

Hattersley-Smith (1963) and Hattersley-Smith and Searson (1970) have conducted mass balance studies on the Gilman Glacier, on the Ward Hunt Ice Shelf and on the adjacent Ice Rise. The Gilman Ice Cap studies show that the net balance, as measured in a pit in the accumulation area, had a distinct maximum in 1950-51 and fell off steadily until 1956-57. Then it started to rise until the end of the record in 1960-61.

On the Ward Hunt Ice Rise (Fig. 7) the period 1958-62 had a negative balance; it was followed by 3 years of positive balance. After 1965 the mass balance was once again negative, but less so than in the early part of the decade. The published observations terminated in 1968.

The record shows a remarkable similarity to that from Scandinavia, e.g., the one from Blåisen Glacier.

Axel Heiberg Island

Müller (1968) has published mass balance data from the White Glacier for the period 1959-62. The 3 years, 1959-60 to 1961-62 had mass balances of -40.5, +5.0 and -74.5 g/cm^2, respectively. For the three following years the mass balance is only given as "negative" "strongly positive" and "positive", respectively. Since then, only data on the elevation of the equilibrium line have been given, and Fig. 8 shows its trend over a 10 year period.

The equilibrium line elevation can be used to give a qualitative expression for the mass balance when used in conjunction with some actual mass balance data. These show that an equilibrium line elevation of about 950 m corresponds to a zero mass balance. The higher the equilibrium line the more negative is the mass balance.

The record from White Glacier shows that the glacier lost volume in the early part of the 1960's, but that it has had predominantly positive mass balance since: the year 1965-66 was an exception. This year, it will be recalled, was an exception in Scandinavia, where it also was recorded as a year of particularly large negative value.

Müller (1968) also reports the results from stratigraphic studies in a deep snow pit, where he found that the mass balance changes show little trend since about 1930. The study shows that between 1930 and 1920 (the bottom of the pit) there were fewer ice layers, i.e., the summers were cooler.

Meighen Island Ice Cap

Arnold (1965) and Paterson (1969) have published

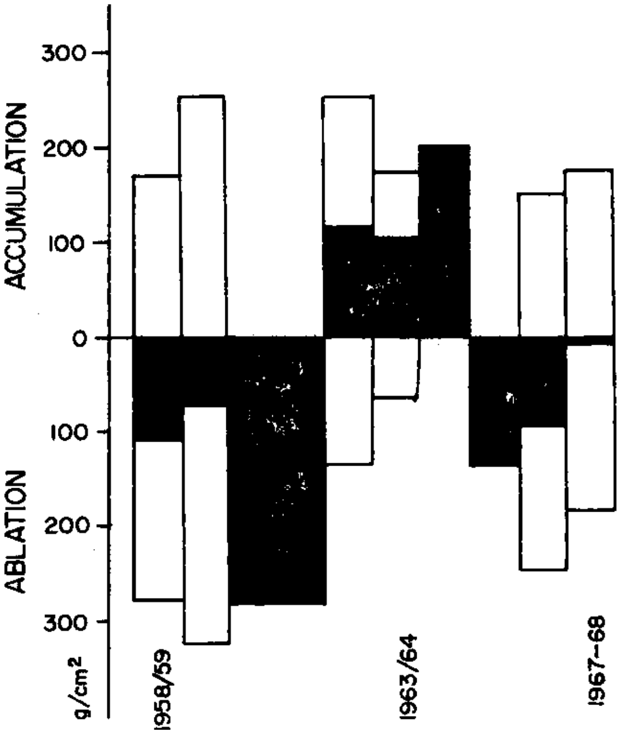


Fig. 7. Mass balance record from Ward Hunt Ice Rise, Ellesmere Island (data from Hattersley-Smith and Serson, 1970).

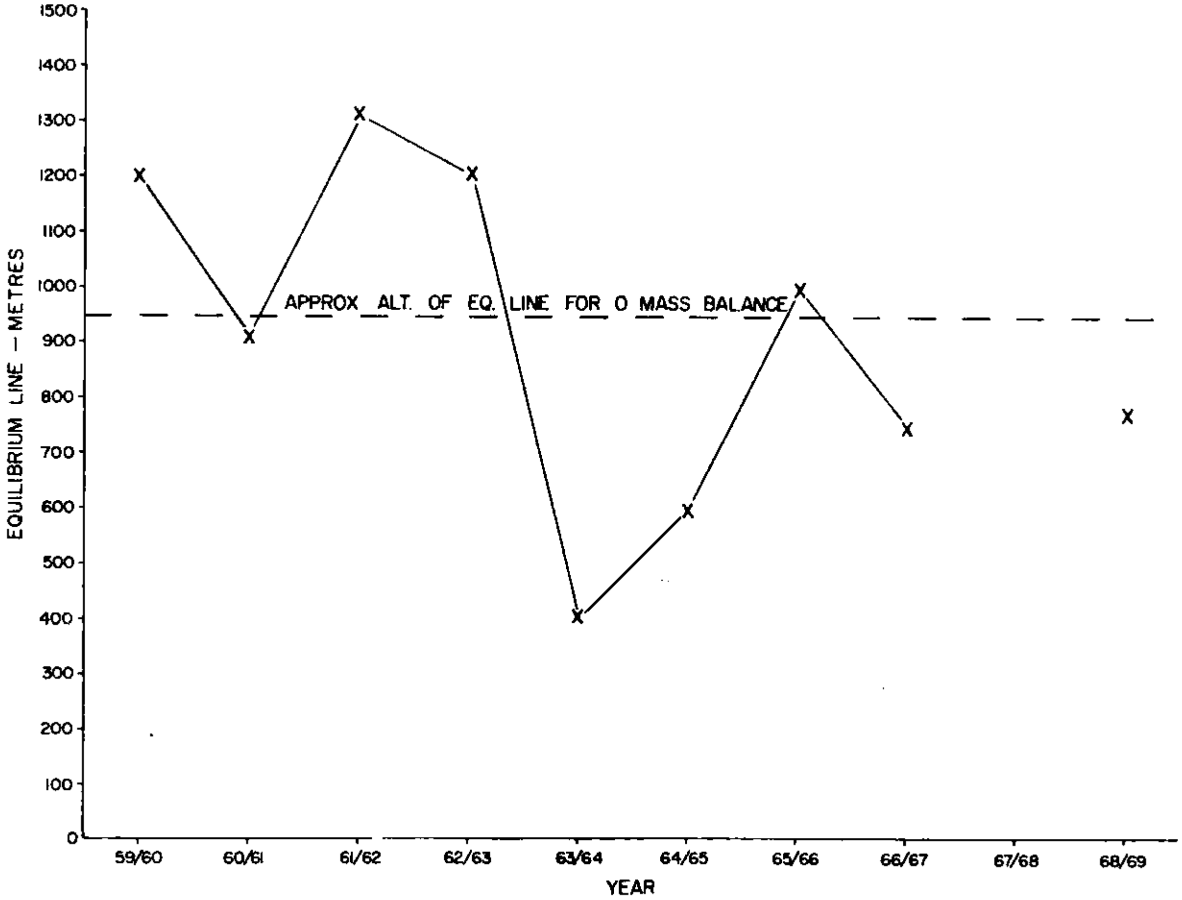


Fig. 8. Equilibrium elevations for White Glacier, Axel Heiberg Island as a function of time, (after data from Müller [1969] and "Ice" 1967, 1968, and 1970).

mass balance data from this ice cap; the latter author provided the longest record and the following is largely based on his publication. Figure 9 shows the mean values of stake readings from each of the 7 years of record. Arnold (1965) shows that the stakes are not representative for equally large areas and the mean values are therefore not necessarily representative for the whole ice cap. However, they are considered to be useful for comparison between the various years. They also give an indication of the mass balance for the whole ice cap.

Two means are plotted for each year; one is the mean of *all* stake readings for each year (the number varied from 20 to 35), the other mean is based on readings at the 18 stakes that were read *each* year. The most northern and lowest part of the ice cap is underrepresented in the latter group, but the rather small difference between the two sets of figures is taken as proof that the mean figures actually are good expressions for the overall mass balance of the whole ice cap.

The data show that negative mass balances prevailed from 1959 to 1963, with 1961-62 as the most negative year. The two following years, 1963-64 and 1964-65 were both positive, while a negative mass balance was recorded in 1965-66. The pattern follows closely that recorded on White Glacier on Axel Heiberg Island. This is to be expected as the distance between the two glaciers is only about 100 km.

Devon Island Ice Cap

Koerner (1970) provides the most detailed mass balance data from this area. He divides the ice cap into four sectors. The northwest sector has the most detailed record, hence, only data from that part is considered here; they are shown graphically in Fig. 10. The great losses in 1960-61 and particularly in 1961-62 are followed by the more positive years thereafter; it follows again the overall pattern of other glaciers described from Arctic Canada. It should be noted, however, that the

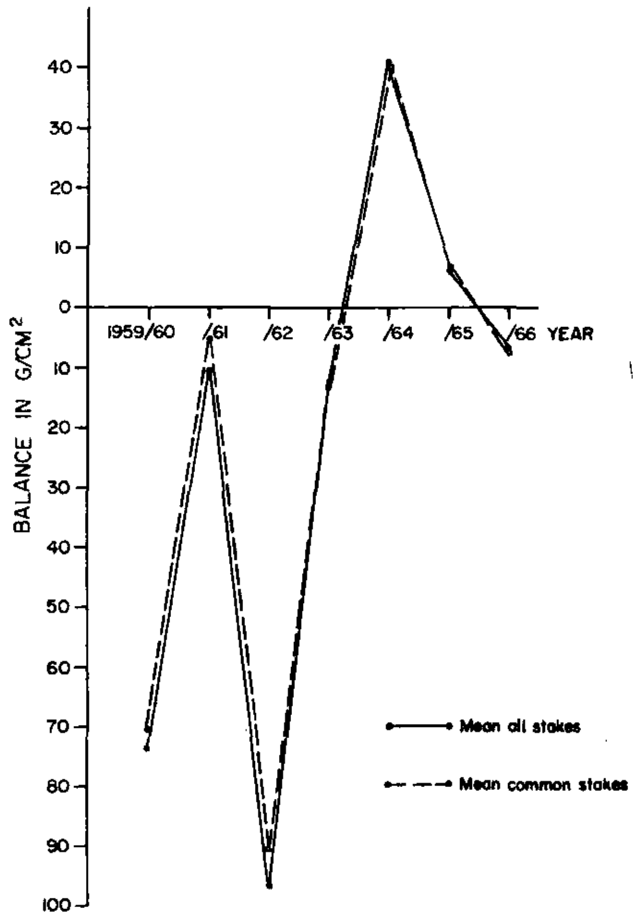


Fig. 9. Mass balance data from Meighen Island Ice Cap (data from Paterson 1969).

year 1962-63 was positive on the Devon Ice Cap and on the Ward Hunt Ice Rise, but it was a negative year on White Glacier and on the Meighen Ice Cap. Thus these western glaciers appear to behave somewhat differently than the more eastern ones.

Baffin Island

Løken and Sagar (1968) reports mass balance data from the Barnes Ice Cap, but the longest and most detailed mass balance record from Baffin Island is from Decade Glacier (Østrem, Bridge and Rannie, 1967; and U. Embacher, personal communication) which has been surveyed since the beginning of the IHD (Fig. 11). The 6 year record is dominated by 2 years (1965-66 and 1968-69) with extremely large negative mass balances. The other years show small absolute values. The two negative years coincides with major negative years of most of the glaciers described above.

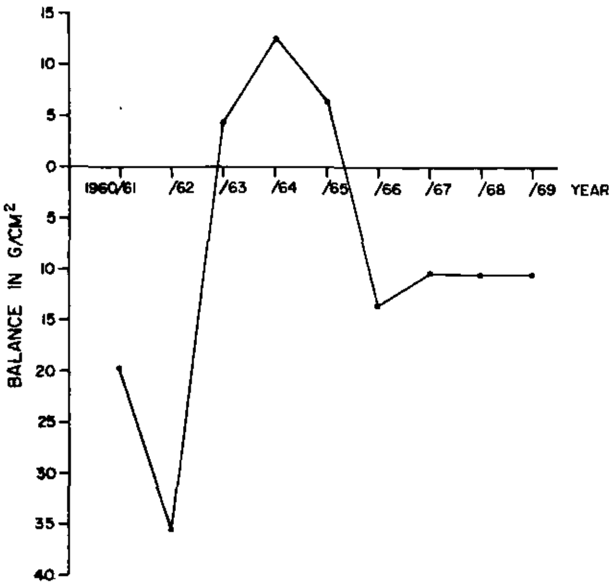


Fig. 10. Mass balance record from northwestern sector of Devon Island Ice Cap (data from Koerner 1970).

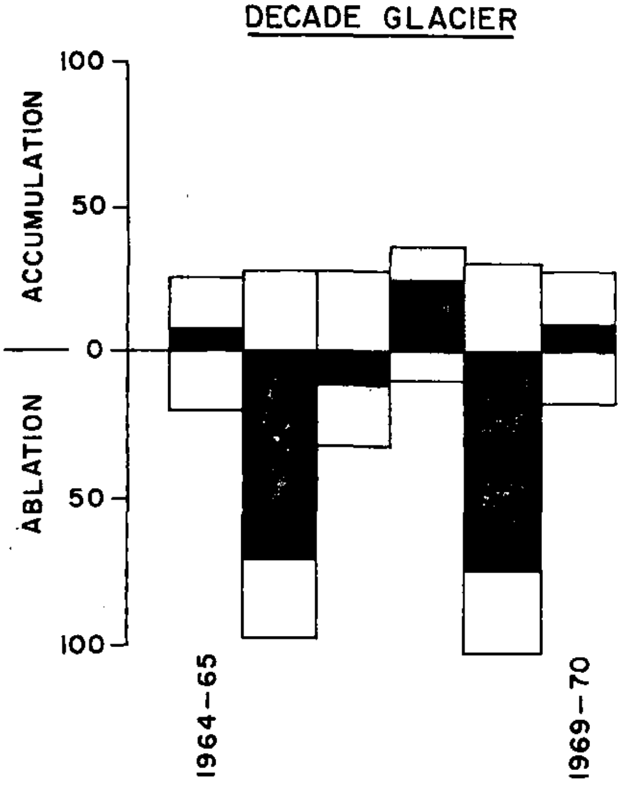


Fig. 11. Mass balance record from Decade Glacier, Baffin Island (from U. Embacher, personal communication and Østrem, Bridge and Rannie, 1967).

An interesting feature of Decade Glacier is the almost constant accumulation which vary little from year to year. The rather steep slope of the glacier surface and the predominantly windy winter climate are believed to be responsible for this phenomenon.

Discussion

It is evident from the preceding sections that the glaciers in the North Atlantic area has diminished in volume over the last 2-3 decades. This is clearly seen from the records for Storbreen and Storglaciären in Scandinavia and it is inferred from the other glaciers as their behaviour has been very similar during the periods of overlapping records. The glaciers in the Polar Urals show a different pattern of behaviour; they appear to diminish at an even faster rate.

The glaciers discussed in this paper show a surprising similarity in their mass balance, considering the large area over which they are scattered. What is valid for the North Atlantic region can however not necessarily apply to a larger area: for example, Hoinkes (1968) has shown that the Scandinavian glaciers seem to be out of phase with those in the Austrian Alps; positive mass balances in the former area frequently coincide with negative values from the latter. The glaciers of the Caucasus region seem to behave in the same way as the Austrian glaciers. Lednik Dzhankuat thus had a strongly negative mass balance in 1967-1968 while in the next year the mass balance was almost 0 and in the following year 1969-70 was strongly positive. This pattern is very different from that followed by the North Atlantic glaciers.

It is not surprising that this difference occurs because the meteorological conditions which govern the mass balances of glaciers are sensitive to the changes in the general circulation, particularly to the position of the major pressure systems and the jet stream. It is therefore natural that if a change in the circulation pattern leads to a more favourable mass balance on some glacier it will lead to the opposite on some others.

Observations of the ice margin retreat have been published for some of the glaciers discussed above and an excellent record is available for Storbreen (Liestøl 1967). The record shows that the margin has retreated every year since 1950. Although there are 2 years without observations the measurements taken the following year leaves little doubt that the margin retreated every year. The greatest single year retreat was recorded in 1961-62 when the margin receded 25 m. Even in 1952-53 and 1953-54 the retreat was 20.0 and 18.3 m respectively.

Most glaciers show a steady retreat of the ice margin over the last 2 decades, but not all are retracting, as Briksdalsbreen in Norway has advanced in 12 of the last 20 years. This illustrates the pitfalls of using snout variations as indicators of growth or decay of glaciers.

Throughout this discussion emphasis has been on the mass balance alone rather than on its composite parts; accumulation and ablation. A more detailed investigation of the glacier climate relationship must be based on a study of both of these, to see what part the accumulation and the ablation play. While there can be no doubt that we can gain a much better understanding of the nature of glacier variations than we have now, it may not be possible to find a direct relationship between the two. This is due to the carryover effect from one year to the next. A year of positive mass balance will leave a large amount of firn with high albedo which will tend to delay ablation in the following year. Had this second year on the other hand been preceded by a negative mass balance year we would have had much more extensive melting under the identical climatological conditions.

It is believed that much can be learned about the glacier-climate relationship by detailed studies of the prevailing meteorological conditions during two years with extreme negative and extreme positive mass balances respectively. Detailed studies of special anomalous situations would also be usefull. In view of the overall parallel trends in the mass balance fluctuations in the North Atlantic area it would be instructive to examine the conditions in 1961-62 when all the glaciers in the Canadian Arctic and also Nigardsbreen in Norway had large negative balances while Storbreen and Storglaciären both had positive balances.

Acknowledgements

I am greatly indebted to Mrs L Arsenault for gathering much of the data for this paper and for preparing the diagrams. Mr C. S. L. Ommanney and Dr A. D. Stanley read the manuscript and provided many valuable comments.

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Discussion

Dr Rodewald: *"Last week in Hamburg, I heard a lecture on modern polar research given by Professor Fritz Loewe from Melbourne, Australia. Among other things he mentioned that the International Glaciological Greenland Expedition (EGIG) had determined the altitudes of the Greenland ice-cap by levelling in 1959 and again in 1968. The second measurements showed a mean increase in altitude of 0.8 m. A decrease only occurred at the outer western edge (0-15 km) of the ice-sheet."*

Dr Løken: *"I know something about this. This certainly shows that the ice-cap is building up in the middle. These, I think, are the first absolute measurements which we have of what is really going on in that section of the ice-cap."*

Dr Dickson: *"If I understood you correctly, Dr Løken, you are looking for conditions of stable persistence of "blocking" over Northwest Europe since these situations lead to decreased accumulation and increased ablation as far as the Scandinavian ice sheets are concerned, and hence, years with this persistent situation are the negative mass-balance years. We, at Lowestoft, have already looked at the occurrence of this type of situation since it appears to have a great bearing on the inter-annual change in the hydrography of the European shelf seas. Every 4 years or so a high pressure anomaly cell becomes established over Scandinavia and through ocean atmosphere feedback effects, may persist for up to a year. Our charts cover each 6-month period from 1949 to the present and refer to the 500 millibar level. The incidence of this persistent blocking situation in the last twenty-odd years corresponds almost exactly to your incidence of negative mass balance. If you would like to see these charts I can let you have them. I might add, incidentally, in reference to something Dr Campbell said that I don't think we have evidence of long-term changes in what Dr Løken has shown us. If I might use a geological expression we have the difference between an anticline and an anticlinorium. He is describing the bumps while I was describing the underlying long-term trend."*

Dr Løken: *"Yes, I think we would have to look at specific weather types or weather situations and see what is happening at the glaciers during this set of conditions. I am interested in your comment that this blocking situation has existed for at least 1 year and it will certainly be useful to compare the results of your blocking studies with the glaciological results. If this would enable us to relate the mass-balance variations to the occurrence of the weather type we could then look at what the general circulation specialists could provide in terms of long-range forecasts for the atmospheric circulation. If the glaciologist and meteorologist will be able to get together and establish a link between what is going on at say the 700 millibar level and at the surface of the glaciers then I think we will have made a big step forward. This is something which we hope to do in Canada. With increasing hydroelectric power development in the western part of the country it is important to forecast in the early part of the summer what quantity of water we can expect to get from melting glaciers in the late part of the summer when the melt is most intense. Operators of big hydro developments virtually get tears in their eyes when they see water running over the dam in late summer due to sudden and intense glacier melt which they had not foreseen. With the type of developments we have on the Columbia River, for example, an inch of water is estimated to be worth a half-million dollars in electric power. Thus considerable economic interest is attached to this type of study."*

Dr Dickson: *"It might cheer Dr Løken to know that, in at least one case, a persistent blocking situation over Northwest Europe was linked by Bjerknes to anomalous events in the eastern tropical Pacific. It looks as though he may be in for some warmer fieldwork in future."*

Dr Løken: *"I am quite sure that we will have to go to the Pacific for this type of information. The relation which has been established between sea surface temperature and the displacement of the Rossby waves in the upper atmosphere must have implications for our glacier studies. I think, however, that part of the problem is fairly well in hand; the critical part is between the 500 millibar level and the glacier surface. Here we know very little about the linking mechanism. I am therefore continually emphasizing the need for mesoclimatic investigations rather than the micrometeorological studies which have been made in so many cases."*

Dr Dickson: *"Do you mean that a displacement in the position of pressure cells can occur between the 500 millibar level and the surface? We looked into this carefully since it is also obviously important to our work. We found, in fact, that there was no significant lateral displacement between 18,000 ft and ground level."*

Dr Løken: *"Now you are referring to individual pressure systems and what you say is true. I was thinking in terms of a link between the events at the 500 millibar level and the heat and mass exchange at the surface because this is really the fundamental problem."*

Mr Fichaud: *"You mention that in setting the growth or decay of glaciers you should not really go too far afield and should also limit yourself to perhaps a year-to-year type of study. But if you do this and if you try to draw conclusions about the climate that you find to be the cause of these decays in growth, would you not be restricting yourself to perhaps a study of the microclimate or at best the mesoclimate and not the macroclimate? Don't we want to look at the macro changes on a large scale and a fairly large time span?"*

Dr Løken: *"I think we might have different interpretations of macro and meso scales. If you are averaging on the macro scale, you might lose, for example, the changes in the north Pacific because these might be compensated by opposite changes in the Atlantic. If that happens you have lost a vital fact. It's this type of averaging which is extended over too large an area that I am afraid of."*

Mr Fichaud: *"But if you restrict yourself to one glacier and are not even concerned with the next glacier?"*

Dr Løken: *"When I talk about meso scale I am talking about scales of up to hundreds of kilometers, when I talk about macro scale I am talking about thousands of kilometers. I am very much aware, as I mentioned in my introduction, that we have to make sure that the glaciers we are studying are representative and that they are not a unique feature which will react in a very special way different from the adjacent glaciers. This problem intrigues us very much and is being given careful attention. The question of providing forecasts is also important. There is a tendency to consider a large amount of scientific research as an attempt to explain today with a fancy theory what we already know happened yesterday without much thought about predictions. The aim of all studies, I think, is to develop models which will allow us, on the basis of what we know today, to predict what is going to happen tomorrow, next week or next month. This is the direction in which we are developing our studies of the glacier-climate relationship."*

Contribution Number 5

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Ice and Its Drift Into the North Atlantic Ocean

By Captain Robertson P. Dinsmore[1]

Abstract

A chronological review is made of sea and berg ice conditions prevailing over the Northwest Atlantic Ocean during the last half century. Emphasis is placed on maximum and minimum conditions. Attempts are made to correlate conditions with periodic cycles and meteorological conditions.

A discussion is presented of the development of ice reports and observation techniques from shipboard reports through serial reconnaissance and satellite observation. The observation is made that earlier, more severe, records are based on a paucity of ship data which, however scarce, are often duplicative and resulted in the interpretation of heavier ice conditions than modern ice reconnaissance will substantiate.

The developing usage of satellite observations and remote sensing imagery shows the need for new approaches to ice reporting and codes. This development also must keep pace with the increasing need for ice information by the oil, fishing and sea transportation industries.

Introduction

Arctic ice every spring and summer drifts southward into the North Atlantic Ocean (often surprisingly far) and presents at such times a menace to ships traversing the ocean, north of 40°N lat.

Although Southwest Greenland also experiences an ice season in both sea ice and icebergs, the ice menace to navigation is greatest off Newfoundland where the prevalence of fog, the congregation of icebergs, the concentration of traffic, and the presence of fishing vessels all contribute to a situation which in the past has resulted in marine disaster.

The general regime of ice drift into the North Atlantic Ocean is shown in Fig. 1. A perusal of the history of navigation prior to the turn of the century impresses one with the great number of casualties that befell vessels in these waters. Collisions between east and westbound ships first elicited more attention than did the perils of ice, and thus it was in 1855, that there were proposed separate lane routes, with the eastbound lane just south of Cape Race and the westbound lane near the Tail of the Grand Banks. Serious mishaps with ice continued to be frequent until 1875 when the Cunard Line adopted a system of tracks, the southern ones of which were laid south of the normal ice limits. The added safety of these provisions caused other large companies to join, and the North Atlantic Track Agreement was formed in 1898. The loss of the Titanic in 1912 brought about the International Convention for the Safety of Life at Sea which further established the International Ice Patrol. In 1970, however, shipping companies abrogated the Track Agreement on the premise that modern communications and radar minimized the ice hazard. It is therefore especially important that scientific and systematic studies of ice conditions be diligently continued to provide the mariner and ocean industries with greater information on this significant environmental factor.

Ice Terminology

In describing the nature and behavior of any natural phenomenon it is important to define the terms which are used. There is then some assurance that the interpretation of information will be uniform and close to what is intended. The World Meteorological Organization (WMO) has done much work in ice definitions and

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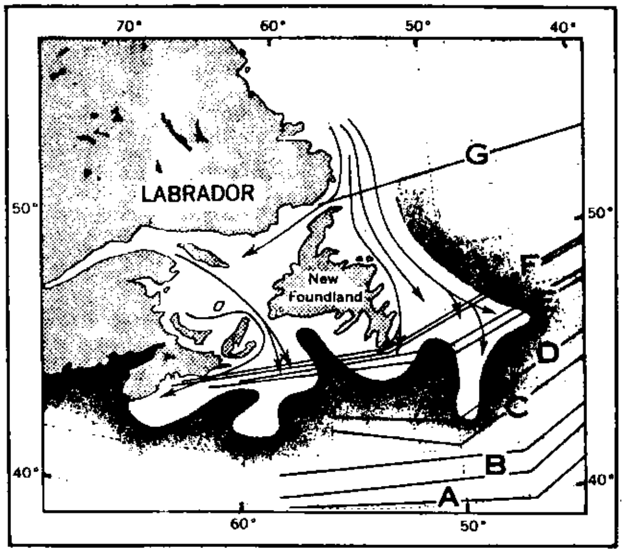

Fig. 1. Major drifts of ice and shipping tracks in the Northwest Atlantic Ocean.

in standardizing the terminology used in describing sea ice and the codes for reporting it. This organization is currently working towards uniform charting methods and is publishing an illustrated Glossary. A summary of the WMO definitions is included in Table 1.

Ice in the sea consists mainly of either ice formed by the freezing of top layers of the ocean or icebergs originating from glaciers or continental ice sheets. Sea ice probably accounts for 95% of all ice encountered; however, bergs are important because of the manner in which they drift from their points of origin to become navigation hazards. A certain amount of ice in the sea originates as fresh water ice in rivers or estauries; however, as it is already in a state of deterioration when it reaches the open sea, its importance is local.

Glacial ice is described generally by its size. *Ice Islands* may range from several hundred meters to several nautical miles in breadth, are low (5-10 m) and tabular in above water shape. *Icebergs* are usually irregular in shape and are the size of a large building or ship. *Bergy bits* and growlers are smaller masses having approximate above water sizes of large and small houses respectively (Fig. 2).

Sea Ice must be decribed by several terminologies. These include:

Ice Age – which refers to its stage of development or thickness; Concentration – which is the amount of ice cover on the sea surface; Ice Forms – which refer to the size and arrangement of the ice floes and the deformation processes (relief and topography).

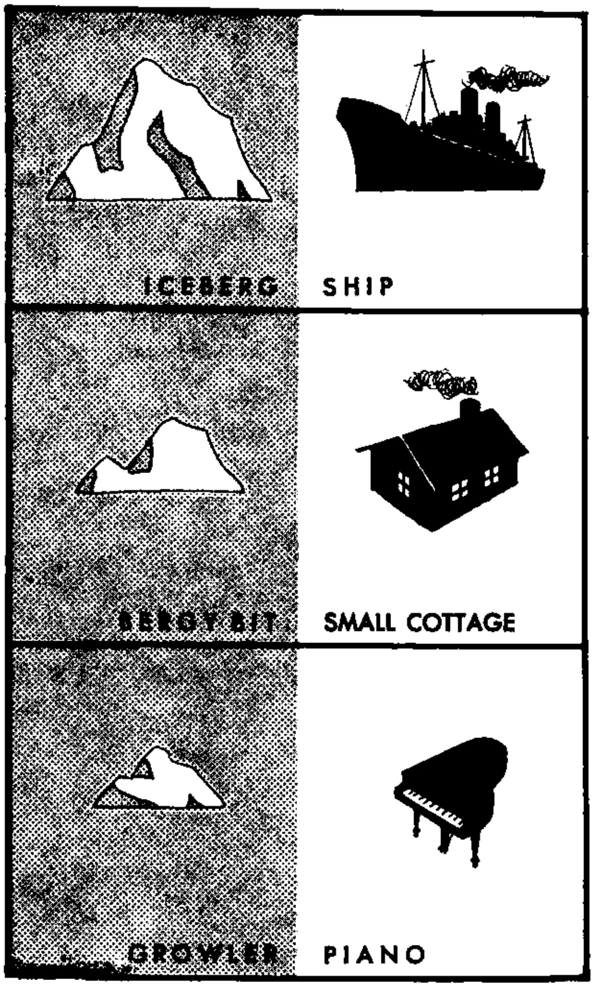

Fig. 2. Comparative sizes of glacial ice.

Each of these can be further described as follows: (see also Table 1)

Ice Age

New Ice – A general term for recently formed ice which includes frazil ice, grease ice, slush, and shuga. These types of ice are composed of ice crystals which are only weakly frozen together (if at all) and have a definite form only while they are afloat.

Nilas – A thin elastic cruist of ice, easily bending on waves and swell and under pressure, thrusting in a

TABLE 1. Glossary of Ice Terms (after World Meteorological Organization, 1968).

Anchor Ice – Submerged ice attached or anchored to the bottom, irrespective of the nature of its formation.

Bare Ice – Ice without snow cover.

Belt – A large feature of pack ice arrangement; longer than it is wide; from 1 km to more than 100 km in width.

Bergy Bit – A large piece of floating glacier ice, generally showing less than 5 m above sea level but more than 1 m and normally about 100-300 sq m in area.

Beset – Situation of a vessel surrounded by ice and unable to move.

Bight – An extensive crescent-shaped indentation in the ice edge, formed either by wind or current.

Brash Ice – Accumulations of floating ice made up of fragments not more than 2 m across, the wreckage of other forms of ice.

Bummock – From the point of view of the submariner, a downward projection from the underside of the ice canopy; the counterpart of a hummock.

Calving – The breaking away of a mass of ice from an ice wall, ice front, or iceberg.

Close Pack Ice – Pack ice in which the concentration is 6/8 to less than 7/8 composed of floes mostly in contact.

Compact Pack Ice – Pack ice in which the concentration is 8/8 and no water is visible.

Concentration – The ratio in eighths or tenths of the sea surface actually covered by ice to the total area of sea surface, both ice covered and ice free, at a specific location or over a defined area.

Consolidated Pack Ice – Pack ice in which the concentration is 8/8 and the floes are frozen together.

Consolidated Ridge – A ridge in which the base has frozen together.

Crack – Any fracture which has not parted.

Dark Nilas – Nilas which is under 5 cm in thickness and is very dark in color.

Deformed Ice – A general term for ice which has been squeezed together and forced upwards in places (and downwards). Subdivisions are rafted ice, ridged ice, and hummocked ice.

Fast Ice – Sea ice which forms and remains fast along the coast, where it is attached to the shore, to an ice wall, to an ice front, between shoals or grounded icebergs. Vertical fluctuations may be observed during changes of sea level. Fast ice may be formed in situ from sea water or by freezing of pack ice of any age to the shore, and it may extend a few meters or several hundred kilometers from the coast. Fast ice more than one year old may be prefixed with the appropriate age category, old, second-year, or multi-year. If it is thicker than about 2 m above sea level it is called an ice shelf.

Finger Rafting – Type of rafting whereby interlocking thrusts are formed, each floe thrusting "fingers" alternatively over and under the other. Common in nilas and gray ice.

First-year Ice – Sea ice of not more than one winter's growth, developing from young ice; thickness from 30 cm-2 m. May be subdivided into thin first-year ice/white ice, medium first-year ice, and thick first-year ice. Formerly termed winter ice.

Flaw – A narrow separation zone between pack ice and fast ice where the pieces of ice are in chaotic state, that forms when pack ice shears under the effect of a strong wind or current along the fast ice boundary (cf. shearing).

Floe – Any relatively flat piece of sea ice 20 m or more across. Floes are subdivided according to horizontal extent as follows:
Giant – Over 10 km across
Vast – 2-10 km across
Big – 500-2,000 m across
Medium – 100-500 m across
Small – 20-100 m across

TABLE 1. (continued)

Floeberg – A massive piece of sea ice composed of a hummock, or a group of hummocks, frozen together and separated from any ice surroundings. It may float up to 5 m above sea level.

Fracture – Any break or rupture through very close pack ice, compact pack ice, consolidated pack ice, fast ice, or a single floe resulting from deformation processes. Fractures may contain brash ice and/or be covered with nilas and/or young ice. Length may vary from a few meters to many kilometers.

Frazil Ice – Fine spicules or places of ice suspended in water.

Frost Smoke – Fog-like clouds due to contact of cold air with relatively warm water, which can appear over openings in the ice or leeward of the ice edge and may persist while ice is forming.

Glacier Ice – Ice in or originating from a glacier, whether on land or floating on the sea as icebergs, bergy bits, or growlers.

Glacier Tongue – Seaward projecting extension of a glacier, usually afloat. In the Antarctic, glacier tongues may extend over many tens of kilometers.

Grease Ice – A later stage of freezing than frazil ice when the crystals have coagulated to form a soupy layer on the surface. Grease ice reflects little light, giving the sea a matte appearance.

Gray Ice – Young ice 10-15 cm thick. Less elastic than nilas and breaks on swell. Usually rafts under pressure.

Gray-White Ice – Young ice 15-30 cm thick. Under pressure more likely to ridge than to raft.

Grounded Ice – Floating ice aground in shoal water.

Growler – Smaller piece of ice than a bergy bit or floeberg, often transparent but appearing green or almost black in color, extending less than 1 m above the sea surface and normally occupying an area of about 20 sq m.

Hummock – A hillock of broken ice which has been forced upward by pressure. May be fresh or weathered. The submerged volume of broken ice under the hummock, forced downwards by pressure, is termed a bummock.

Hummocking – The pressure process by which sea ice is forced into hummocks. When the floes rotate in the process it is termed screwing.

Iceberg – A massive piece of ice of greatly varying shape, more than 5 m. above sea level, which has broken away from a glacier, and which may be afloat or aground. Ice bergs may be described as tabular, dome-shaped, sloping, pinnacled, weathered, or glacier bergs.

Iceberg Tongue – A major accumulation of icebergs projecting from the coast, held in place by grounding, and joined together by fast ice.

Ice Blink – A whitish glare on low clouds above an accumulation of distant ice.

Icebound – A harbor, inlet, etc., is said to be icebound when navigation by ships is prevented by ice, except possibly with the assistance of an icebreaker.

Ice Boundary – The demarcation at any given time between fast ice and pack ice or between areas of pack ice of different concentrations (cf. ice edge).

Ice Breccia – Ice pieces of different age frozen together.

Ice Cake – Any relatively flat piece of sea ice less than 20 m across.

Ice Edge – The demarcation at any given time between the open sea and sea ice of any kind, whether fast or drifting. It may be termed compacted or diffuse (cf. ice boundary).

Ice Field – Area of pack ice consisting of any size of floes, which is greater than 10 km across (cf. ice patch).

TABLE 1. (continued)

Icefoot – A narrow fringe of ice attached to the coast, unmoved by tides, and remaining after the fast ice has moved away.

Ice Free – No sea ice present. There may be some ice of land origin (cf. open water).

Ice Front – The vertical cliff forming the seaward face of an ice shelf or other floating glacier varying in height from 2-50 m or more above sea level.

Ice Island – A large piece of floating ice extending about 5 m above sea level which has broken away from an Arctic ice shelf, having a thickness of 30-50 m and an area of a few thousand square meters to 500 or more square kilometers, usually characterized by a regularly undulating surface which gives it a ribbed appearance from the air.

Ice Jam – An accumulation of broken river ice or sea ice caught in a narrow channel.

Ice Keel – From the point of view of the submariner, a downward-projecting ridge on the underside of the ice canopy; the counterpart of a ridge. Ice keels may extend as much as 50 m below sea level.

Ice Limit – Climatological term referring to the extreme minimum or extreme maximum extent of the ice edge in any given month or period based on observations over a number of years. Term should be preceded by "minimum" or "maximum" (cf. mean ice edge).

Ice Massif – A concentration of sea ice covering hundreds of square kilometers found in the same region every summer.

Ice Patch – An area of pack ice less than 10 km across.

Ice Rind – A brittle shiny crust of ice formed on a quiet surface by direct freezing or from grease ice, usually in water of low salinity. Thickness to about 5 cm. Easily broken by wind or swell, commonly breaking into rectangular pieces.

Ice Shelf – A floating ice sheet of considerable thickness showing 2-50 m or more above sea level and attached to the coast. Usually of great horizontal extent and with a level or gently undulating surface. Nourished by annual snow accumulation and often also by the seaward extension of land glaciers. Limited areas may be aground. The seaward edge is termed an ice front (q.v.).

Ice Stream – An ice pack area that is under the established influence of any permanent ocean current.

Ice Under Pressure – Ice in which deformation processes are actively occurring, hence a potential impediment or danger to shipping.

Lake Ice – Ice formed on a lake, regardless of observed location.

Large Ice Field – An ice field over 20 km across.

Lead – Any fracture or passageway through sea ice which is navigable by surface vessels.

Light Nilas – Nilas which is more than 5 cm in thickness and rather lighter in color than dark nilas.

Medium First-year Ice – First-year ice 70-120 cm thick.

Medium Floe – (See Floe).

Medium Fracture – 200-500 m wide.

Medium Ice Field – An ice field 15-20 km across.

Multi-year Ice – Old ice up to 3 m or more thick which has survived at least two summer's melt. Hummocks smoother than in second-year ice, and the ice is almost salt-free. Color, where bare, is usually blue. Melt pattern consists of large interconnecting irregular puddles and a well-developed drainage system.

New Ice – A general term for recently formed ice which includes frazil ice, grease ice, slush, and shuga. These types of ice are composed of ice cystals which are only weakly frozen together (if at all) and have a definite form only while they are afloat.

Nilas – A thin elastic crust of ice, easily bending on waves and swell and under pressure, thrusting in a pattern of interlocking "fingers" (finger rafting). Has a matte surface and is up to 10 cm in thickness. May be subdivided into dark nilas and light nilas.

TABLE 1. (continued)

Nip – Ice is said to nip when it forcibly presses against a ship. A vessel so caught, though undamaged, is said to have been nipped.

Old Ice – Sea ice which has survived at least one summer's melt. Most topographic features are smoother than on first-year ice. May be subdivided into second-year ice and multi-year ice.

Open Pack Ice – Pack ice in which the ice concentration is 3/8 to less than 6/8 with many leads and polynyas, and the floes are generally not in contact with one another.

Open Water – A large area of freely navigable water in which sea ice is present in concentrations less than 1/8. When there is no sea ice present the area should be termed ice free, even though icebergs are present.

Polynya – Any nonlinear-shaped opening enclosed in ice. Polynyas may contain brash ice and/or be covered with new ice, nilas, or young ice; submariners refer to these as skylights. Sometimes the polynya is limited on one side by the coast and is called a shore polynya, or by fast ice and is called a flaw polynya. If it recurs in the same position every year, it is called a recurring polynya.

Puddle – An accumulation of melt water on ice, mainly due to melting snow but in the more advanced stages also to the melting of ice. Initial stage consists of patches of melted snow.

Rafted Ice – Type of deformed ice formed by one piece of ice overriding another (cf. finger rafting).

Rafting – Pressure processes whereby one piece of ice overrides another. Most common in new and young ice (cf. finger rafting).

Ram – An underwater ice projection from an ice wall, ice front, iceberg, or floe. Its formation is usually due to more intensive melting and erosion of the unsubmerged part.

Ridge – A line or wall of broken ice forced up by pressure. May be fresh or weathered. The submerged volume of broken ice under a ridge forced downwards by pressure is termed an ice keel.

Ridged Ice – Ice piled haphazardly one piece over another in the form of ridges or walls. Usually found in first-year ice (cf. ridging).

Ridging – The pressure process by which sea ice is forced into ridges.

River Ice – Ice formed on a river, regardless of observed location.

Rotten Ice – Sea ice which has become honeycombed and which is in an advanced state of disintegration.

Sea Ice – Any form of ice originating from the freezing of sea water.

Second-year Ice – Old ice which has survived only one summer's melt. Because it is thicker and less dense than first-year ice, it stands higher out of the water. In contrast to multi-year ice, summer melting produces a regular pattern of numerous small puddles. Bare patches and puddles are usually greenish-blue.

Shearing – An area of pack ice is subject to shear when the ice motion varies significantly in the direction normal to the motion, subjecting the ice to rotational forces. These forces may result in phenomena similar to a flaw (q.v.).

Shore Lead – A lead between pack ice and the shore or between pack ice and an ice front.

Shuga – An accumulation of spongy white ice lumps, a few centimeters across; formed from grease ice or slush and sometimes from anchor ice rising to the surface.

Slush – Snow which is saturated and mixed with water on land or ice surfaces, or as a viscous floating mass in water after a heavy snowfall.

Small Fracture – 50 to 200 m wide.

Small Ice Cake – An ice cake less than 2 m across.

Small Ice Field – An ice field 10-15 km across.

TABLE 1. (continued)

Strip – Long narrow area of pack ice, about 1 km or less in width usually composed of small fragments detached from the main mass of ice and run together under the influence of wind, swell, or current.

Tabular Berg – A flat-topped iceberg. Most tabular bergs form by calving from an ice shelf and show horizontal banding (cf. ice island).

Thick First-year Ice – First-year ice over 120cm thick.

Thin First-year Ice/White Ice – First-year ice 30-70 cm thick.

Tongue – A projection of the ice edge up to several kilometers in length caused by wind or current.

Vast Floe – (See Floe).

Very Close Pack Ice – Pack ice in which the concentration is 7/8 to less than 8/8.

Very Open Pack Ice – Pack ice in which the concentration is 1/8 to less than 3/8 and water preponderates over ice.

Very Small Fracture – 0 to 50 m wide.

Water Sky – Dark streaks on the underside of low clouds indicating the presence of water features in the vicinity of sea ice.

Weathered Ridge – Ridge with slightly rounded peaks; slope of sides usually 30° to 40°. Individual fragments are not discermible.

Weathering – Processes of ablation and accumulation which gradually eliminate irregularities in an ice surface.

Winter Ice – Formerly used to describe first-year ice. Used locally in Greenland to refer to fast ice.

Winter Pack – A conglomerated pack ice located in regions of first-year ice. These include Baffin Bay, Hudson Bay and Gulf of St. Lawrence.

Winter Ice Stream – A moving conglomerate of winter pack in a permanent current or prevailing winds.

Young Coastal Ice – The initial stage of fast ice formation consisting of nilas or young ice, its width varying from a few meters to 100-200 m from the shoreline.

Young Ice – Ice in the transition stage between nilas and first-year ice, 10-30 cm in thickness. May be subdivided into gray ice and gray-white ice.

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pattern of interlocking "fingers" (finger rafting). Has a matte surface and is up to 10 cm in thickness. May be subdivided into dark nilas and light nilas.

Young Ice – Ice in the transition stage between nilas and first-year ice, 10-30 cm in thickness. May be subdivided into gray ice and gray-white ice.

First-Year Ice – Sea ice of not more than one winter's growth, developing from young ice; thickness from 30 cm to 2 m. May be subdivided into thin first-year ice/white ice, medium first-year ice, and thick first-year ice. Figure 3 shows first-year, open pack ice.

Old Ice – Sea ice which has survived at least one summer's melt. Most topographic features are smoother than on first-year ice. May be subdivided into second-year ice and multi-year ice.

Concentration – The ratio in eighths (oktas) of the sea surface actually covered by ice to the total area of sea surface. Representative Concentrations are shown by Fig. 4.

Compact Pack Ice – Pack ice in which the concentration is 8/8 and no water is visible.

Consolidated Pack Ice – Pack ice in which the concentration is 8/8 and the floes are frozen together.

Very Close Pack Ice – Pack ice in which the concentration is 7/8 to less than 8/8.

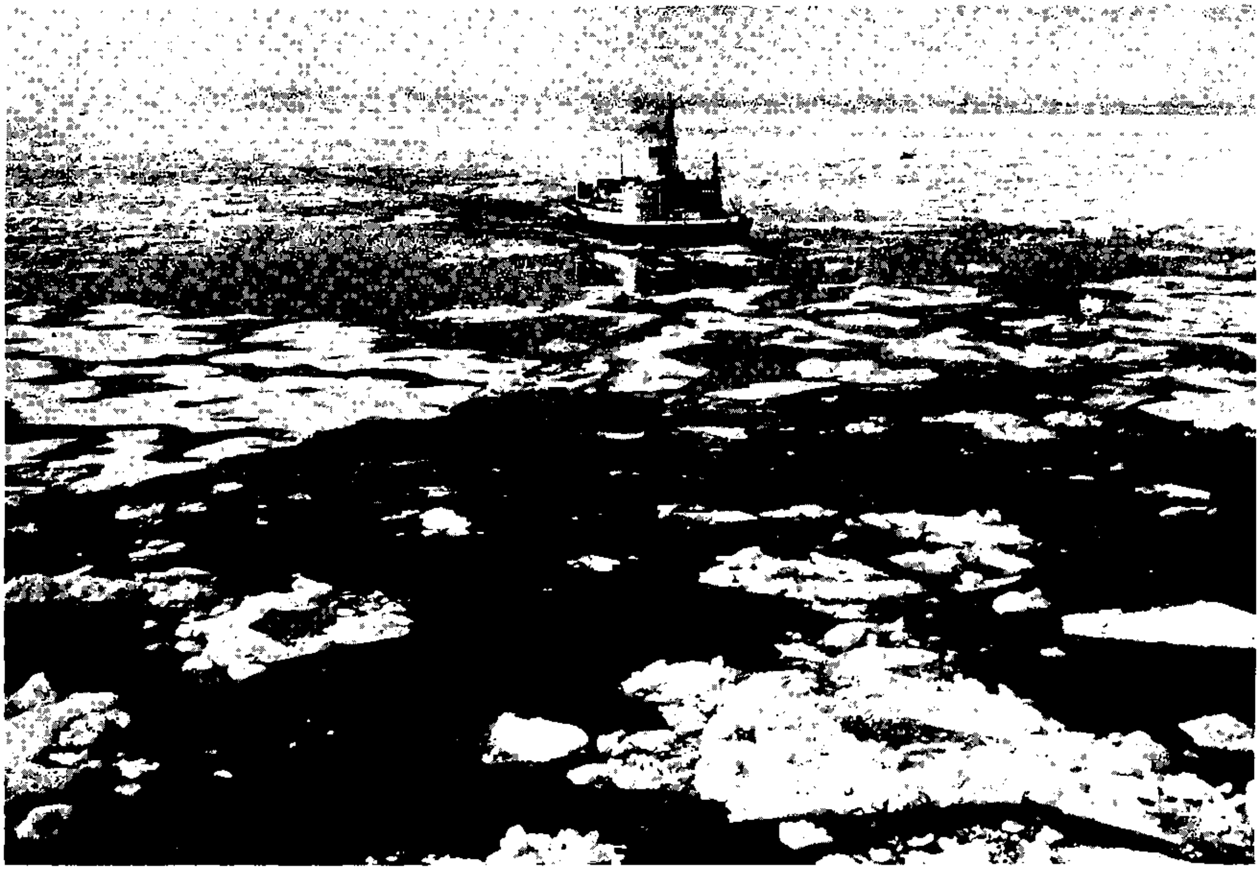


Fig. 3. Open pack ice of first year (winter ice).

Close Pack Ice – Pack ice in which the ice concentration is 6/8 to less than 7/8, composed of floes mostly in contact.

Open Pack Ice – Pack ice in which the concentration is 3/8 to less than 6/8 with many leads and polynyas, and the floes are generally not in contact with one another.

Very Open Pack Ice – Pack ice in which the concentration is 1/8 to less than 3/8, and water preponderates over ice.

Open Water – A large area of freely navigable water in which sea ice is present in concentration less than 1/8. When there is no sea ice present the area should be termed ice free, even though icebergs are present.

Ice Forms

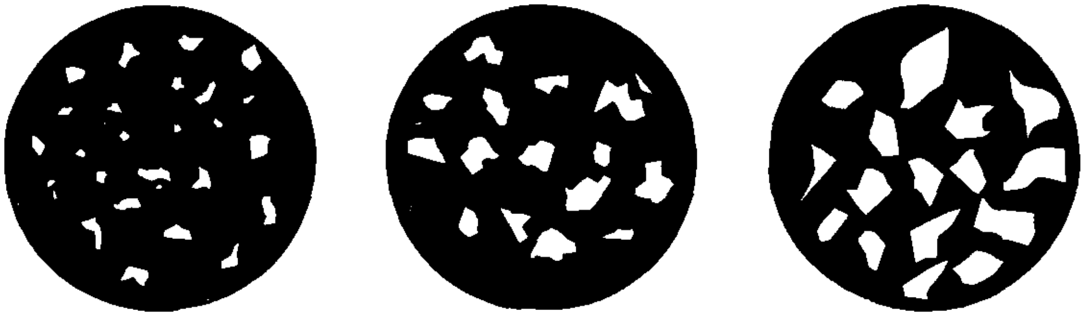
Ice Floes – Floe size ranges from ice cakes (2-10 m in width) through small, medium, big, and vast to giant floes which are over (10 km) in width (Fig. 5).

Deformation – When the floes are pressed together, the process is termed "rafting" if one floe over-rides another with little fracturing; "ridging" if a tent-shaped line of broken fragments is formed across the floes, and "hummocking" when random disconnected piles of fragments develop (Fig. 6).

Other terms applied regionally to ice forms or descriptions are dealt with under the particular region.

Formation and Properties of Ice

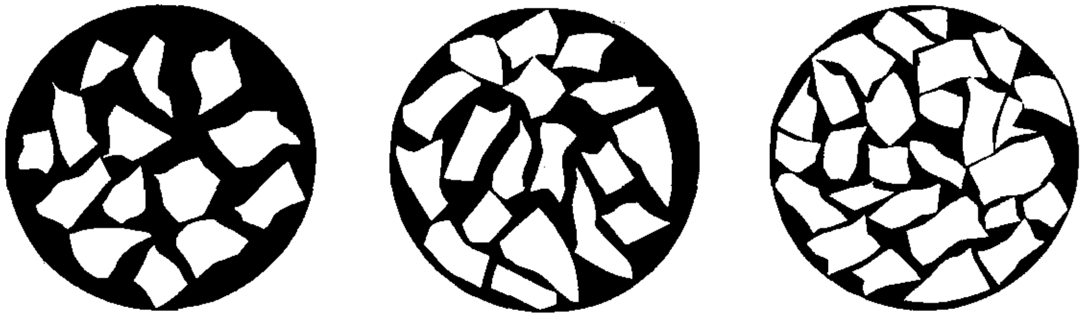
The density of pure water increases as it is cooled until it reaches a temperature of 4°C; thereafter its density decreases to the freezing point. When a freshwater lake is cooled this results in vertical mixing, as the surface becomes more dense than the sub-surface layers. The process will continue until the lake reaches 4°C throughout its depth, and then only the surface layers are chilled until ice is formed. In a saline solution,



LESS THAN 1 OKTA
OPEN WATER

1 OKTA
VERY OPEN PACK

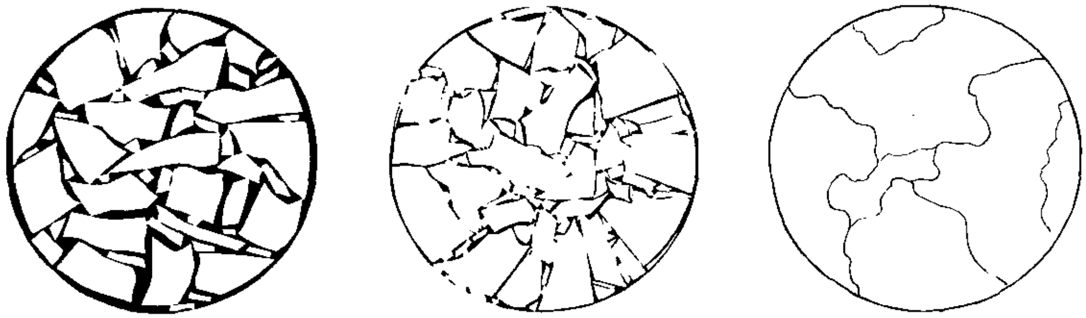
2 OKTAS
VERY OPEN PACK



3 OKTAS
OPEN PACK

4 OKTAS
OPEN PACK

5 OKTAS
OPEN PACK

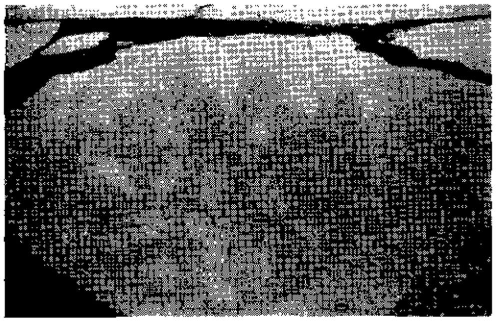


6 OKTAS
CLOSE PACK

7 OKTAS
VERY CLOSE PACK

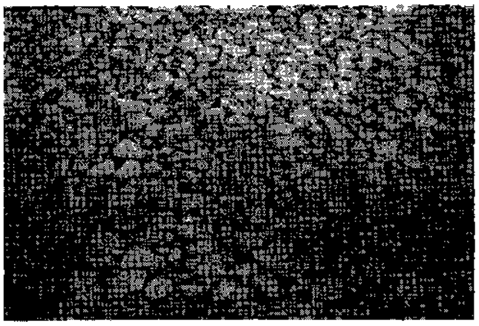
8 OKTAS
COMPACT PACK

Fig. 4. Representative ice distributions.

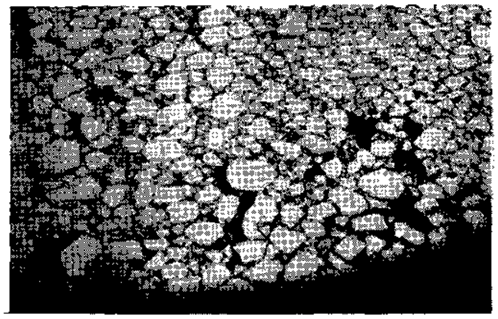


Medium floes off Labrador Coast

Medium floes breaking into small floes off northern Newfoundland



Small floes in close pack ice off Newfoundland Coast



Small floes off Newfoundland

Fig. 5. Ice floes off the Labrador and Newfoundland Coasts.

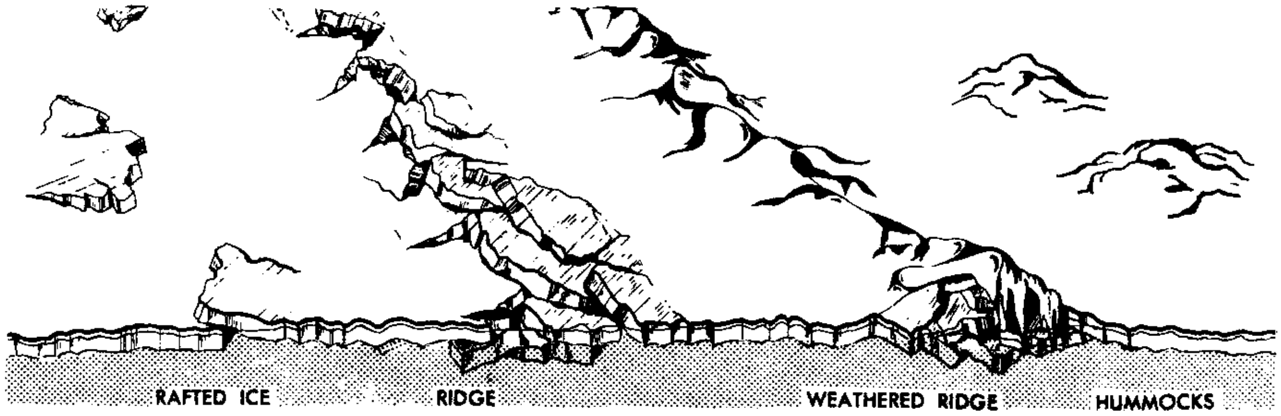


Fig. 6. Various types of ice topography caused by pressure.

the temperature of maximum density becomes lower as the amount of salts increase. The same is true of the freezing point, but its rate of lowering is somewhat slower. As a result, a point will be reached where the temperature of maximum density occurs at the freezing point. This occurs at a temperature of 1.3°C and a salinity of 24.7°/oo (Fig. 7).

Ocean waters have a greater salinity than 24.7°/oo, so that sea water when cooled increases steadily in density until the freezing point is reached. The significance of this is in the vertical circulation set up in sea water when it is cooled by coming into contact with cold air. As the surface water is cooled, it contracts and sinks, being replaced by warmer and lighter water at the surface. The cooled water may penetrate downward until it reaches the sea bottom. As a result the entire body of water must be cooled to the freezing point (-1.7°C for water of salinity 32°/oo) before sea ice can form. In actuality oceans are usually stratified, with different temperatures and salinities at each depth. Despite these variations, the water must be basically stable and its density must increase with depth otherwise vertical currents will develop to correct this anomaly. Cooled surface water will thus sink to a level where the density of the surrounding water is the same as its own, rather than to the bottom as in the simple case. A large volume of water must thus be cooled to the freezing point. This factor combines with the difference in temperature of maximum density is the reason for the delayed formation of coastal sea ice in comparison to nearby inland waters.

The first sign of sea surface freezing is an oily, apaque appearance of the water. This appearance results from formation of spicules, minute ice needles, and thin plates of ice known as frazil crystals which increase in number until the sea surface attains a thick soupy consistency. This is known as grease ice. Upon further freezing, and depending on wind exposure, waves, and salinity, the grease ice develops into nilas, an elastic crust with a matte surface, or into ice rind, a brittle shiny crust. Except in wind-sheltered areas, the thickening ice usually separates into masses, frequently in a characteristic pancake form. The raised edges and rounded shapes result from collisions between the cakes. With continued low temperatures the cakes freeze together to form a continuous sheet.

Ice has a crystal structure which is intolerant of impurities such as the inorganic salts which make up most of the dissolved material in sea water. If sea water is frozen very very slowly a cover of pure ice will form

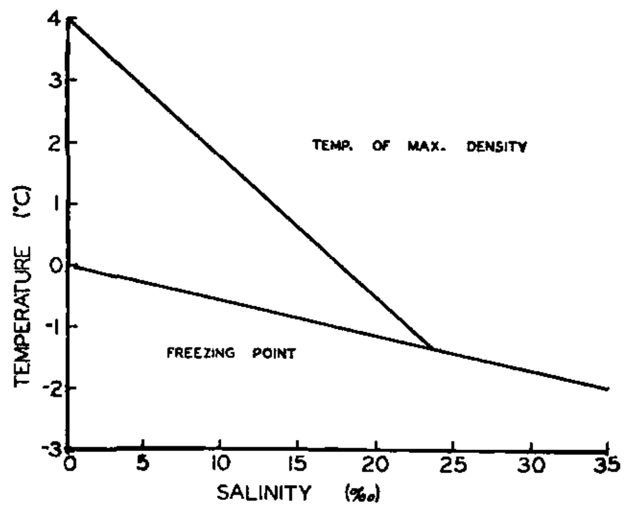


Fig. 7. Formation of sea ice.

Fig. 8. A system of highly developed puddles in multi-year ice.

Fig. 9. Old ice showing the uneven surface as a result of the differential melting of puddles and old hummocks.

with impurities being rejected into the water below the ice. Natural freezing is never this slow however, and sea ice always contains a certain amount of entrapped salts. The actual amount is mostly dependent upon the freezing rate, but typical sea ice has a salinity of 4-6°/oo, 80-90% of the salt being excluded from the ice. The presence of this small proportion of salt has an extremely important effect on the physical properties of the ice. The detailed crystal structure which results dominates these properties to the extent that one can relate any of them to "brine volume". The brine volume is defined as the fraction of the volume of sea ice occupied by fluid (liquid brine or air bubbles).

Even fresh ice is an inhomogeneous material with varying crystal sizes and orientations, so all strength measurements show a great scatter of results and statistical methods must be employed to get averages of any validity. Another complicating factor is that ice readily undergoes plastic flow so that results vary with rates of stress. On the average, with rapid stress application at -5°C, the ultimate strength of fresh-water ice is 15 kg/cm^2 in tension, 7 kg/cm^2 in shear, 17 kg/cm^2 in flexure and 35 kg/cm^2 in unconfined compression. In dealing with sea ice these values should be adjusted by the formula,

$$s = s_o\,(1\text{-}1.9v),$$

where s_o is the strength cited above for fresh ice and v is the brine volume.

The response of ice to a rapidly varying force is elastic unless crushing occurs. Sound transmission, response to moving vehicles, and impact from landing aircraft all fall into this category; but deflections from static or slowly varying loads do not, for, in these cases, ice flows plasticly. Although this plastic flow has been studied in glaciers, there has been little data available on sea ice.

Sea ice transmits acoustic energy readily, particularly at low audio frequencies, and bathymetric observations can be made through an ice cover by bonding a flat transducer to the sea ice surface with a thin film of non-freezing liquid. Reflection at the sea ice – water interface is small.

The initial stages of ice growth are always dependent upon heat loss from the sea. This heat must flow upward through the ice layer and through any snow lying on the ice so that the insulating qualities of these two materials are important factors in determining the rate and amount of ice growth. Air temperature and the amount of radiant energy falling on the surface are also important factors.

In the Arctic, radiation is almost completely dominant in determining the ice surface temperature and hence the duration of ice growth. This will continue until the increasing solar radiation changes the heat budget of the ice from a loss to a gain. This usually happens before the air temperature rises above the melting point of the snow cover. Pure white snow reflects as much as 90% of the radiation falling on it, and although the quantity of heat absorbed by the ice is small at first, spring conditions result in a gradual increase in the temperature of the ice. When the air temperature reaches the melting point, the snow surface begins to melt rapidly; it absorbs about 60% of the radiant energy and puddles of melt water become very extensive.

Puddling is the first apparent stage of deterioration of the ice cover. The water surface absorbs heat readily, permitting the puddle to widen and deepen and also to warm the ice. Later flaws and cracks develop in the floe through which much of the surface water drains away leaving dry hummocks of ice separated by ponds and streams of melt water. In temperate latitudes the ice is reduced to a grey, water-saturated matrix (rotten ice) which finally melts and the cycle is complete. Farther north, the summer is too brief for complete melting to take place and by late August or September puddles begin to freeze and new ice forms between the floes. After a time the floes themselves start growing again. A floe formed in 1 year which survives through the following summer differs chemically and physically from ice that is less than 1 year old. During the summer most of the brine drains out of the ice so that the typical salinity of old ice (second or multi-year ice) is about 0.5 to 1.0°/oo. Melt water from this ice is quite potable. The crystal structure of the ice becomes less regular, the crystals themselves are small and the ice is extraordinarily tough, even in summer. Figure 8 shows a system of highly developed puddles in multi-year ice.

In summer, old floes may be distinguished from first-year ice by their color. The melt water puddles on an old floe have a very characteristic pale blue color which persists after they freeze. On first-year ice, these puddles have a green to brownish appearance. The old ice itself has a pale blue color whereas first-year ice is much more a greenish white color. The surface of first-year ice is comparatively smooth except for pressure ridges and hummocks. Old ice has a characteristic uneven surface as a result of the differential melting of puddles and old hummocks (Fig. 9).

The Drift of Ice Into the North Atlantic Ocean

Ice conditions in the North Atlantic Ocean, as well

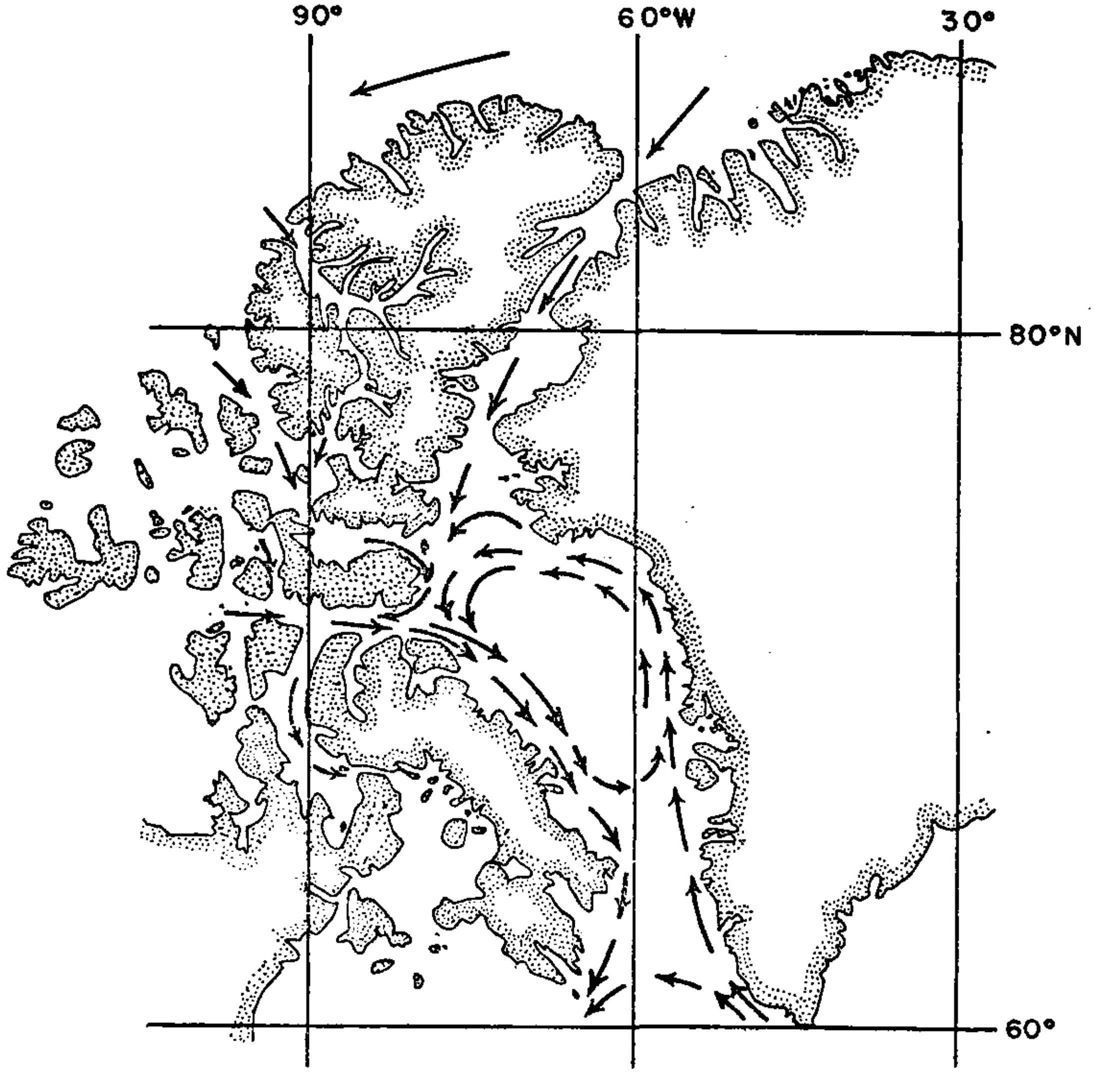


Fig. 10. General circulation of Baffin Bay and Davis Strait.

as any other ocean, can only be properly considered on a regional basis for not only is its seasonal variation considerable, but also cold or warm ocean currents and weather systems have a very marked effect. Ice forms every winter in the Gulf of St. Lawrence in 46°-50°N lat for example, whereas the Norwegian coast from 60° to 70°N lat is ice-free. In general cold, south-flowing currents on the east coast of North America permit ice formation as far south as 45°N lat and water currents may carry it even farther south in early spring. A similar flow carries ice down the East Greenland coast. Because the occurrence and ultimate fate of both sea ice and icebergs in the North Atlantic proper is the result of these permanent ocean currents, it is important to examine briefly the regime of ocean currents which affect the ice.

The East Greenland Current is a true polar current having its origin in the Arctic Ocean and flowing southward along the East Coast of Greenland as far as Kap Farvel. Between lat 65°N and 58°N this current mixes with the warm Atlantic Irminger Current. The West Greenland Current is formed by the combination of the East Greenland Current and the Irminger Current near Kap Farvel. The West Greenland Current never flows directly across the Kap Farvel to Newfoundland.

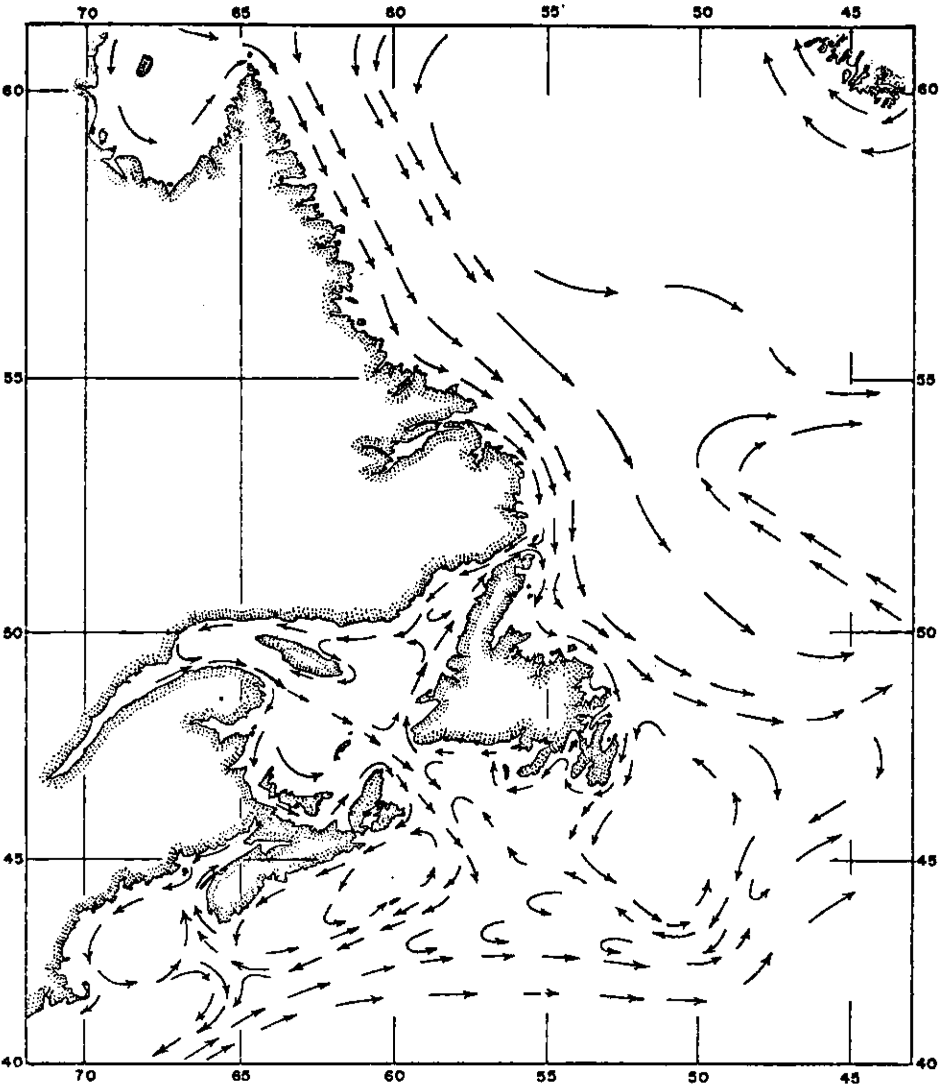


Fig. 11. General circulation of the Labrador Sea and Grand Banks.

The tempering effect of the Irminger Current, which is a recurving branch of the North Atlantic Current of the Gulf Stream system, makes the West Greenland Current relatively warm and salty. The West Greenland Current flows northward along the West Greenland coast, with velocities often of 1 knot from Kap Farvel to Nunarssuit, yet steadily losing volume through low-velocity westward branching, as water is fed into the cyclonic circulatory system of the Labrador Sea. Just south of Davis Strait ridge a major westward branching occurs, the remainder of the West Greenland Current continuing across Davis Strait ridge into Baffin Bay where it feeds the eastern edge of another cyclonic circulatory system (Fig. 10).

In the northern part of Baffin Bay and Smith Sound there is an area described as "North Water". In this region summer sea surface temperatures have been reported as consistently warmer than in surrounding areas. In winter various sources attribute it to be ice free during all or most of the year. Various reasons for the existence of the "North Water" have been advanced. The most recent one considered is that it is due to the nature of the circulation which keeps it ice-free and hence allows the surface waters to be warmed through insolation. The Labrador Current is formed by the junction of that portion of the Baffin Island Current which flows southward across Davis Strait ridge along the Baffin Island side, with the branch of the West

Greenland Current which curves westward just south of this ridge. The resulting stream, the Labrador Current, flows southward along the Labrador Coast with its axis over and paralleling the continental slope. The frigid (Baffin Island) component, by which the Labrador Current is best known, is on the coastal side of the axis; and the warmer (West Greenland) component is on the offshore side of the axis. The Labrador Current retains these characteristics with remarkably little change all the way to the Tail of the Grand Banks. The Baffin Island Current enters Hudson Strait along its northern side (Greenland icebergs having been sighted near Big Island, 150 miles in from the entrance) and leaves by the southern side. Arctic water and a few bergs enter the troughlike Strait of Belle Isle on the Labrador side and, more or less conforming to the bottom configuration, the current discharges on the Newfoundland side, but the extent of Arctic intrusion into the Gulf of St. Lawrence is largely controlled by tides, winds, and barometric pressures. The major portion of the Labrador Current continues southward along the east coast of Newfoundland to the northeastern part of the Grand Banks. Here it divides; one branch sets southwestward along the Avalon Peninsula; another and usually major branch continues southward along the eastern edge of the Grand Banks, this being the portion of the Labrador Current that bears the ice farthest south and consistutes the menace to the streamship tracks between the United States and Europe (Fig. 11).

In the Gulf of St. Lawrence, the circulation of the waters are in general counter-clockwise. Water that enters the Gulf past Cape Ray are deflected to the right to flow northeastward along the west coast of Newfoundland. Along the north shore of the Gulf the waters drift westward mixing with the Labrador Coast water along the south coast of Anticosti Island where it enters the general circulation of the estuary of the St. Lawrence River. In the Gaspé Passage, between Anticosti Island and the Gaspé Peninsula, there is a predominance of movement to the east which is known as the Gaspé Current. This current is very strong and its effects can be seen and felt for many miles from the coast. It keeps well to the Gaspé Peninsula but loses a considerable amount of its velocity on passing into the shallows of the southern portion of the Gulf. The waters in this southern St. Lawrence Gulf area form a general eastward flow which works toward Cabot Strait, where the main efflux of the Gulf of St. Lawrence takes place. This current in Cabot Strait is known as the Cape Breton Current.

Crossing the deep oceanic triangle between the Nova Scotian and the Grand Banks, the Gulf Stream encounters the Tail of the Grand Banks around which it bends. On passing the Tail of the Grand Banks, the Gulf Stream system becomes known as the North Atlantic Current and spreads fanlike to form many highly complex tongues and eddies as it continues to the northeast and east past the Tail of the Banks. The surface temperature of the northern edge of the stream near the Grand Banks is approximately 12°C in winter and 18°C in summer, with an annual range of about 6°C.

The region around the Tail of the Grand Banks where the Arctic and Gulf Stream waters meet exhibits

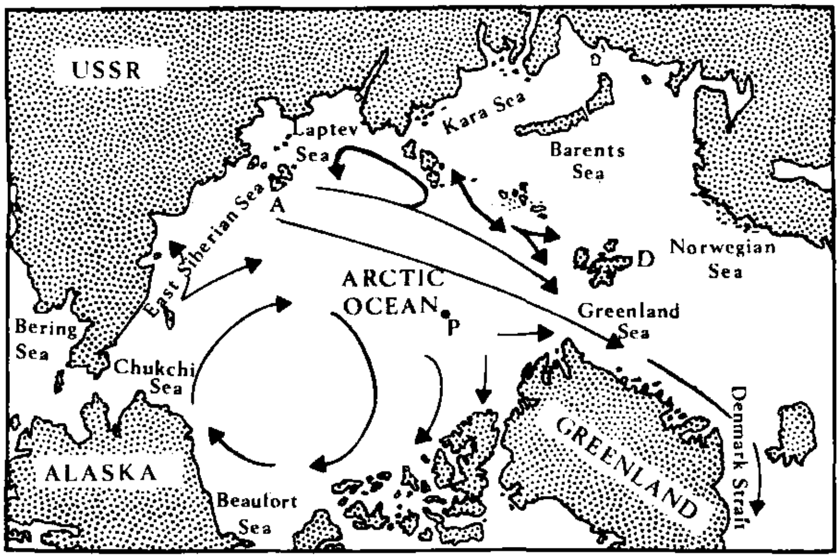


Fig. 12. General pattern of sea-ice drift in the Arctic.

among the greatest hydrographical contrasts to be found anywhere in the world. The Labrador Current curves to the east paralleling the Atlantic Current on the northern border of the latter, and gradually loses its identity through mixing.

Seasonal Distribution of Sea Ice in the Northwest Atlantic Ocean

Knowledge of Arctic ice distribution and of environmental influences which control the distribution is far from complete. In attempting to describe it certain macroscale definitions must be understood. These relate to geographic and regional concepts as contrasted to the mesoscale terminology of the WMO ice glossary. Chief among these are

Ice Stream – an ice pack area that is superimposed on any established permanent ocean current.

Winter Pack – a conglomerated pack ice located in regions where ice usually consists of winter or one year ice. These areas include Baffin Bay, Hudson Bay, and the Gulf of St. Lawrence.

Winter Ice Stream – a moving conglomerale of winter pack in a permanent current or prevailing winds.

Polar Ice Stream – Multi-year Arctic pack ice in a region having a significant velocity.

Archipelagic Pack – quasi-stationary winter pack or polar pack advected into shallow archipelagic waters.

East Greenland

The East Greenland Ice originates mainly in the Arctic Ocean, but is supplemented by locally formed ice. This drift is shown in Fig. 12. It characteristically contains a high proportion of old ice known locally as Storis. This ice is charactistically 2 to 3 years old and has attained an unusual thickness and hardness as a result of repeated freezing and rafting. Storis, together with a small number of icebergs from glaciers in Northeast Greenland, is carried south by the East Greenland Current as a broad belt which is about 500 km wide north of Scoresby Sount (70°N) in late winter. North of this latitude there is ice at all seasons, but the belt narrows and concentrations decrease in late August. Beginning in September, the southern and southeastern margin of the pack ice expands southwards until by December or January a tongue of pack ice has reached Kap Farvel and has effectively blocked the east coast. Between January and May it continues round Kap Farvel up the west coast as far as Frederikshaab, or occasionally in very severe years as far as Godthaab.

The maximum cover of East Greenland Ice is reached in March and April, and in very heavy ice years there is at that time continuous pack ice from southern Greenland to Svalbard and eastwards to Novaya Zemlya. The margin of the ice begins to withdraw in May by melting on the outer side and by the development of inshore leads between the coast and the open pack. Great fluctuations have been recognized in the extent of the East Greenland Ice and this is clearly related to the rate of transport by the current and amount of ice that is offered from the Arctic Ocean. In general it appears to have been less in the 20th Century than in the previous three centuries. However, in the mid-to-latter 1960's a severe ice intrusion has occurred across the northern coast of Iceland. Storis reaches a mean maximum thickness of about 3.5 m. Seasonal conditions of East Greenland ice, along with other regions, are shown in Figs. 13-16.

Baffin Bay and Davis Strait

Owing to the presence of a warm northerly West Greenland Current off the Greenland coast and a cold southerly current off Baffin Island, there is considerable contrast between ice conditions on the east and west sides of Baffin Bay and Davis Strait. The east side remains clear in winter, except for fast ice in the fjords, almost as far north as Disko Bay, while the west side is completely ice-covered. In summer the ice clears out completely or almost completely, the remaining ice, if any, being on the west side of Baffin Bay. The ice is therefore mostly of 1 year's growth and is termed West Ice by Greenlanders because it arrives, wind driven, on the Greenland coast from the west and remains north of lat 66°N until spring.

Ice formation starts in the north in late September and advances southwards to Davis Strait by mid October. The whole area to the limits of ice formation is covered with pack ice by early November. Thicknesses by late December have reached about 0.7 m in northern Baffin Bay and 0.4-0.5 m in the Disko-Cape Dyer area; and maximum growth, which is reached around the end of April, is about 1.5 m in northern Baffin Bay and 1.3 m in Davis Strait.

Beginning in early November and continuing through July the winter ice stream enters Frobisher Bay and Hudson Strait along the northern side in each case. That entering Frobisher Bay, except for a portion which moves into Hudson Strait via Gabriel Strait, is mostly

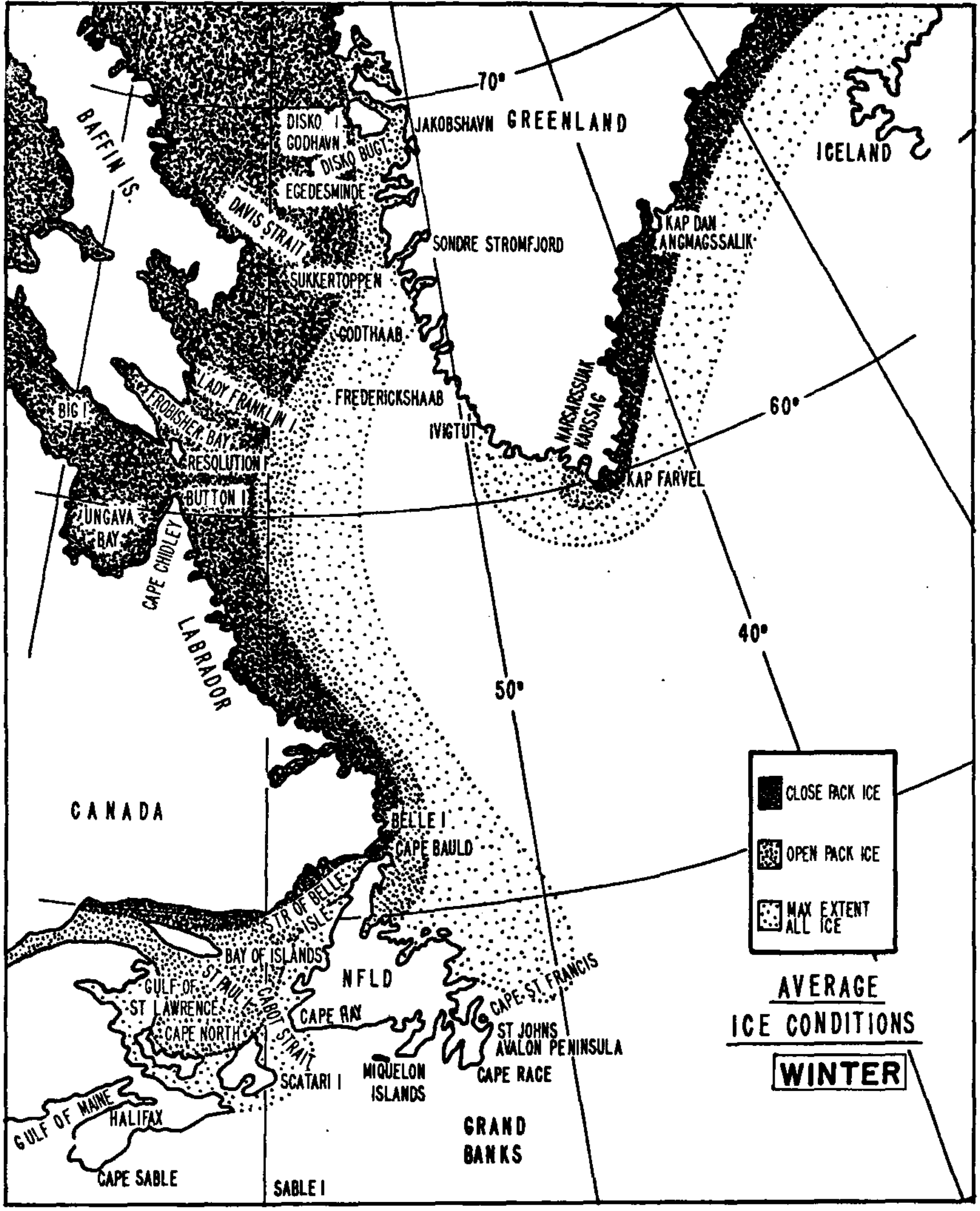


Fig. 13. Average winter ice conditions in the Northwest Atlantic.

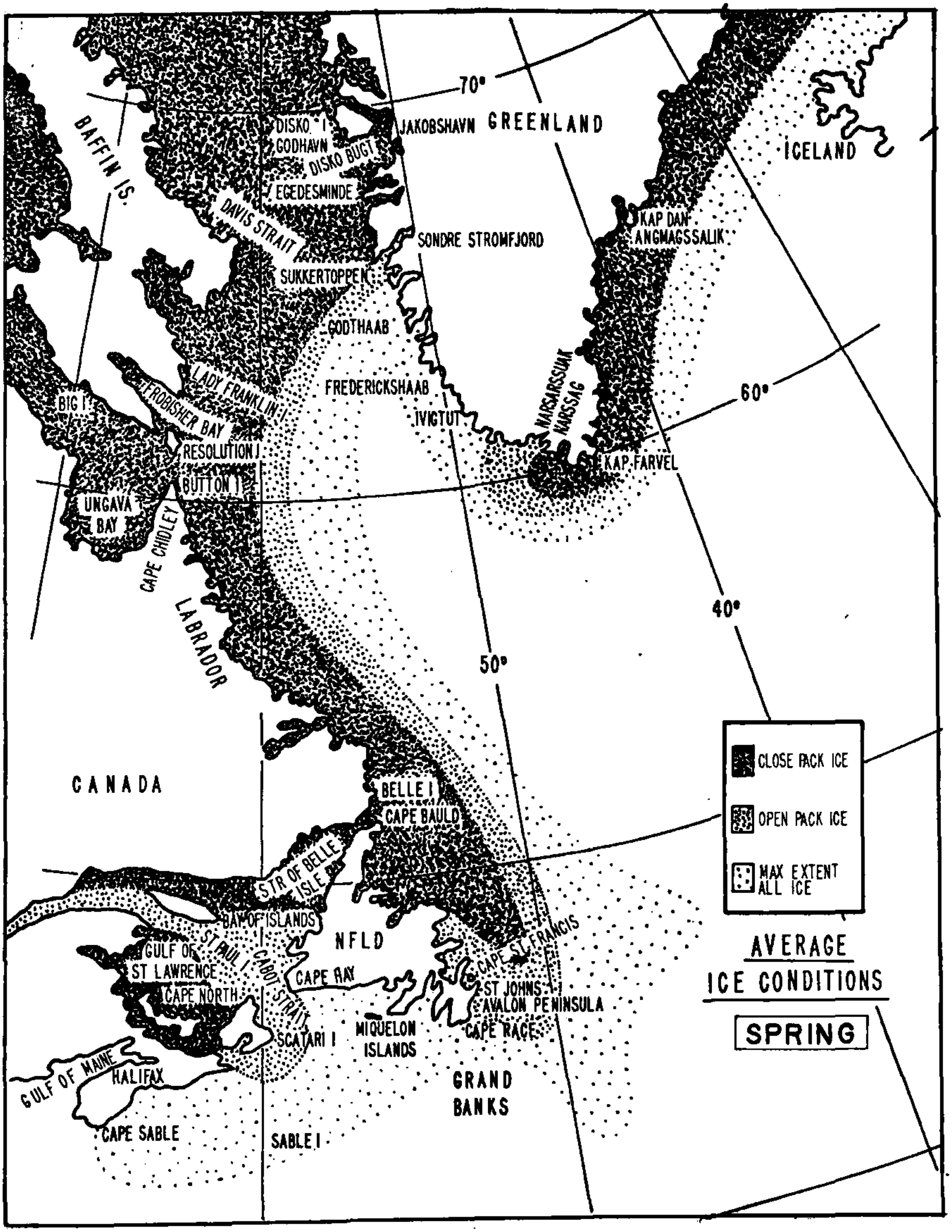


Fig. 14. Average spring ice conditions in the Northwest Atlantic.

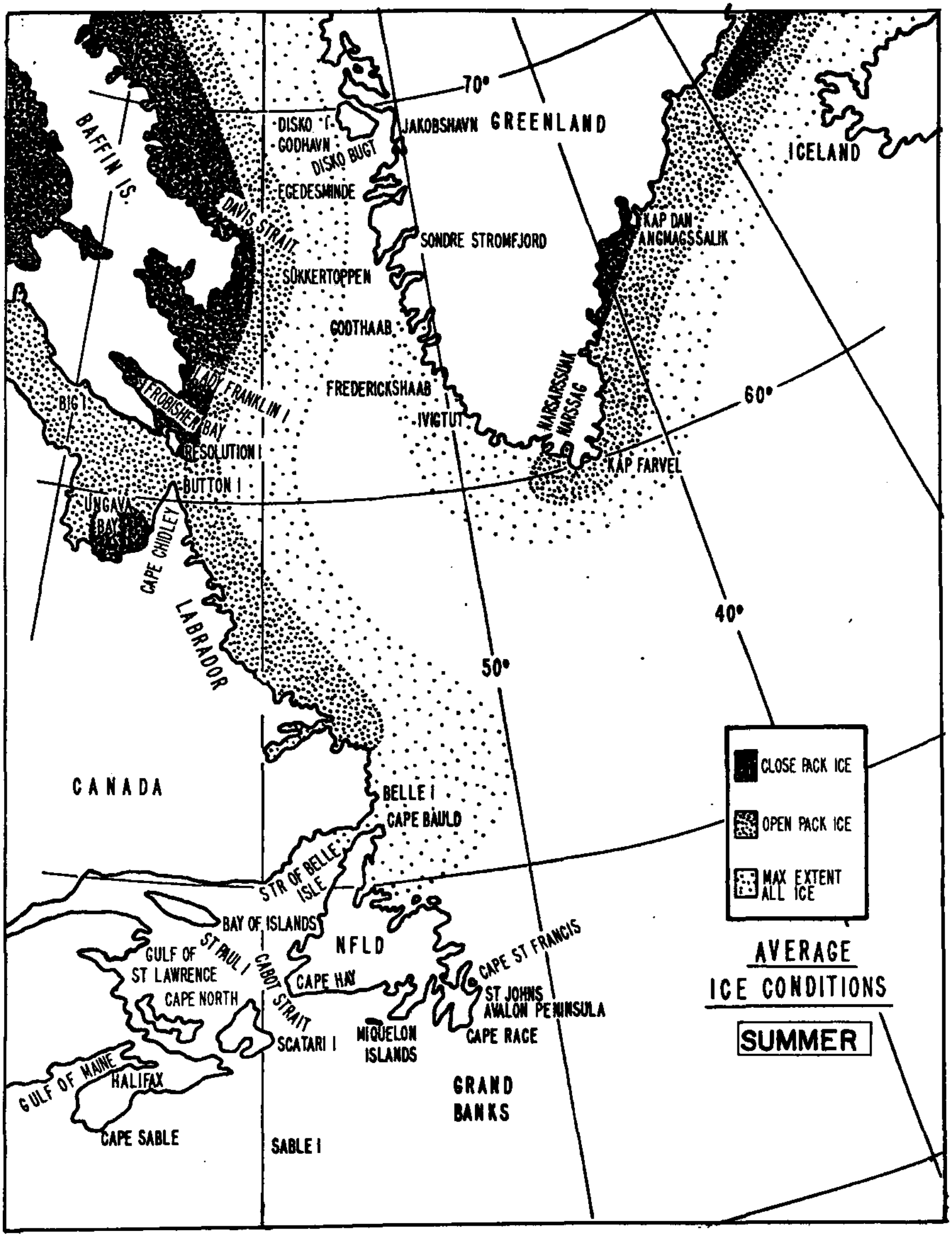


Fig. 15. Average summer ice conditions in the Northwest Atlantic.

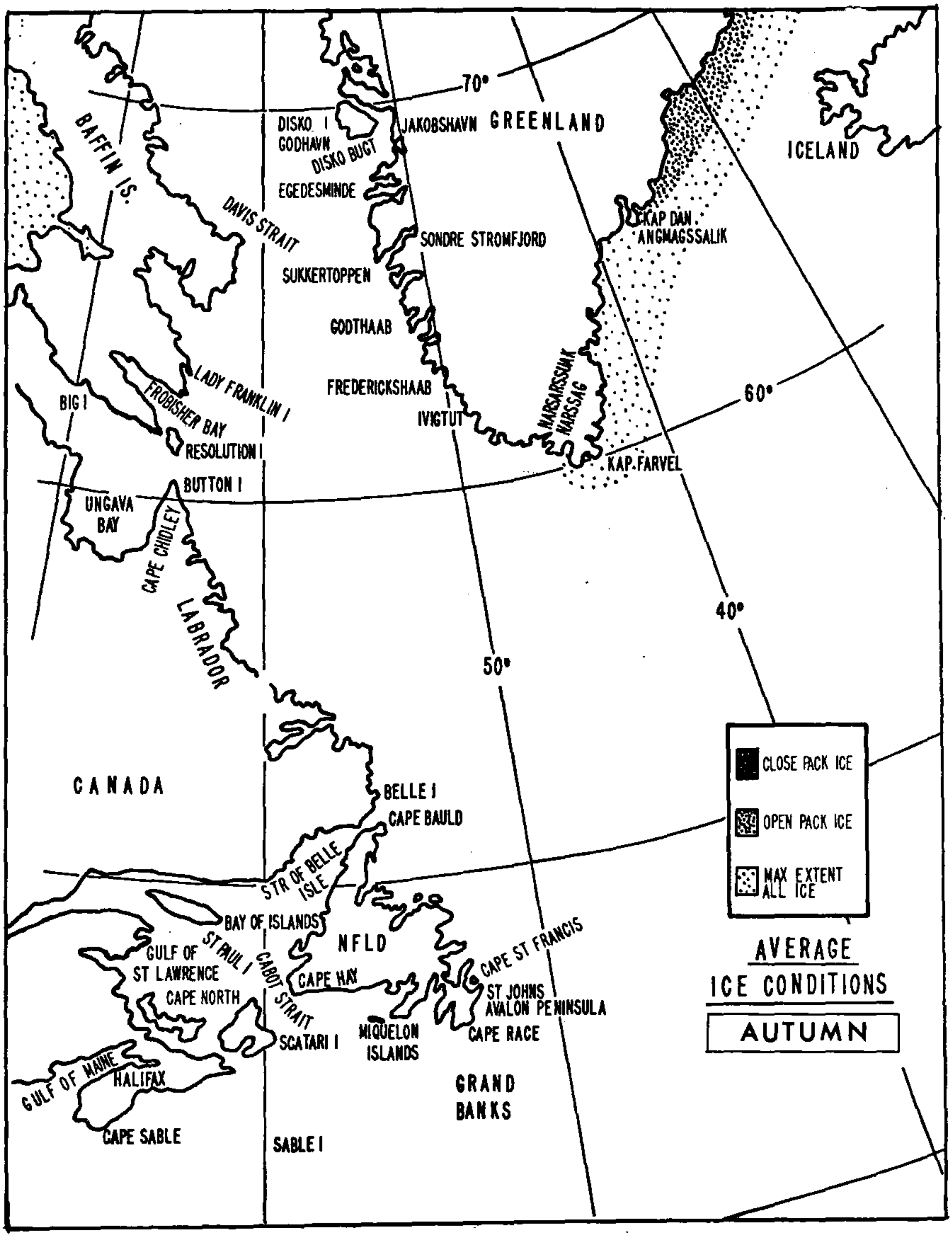


Fig. 16. Average autumn ice conditions in the Northwest Atlantic.

trapped and destroyed there. That entering Hudson Strait, together with the ice from Frobisher Bay leaves the Strait on the south side past Cape Chidley and the Button Islands, and then moves southward along the Labrador coast with the main floes. The other source of pack ice in this area is Foxe Channel ice which accumulates in Foxe Basin and with northerly winds debouches into the western end of Hudson Strait and northern Hudson Bay. The initial movement of this ice is almost entirely due to wind as the residual current system is weak and the water movement governed by the strong but oscillatory motion of the tidal currents.

In southern Greenland the Winter Ice seldom becomes very thick except in the inner ends of long fjords. This is partly because of the milder temperatures and partly because weather conditions are so unsettled and stormy that the ice does not have a chance to get very thick. The most landlocked waters are not open until sometime in June, but less protected waters are naviagable as early as April. In several places, notably in the outer parts of fjords, no Winter Ice is formed because of the currents and stormy weather.

Most mariners not acquainted with west Greenland waters are under the impression that they are much icier and remain closed for longer periods than is the case. It may be surprising therefore, to learn that practically all of the larger coastal harbors of west Greenland as far north as lat 66°N, viz, Julianehaab, Frederickshaab, Fiskernaesset, Faeringerhavn, Godthaab, and Sukkertoppen, are navigable throughout the winter.

By early June general melting has started. An open water route is usually available through Melville Bay by late July. The last ice usually persists off the east coast of Baffin Island in the area of Home Bay. In some years ice remains here all summer, but more usually all ice has gone by September. Navigation in virtually open water is available in the area from late July to October.

Labrador and Newfoundland

The two chief sources of Arctic ice for the western Atlantic are Davis Strait and Foxe Channel via Hudson Strait. The winter ice stream from Baffin Bay reaches Hudson Strait in late October or early November and is there joined, at Cape Chidley, about the first of November, by the heavy floes from Foxe Channel. The combined streams move down the Labrador coast arriving off Belle Isle in December. The first ice to arrive is, of course, open strings of young ice but this is soon replaced by heavy floes of true Arctic character which often extend 100 miles east of the entrance of the Strait of Belle Isle. The pack ice is of various ages but first-year ice predominates. Much of this ice enters the Strait along the northern side and soon fills the entire Strait. This action ordinarily closes the Belle Isle route to navigation until the latter part of June.

By January or early February this ice has reached the northern edge of the Grand Banks and by March the Pack has spread generally over the northern part of the Banks, often as far south as lat 46°N (Fig. 14). Again the first ice to appear is deceptively soft and open but is rapidly followed by heavier, more compact fields extending as far as the eye can see. In these latitudes, at this time of year, this ice has little of the appearance of the original winter pack. However, it is heavy and compact enough to stop a vessel and may seriously damage any ship attempting to force a passage. There are two possible extensions of this ice following the movement of the major ocean currents of the region described above. Large quantities drift south along the eastern edge of the Banks breaking up into ice patches and belts as it moves south of 45°N lat, being mostly destroyed before reaching the Tail. If it survives to this latitude (43°N) it is generally very quickly melted in the warmer water south and east of the Tail. This ice, in the last stage of disintegration, is seldom a menace to navigation. In occasional instances of extremely heavy ice seasons dangerous floes extend to the Tail of the Banks but rarely south or west of this point. There are always a few bergs in the ice fields when they arrive at the Banks and often a great number. The other extension of the ice is along the east coast of Newfoundland and around Cape Race. From this point it spreads south and southwestward over the neighboring banks. There is no appreciable amount of ice experienced southwest of the shelf and a clear passage can regularly be found in the mouth of the deep Laurentian Channel leading toward Cabot Strait. In some years the ice spreads westward from Cape Race completely blocking the harbors on the south coast of Newfoundland as far west as the Miquelon Islands.

In April and May the winds tend to become westerly, the ice is driven eastward into warmer water and melts, starting a northward retreat of the pack. It clears Belle Isle Strait about the end of May in most years and the northern Labrador coast about the third or fourth week in July.

Gulf of St. Lawrence

Ice conditions in the Gulf of St. Lawrence vary in extent from year to year, and range from minimum ice cover in the west and southwest parts of the Gulf only, to the other extreme of virtually complete ice cover in the Gulf which may persist for as much as 4 weeks.

Except for a small amount of older ice which sometimes comes through the Strait of Belle Isle in the spring, all ice is of one season's growth only, and much of it never gets past the young ice stage of development. Maximum thickness is about 0.7 m.

Ice formation usually starts in the river below Quebec about mid December, and in Chaleur Bay and Northumberland Strait in the second half of the month. Maximum extent is reached about mid or late February, but growth is not always continuous up to this time. Warm temperatures can occur at any time during the growth season and temporarily reverse the trend.

The ice stream departs the Gulf of St. Lawrence more or less constantly past St. Paul Island and Cape North spreading south and southeast toward Sable Island, frequently completely covering the banks north of that island. The ice in heavy years completely surrounds Sable Island, for short periods when the wind favors such drifts. This ice, as it leaves the Gulf, consists of heavy, tightly packed, rafted ice and forms a difficult barrier to all navigation. With favorable winds, when the fields have their greatest extension, the ice spreads east-northeastward toward the southern shore of Newfoundland, though seldom actually reaching that coast. Some also moves southwestward from Scatari Island along the Nova Scotia coast, though rarely drifting as far as Cape Sable. This last ice is always open and navigable when it reaches Halifax and it is doubtful whether field ice reported in the vicinity of Cape Sable or in the Gulf of Maine is Arctic ice.

The final retreat of the ice normally starts sometime in March, and complete clearing occurs anytime from late April to the end of May. Open water passage through the Gulf is nearly always possible anytime after the end of March and sometimes sooner.

From this description it might appear that the Gulf of St. Lawrence ice would present few problems to navigation, but this is not always the case. Stormy weather causes much ridging and rafting of the ice, and pressure at times can be severe. Polar icebreakers have, at times, been halted by the severe ice bridge which can develop across Cabot Strait.

Icebergs and Their Drift Into the North Atlantic Ocean

Icebergs are masses of glacial terminus and drifted seaward. The principal origin of icebergs which reach the North Atlantic Ocean are from the 100 tidewater glaciers of West Greenland where approximately 15,000 bergs are calved each year mostly from 20 major glaciers between Jacobshavn and the Humboldt Glaciers (Fig. 17). It is estimated that these account for 85% of the average 400 icebergs which reach the Grand Banks of Newfoundland each year normally from March through June. Other sources of iceberg origin are East Greenland glaciers where it is estimated that about half as many are calved as West Greenland but account for only 10% of the number reaching the North Atlantic. The remaining 5% are thought to come from the glaciers and ice shelves of northern Ellesmere Island.

Fig. 17. Principal iceberg producing glaciers of West Greenland.

Icebergs are properly termed such when they are larger in size than a large building or ship. Smaller pieces the size of houses and garages are termed bergy bits and growlers respectively. However, the classification is quite relative and what might be a respectable iceberg at the Tail of the Grand Banks might scarcely pass for a growler in Baffin Bay.

East Greenland icebergs drift southward along the coast to Kap Farvel (Fig. 18) rounding that cape and then drifting northward under the influence of the West Greenland Current. Occasionally with the effect of wind

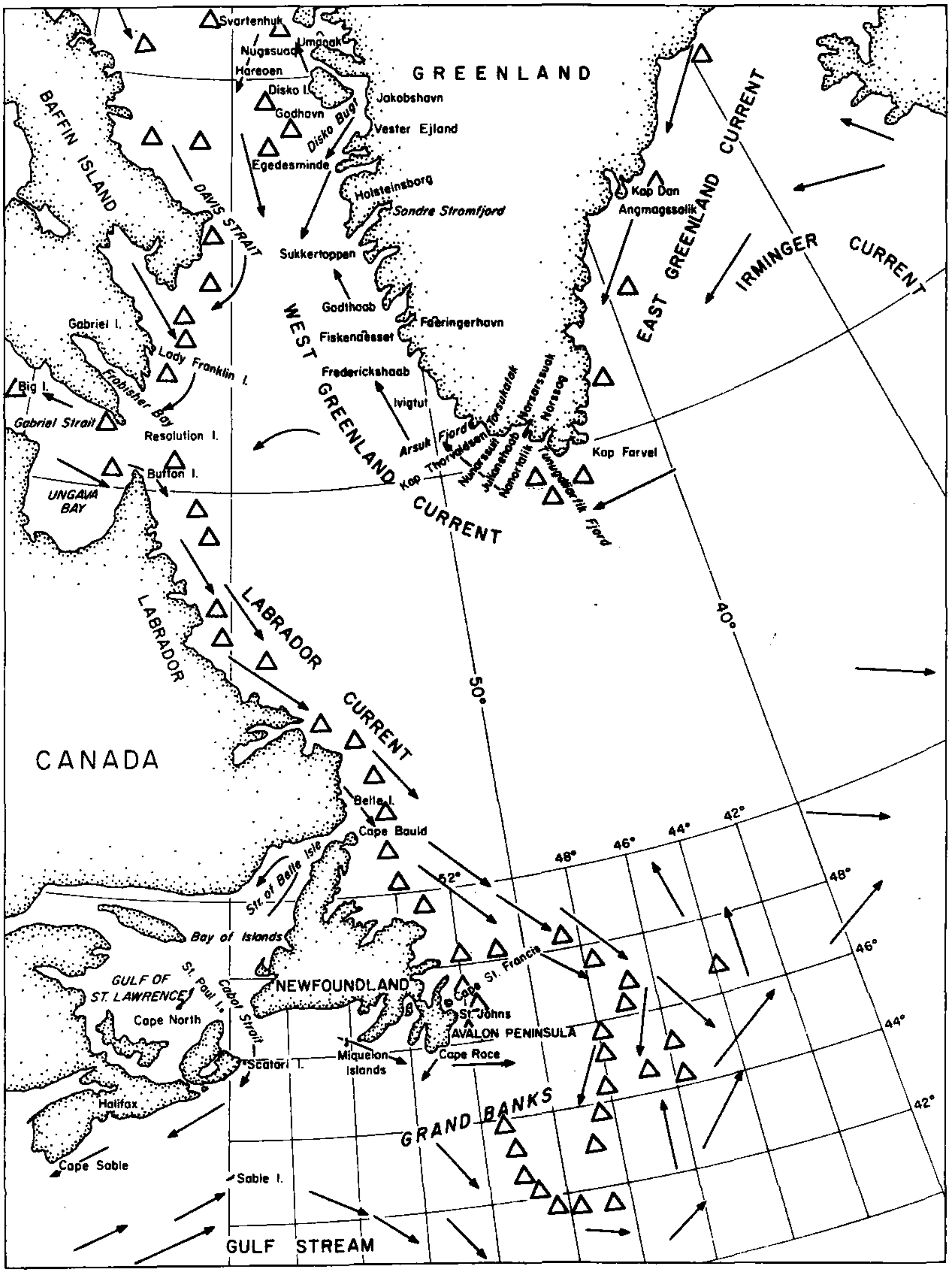


Fig. 18. Drift of icebergs from their source into the North Atlantic Ocean.

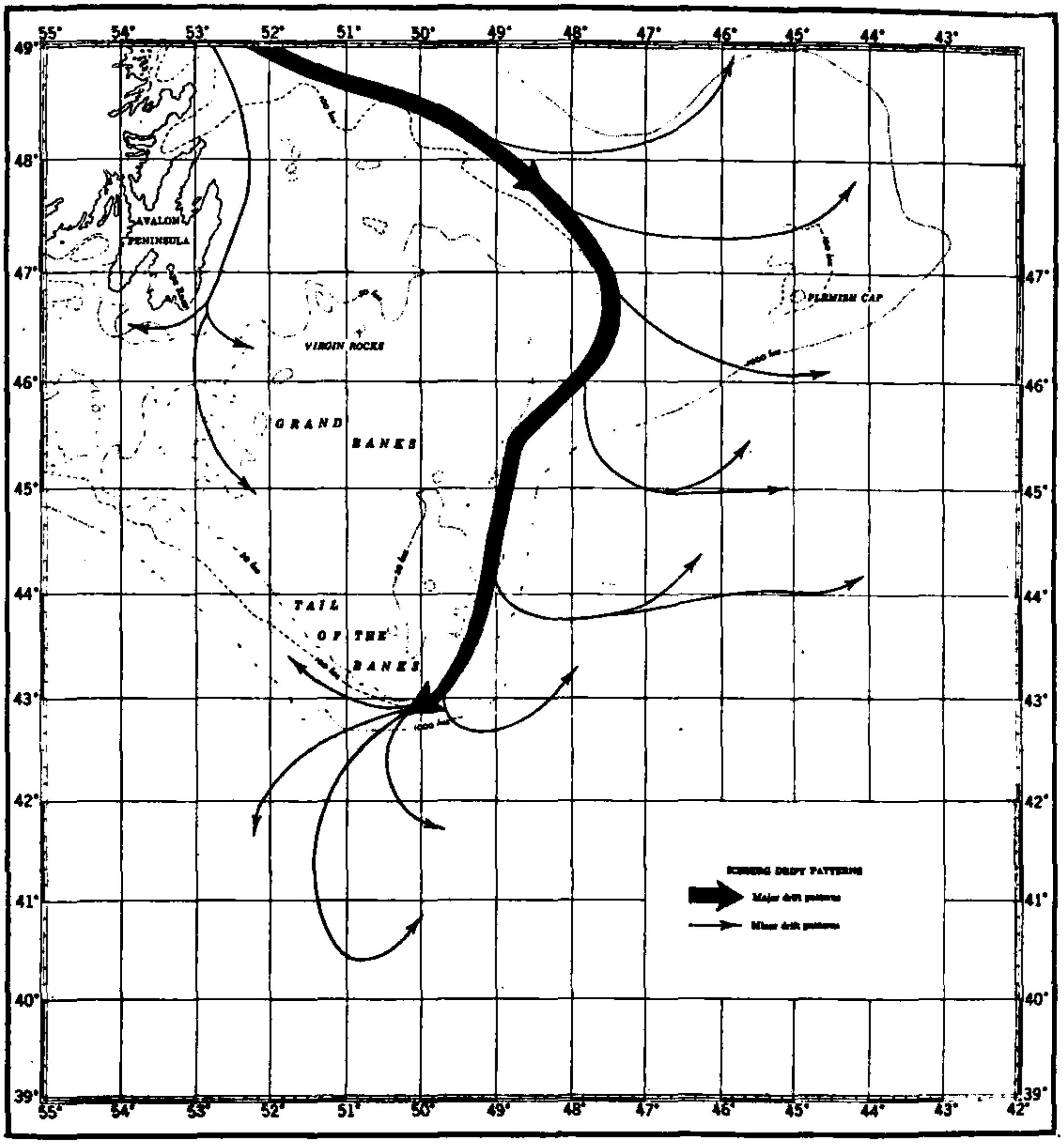


Fig. 19. Iceberg drift patterns on the Grand Banks of Newfoundland.

or in the absence of a well developed Irminger Current, bergs may continue past Kap Farvel reaching as far as 100-200 nautical miles to the south or southwest. The majority under the effect of the warm West Greenland Current disintegrate rapidly; seldom drifting north of lat 65°N along the West Greenland coast. A few drift westward across the southern Davis Strait to the Labrador and Baffin Island Coasts where they join the main stream drifting southward.

Icebergs of West Greenland origin make their appearance at Disko Bay where the Jakobshavn Glacier and its spectacular icefjord is a major producer. Thereafter in their travel up the west coast of Greenland and around to the western side of Baffin Bay, they become prodigous in number. The majority of bergs are conconcentrated near the coast generally within 20 nautical miles on the Greenland side and 60 miles on the Canadian side.

Icebergs in their southward drift from Baffin Bay to Newfoundland display drifts much the same as the sea ice stream of the Arctic pack, thus indicating they are under the overall influence of the same current regime. The drift season corresponds also and the arrival of the winter pack along the Labrador and Newfoundland Coasts usually marks also the arrival of the seasons iceberg count. This is because, although the Arctic supply is inexhaustible, few bergs survive on unseasonable drift southward into warmer waters unprotected by pack ice. Wave erosion is a major factor in iceberg deterioration.

The drift of icebergs from their origin on the coast of West Greenland to the coast of Newfoundland is about 1,800 nautical miles and takes an average of about 3 years. Owing to their deep drafts of 100-200 m larger bergs are often slowed by grounding.

An average of about 1,000 bergs each year reach the region offshore from Belle Isle and about 400 reach the northern edge of the Grand Banks (lat 48°N). Here the Labrador Current divides carrying part of the stream close along the Avalon Pennisula and the other stream to the east and south along the slope of the Banks generally between the 200 m and 2,000 m isobaths (Fig. 19). At this point the velocity of iceberg drift increases from about 10-15 miles per day along the Labrador and Newfoundland coasts to 25-30 miles per day along the slope of the Banks.

Occasional bergs may be reported south of Newfoundland even during November, December, and January when their numbers are at a minimum for the year. The maximum occurs during April, May, and June. Along the eastern edge of the Grand Banks the first bergs of the season are usually sighted in February or early March. In heavy ice years and with a well developed Labrador Current, icebergs frequently reach lat 43°N and about long 50°W before being rapidly disintegrated by the warm waters of the Gulf Stream. Few icebergs drift south of lat 40°N. This has occurred only four times since 1900. The Labrador Current and the ice follow a definite course from the Arctic to the Tail of the Grand Banks. The effect of wind upon the drift of bergs being small, their subsequent movements depend largely upon the complex and variable current pattern which exists along the boundary between the Labrador and the North Atlantic Currents in this region. During the latter part of March and the first part of April the flooding Labrador Current holds closely to the eastern slope of the Banks and sometimes curls around the Tail and extends for a considerable distance northwestward along the southwestern slope. As the volume of the discharge from the north increases, the mixing zone between the two currents moves farther offshore to the southwest, south, and southeast of the Tail and bergs fan out along the northern edge of the Gulf Stream system. During the summer, however, the Labrador Current dwindles in volume in this region and bergs remain nearer the Banks or are returned northeastward by the currents before reaching the Tail of the Banks. During this cycle in the flow of the Labrador Current, the volume of flow of the North Atlantic Current is also changing and the resulting changes in relative strength of the two currents produce complicated variations in the location of their common boundary and in the courses, drift rates, and life expectancy of the bergs reaching this vicinity.

Bergs do not ordinarily drift very far into the northern edge of the Gulf Stream system which carries them off to the east and northeast, but occasionally a berg journeys as far south as the latitude of the Azores, most often during the height of the ice season or during a bad year, such as 1912. Comparatively few bergs drift south of the 40th parallel.

Disintegration is accelerated by high waves and heavy swell, the effects of which are noticeable on bergs which drift offshore in the Atlantic early in the season. A berg of average size in the mixed water south of the Tail of the Banks is likely to survive as a menace to navigation for a period of 12 to 14 days during April, May, or June, but during July or August is not likely to survive longer than 10 to 12 days. A similar-sized berg in the warmer waters of the North Atlantic Current is not likely to survive more than 7 days. Observations by the International Ice Patrol indicate the following deterioration rates for an average sized iceberg (50 m high and 200 m in breadth):

Water temperature (°C)	Iceberg melting rate
-2.0 to 0	Nil
0 to 4.5	Immeasurable
4.5 to 7.5	Measurable but slight
7.5 to 10.0	Disintegration in 2-3 weeks
10.0 to 15.0	Disintegration in 1½-2 weeks
15.0	Disintegration in 1 week or less

The iceberg season south of Newfoundland is largely covered by the months March to July, inclusive, with most of the bergs being melted between the middle of March and the middle of July. The bergs decrease in numbers noticeably after the middle of June and from the middle of July on until the following spring the area south of the Grand Banks is practically free from them. An isolated berg or two may drift south to the Tail, but not often south of it as late as October. After that month it is unusual to sight bergs in the latitude of the Banks until the following February.

Records of icebergs drifting in the North Atlantic have been kept by the International Ice Patrol. The number reaching the Banks varies from zero (in 1958

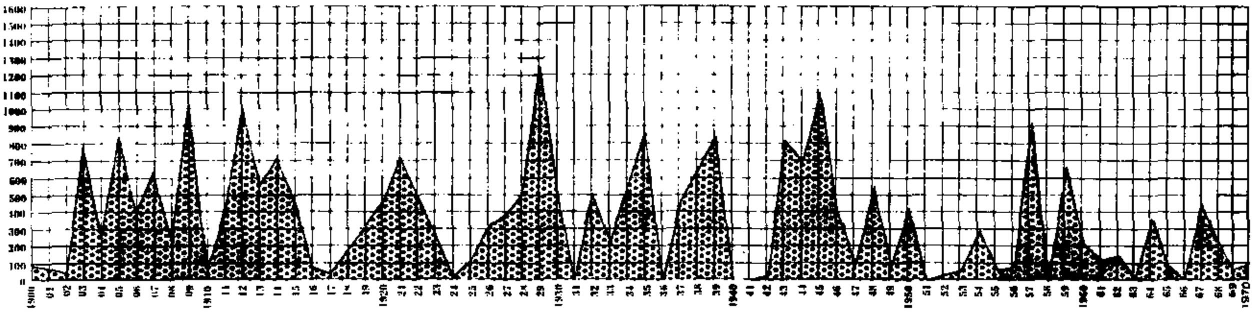


Fig. 20. Annual number of icebergs drifting south of 48°N from 1900 to 1970.

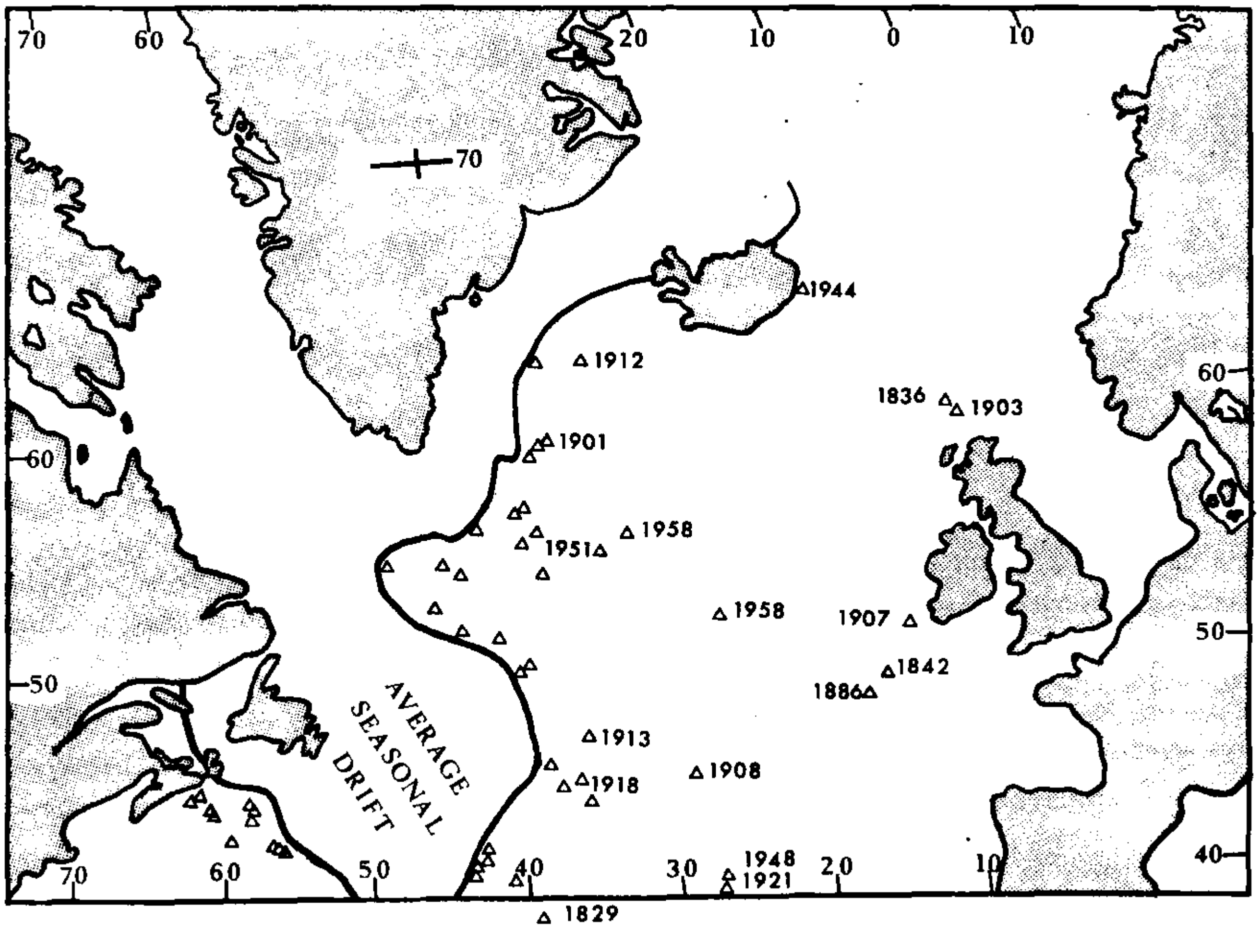


Fig. 21. Positions of unusual iceberg sightings in the North Atlantic Ocean.

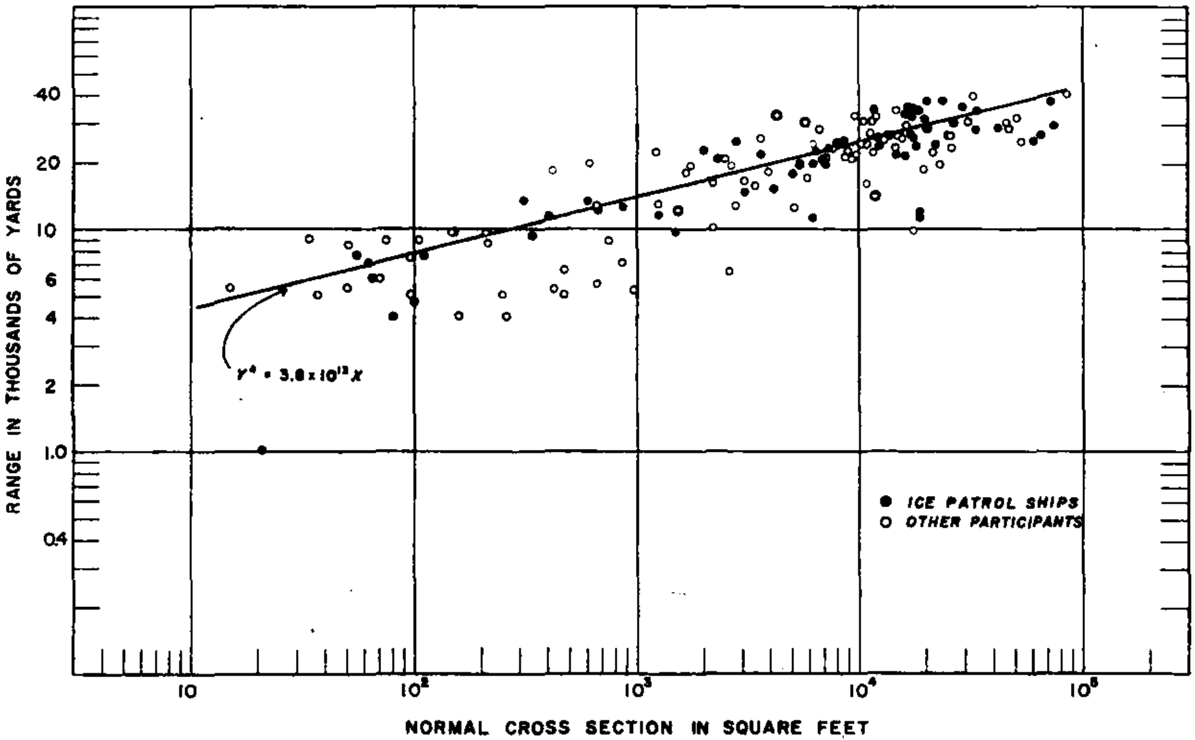


Fig. 22. Relation between radar maximum range of detection and iceberg physical cross-sectional area illumunated at the maximum range.

and again in 1966) to 1,350 bergs (in 1929). The annual cycle is shown in Fig. 20.

Unusual sightings of icebergs have been variously reported almost over the entire North Atlantic north of lat 38°N. A chartlet portraying the unusual iceberg limits and positions of unusual but reliable sightings is shown in Fig. 21.

Ice Detection by Radar

With the advent of radar during World War II there became available to the mariner a valuable aid for the detection of ice over the fog shrouded regions of the Northwest Atlantic Ocean. However, the precise effectiveness of radar in ice was not known and conflicting reports existed on the behavior of radar for ice navigation. These reports together with the increased speed of merchant ships and the reliance placed upon radar emphasized the need for previse and trustworthy information.

A method for quantitative analysis of the ability of radar as an ice detection instrument was devised and field work carried out on the Grand Banks in 1945 by the United States Navy and in 1946 by the International Ice Patrol. Qualitative analyses of radar ice reports submitted by Hudson Bay shipping were made by the National Research Council of Canada from 1953 to 1957. The results of these surveys and additional reports from merchant ships transiting ice areas indicate that growlers are inconsistent targets, and that vessels relying on radar for safe navigation through ice infested areas might, in so doing, compromise their safety. It was established that in calm or slight seas dangerous ice formations of all types should be detected at ranges varying from 10 to 15 nautical miles for icebergs to 2-3 miles for small growlers and sea ice; however, during moderate and rough sea conditions when sea clutter extends beyond 2 miles on the radar scope presentation, growlers large enough to cause serious damage to ships might not be detected.

In 1959 and in view of advances in radar systems and the need for a quantitative evaluation of radar reliability and anticlutter device effectiveness a comprehensive study was commenced by the International Ice Patrol. Over a 3 year period using carefully controlled measurements by facilities of the Ice Patrol and from reports of cooperating ships, 343 precise observations were obtained upon which quantitative analyses could be based.

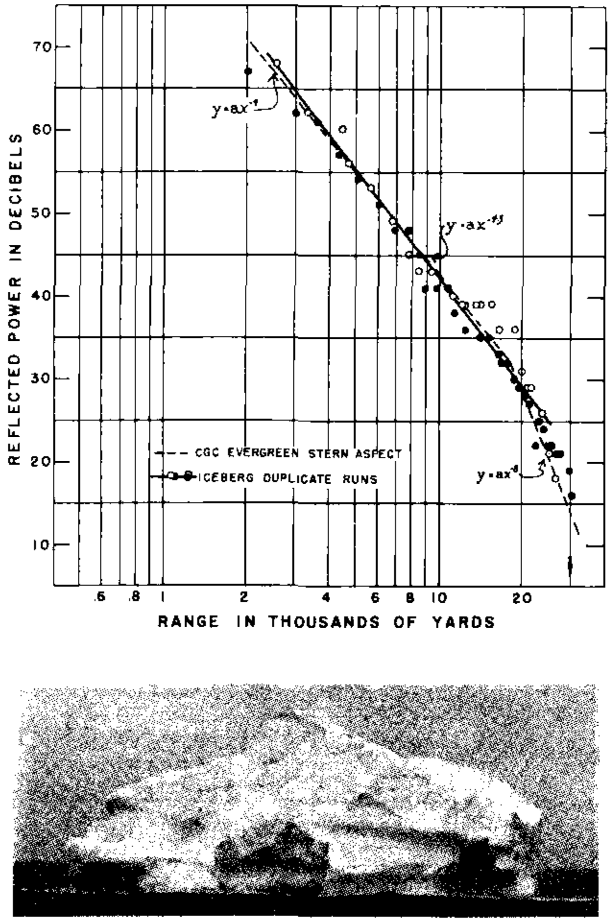


Fig. 23. Radar signal as a function of range of a large iceberg (44,000 sq ft) and of the ice patrol ship stern (740 sq ft).

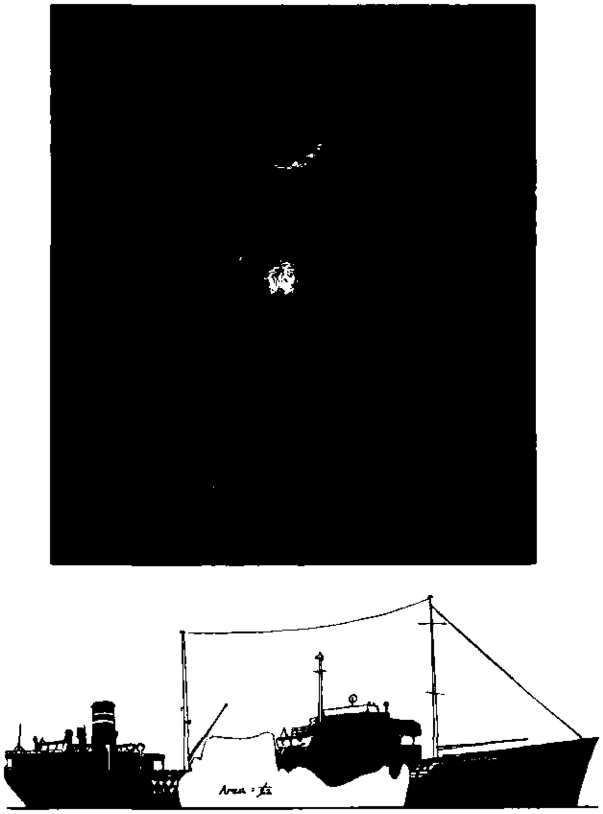


Fig. 24. Radar scope photograph comparing blip intensity of a ship with that of an iceberg (27 X 84 ft) and the vertical cliffs (300 ft) of Cape Race, Newfoundland. The ship is at 295°T, 8,200 yds; the iceberg is at 355°T, 8,200 yds; and Cape Race is north at 18,000 yds. Other targets are large icebergs.

Maximum Range of Detection

A plot of maximum ranges of detection is shown by Fig. 22. It can be seen that for large icebergs having broad cross sections, the expected detection range is between 10 and 15 nautical miles. However for sea ice floes and growlers having exposed cross sections of 10 and 100 sq ft[2] the maximum range of detection is 2 and 4 nautical miles respectively.

Reflectivity of Ice

The behavior of ice as a reflector can best be measured by either laboratory or field observations of the quantity of power returned from an iceberg. Observations were made on a large iceberg measuring the echo signal strengths with a test set. Results shown in Fig. 23 show the least squares best fit to have a slope of -4.3 which is in general agreement with the inverse fourth power law.

Similar reflected power was obtained using the stern aspect of one of the participating ships. This also is shown on Fig. 23. The iceberg had a radar cross section of 43,900 sq ft and the ship aspect had a cross section of 740 sq ft. A rough conclusion of interest to the navigator drawn from these measurements is that a ship is a better reflector of radar by a factor of about 62.

Another test is shown on Fig. 24 where signal strengths of both a ship and an iceberg were measured at the same range. Signal strength of the ship was 200 times greater than the iceberg which had a radar cross section 0.31 of the ship. The reflectivity ratio is 62.5 in this case.

Radar Propogation

In many instances those conditions which give fog and create the most need for radar also cause subnormal

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[2] The results of the radar ice detection tests by the International Ice Patrol are reported in English units of measurement (square feet).

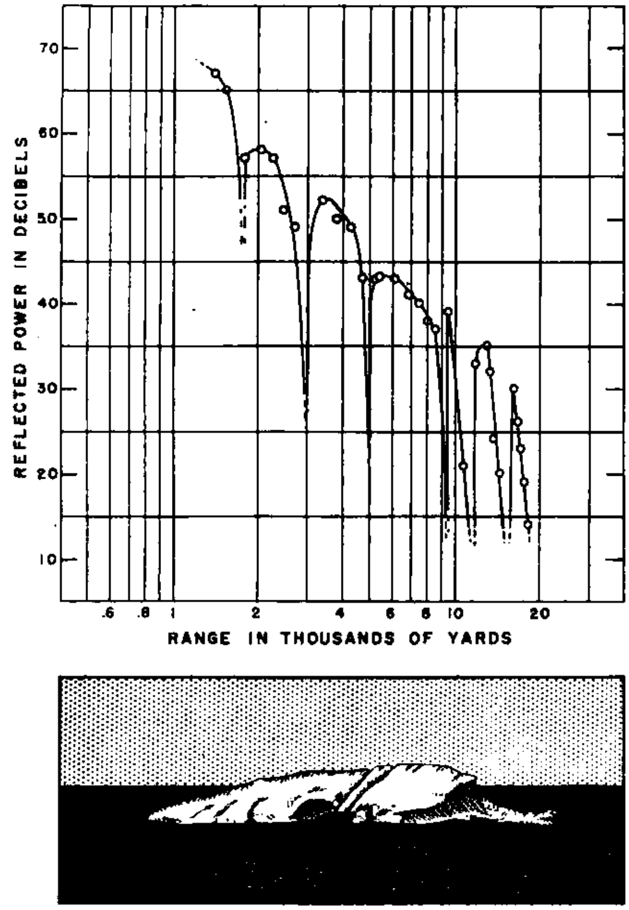


Fig. 25. Radar signal as a function of range for a low lying iceberg (33 X 247 ft, 5,360 sq ft). Maximum range of detection was 19,600 yds. Sea was calm. Subnormal propagations existed. Fade areas were well observed.

propagation of radar waves. When moist warm air from the Gulf Stream, or Atlantic Current continuation thereof, flows over the colder water of the Labrador Current and Grand Banks, advection fog normally results. This is a common occurrence in the Grand Banks area during much of the ice hazard season. In fact, the Grand Banks off Newfoundland and the potential iceberg drift area of the North Atlantic Shipping Lanes are the poorest visibility area of all the oceans during the entire year with the exception of an area south of the Kamchatka Peninsula, Pacific Ocean, during June, July, and August. The advection fog is often accompanied by strong southerly winds and concomitant radar sea return.

An example of subnormal propogation is given in Fig. 25 where five distinct "fades" are observed. Corresponding to these measurements, atmospheric observations and refractive ray diagrams corroborated the evidence of poor radar return. At this time it cannot be said with certitude to what extent the conditions on the Grand Banks may reduce radar ranges; however, there is little doubt that the subnormal propagation conditions are the rule rather than the exception.

Sea Clutter

Although it has been established that icebergs are very poor reflectors of radar, and that reflection might be further decreased by aspect and subnormal propagation conditions which exist on the Grand Banks during spring, it is well established that most icebergs do provide good targets and during calm sea conditions some reliance can be placed on radar. However, it appears that distinguishing small ice targets in heavy seas

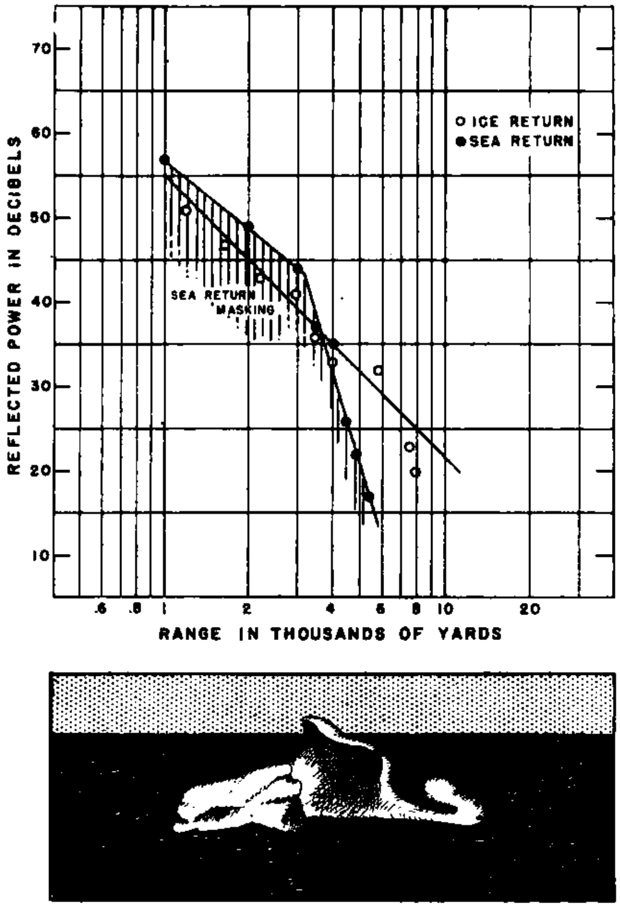


Fig. 26. Radar signal as a function of range for a growler (22 X 76 ft, 665 sq ft) and sea return (4.5 ft high, 200 ft long). Standard radar propogations.

might be the limiting factor in the reliability of radar as an instrument for providing safe navigation through ice infested waters. The basic phenomena of sea return or sea clutter have not yet been definitely established; however, it is well known that sea echo from waves acts as a built-in jammer, blanketing and obscuring small target echoes. Other things being equal, the strength of the sea return depends upon the state or roughness of the sea, which in turn depends largely upon the wind force. Experiments have shown that the range of sea clutter on the scope is very nearly a linear function of the sea state. The radar cross section and echo strength of sea return are difficult quantities to measure or compute because among other things the reflection surfaces extend from the ship to an indifinite range. The variation of reflected power with range does not necessarily follow the same relation as that for a ship or iceberg target as is apparent from the fact that the reflection from waves is less at longer ranges due to a decrease in the angle of incidence. In general, the decrease in sea return with range is more rapid than that for other targets. Experimental observations have confirmed that sea return can significantly mask the minimum range of detection. An example is shown by Fig. 26 where growler ice was initially detected at 8,000 yd range but was masked by the sea clutter effect of waves 4.5 ft in height at a minimum range of 4,000 yd rather than 1,000 yd without sea clutter effect.

This can be made more understandable when the theoretical reflection coefficients of water and ice are computed from Fresnels equations. For angles of incidence lower than 45° the reflection coefficient of water is three times greater than for ice. This infers that water is a better radar reflector than ice and is thus not surprising that sea clutter can entirely mask ice.

To the navigator the significance of Fig. 26 is that a ship proceeding toward a piece of ice of 5,000 tons mass at a speed of 15 nautical miles per hour will have that ice visible on its radar scope for 8 minutes.

A summary of the results of the International Ice Patrol radar ice detection experiments indicate the following results for prudent radar use in navigation.

Iceberg ice on the Grand Banks has a reflection coefficient of approximately 0.33 and reflects radar waves 60 times less than a ship of equivalent physical cross-sectional area.

The maximum range of radar contact is proportional to the fourth root of the physical cross-sectional area of icebergs. A statistical relation derived from 152 observations shows that growlers and medium ice floes normally cannot be detected at ranges over 4 miles.

The Grand Banks and contiguous areas of the North Atlantic Ocean exhibit conditions of subnormal radar propagation during the spring months when fog and ice hazards are most prevalent.

Waves over 4 ft in height might obscure a dangerous growler even with the expert use of anticlutter devices. If an ice target is not picked up beyond the sea return, it will not be detected at all.

Ice is not frequency sensitive. The response of ice to various radar bands is the same.

The use of sector scan, trained radar operators, and constant surveillance of the radar scope increases the probability of detecting ice by radar.

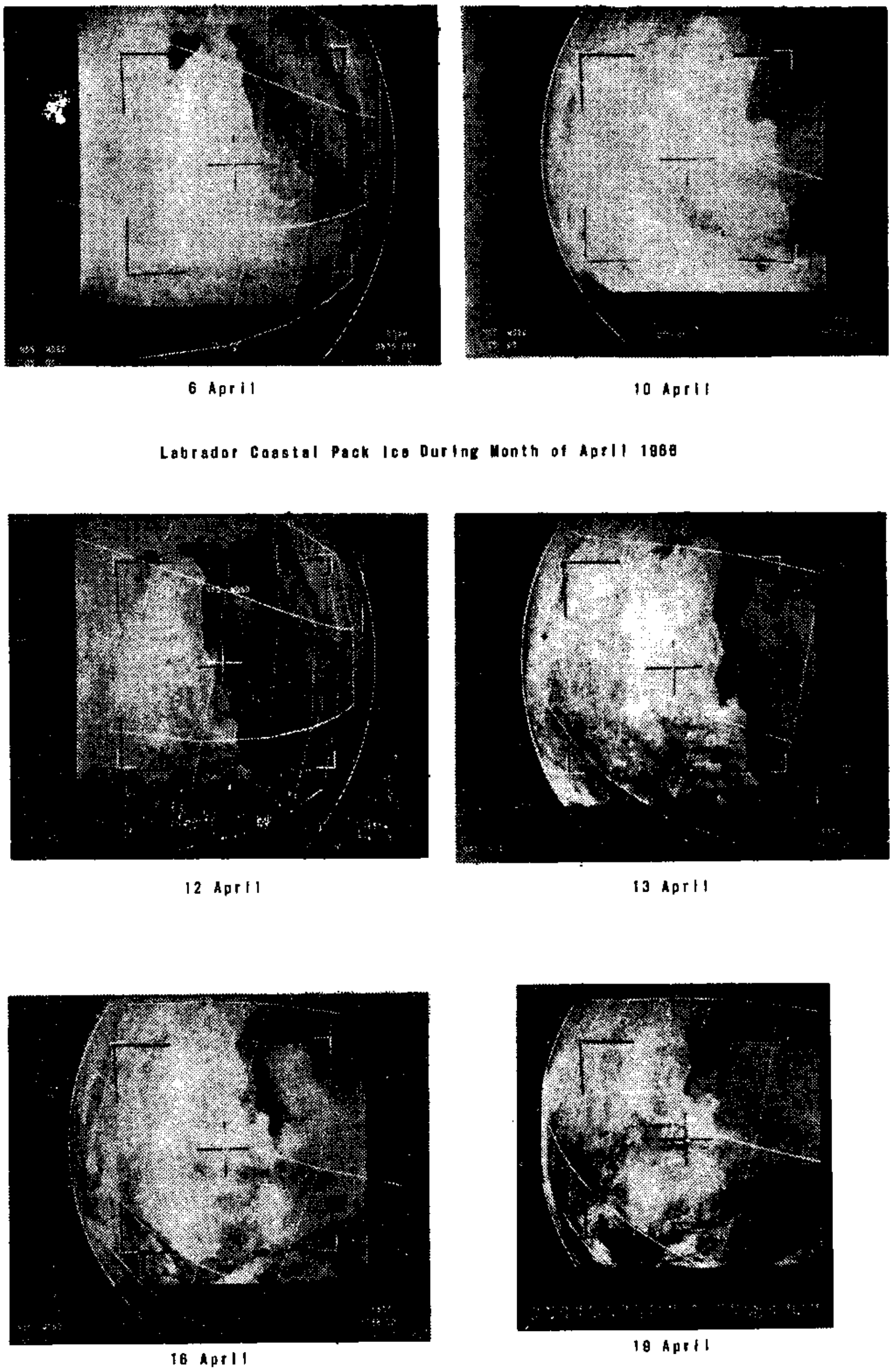
Commercial radar in common use on the ships of the world today cannot be relied upon for the detection of all ice drifting in the North Atlantic Ocean. It is definitely an aid, but it does not provide an assurance against the presence of all floating ice which might sink a ship upon collision.

Satellite Ice Surveillance

Interest in the use of satellites for sea ice surveillance first developed in 1960 when pictures from the weather satellite TIROS I revealed ice boundaries in the Gulf of St. Lawrence. A thorough evaluation study of this application was conducted by the United States Environmental Satellite Center, the Canadian Defence Research Board, and the Canadian Department of Transport during Project TIREC (TIROS Ice Reconnaissance) in February and April 1962 (Canadian Defence Research Board). TIROS IV provided satellite observations in the Gulf of St. Lawrence area during this period. The Canadian Department of Transport (CANDOT) co-sponsored the project and several other agencies cooperated. That study concluded that broad scale ice surveillance, with the charting of ice field of four tenths concentration or greater to an accuracy of plus or minus seven miles, was decidedly within the limits of the camera systems aboard the TIROS vehicle. Limitations were imposed by the satellite's orbital characteristics and control system.

The Canadian Meteorological Service and U.S. Weather Bureau conducted a semi-operational satellite ice surveillance study in April and May of 1963 using TIROS vehicles V and VI.

The National Research Council of Canada and the Meteorological Service of Canada conducted "APT Project NAIREC" at Frobisher Bay, Northwest Territories, in September 1964 which used transmissions



6 April

10 April

Labrador Coastal Pack Ice During Month of April 1966

12 April

13 April

16 April

19 April

Fig. 27. Photographs from the ESSA 1 satellite showing the Labrador coastal pack ice during April 1966.

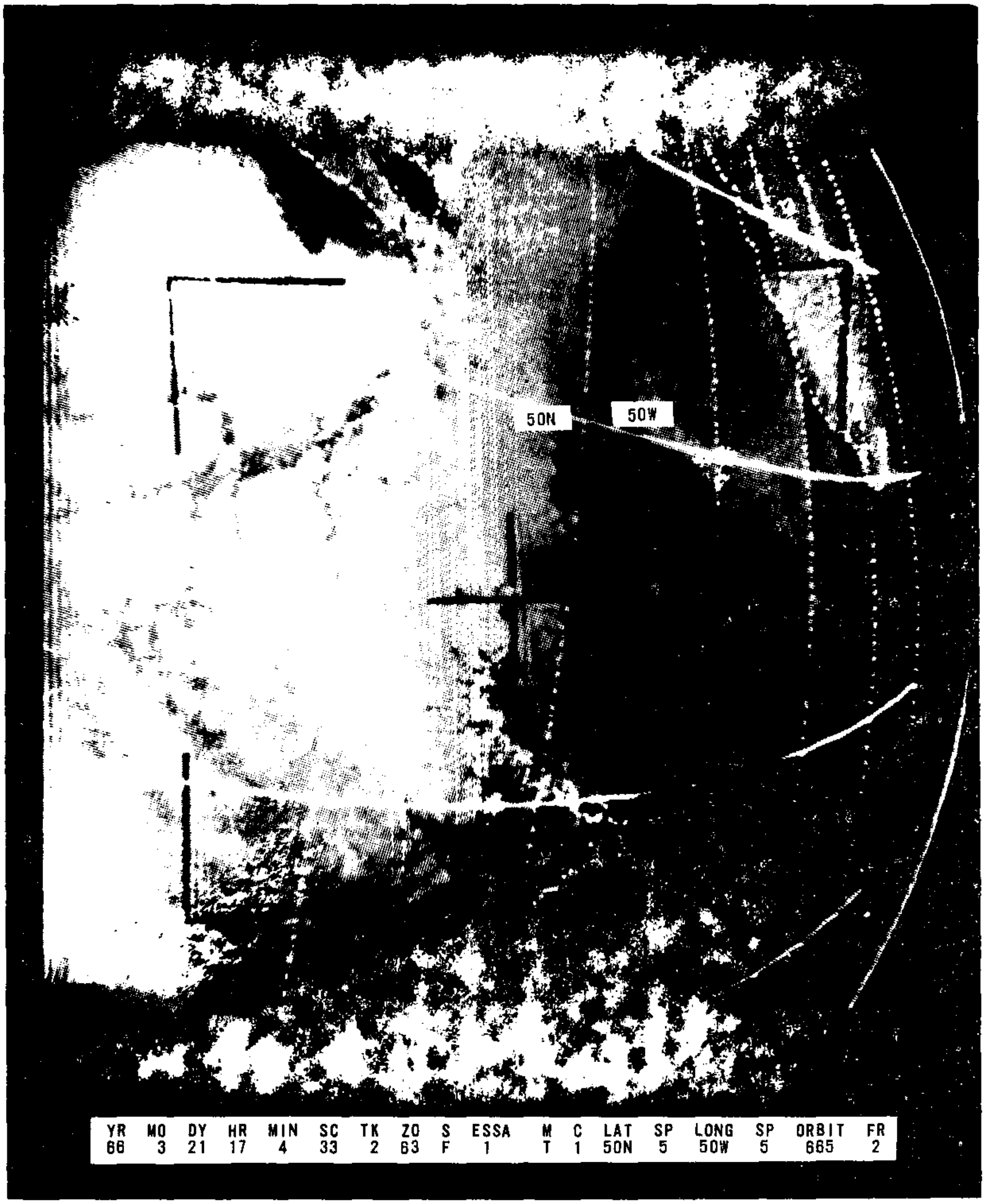


Fig. 28. Photograph from the ESSA I satellite showing open pack ice off the Newfoundland coast on 21 March 1966.

from NIMBUS I and learn how the data would fit into the Arctic ice forecasting system under nearly-operational conditions.

When the TIROS Operational Satellite (TOS) system became effective upon the successful launch and operation of ESSA I in February 1966, the International Ice Patrol set up a satellite ice surveillance project for the 1966 season of the International Ice Patrol (IIP). The primary objective of this study was to determine the potential for increasing the effectiveness of the Ice Patrol. The results established the effectiveness of satellite observations in mesoscale sea ice surveillance. Although icebergs have not been reliably tracked by satellite methods, the International Ice Patrol now routinely uses satellite ice information to establish the sea ice boundaries in the North Atlantic.

A series of photographs using conventional vidicon are shown by Fig. 27. These are from the ESSA I satellite at an orbital altitude of 400 nautical miles with a 480 line scan. The series demonstrates the well defined close pack fields with 6/8 to 8/8 concentrations of medium and thick winter ice, which are in some instances visible through thin overcasts. Discrete changes in the Labrador ice pack are observable from the 6 to 24 April 1966. A reconnaissance aircraft with a 280 knots cruising speed would require approximately 55 hr in flight to acquire the same information represented by the seven satellite photographs.

The greatest problem confronting the use of satellites for ice surveillance is the discrimination between open pack ice of 4 oktas concentration or less and certain types of cloud formations appearing to be similar in texture and brightness. Figure 28 is a good example of ice fields averaging 4/8 concentration which initially presented an evaluation problem because of their appearance. In this case the lower contrast and absence of sharp ice boundaries raise some uncertainties in evaluating the formations as ice or cloud. As evidenced by the results of an aircraft reconnaissance conducted on the same day the formations were definitely ice fields of 4/8 average concentration. A plot of the formations from the photograph shows excellent agreement with the aircraft reconnaissance data. These are compared in Fig. 29. A camera system providing higher resolution and greater grey tone distinction would help considerably in evaluating the formations appearing on the photograph discussed above.

Satellites currently in orbit such as the ESSA and NIMBUS carry Advanced Vidicon Camera Systems (AVCS).

A mosaic or montage is shown in Fig. 30 to demonstrate the arrangement of AVCS triads to produce a map-like display of broad areas. Shown also for comparison is the same area by ESSA III AVCS imagery provided by a higher flying vehicle.

An aid in removing transitory cloud regimes, and thereby revealing the image of the underlying surface, is the computer-processed satellite data. These data have been available from ESSA VII and ESSA IX of the TIROS Operational Satellite (TOS) System of the U.S. National Environmental Satellite Service. As the onboard vidicon camera shutter is activated, the response is stored on the satellite's video tape recorder. A readout command delivers the signal to a receiving center where the video signal is next digitized and mapped video arrays are produced and stored in a computer.

Because snow and clouds have nearly the same reflectivities, a problem exists in differentiating between the two. A technique of minimum compositing over a selected time period tends to minimize cloud interference. By saving only the minimum response of each spot for a selected number of days, any relatively bright feature which does not show up over a spot on all days

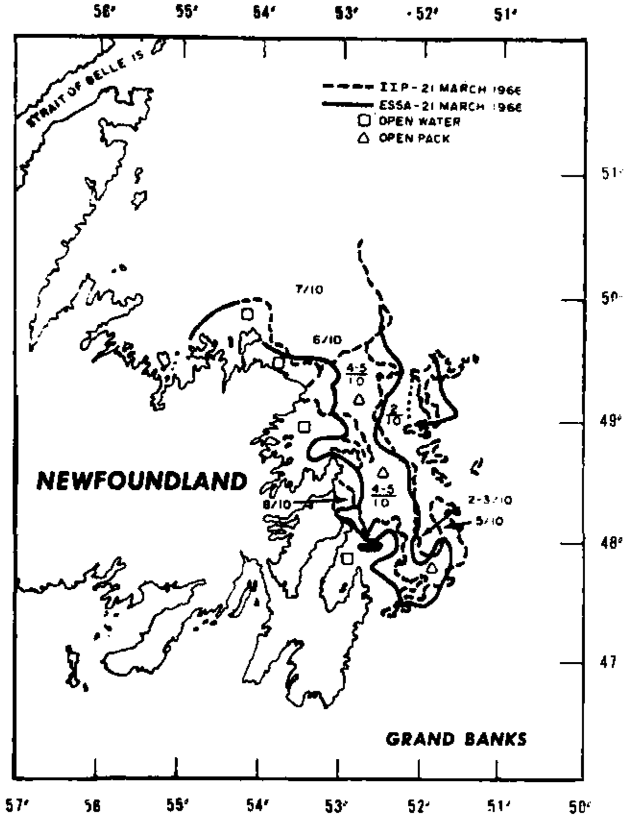


Fig. 29. Aircraft ice observation by International Ice Patrol compared to satellite photograph (Fig. 28) for the same day, 21 March 1966.

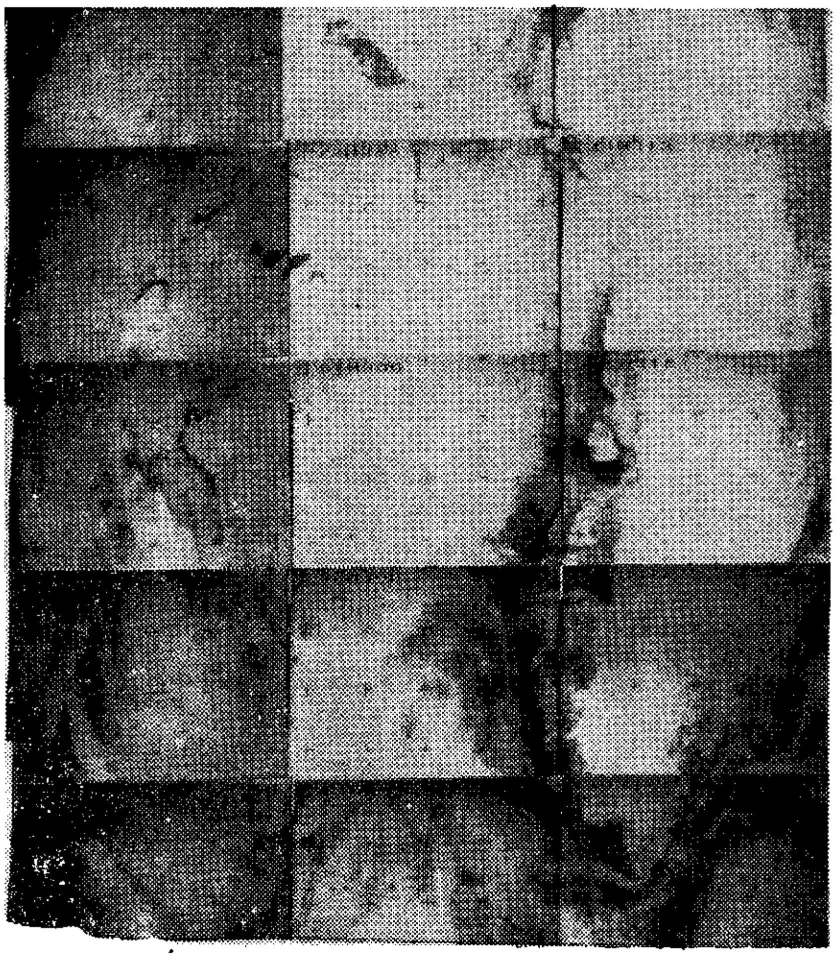
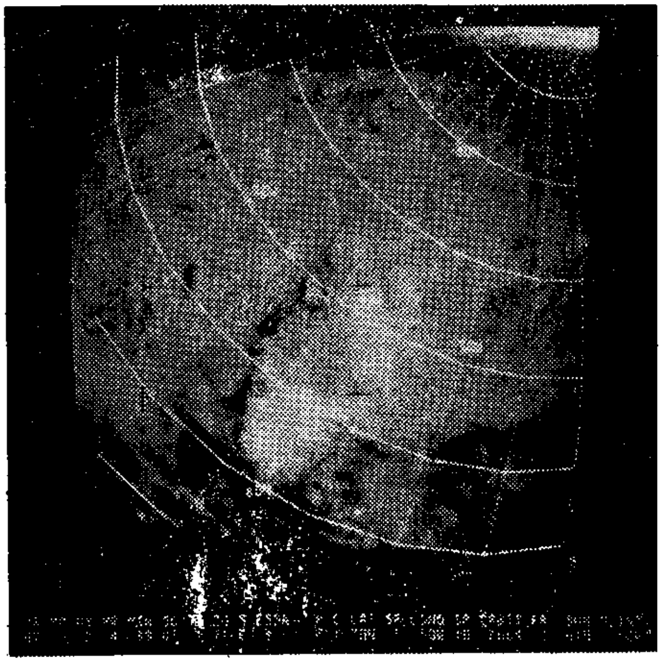


Fig. 30. (A) Nimbus II mosaic – Labrador Sea, Davis Strait, and Baffin Bay.



(B) Example of ESSA III A.V.S. imagery provided by a higher flying vehicle.

Fig. 31. Five day computer generated ice map – 14-18 May 1970.

Fig. 32. APT imagery received from ESSA VIII by the University of Dundee, Scotland, 31 March 1971. The ice edge off Greenland is clearly distinguishable. The receiving equipment was constructed at a low cost by the university electronics laboratory.

of the period is eliminated. Thus, transitory features such as clouds would be eliminated, whereas more permanent features such as snow or ice would be retained in the data field. The degree of cloud elimination varies according to the persistency of cloud systems in the area in question.

Time changes in minimum brightness can be produced by substracting the brightness values of two minimum composite periods and dividing the data field into three categories – increase, decrease, and no change in minimum brightness. Minimum composite charts have been produced for 5-day periods. One of these charts is shown by Fig. 31 and has proved to be an excellent source for the definition of ice pack boundaries. Persistent fog banks continue to mark underlying regions.

Fig. 33. Direct readout infrared imagery received at the University of Dundee from ITOS I satelite 4 February 1971. Note ice fields off Labrador Coast.

Many users are now employing operational satellites for surveillance purposes by means of the Automatic Picture Transmission (APT) System whereby the investigator has his own receiver for direct transmissions. As of January 1971 approximately eighteen APT receivers were in regular use in about six nations. Figure 32 shows an APT vidicon picture received at the University of Dundee, Scotland, from the ESSA VIII satellite. Here the ice edge off Greenland is strikingly apparent. Figure 33 is Direct Readout Infrared (DRIR) imagery received also at the University of Dundee from the ITOS I satellite. Generally infrared imagery requires greater interpretation than vidicon. However, in Fig. 33 the ice fields off Labrador and Greenland are plainly discernible.

Ice islands have been successfully tracked down the East Greenland Coast and large icebergs have been identified off Antarctica using satellite imagery. However, present satellites are not able to assist in detecting icebergs in the North Atlantic. It is hoped that future satellites with sensors designed for earth features and having greater resolution will provide this service.

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Discussion

Mr Markham: *"Do you have documented evidence of icebergs from around Cape Farewell crossing over Davis Strait onto Baffin Island?"*

Captain Dinsmore: *"Perhaps our best example is an iceberg which was sighted by Ocean Weather Station Bravo which is half way between Cape Farewell and the Strait of Belle Isle. During the late 1950's this Ocean Weather Station has sighted a large number of passing icebergs. The Station was asked to track these icebergs in an attempt to identify them. The information was not complete but the drift patterns gave every reason to believe they came from the Cape Farewell area. One iceberg had three polar bears aboard. Discussing this with people familiar with polar bears, it was felt that almost certainly this iceberg was an East Greenland iceberg. For the most part, however, such drifts are rare and we do not consider them significant."*

Dr Rodewald: *"I would like to ask you a question about the decline in number of icebergs in the last 20 years. With more and better information available through the use of radar and satellites, there seems to be no doubt that the number of icebergs has declined. However, is this decline related to their transport or in production? Since the data you have presented are only from below 48°N, you may have more northerly records which could suggest the main cause as production or transportation."*

Captain Dinsmore: *"As near as we can determine, it is a matter of transport of the icebergs. Although the Greenland glaciers which produce icebergs are generally retreating in much the same manner as Dr Løken has pointed out, this has not significantly affected the number of icebergs sighted in the Arctic. One glacier formerly producing bergs no longer reaches tidewater but there are two anomalies where iceberg producing glaciers are actually advancing today. The number of icebergs in Melville Bay and Baffin Bay, which is the main storage area of icebergs, was measured 25 years ago at something like 40,000 icebergs. Just recently we have verified that count. The supply is virtually inexhaustible but it is the transport that seems to be most important. We generally attribute the occurrence of icebergs on the Grand Banks as a function of the volume of the flow of the Labrador Current. In years when fewer numbers of icebergs have occured, there seems to have been a reduced volume of flow of the Labrador Current which has its initial beginnings in Baffin Bay. Farther south it is, of course, a complex function of mixed water of the Labrador Current with the Gulf Stream. Another reason for an apparent decrease in sightings below lat 48°N is that better observational techniques prevent duplications which we are sure existed in earlier years."*

Dr Løken: *"Do you have any method for estimating the depth of these icebergs or have you judged their depth from their height above sea level?"*

Captain Dinsmore: *"We have made some sonic measurements of their depth and from a simple statistical inference of how and where they ground, we find that icebergs rarely extend more than 200 m in depth. A large iceberg characteristically has a draft of about 200 m. We have found that the draft, not the underwater mass, of an iceberg is generally about 3 below to 1 above the sea surface."*

Dr Campbell: *"Is the state of the art of satellite detection of icebergs to the point where one can distinguish an iceberg from an ice field or an ice flow?"*

Captain Dinsmore: *"We are unable to even find an iceberg on a satellite picture yet. There was one large ice island tracked successfully by satellite observation – perhaps Mr Markham was more aware of this – down the east coast of Greenland. We have identified large icebergs in the Antarctic but, up to now as far as operational usage over the Grand Banks area is concerned, we have not detected an iceberg by satellite. The new APT satellites are absolutely superb for measuring limits of field ice and I think perhaps when we do get icebergs on the satellites we may discover, as Dr Rodewald suggested, there is more there than we realized."*

Contribution Number 6

Contributors to the Discussion

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A Look at the Bottom Marine Geology of the Northwest Atlantic

By B. D. Loncarevic[1] and A. S. Ruffman[2]

Abstract

The continental margins around the Northwest Atlantic are considered to be a good prospect for oil exploration. The resulting activities on the continental shelves of Nova Scotia, Newfoundland, Labrador, and southwest Greenland have greatly increased our knowledge of bottom configuration and structure. The surficial geology mapping has been extended by addition of substantial number of bottom samples. New interpretations of geological structures are possible with the aid of seismic survey and other geophysical results.

In the deep ocean the seismic reflection profiling, deep-sea drilling (JOIDES) and systematic magnetic surveys have been used for a number of hypothetical reconstructions of the geological history of the region. The major advances are due to a better understanding of the mechanism of generation of ocean floor within the context of the "Plate Tectonics". The present conjecture is that the Labrador sea floor was created during a period of 60 to 40 million years (m.y.) before present. The area has remained relatively stable since that time though vertical tectonic movements could have occurred throughout the last 40 m.y. Of particular interest is a discovery of a continental fragment, the Orphan Knoll, 260 km north of Flemish Cap (460 km from the coast of Newfoundland). This fragment is now at a depth of 2,000 m but the indications are that it was at a shallow depth 65 m.y. ago.

The ocean floor studies are one of the scientific frontiers which are being pushed with great urgency. An estimate made a few years ago would have suggested that it would take us till the end of this century to complete even reconnaissance type surveys over the major portions of the world ocean. The expectation of finding vast natural resources below the ocean floor provides a basis for a new thrust in exploration. Although in the next decade the advances in geographical knowledge will be great, the most important accomplishment might be in the field of the International Law if and when an agreement is reached concerning the ownership and management of the sea floor.

Introduction

The decade of the 60's has been a most remarkable one, not just for outer space enthusiasts, but also for oceanographers, in particular for those oceanographers who work in geological and geophysical studies of so-called 'inner space'. In the decade just past Geology-Geophysics has undergone a scientific revolution of the sort that probably only occurs once a century in any particular discipline. It is interesting to note that many of the advances have occurred between ICNAF's two environmental symposia in 1964 and 1971.

By 1964 scientists had developed general ideas about the nature and structure of the ocean basins. They had described the ocean's main features, the striking linear mid-ocean ridges, the abyssal plains, the zebra-like striped pattern of magnetic anomalies seen over the ocean floor and the continental slopes. However, prior to 1963 there was no generally accepted comprehensive scheme that tied all this knowledge together. It was perhaps Vine and Matthews' (1963) explanation of the striped ocean-floor magnetic pattern that set in motion geologic thought and produced today's 'new global

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tectonics' or 'plate tectonics' theories. This breakthrough allowed the combination of the old continental drift theory of Wegener's (1915) with that of sea floor spreading (Dietz, 1961; Hess, 1962) and eventually with the theory of transform faults (Wilson, 1965).

By 1968-69, after a deluge of publications, some of which will remain for some time as standard references (Isacks *et al*, 1968; Le Pichon, 1968; McKenzie and Morgan, 1969), earth scientists had reached a much higher level of understanding of the structure of the earth and to some extent of the processes that shape it. In the past 2 or 3 years more detail has been added and parts of the theory confirmed.

The plate tectonic theory of formation of the earth's crust divides the whole of the earth's surface into six or seven major crustal plates consisting of both continental and oceanic crust. The plates are believed to be in continual relative motion, with their boundaries either at mid-ocean ridges such as Iceland and the Reykjanes Ridge or at the deep ocean trenches that are found seaward of the island arcs of the Pacific and Atlantic and west of the great Andes – Rockies mountain chain. The Puerto Rican trench is the eastern boundary of a minor plate.

It is believed that new oceanic material is continuously being added to the edges of the plates as a series of intrusions of rock that originated in the earth's mantle at a depth of about 120 km. This is in part confirmed by the very tight concentration of earthquakes over the mid-ocean rifts. As new material is being accreted to the mid-ocean edges of the plates the geometry of a sphere demands that other material be removed from the plates to avoid gross crustal deformation. This consumption of crust occurs at the oceanic trenches where the leading edges of the moving plates sink back into the mantle and are 'subduced' or reabsorbed. Thus the trenches are the focus of some of the earth's most powerful earthquakes. The plates appear to be moved by a series of major convection cells, as yet poorly understood, with hot material rising as molten intrusions at the ridges, cooling and moving outwards over great distances at rates of up to 8 cm/y, then finally sinking at the trenches perhaps as much as 100 million years after the original intrusion at a mid-ocean rift.

It is now believed by most geologists and geophysicists that the major features of the Earth can be explained in terms of plate tectonic processes or by the interaction of plates along their coincident boundaries. However, it is only fair to say that the theory does not answer all the problems or explain all the details and that some geologists have been most hesitant to accept it for this reason (Beloussov, 1970; Meyerhoff, 1970 *a* and *b;* Meyerhoff and Teichert, 1971).

One of the projects that is systematically filling in detail is the Deep Sea Drilling Project of the National Science Foundation. Since 1968 Scripps Institution of Oceanography has operated the drilling ship, *Glomar Challenger* (Fig. 1). Unlike conventional offshore oil rigs *Glomar Challenger* is specially designed to drill for scientific purposes in deep water. It can drill in water up to 6 km deep and can drill holes in the ocean bottom up to 1 km in depth. The drilling operation at sea represents a high degree of technical sophistication. With the use of an automatic dynamic positioning system that homes on a beacon dropped to the bottom, the ship's position can be maintained for up to 5 days within 15-25 m, even in winds up to 35 knots. Modifications to conventional drilling equipment have enabled this ship, during 20 two-month cruises, to make a number of exciting finds including one confirmation of continental drift during a traverse across the South Atlantic (Maxwell *et al.*, 1969). In the summer of 1970 *Glomar Challenger* entered the Labrador Sea on Leg 12 with an international team of 10 scientists representing 7 countries. The work of the drill-ship in the area of ICNAF's concern will illustrate the advances in understanding of sea floor geology.

Labrador Sea – Secondary Topography

In looking at the Labrador Sea (Fig. 2) the importance of secondary sedimentation processes is evident. Traditionally it has been considered that the deep oceans have been subjected to only a steady rain of pelagic sedimentation in the form of microscopic clay particles or organic carbonate shells such as cocoliths or foraminifera. It was thought that the pelagic sediment would evenly blanket the deep-ocean floor.

However, it is now known that periodic turbidity currents (or submarine avalanches) contribute a large amount of material from the margins of the continental shelves to the abyssal plains. These turbidity currents are in fact responsible for burying in places all topographic relief and creating the extremely flat abyssal plains such as the small plain northeast and east of Flemish Cap.

As a result of the widespread use of continuous seismic profilers it is now realized that the deep oceans are not evenly blanketed by a constant thickness of sediment. Rather it is now known that in addition to turbidity currents, dynamic deep-ocean currents, probably resulting from thermo-haline circulation, are redistributing, and in some areas piling up, oceanic sediments (Johnson and Schneider, 1969). The resultant topography is referred to as 'secondary topography'. The present bottom topography of the Labrador Sea is now

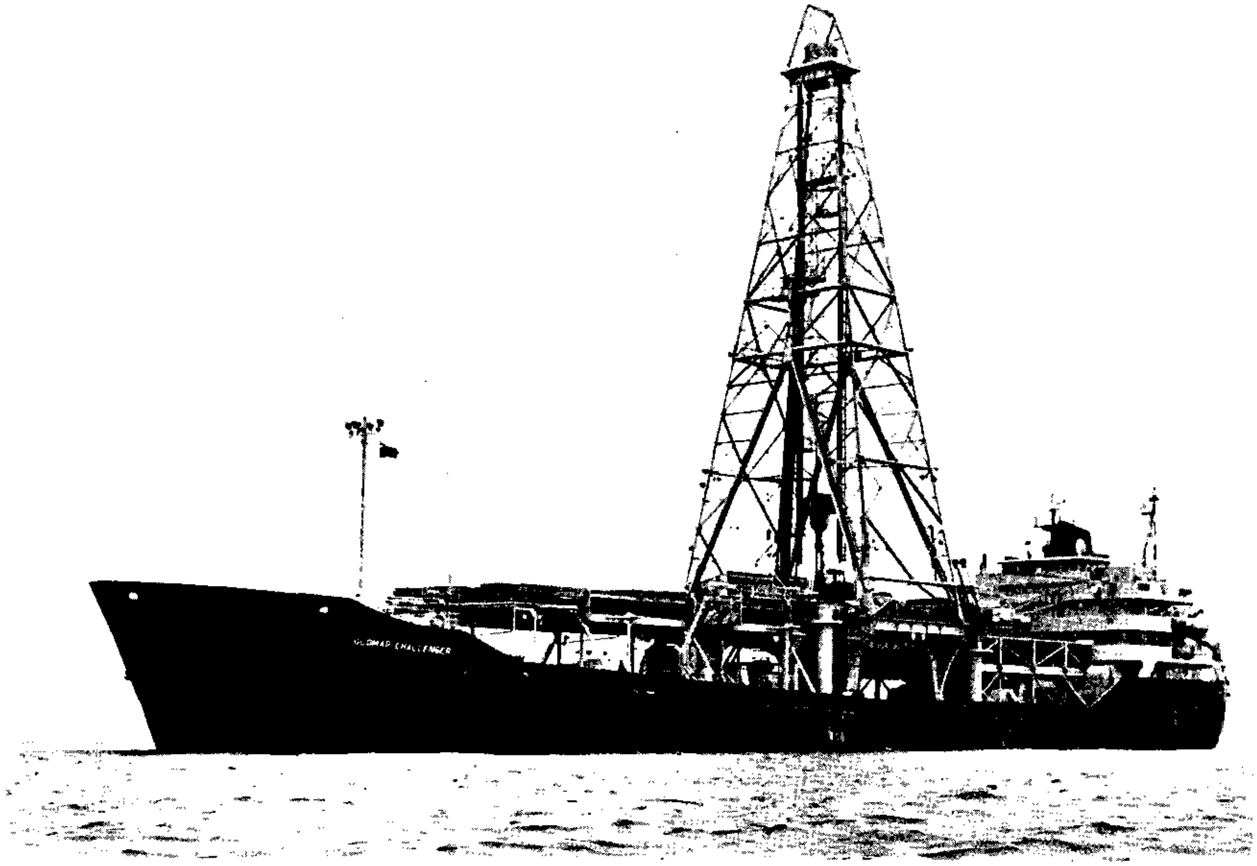

Fig. 1. Port view of *Glomar Challenger*. The drilling vessel is owned and operated by Global Marine Inc. which holds a subcontract with Scripps Institution to do drilling and coring. *Glomar Challenger* weighs 10,400 tons, is 400 ft long and the million-pound hook-load capacity drilling derrick stands almost 200 ft above the waterline. Forward is the automatic pipe racker which holds 24,000 ft of 5-inch drill pipe; crew's quarters for 69 are towards the stern. A complete second drill string is held below decks. Photo courtesy of Deep Sea Drilling Project.

virtually completely controlled by secondary processes and the original jagged basaltic terrain has been muted by sedimentary cover (Johnson *et al.*, in press; Davies and Laughton, in press). Figure 3 shows two sections of continuous seismic profile from *Glomar Challenger* illustrating how, as the result of bottom currents, the present sedimentary cover is uneven and bears no relationship to the underlying bedrock. This figure also shows ripple marks on the bottom that are often associated with the redistribution of sediments by bottom currents. These ripple marks are of 1 to 2 km wavelengths and heights of up to 35 m.

A dominant feature of this type of secondary topography in the North Atlantic is the so-called contour current ridge or sediment ridge. The Feni Ridge, named after an ancient Irish tribe, lies along the eastern and southern base of the Rockall Plateau (Jones *et al.*, 1969). A second well-known ridge, the Gardar Ridge, named after Gardar Svavarsson who was first to explore Iceland, lies parallel to and along the eastern flank of the Reykjanes Ridge (Johnson and Schneider, 1969). The third ridge shown on Fig. 2 trends southwest from Cape Farewell. Johnson and Schneider (1969) have proposed to name it the Eirik Ridge after Eirik the Red, the first explorer and settler of Greenland. Figure 2 shows several other sedimentary ridges, often not well documented, that are thought to traverse the Labrador Sea floor from northeast to southwest, (Laughton, in press).

It is believed that the bottom currents of the Labrador Sea were persistent and they appear to have maintained the same general pattern for as much as the last 30 million years (m.y.) This was established by the

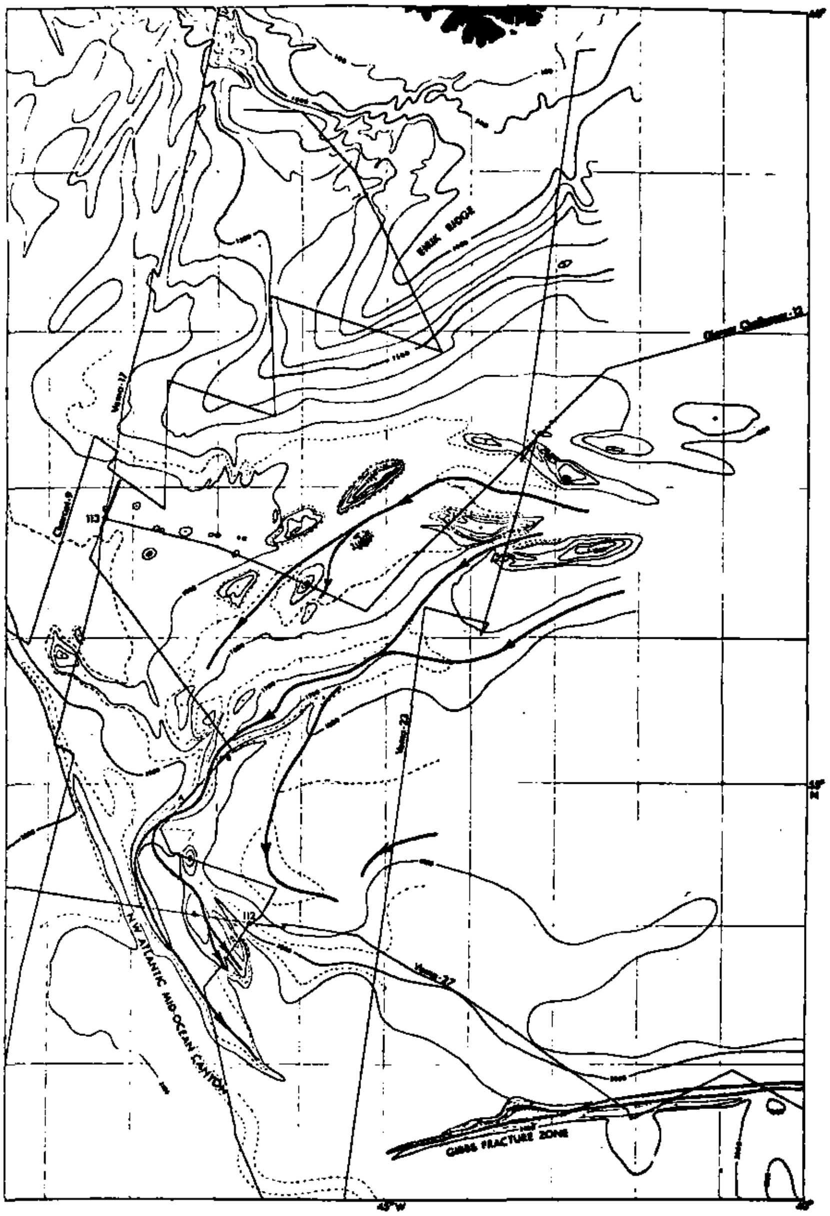

Fig. 2. Bathymetry of the southern Labrador Sea modified from Johnson *et al* (in press) by Laughton (in press). The Eirik Ridge is shown trending southwest from Cape Farewell. A series of sedimentary ridges are shown trending southwest from the area of 46° 30′N and 43°W. They are contained within a semicircle of basement highs which are thought to represent the old spreading centres of the Labrador Sea. Sedimentation within the semicircle of basement highs is controlled mainly by bottom currents while to the west and north the main control of sedimentation is by continentally derived turbidity currents. The Northwest Atlantic Mid-Ocean Canyon which stretches from the northern Labrador Sea to the Sohm Abyssal Plain is shown in part (Heezen *et al.*, 1969). More detailed bathymetry of the northern Labrador Sea may be seen in Johnson *et al.* (in press).

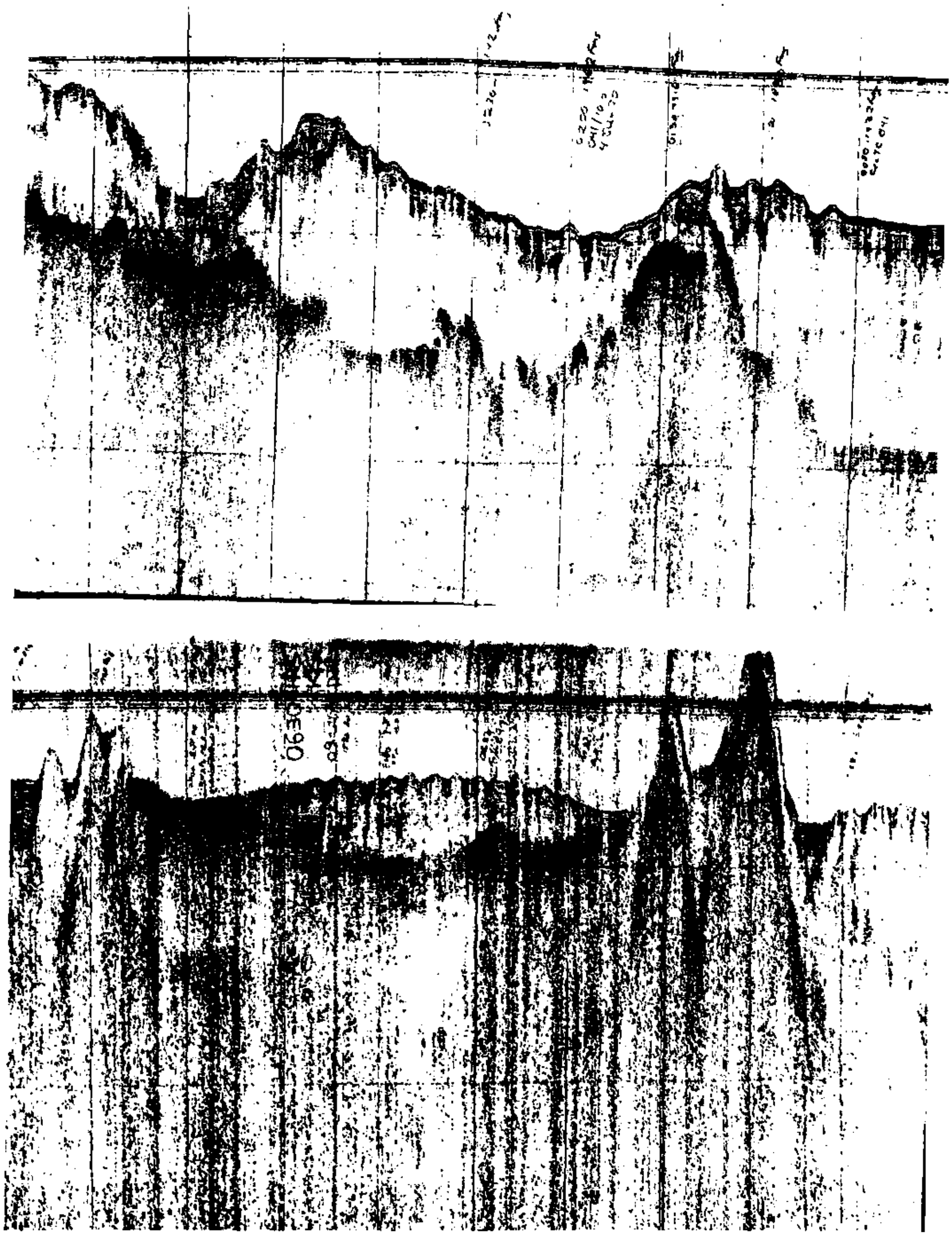

Fig. 3. Upper photo: Section of the *Glomar Challenger's* continuous seismic profile running from B through A on Fig. 2 (left to right). The upper surface represents the reflection from the sea bottom. The ship has crossed the same sedimentary ridge twice. The lower reflector is the bedrock surface (probably basalt) and it can be seen that the sedimentary thickness bears no relationship to the underlying bedrock surface. This is because the bottom currents are often not controlled by bottom elevation. One second of two-way travel time is represented by the distance between the horizontal lines in both photos and the vertical exaggeration is about 18:1.

Lower Photo: Seismic profile over the old Labrador Sea rift valley showing a lens of current-laid sediment, ripple marked on its upper surface, and contained between peaks of basaltic outcrop. Notice how the body is thickest in the centre and the margins are lower where presumably current velocities were too high to permit deposition. Profile is centered at 47°N on the *Glomar Challenger's* track on Fig. 2. Photos from Laughton *et al.* (in press).

Glomar Challenger's drilling at site 112 (Fig. 2) and by the use of continuous seismic profiles which provide a three-dimensional picture of the sediment and permit extra polation of sedimentary horizons away from the drill hole. At site 112 a prominent seismic reflector was dated and found to be a layer of sediments laid down by bottom currents during the Oligocene period (37 m.y. ago). From seismic profiles collected by the research ship *Vema* and by *Glomar Challenger* it was found that this reflector extends to the north and east of the drilling site (Fig. 2). Sediment deposition by similar currents appears to have persisted over the area up to the present time.

Labrador Sea – History of Development

Another of the *Glomar Challenger's* drilling sites, site 111 on Orphan Knoll, provided an insight into the early history of the Labrador Sea. The Knoll was known as early as 1917 as a shallow area of about 1,800 m surrounded by water up to 3,400 m deep, lying 250 km north of Flemish Cap (Fig. 5). Several sounding lines were obtained over the Knoll during the next 50 years including two during the ICNAF NORWESTLANT Surveys of 1963. However, no scientific work was done over Orphan Knoll till 1969 and in 1970 the Deep Sea Drilling Project drilled a 250-m hole near the centre of the Knoll.

This hole produced exciting results. It demonstrated that 175 m.y. ago Orphan Knoll was a part of a continental landmass, was above water, and was receiving sediment probably from a series of nearby streams. There was then some uplift and erosion. About 120 m.y. ago the Knoll began to sink and was covered by shallow water limestones. At some point in this period the Knoll was separated from the continent. Slow sinking continued till about 75 m.y. ago when the Knoll apparently began to sink rapidly and subsided from about 500 m depth to its present depth of 1,800 m very quickly (within 10-15 m.y.) (Laughton *et al.*, in press). Orphan Knoll is a continental remnant that has stood isolated and neglected for some 60 m.y., hence the name Orphan.

In Fig. 4 one can see a west-to-east seismic profile across the Knoll with the approximate drilling site marked. On the east the Knoll slopes very steeply to the abyssal plain while on the west the slope is less steep and the turbidity current-laid sediments are banked up against the Knoll. The top surface of the eastern edge of the Knoll is cut by a marked series of intrusive hills of an unknown nature. The top of the shallow water limestones discovered during drilling can be seen as the pronounced internal reflector 0.2 sec (190 m) below the sea floor at the drilling site (Fig. 4).

The detailed sinking history of the continental margin revealed at Orphan Knoll was only one insight into the early history of the North Atlantic. The second insight came from the fragments of anthracite coal contained in the 175 m.y. old continental sediments. Trace element studies showed that this anthracite was more closely related to the anthracite of south Wales than any known deposit in eastern North America (Bloxam and Kelling, in press). If the anthracite on Orphan Knoll is unrelated to coals in the Canadian Maritime provinces and that elsewhere in eastern North America but is related to anthracite in south Wales, then it is not unreasonable to suggest that at the time of deposition 175 m.y. ago Orphan Knoll may not have been too distant from Wales. This single item by no means proves the theory of continental drift. However, it was an earlier collection of a series of similar arguments that led Wegener (1915) to suggest his theory of continental motion. In the Labrador Sea there is more evidence for sea floor spreading.

This evidence is found in the striped pattern of magnetic anomalies. The Vine and Matthews' (1963) theory suggested that the striped patterns of magnetic anomalies found symmetrically on either side of the central rift zones were caused by intrusions of molten basalt which when solidifying is magnetized in the direction of the earth's magnetic field at the time of intrusion. The earth's magnetic field is known to have reversed itself some 30 to 40 times in the last 80 m.y. Thus as new material from deep within the Earth is being added along the rift zones it is being magnetized with either a normal or reversed magnetization depending on the incumbent field at the time of intrusion. As new material is intruded along the ridge axis the older lavas move outwards away from the axis and carry with them the positive or negative magnetic anomaly record of the earth's normal or reversed field. Hence a striped pattern of magnetic anomalies, symmetrical about the ridge axis, is developed. The anomaly pattern can be mapped and related to the spreading history of the land masses. Geophysicists are now able to date the time of creation of certain prominent magnetic anomaly stripes with a fair degree of certainty (Heirtzler *et al.*, 1968; McElhinny and Burek, 1971). It is important to remember that a certain magnetic anomaly strip on the ocean floor represents an isochron and as such may be used to match with other stripes of the same age. Traditionally the geophysicist numbers the anomaly stripes outwards from the ridge axis and assigns an age to them – hence anomaly 5 is 10 m.y. old and anomaly 31 about 72 m.y. in age.

Laughton (in press) has compiled the magnetic anomaly data of the Labrador Sea and Figs. 5 and 6 show the progressive development of the North Atlantic

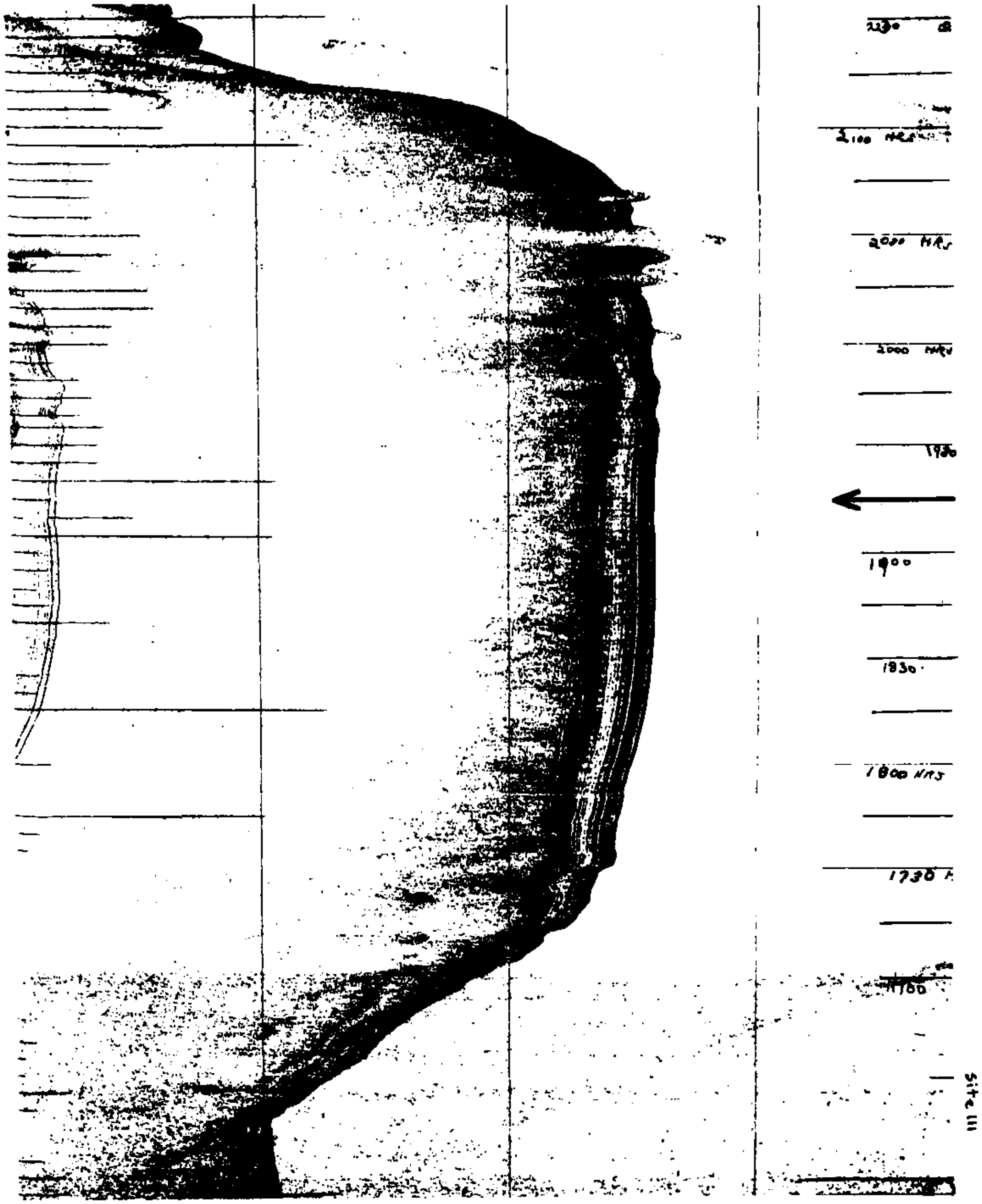

Fig. 4. West to east (left to right) seismic profile across Orphan Knoll. The separation of the horizontal lines represents one second of two-way travel time. Vertical exaggeration about 18:1. The marked reflector 0.2 seconds below the top of the Knoll is the top of the shallow water carbonates that were laid down between about 120 and 75 m.y. ago. Approximate drilling site (site III) is marked by arrow. Photo from Laughton *et al.* (in press).

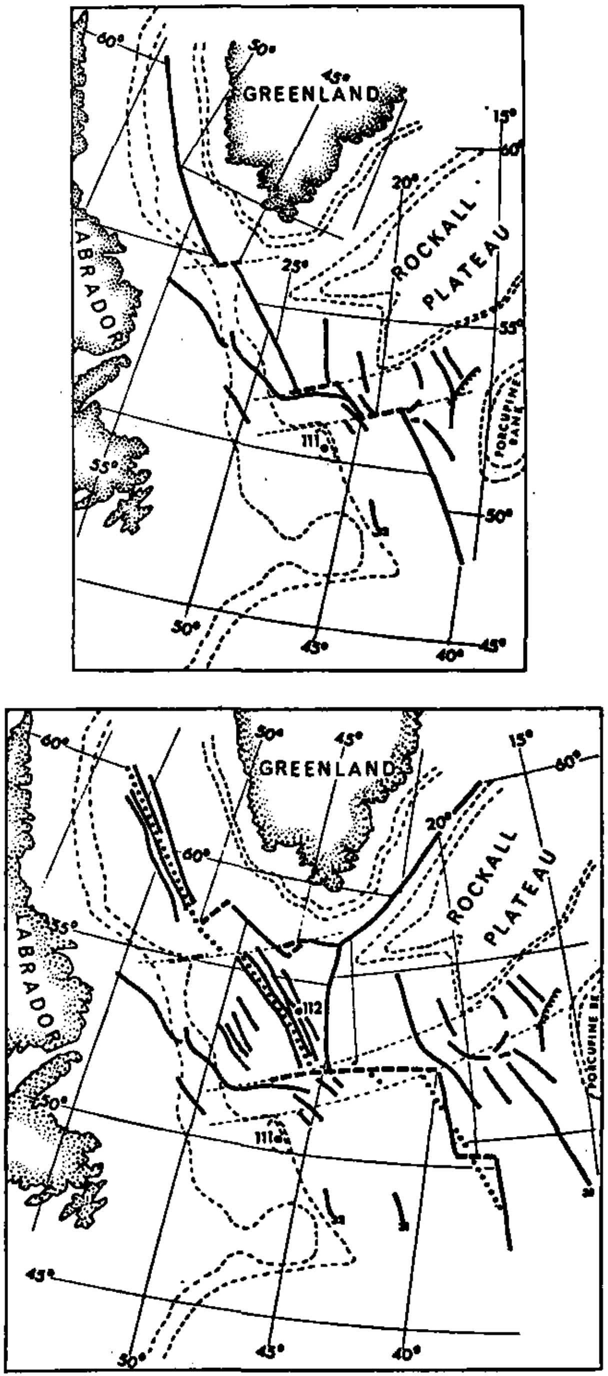

Fig. 5. Top: Palaeogeographic reconstruction of the North Atlantic at anomaly 31 (72 m.y. –Late Cretaceous). The North Atlantic has just begun to open with Greenland, Rockall Plateau, and Europe acting as one plate and North America the other. In Figs. 5 and 6 the top and bottom of the continental slope are shown as dashed lines, positive magnetic anomalies shown by heavy solid lines, fracture zones by heavy dashed lines and the dotted lines represent either extinct or present spreading axes.

Bottom: Palaeogeographic reconstruction at anomaly 24 (60 m.y. – Palaeocene). Labrador Sea has partially opened. At this point a triple point develops and Greenland begins to move northward relative to the Europe-Rockall plate and to the North American plate. The dotted line shows the spreading axis prior to 60 m.y. Figures from Laughton (in press).

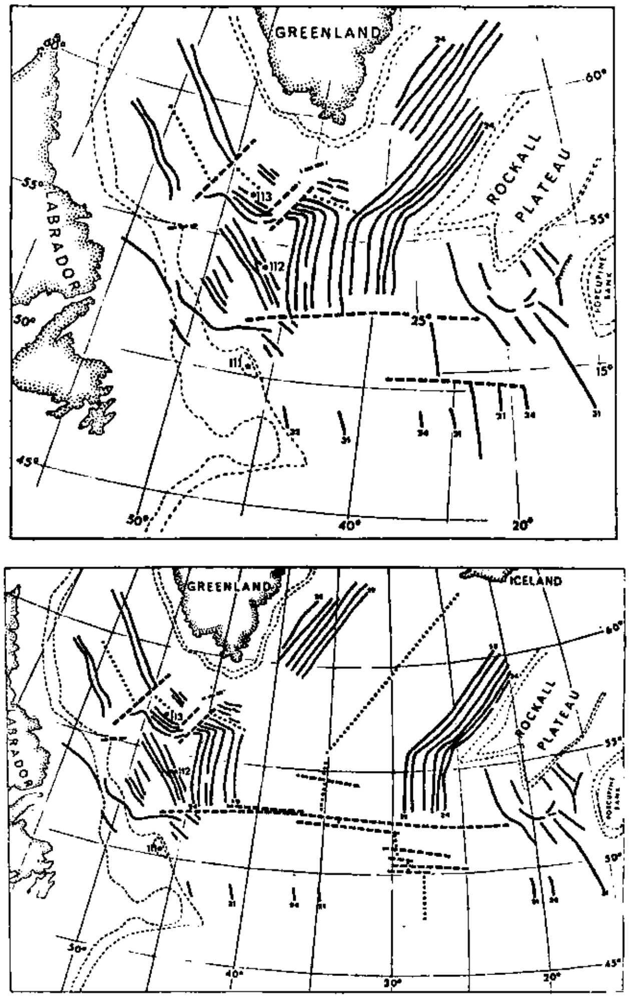

Fig. 6. Top: Palaeogeographic reconstruction at anomaly 19 (47 m.y. – Middle Eocene). Labrador Sea has finished opening and Greenland acts as single plate with North America right up until the present time.

Bottom: Present magnetic trends in North Atlantic. No trends younger than 47 m.y. are shown. Figures from Laughton (in press).

beginning about 80 m.y. ago. Thus the following sequence of events took place to develop the 'fishbowl' with which this symposium is concerned.

1) Approximately 175 m.y. – uplift of central part of the large northern continent of 'Laurasia'. Deposition and subsequent erosion in the Orphan Knoll area.

2) 175 m.y. to 82 m.y. – some dyke intrusion, perhaps minor opening and development of a shallow proto North Atlantic Ocean between North America and the European plate.

Fig. 7. Palaeogeographic reconstruction of North and South America with Europe and Africa done by matching the 500 fathom lines using a computer. From Bullard *et al.* (1965).

3) 82 m.y. – European plate (with Rockall Plateau and Greenland) breaks from Labrador pulling off fragments like Porcupine Bank, Flemish Cap and Orphan Knoll (Fig. 5, top).

4) 82 m.y. – 60 m.y. (anomaly 24) – Labrador Sea and North Atlantic open. Orphan Knoll sinks slowly (Fig. 5, top).

5) 60 m.y. (anomaly 24) – Rockall Plateau breaks from Greenland initiating a triple junction and a shift of spreading axes and fracture zones to accommodate the new geometry (Fig. 5, bottom).

6) 60 m.y. – 47 m.y. (anomalies 24-19) – simultaneous opening of Labrador Sea, Reykjanes Ridge and North Atlantic (Fig. 6, top).

7) 47 m.y. (anomaly 19) – Greenland stops moving relative to Labrador and Labrador Sea growth finishes (Fig. 6, top).

8) 47 m.y. – present (anomalies 19-0) Reykjanes Ridge and North Atlantic grow as the European plate separates from the Greenland-Labrador plate. Iceland develops as a subareal part of the Reykjanes Ridge about 16 m.y. ago (Fig. 7, bottom).

9) 1947, Mount Hekla erupts in Iceland, 1958 eruption on island of Fayal in the Azores and in 1963 new island of Surtsey appears off southern Iceland as the process of opening the North Atlantic continues through to the present day.

Thus by working out the details of continental drift one may reconstruct the relative positions of continents before drift began. One early fit is that of Bullard *et al.*, 1965 (Fig. 7). This fit, done by matching the edges of the continental margins, is known to be no longer exact and detailed work such as that done by Laughton (in press) will greatly improve the fit.

Greenland and Baffin Island Shelves

Next we consider the shallow submerged edges of the Labrador Sea, the continental shelves, beginning with the Greenland Shelf and working counterclockwise to the Scotian Shelf. The Greenland Shelf along with that of Baffin Island are the least known of all the shelves within ICNAF's concern. Holtedahl's (1970) compilation of the bathymetry off West Greenland is seen in Fig. 8. The area is a typical glaciated continental shelf, highly dissected and with a marginal channel paralleling the coast.

Olaf Holtedahl (1950) and his son Holtedahl (1958) were first to recognize these marginal channels as being glacial in origin (Holtedahl and Holtedahl, 1961). The marginal channels are separated from the shelf edge by a number of banks well known for their fishing. A number of ice-cut transverse troughs provide access from the marginal channel to the open ocean. It appears that during glacial times ice moved longitudinally down the axes of the marginal channels and drained through the openings across the shelf to the glacially lowered ocean. A companion paper to that of Holtedahl's (1970) on the West Greenland Shelf is that of Løken and Hodgson (1971) describing the shelf along the east coast of Baffin Island.

Labrador Coast

The Labrador coast shows much the same glaciated bathymetry as seen on the Greenland and Baffin Island Shelves (Fig. 9) (Grant, 1970). The marginal

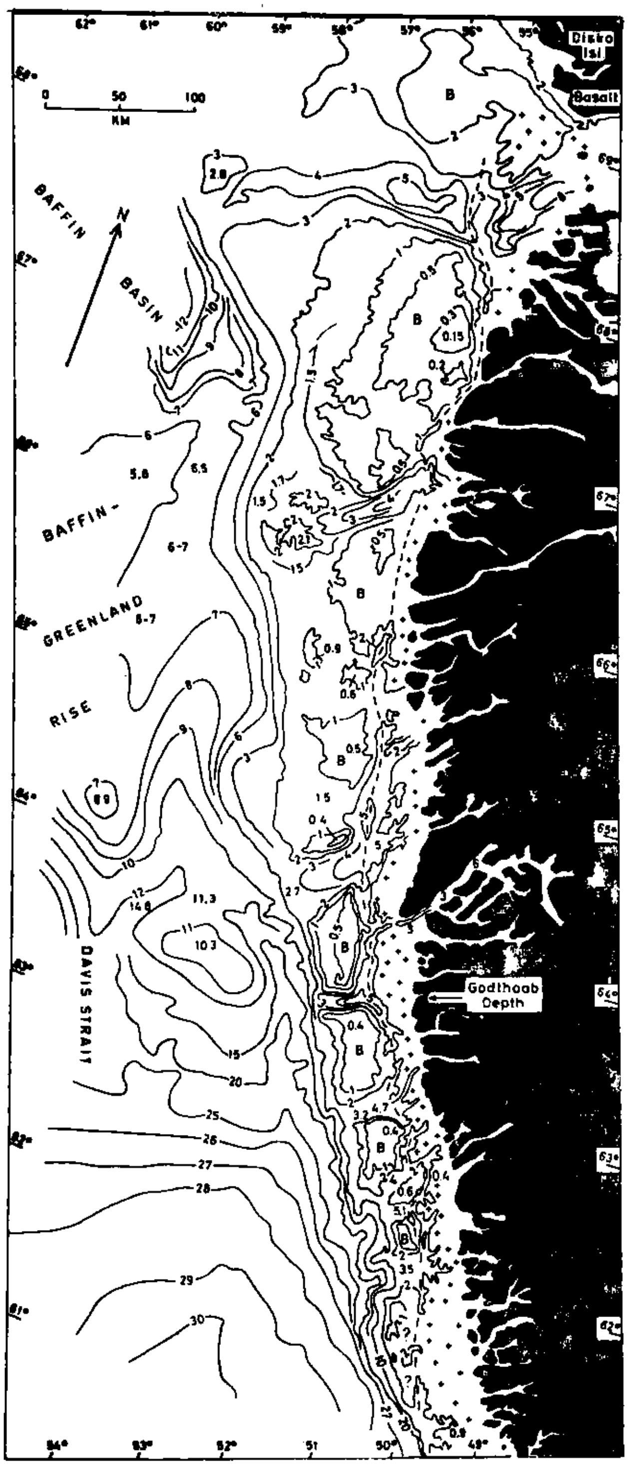

Fig. 8. Bathymetry of West Greenland showing a glaciated fjord system on the coast and a series of marginal channels and transverse troughs offshore. Contour interval is 100 m. From Holtedahl (1970).

channel is much more pronounced and marks the geological contact between the hard crystalline rocks of the Precambrian and the younger, seaward-dipping, sedimentary rock (Fig. 10). Grant believes that the eroded material from the marginal channel can, to the first approximation, be accounted for in the volume of recent material forming the offshore banks.

Flemish Cap – Grand Banks – Scotian Shelf

The area between Orphan Knoll and the coast of Labrador and between the Knoll and Flemish Cap is thought possibly to be a foundered block of continental crust that sank at some point and left Orphan Knoll standing high. Considerably more field work in deep

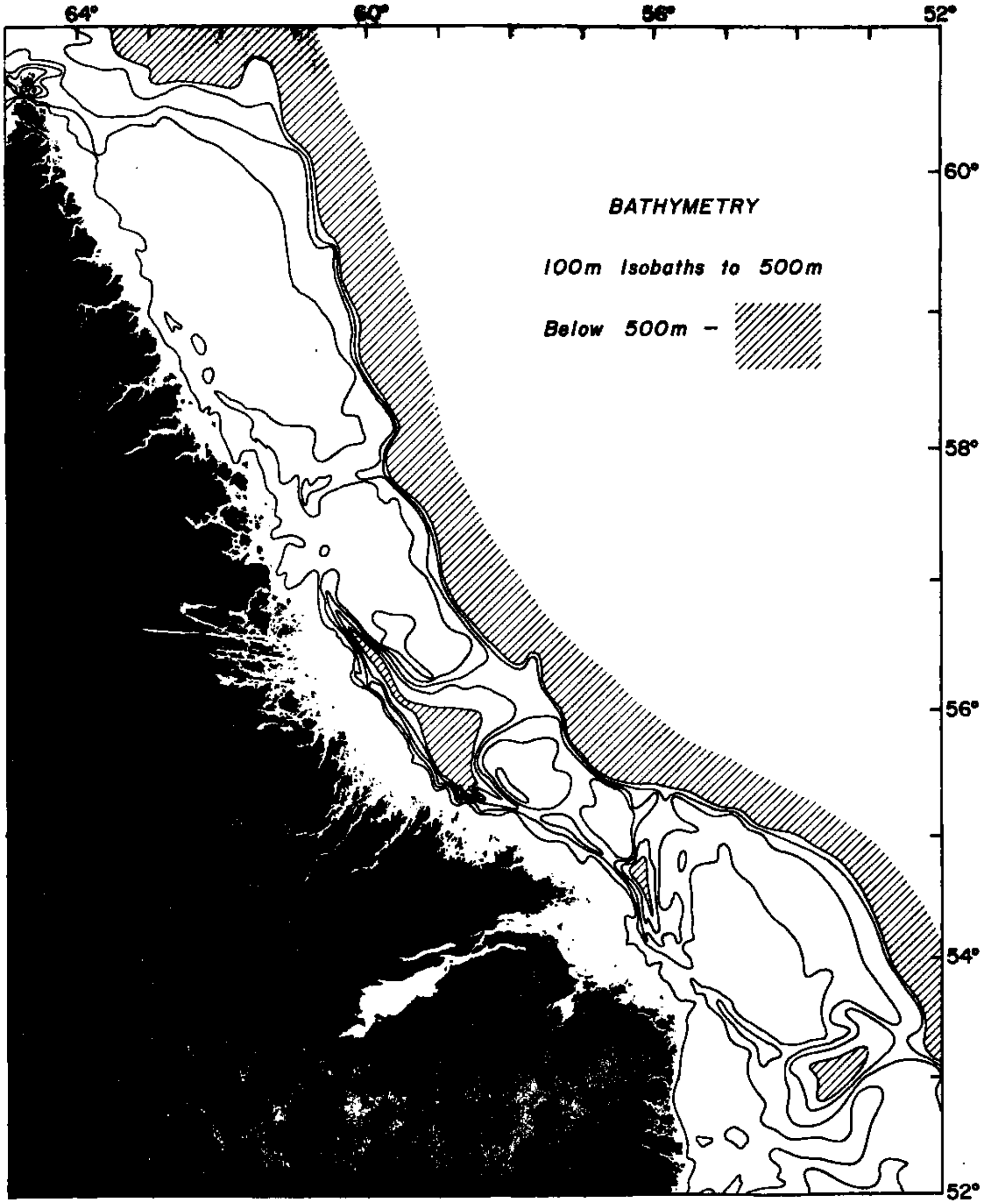

Fig. 9. Bathymetry off the east coast of Labrador. A very pronounced marginal channel with apparent overdeepening has been developed at 56°N. Ice-cut transverse troughs lead across the shelf to the continental margin. From Grant (1970).

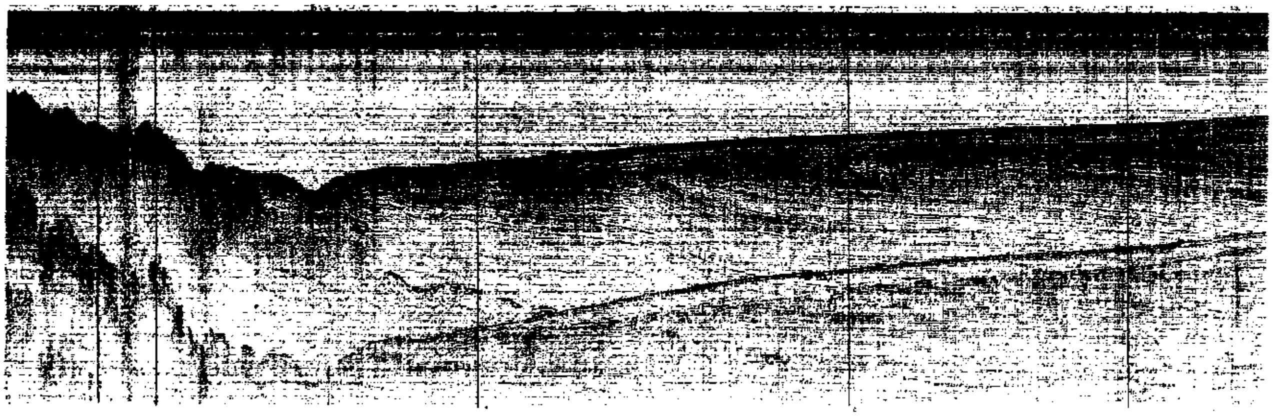

Fig. 10. A seismic profiler recording of a typical transect of the marginal channel off the Labrador coast going from west to east (left to right). Rugged crystaline Precambrian rocks on the left hand or shoreward end of the profile abut layered sedimentary rocks in almost the deepest part of the channel. The seaward dip of the sedimentary rocks has led Grant (1970) to suggest there has been recent tectonic activity along the axis of the marginal channel. From Grant (1970).

water is required before this can clearly be demonstrated.

Flemish Cap itself is smooth and almost devoid of soft sediments. Bedford Institute has drilled a 10-cm core of granite from the crest of the Cap. This granite was dated at 590 ± 20 m.y. (Pelletier, in press). The seismically hard central core of granite is surrounded by much younger sedimentary beds that dip outwards from the Cap (Grant, in press). In geological terms, Flemish Cap, even though it is separated from the Grand Banks by up to 1,000 m of water, is probably a continuation of the continental block of Newfoundland.

A number of methods are available for observing and sampling the bottom on the shelf. One can send down a grab or a dredge. This works but is very haphazard since in deep water the ship has no real control over where lowered equipment will land. Another approach is to use a still camera or a television camera to photograph the bottom features but this again is a somewhat random technique. Similarly, a drill lowered from the surface will land on unknown rock, mud or boulders unless it can be placed exactly where needed. The continuous seismic profiler offers an excellent opportunity to 'see' the sediment in three dimensions but it must be correlated closely with bottom and sub-bottom samples. Finally, we can use a submersible to directly view the bottom and to remotely sample material or to lock out a diver who can actually bring back the desired sample. Perhaps the most powerful technique is that used by King (1967) in mapping the Scotian Shelf.

King uses a combination of echograms and seismic profiles with routine grab or core samples and bottom photos. He is able to correlate the type of echogram trace obtained during standard hydrographic surveys with the type of surficial sediment on the bottom. Figure 11 illustrates five types of echograms obtained and the sediment types correlated with them. Figure 12 illustrates the range of bottom types mapped on the Scotian Shelf, from very smooth bottom (covered with sand dollars in this case) to a rough bottom covered with rounded glacially derived boulders. Using these mapping methods King (1970) has published a coloured chart of the surficial geology of the Halifax – Sable Island area and an adjoining sheet is in press by G. Drapeau of the Bedford Institute.

The Canadian Hydrographic Service (1969, 1970) have published comprehensive, coloured bathymetric charts of the Scotian Shelf and Grand Banks. These two charts, 801 and 802, are at a scale of 1:1,000,000 and cover the area from Georges Bank to Orphan Knoll and west to the eastern end of Anticosti Island in the Gulf of St. Lawrence. Canada has an ongoing program to map the whole of the Scotian, Newfoundland and Labrador Shelves and to issue Natural Resource charts at a scale of 1:250,000 on a transverse mercator projection. Areas covered by these charts are shown in the index map (Fig. 13) and for each sheet a contoured bathymetric, a total magnetic field, and a free air gravity chart is issued along with a plotting base at the same scale. At present the Gulf of St. Lawrence sheets are complete, as are parts of the Nova Scotian and Newfoundland Shelves. In the summer of 1971 CSS *Baffin* is working on the Northern part of Flemish Cap and Grand Banks. By the summer's

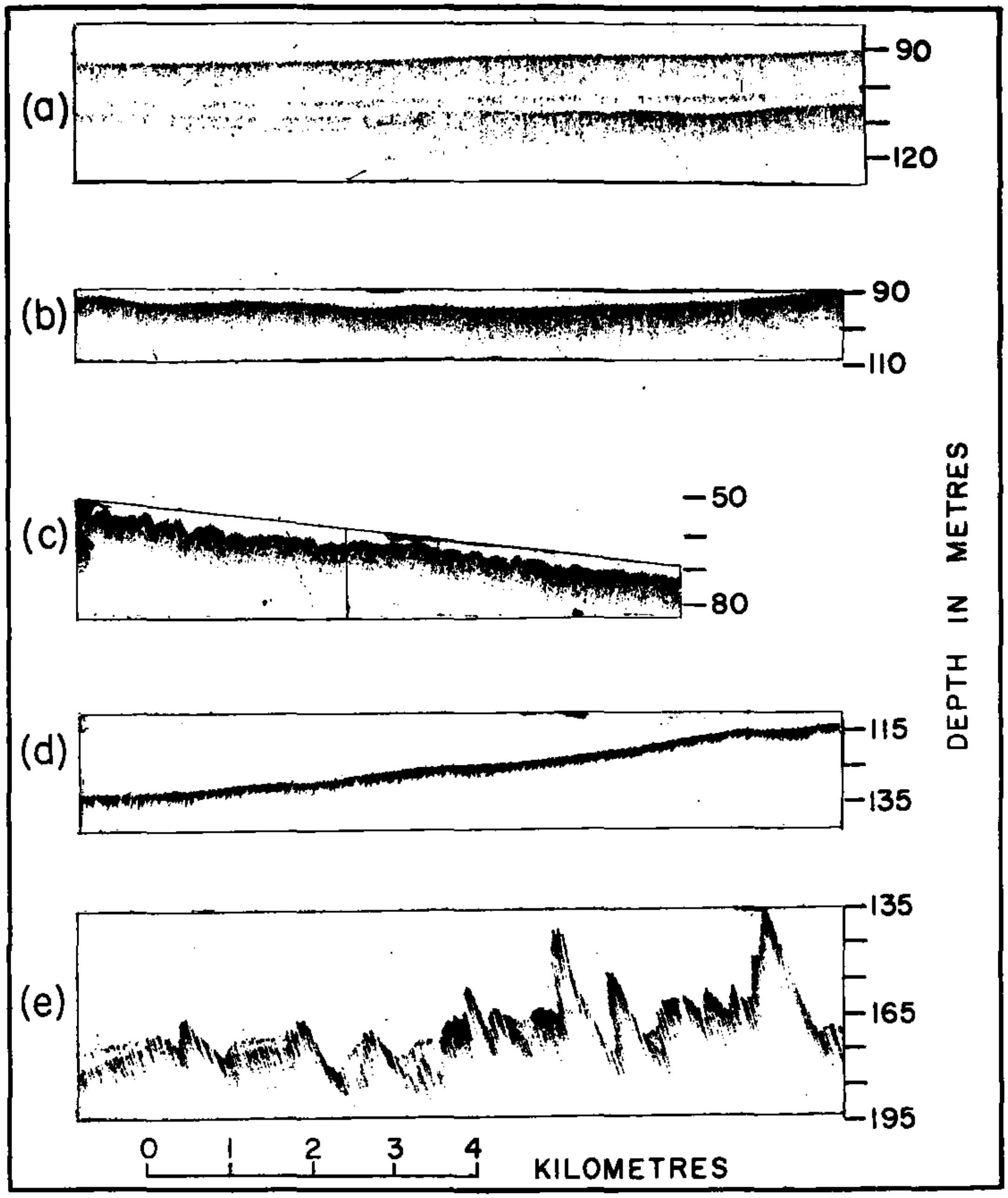

Fig. 11. Five types of echograms calibrated by King (1967) using bottom photos and sampling techniques.

Type (a)	soft uncompacted bottom smooth surface,
Type (b)	semicompacted bottom, smooth surface,
Type (c)	Semicompacted with an undulating surface,
Type (d)	hard compacted bottom,
Type (e)	bottom where sediment types are varying too rapidly to be mapped.

Photo from King (1967)

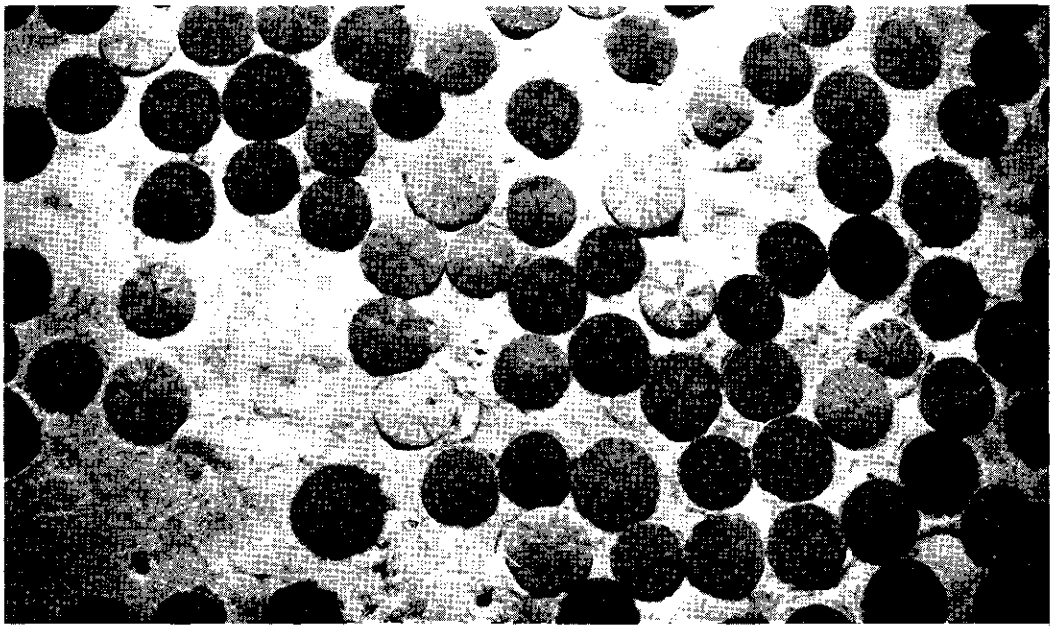

Fig. 12. Top: Smooth bottom of Scotian Shelf covered by sand dollars.

Bottom: Rough hard bottom covered by thick bed of glacially derived boulders. These boulders may have been derived from a beach that became drowned by an encroaching sea. Both photos courtesy of Dr L. H. King, Bedford Institute.

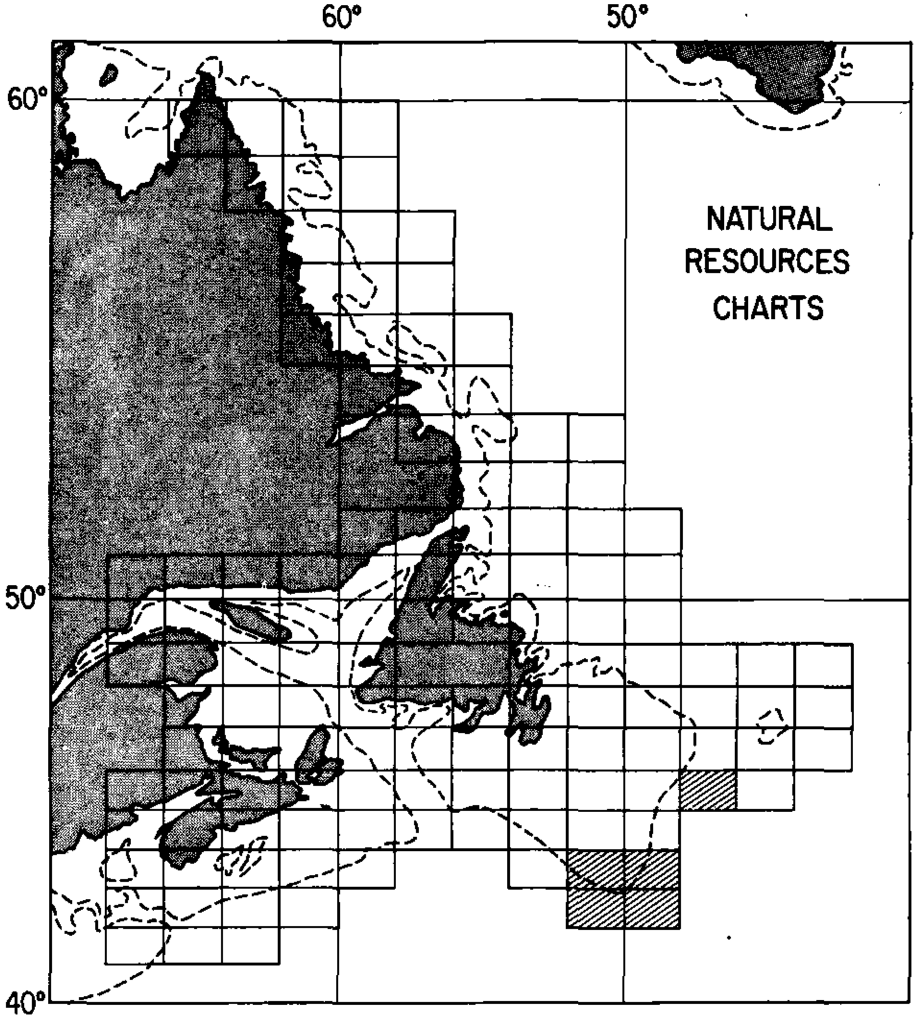

Fig. 13. Index map of Natural Resource Charts. The hachured sheets have been published. Diagram courtesy R. F. Macnab, Bedford Institute.

end the field work on the Grand Banks may be complete.

Nonrenewable Resources and Pollution

Much of the effort expended towards obtaining a comprehensive accumulation of a large body of knowledge on the Scotian and Newfoundland Shelves is intended to facilitate the hunt for nonrenewable natural resources – mainly oil and gas. The search for oil has progressed on the Nova Scotian Shelf to the stage where today it represents an exploration activity of about $100 million per year. There will be five oil rigs drilling off the Canadian coast in the summer of 1971. Exploratory permits have been issued by the Canadian government over almost all the area covered in the index map (Fig. 11) as well as over parts of the east and west coasts of Baffin Island, over central Hudson Bay and over the Arctic island channels.

The Geneva Convention on the Continental Shelf of 1958 set the limits of national jurisdiction only as "adjacent to the coast but outside the area of the territorial sea to a depth of two hundred metres". However, the 1958 Convention also allowed the limits of national jurisdiction to extend to the 'technologically elastic point', "where the depth of the superjacent waters admits of the exploitation of the natural

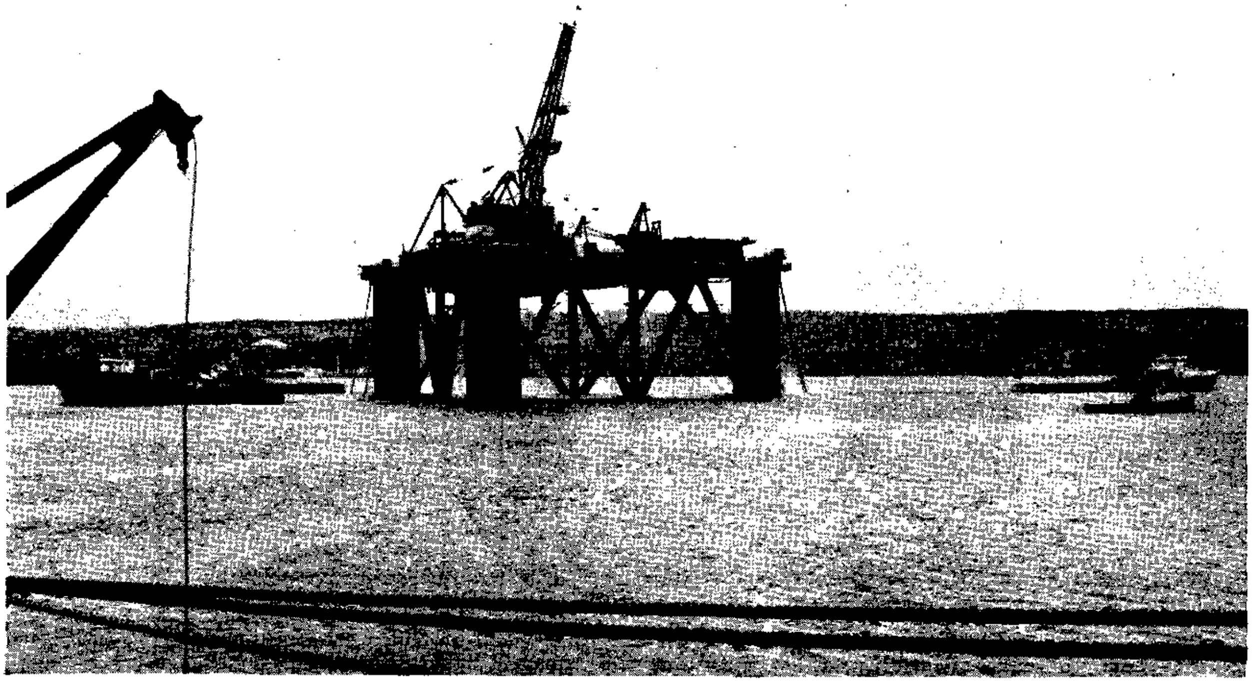

Fig. 14. SEDCO I semisubmersible drilling rig built in Halifax Shipyards. The rig is in Bedford Basin, Nova Scotia, testing equipment. The collapsible derrick is just being raised to the vertical position. Various tugs and tenders are standing by. Photo courtesy of Roger Belanger, Bedford Institute.

resources of the said area . . ." (Hawkins, 1970). Unfortunately, this deeper limit has never been specified by the coastal nations of the world. Hence we have in Canada and throughout the world a gradual creeping of the practical limit of exploitability into deeper and deeper water as technology responds to the lure of possible profits in the deep ocean. In Canada the expansion has appeared through applications by oil companies for exploratory permits over deeper and deeper water down to depths of 3,700 m (Crosby, 1970).

The SEDCO I semisubmersible drill rig pictured in Fig. 14 is the second of three or possibly four drilling rigs to be built in Halifax Shipyards. SEDCO I, its predecessor SEDCO H and other rigs leased by the large oil consortiums are presently drilling on the Grand Banks and Scotian Shelf. The rigs are licensed as vessels and are truly monsters; they are as tall as a 23-storey building with a triangular base over 100 m to a side and carry a crew of up to 65 men. The men work a straight 2 weeks with 12 hr on and 12 hr off. The crew is then completely exchanged, and is flown by helicopter to the drilling companies' base of operations in Sydney, Nova Scotia.

Oil exploration on the Shelf is connected to an environmental symposium in a number of ways. First of all, the platforms represent a semipermanent platform (up to 3 months in one position) from which a number of scientific measurements could be made. They are used for weather and wave predicting at present. Also the platforms, when anchored, make useful reference points for fishing fleets. Similarly, we should remember that each rig has a tender in constant attendance and this ship might well serve in emergencies. Finally, a recent article by Treybig (1971) indicates that the presence of a large number of permanent production platforms in the Gulf of Mexico has led to an increase in the fish catch and an article in the same journal (Anonymous, 1971) suggests automated fish-harvesting platforms could be developed. We are a long way on the Canadian Shelf from such a happy situation and indeed if oil or gas is found in commercial quantities permanent production platforms may not be used because of the ice hazard.

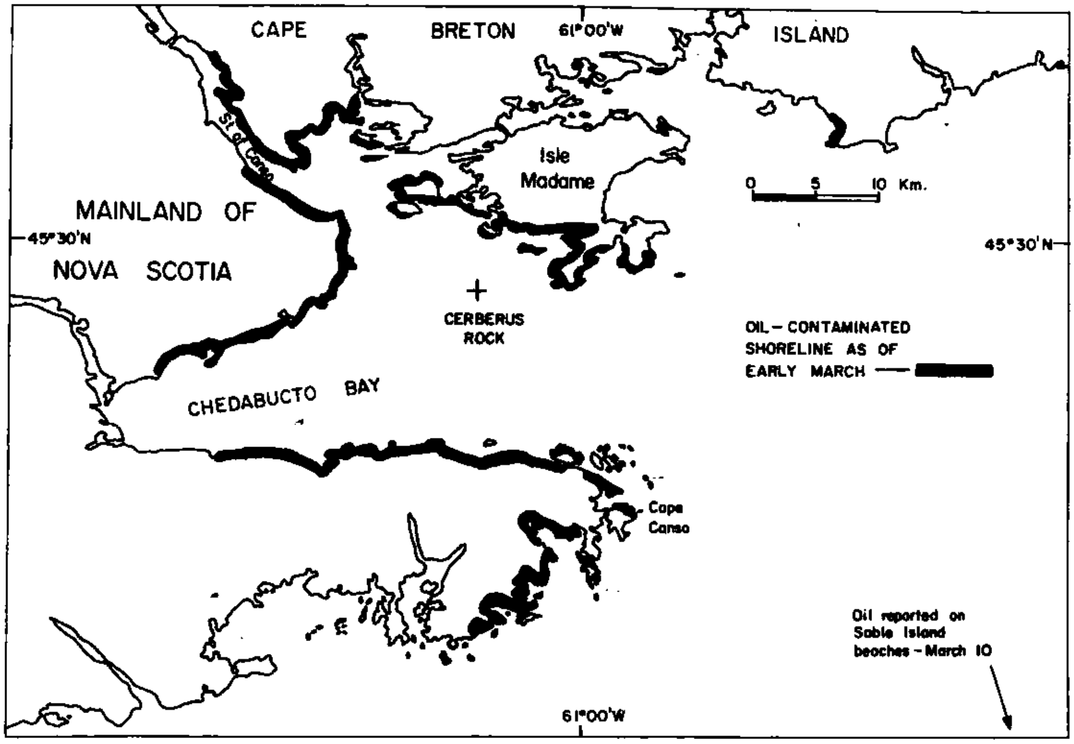

Fig. 15. Chedabucto Bay, Nova Scotia, showing the shoreline contaminated by Bunker C oil one month after the Arrow foundered on Cerberus Rock. Diagram from Forrester (1971).

However, perhaps of greater importance is the risk of environmental pollution as a result of foundering of oil tankers, or of oil rigs. On 4, February 1970, the tanker *Arrow* hit Cerberus Rock in Chedabucto Bay 200 km northeast of Halifax and spilled 2.5 million gallons of Bunker C oil into the Bay and polluted about 200 km of shoreline (Fig. 15). The main pollution threat was to holiday beaches and fishing gear. In addition to massive oil spills, we are becoming more and more aware that there may well be an even greater danger from oil disposed of in the open ocean by freighters pumping bilges or tankers pumping clean tanks during normal passage. The estimates of the quantity of ocean pollution from this source are not very precise and perhaps this should be an immediate task of researchers – to find better means of estimating open ocean pollution and its impact on our long-suffering and all-too-fragile environment.

Conclusion

In this paper we have ranged over a number of topics connected with ICNAF's 'fishbowl'. We did want to acquaint you with the most important and exciting advances made in earth sciences in the 60's. We would not say, however, that all the problems have been solved. The decade of the 1970's is the International Decade of Ocean Exploration and the activities associated with that international program are bound to produce more data and involve new people so we may expect an increase in the understanding of our marine environment.

The 1970's might also give us a very important advance in what might be called 'Legal Oceanography' or perhaps 'Political Oceanography' as it relates to, and potentially controls and protects the oceanic environment. We are all now aware that the largest part of the earth's real estate lies under water and it is presently a no man's land. We also realize that unless some workable international regime is agreed to, we will witness one of the greatest land grabs since Pope Alexander VI arbitrarily divided the western hemisphere in 1494.

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Discussion

Dr Campbell: *"What happened to Iceland?"*

Dr Loncarevic: *"Iceland is a geological province which has only emerged in the last 16 million years as a large volcanic outflowing. We do not understand the mechanism which would concentrate the volcanic activity in one such place for so long, but it is obvious that, on the Mid-Atlantic Ridge in that one place, volcanic activity has persisted over some 16 million years and has built up a volcanic pile which today is Iceland."*

Mr Lee: *"At the end of his talk, Dr Loncarevic indicated the extent to which exploratory permits had been issued on the Canadian side of the Labrador Sea. Is there a similar move to undertake prospecting work along the Greenland side of the Labrador Sea? That is my first question. The second one relates really to Dr Loncarevic's and Captain Dinsmore's talks. We all saw Captain Dinsmore's pictures of the sea-ice and many are no doubt wondering – How safe will these oil rigs be in respect to pollution and possible damage to fishing?"*

Dr Loncarevic: *"The West Greenland offshore area is being explored actively at the present time by the oil companies and by the Greenland administration and the Canadian government. We have information that this July two holes will be drilled near Nugssuaq. They will be land-based rigs but they will be close to the shore to give information on the general offshore geology. I don't know the precise extent of licensing along the Greenland coast but I do know that plans are well advanced.*

"In reference to the second question, there is a section of the Greenland coast north of Disko Island between Melville Bay and Disko Island that appears to have fewer icebergs than any area in the Labrador Sea and Baffin Bay and that area might in fact be one of the first to be actively exploited. As far as ice is concerned, this is of major concern to the offshore exploration companies. The drilling rigs are as good as one can design them on the basis of the known parameters. However, we already know that these parameters are perhaps not good enough. Waves of this magnitude were larger than had previously been assumed in this locality. However, the problem of waves is minor compared to what would occur if a million-ton iceberg collided with an offshore oil rig. The only thing that could be done in such a case is to devise operational procedures which would avoid collision. A great deal of research is being done at Memorial University of Newfoundland on very short-range prediction, spanning a few days or even hours, on iceberg movements and on the probability of an iceberg being located within a certain sector upwind and upstream from a drilling rig. The idea is to predict the danger of collision if an iceberg begins to approach the rig.

"The operational thinking by the drilling operators at the moment is that if the iceberg approaches the drilling rig within a certain distance, they are going to move. They are not going to stay and wait for it and hope it misses.

"A great deal of work is being done at the present time with actual diversion of icebergs. This summer (1971) a Memorial University tug is going to hook onto icebergs to see what sort of power is needed to deflect them. Of course, you don't have to deflect them very much if you are only trying to miss one point. You only have to deflect them a few hundred yards and you are safe."

Editorial Note: *(These experiments were reported in the press in late summer (1971) to have been quite successful.)*

Dr Campbell: *"I could add to Dr Loncarevic's remarks on the question of whether you pull the drill string and risers or not. An exploration company was drilling in Hudson Bay and they had consultants in meteorology and oceanography to give them advance warning of inclement weather and wave conditions. They found that 50% of their consultants' forecasts were inaccurate and after they had pulled the drill string five times, they decided it wasn't worth it. On the next two occasions they were hit with 38-ft waves in August and 40-ft waves in September. The risers broke the string and they lost all their anchors and decided it was time to go home."*

Contribution Number 7

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Some Aspects of Water Circulation in the Northwest Atlantic in 1960–69

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Abstract

Data gathered by PINRO and from relevant literature was analysed to show changes in the circulation of waters in the ICNAF area during the period 1960-69. The variability of main current flows through some sections and of geostrophic circulation in the northern Davis Strait are reported.

Part I. Subareas 1-3

During the last decade the intensity of the West Greenland Current in summer was on the average about the "norm". Fluctuations in the volume transport of the East Greenland component take place in counterphase relative to the fluctuations of the Irminger component.

Analysis of charts of dynamic topography of Davis Strait, obtained from data collected during five Soviet expeditions, the NORWESTLANT Surveys and other sources, make it possible to identify the following two types of water circulation in the Strait: (a) deformation field, when the flows of the West Greenland and Baffin Land Currents are divided by the hollow of the lower sea level; (b) the eddy field, when the eddy is on the Greenland-Canadian Ridge and it appears on the northern extremity of the cyclonic water cycle of the Labrador Sea.

During the last decade the, intensity of the Labrador Current in the Hamilton Inlet Bank area decreased. It is suggested that the fluctuations in the volume transport of the Labrador Current on the eastern and southern slopes of the Grand Bank differ from the fluctuations observed on Hamilton Inlet Bank.

No regularities in year-to-year changes of circulation intensity of the Northwest Atlantic waters were detected.

Part II. Subareas 4-5

Changes in circulation on the Scotian Shelf and Georges Bank could not be considered because of insufficient observations. Only charts of average circulation for winter and summer were compiled.

Average values of temperature and salinity at 1,153 hydrographic stations occupied by AtlantNIRO from 1961 to 1966 on the Scotian Shelf and Georges Bank were calculated. These calculations refer to winter (January, February, March) and summer (July, August, September) seasons by "squares", 20' in lat and 30' in long.

Calculations of geostrophic currents by the dynamic method were made, and charts of horizontal geostrophic circulation for both winter and summer and depths of 0, 50, and 100 m were plotted.

As a result of the analysis, a general scheme of water circulation in this area is described. Some eddies were observed in summer and winter and suggests the quasi-stability of these processes throughout the year.

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Introduction

Marine currents transport immense volumes of water, and consequently, make for the formation of temperature anomalies. The role of currents is especially great in the formation of temperature anomalies in cases where a current transports water which is quite different in its physical and chemical properties from the "local" waters. This phenomenon has been observed in the Northwest Atlantic (the area of ICNAF) (Hachey *et al.*, 1954; Lee, 1968).

The comparison of the charts of dynamic topography drawn by Lee (1968) based on data from the three consecutive NORWESTLANT Surveys from 31 March to 3 August 1963 shows that geostrophic circulation undergoes a certain transformation with time. This transformation shows itself mainly in the change of the kind of eddying of the current field and the velocity of some branches of currents. The main features of general water circulation in each case remain unchanged and correspond to those previously described.

However, the authors' task is to provide conclusions concerning changes in water circulation in the ICNAF area during the years 1960-69. The task is complicated and can not be completed in detail as the necessary observations are not available. Therefore, we shall deal only with the variability of the discharge of the main currents and the nature of the water circulation in some limited areas and over sections where observations were made most frequently during the years under consideration.

The material available from observations carried out over the Scotian Shelf and Georges Bank are even more limited. Therefore, changes in the circulation could not be followed there. However, it seems possible to give the mean picture of horizontal circulation during winter and summer in this region.

Part I of the present paper is written by A. P. Alekseev and B. P. Kudlo (PINRO); Part II, by A. F. Fedoseyev, A. A. Barinov, and V. N. Yakovlev (AtlantNIRO)

Part I. Subareas 1-3

The West Greenland Current

Around Cape Farewell, the East Greenland Current forms the beginning of the West Greenland Current which washes the west coast of Greenland from Cape Farewell to Disko Island in Baffin Bay. Year-to-year fluctuations in the flow of the West Greenland Current, and of its warm Irminger and cold East Greenland components, can be followed on the South Wolf Island, Labrador-Cape Farewell, Greenland section (section 8-A). Figure 1 shows the values for summer discharge of the West Greenland Current and its components calculated from data obtained by Dinsmore and Moynihan (1969). Discharges for 1965 and 1966 are calculated by us from data collected by Soviet vessels. It should be noted that the discharge of the current in July is characteristic of the intensity of the flow during the whole year because there are seasonal fluctuations in the current strength (Fig. 2). In the calculations the 1500-m level was taken as the reference level for 1965, and that of 800 m for 1966.

According to calculations by Dinsmore and Moynihan, the average discharge of the West Greenland Current for 1948-68 is $6.30 \times 10^6 m^3/sec$ and for the previous 10 years (those between 1928 and 1941), it was $5.54 \times 10^6 m^3/sec$. The intensity of the West Greenland Current in summer for the last 10 years can be regarded, on the average, as about the norm.

Figure 1 shows that, on the whole, there are considerable year-to-year changes in both the current and its components. Dinsmore and Moynihan call these accidental fluctuations which appear as the result of the masked long-period fluctuations of a cyclic character having different periods. The nature of these fluctuations is not yet clear. The values of some components do not change proportionally from year to year. Thus, from 1963 to 1964 the total discharge increased by nearly a factor of three. This took place due to an increase in the inflow from the Irminger component, the discharge of the East Greenland component of the current being unchanged. One can conclude that fluctuations in the discharge of the East Greenland component take place in counter-phase relative to the fluctuations in discharge of the Irminger component of the West Greenland Current at Cape Farewell. At least this phenomenon was observed from 1953 to 1962. In other years during the period under consideration (1949-68), this regularity was not distinctly marked.

The average discharge of the East Greenland component (1965 and 1966 are not taken into consideration) amounts to $2.95 \times 10^6 m^3/sec$, and that

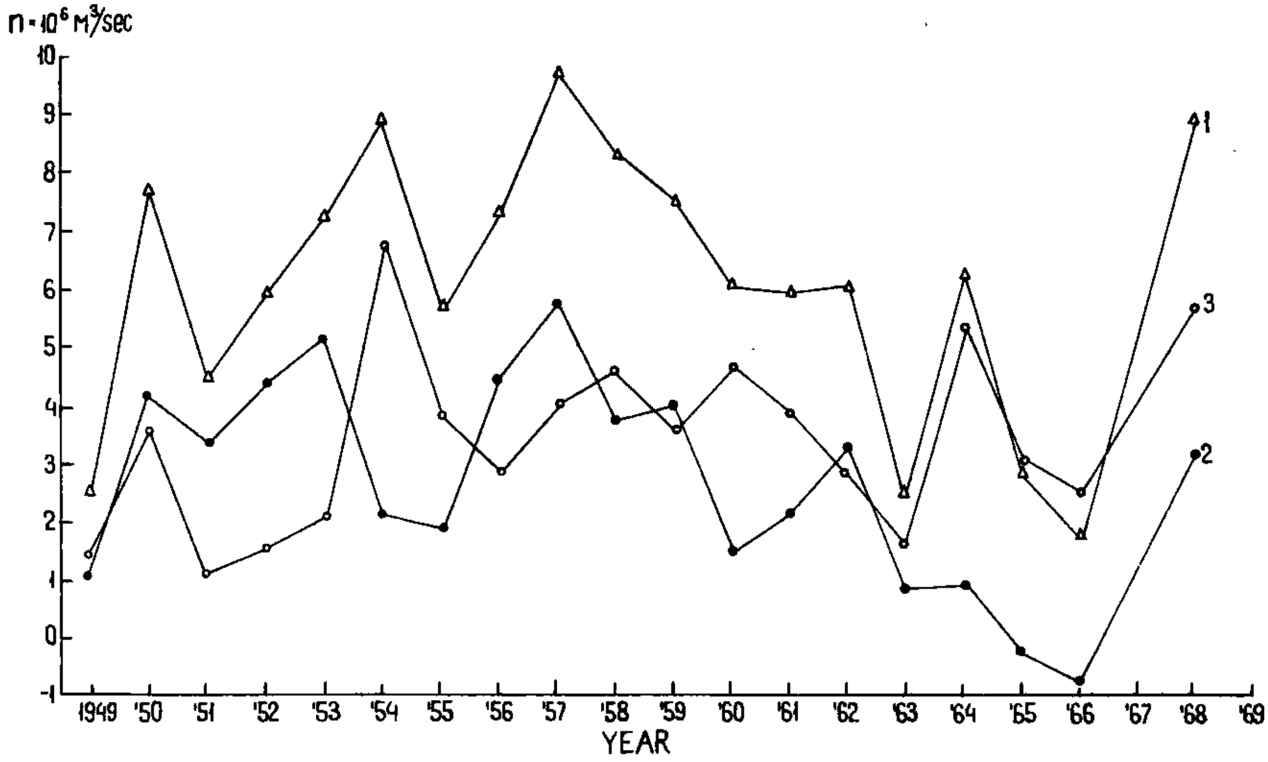

Fig. 1. Fluctuations in discharge of the West Greenland Current (1) and its East Greenland (2) and Irminger (3) components on the Labrador-Cape Farewell section, 1949-68.

of the Irminger component, 3.72 X $10^6 m^3/sec$, i.e., relatively warm waters reach West Greenland in greater volumes than the waters of Arctic origin.

Circulation in Davis Strait

Davis Strait connects Baffin Bay with the Labrador Sea. It plays an important part in the formation of the water circulation system in the extreme northwestern areas of the Atlantic Ocean, since waters of Arctic origin penetrate southward through it and relatively warm Atlantic waters reach Baffin Bay.

Davis Strait has a sill like most of the straits of the northern latitudes of the world. The sill is located at 66°N between Baffin Island and Greenland. The Greenland-Canadian sill has a maximum depth of 732m and divides the deep waters of the adjacent seas (depths more than 2,000-3,000m).

In addition to the narrowest part of the Strait, which is located in the area of the sill between Cumberland Peninsula and Greenland, the whole area between Baffin Island and Greenland between 67° and 60°N is usually considered to be the area of Davis Strait. Redistribution of the West Greenland Current into its western component and a Baffin Bay component takes place within the boundaries given, as does the formation of the Labrador Current.

Investigations by Canadian, American, and Danish scientists have revealed the main features of the water circulation in this part of the Atlantic. However, the seasonal and year-to-year fluctuations of the Davis Strait currents have not been investigated. Therefore, analyzing the temperature data and the conditions of the fishery, one has to use indirect indices of intensity (strength) of separate streams of the currents and of the characteristic features of the field of water circulation, i.e., the distribution of water temperature and the values of temperature anomalies. Such an approach is not always reliable, as fluctuations in water temperature are not determined only by the changes in intensity of currents or intra-water advection of heat. Hence, further detailed investigation of the currents is evidently necessary.

Over the last 10 years five Soviet expeditions carried out more or less detailed hydrographic investigations in Davis Strait (Table 1). Data obtained during

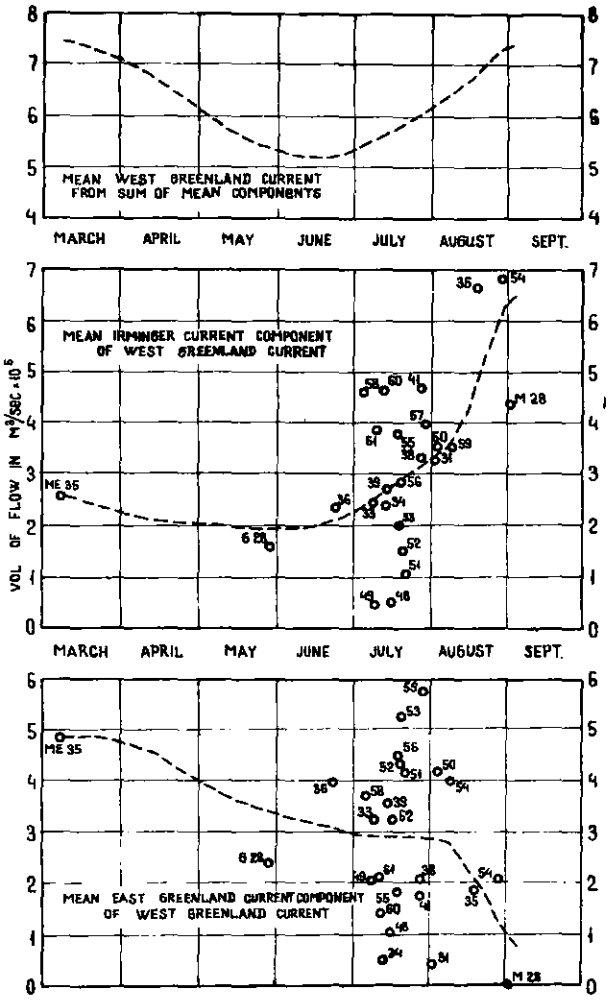

Fig. 2. Experimental average seasonal curves of discharges of the West Greenland Current at Cape Farewell and its Irminger and East Greenland components (according to Soule *et al.*, 1963).

TABLE 1. Soviet hydrographic expeditions providing data used in this paper.

Year	Date of survey	Vessel and cruise	Number of stations
1962	16 June – 2 July	*Topseda*, cruise 37	103
1962	19 Aug – 15 Sept	*Topseda*, cruise 38	141
1967	30 Aug – 2 Oct	*Novorossiisk*, cruise 22	136
1969	16 Aug – 13 Sept	*Perseus III*, cruise 2	76
1970	4 Aug – 18 Aug	*Perseus III*, cruise 5	71

these expeditions and also the results of the international NORWESTLANT expedition along with some other material made it possible to bring out some features of fluctuation of water circulation.

The first two surveys make it possible to draw current schemes for midsummer and early autumn and to compare them. Four surveys were carried out about the same time in August-September, near the beginning, near the middle, and at the end of the 10-year period (1962, 1967, 1969, and 1970).

Observations on water temperature and salinity were made during all the expeditions at standard depths using the normal methods. The bathometer depth was adjusted (a) by means of "slackening the wire" the amount of slackening was determined by the angle between the wire and the vertical line (R/V *Topseda*); (b) by keeping the vessel at the hydrographic station so that deviation of the wire from the vertical line was minimal (FRV *Perseus III*, cruise 5); (c) by means of unprotected thermometer (R/V *Novorossiisk*, FRV *Perseus III*, cruise 2). On the continental shelf the observations were made from surface to bottom and in the deeps of the Strait to a depth of 1,000 m.

Currents were identified by the dynamic method. Dynamic heights were calculated using the computer "Mir" in PINRO. Dynamic heights at shallow water stations were adjusted to the common reference surface using the method suggested by Somov (Zubov and Mamayev, 1956).

The choice of the reference level was made taking into consideration the following facts: the shallow depths on the West Greenland Shelf (50-100-200 m) and in the area of the Greenland-Canadian sill (the maximum depth being 732 m); and the maximum depth (1,000 m) at which the observations were conducted. The area where depths are 1,000 m and more is relatively small and lies in the southern part of Davis Strait. Therefore, this depth could not be taken as the reference level. In this case, at most stations, the dynamic heights would have been adjusted to the 1,000-m level, with the water layer in which the density is determined, being smaller than the water layer with the hypothetical density of water. Therefore, 500 m was accepted as the main reference level. Dynamic topography charts of 0-200 and 0-1,000 db were also drawn as controls.

The dynamic topography charts of Davis Strait for summer and early autumn of 1962 which show seasonal changes of water circulation are given in Figs. 3 and 4. It should be noted that small eddies of the West Greenland Current are not stationary. Eddying and even the formation of countercurrents and vortices increase with the increased general velocity of the flow. This very phenomenon took place in 1962 when from summer

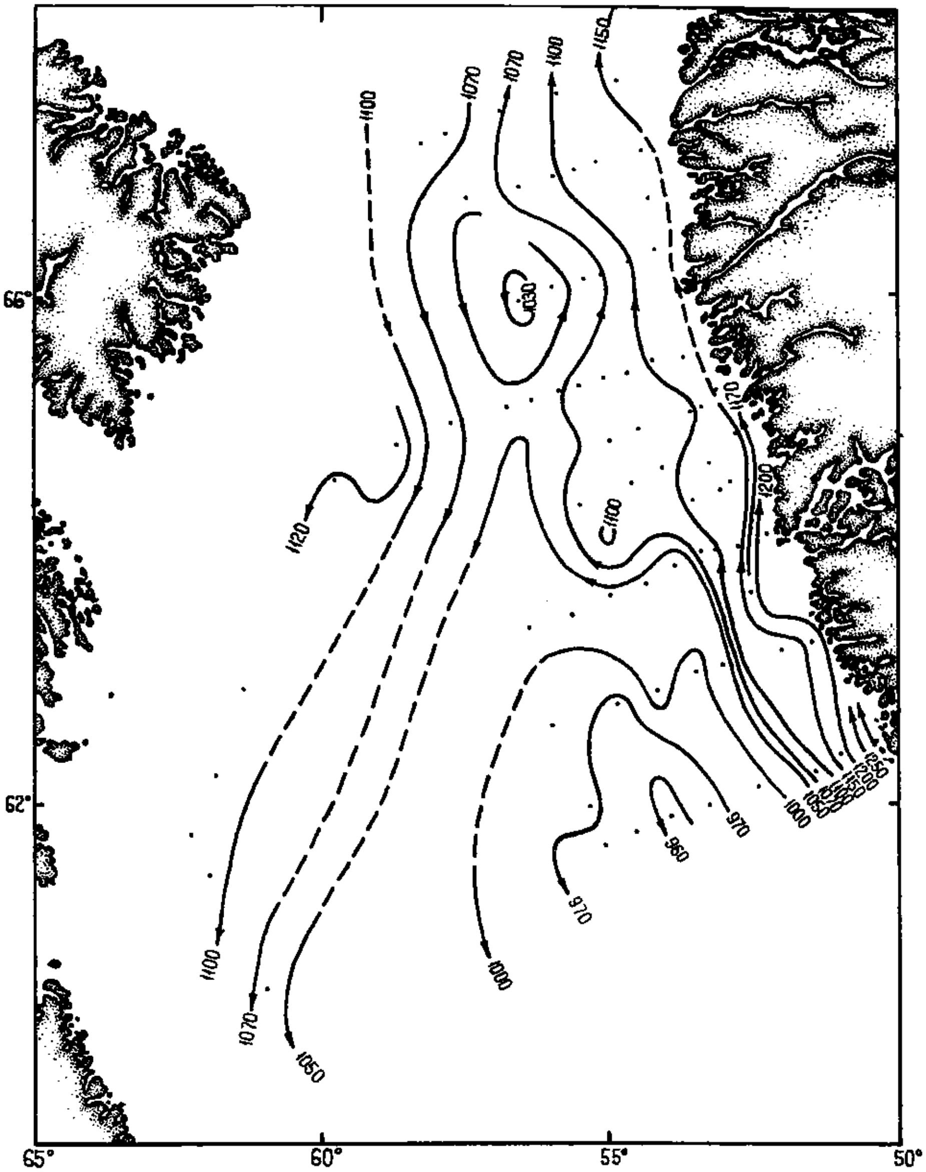

Fig. 3. The 0-500 db dynamic chart of Davis Strait drawn from data obtained by the R/V *Topseda*, cruise 37, 16 June-2 July 1962.

(Fig. 3) to early autumn (Fig. 4), the velocity of the West Greenland Current together with eddying increased considerably. This, in our opinion, is the result of the flow transformation caused by its interaction with the complex bottom relief (Rvachyov, 1963).

The change in circulation features on the sill is the second peculiarity of the seasonal reconstruction of circulation in 1962. In June an eddy with its centre at 66°N, 56°30′W (Fig. 3) was located on the sill. On the dynamic chart prepared by Soule *et al.* (1963) from two sections occupied by the International Ice Patrol (July 1962), this eddy can be followed between parallels 64° and 65° and a little more westwards. By the beginning of September (Fig. 4) the eddy had disappeared from the southern slope of the Greenland-Canadian sill. The intensity of the West Greenland Current and the Baffin Land Current increased and a pronounced deformation field formed on the sill. In this case, we encounter the phenomenon when the offshore portion of the Baffin Land and West Greenland Currents make sharp turns to the left over the sill, and return into Baffin Bay and the

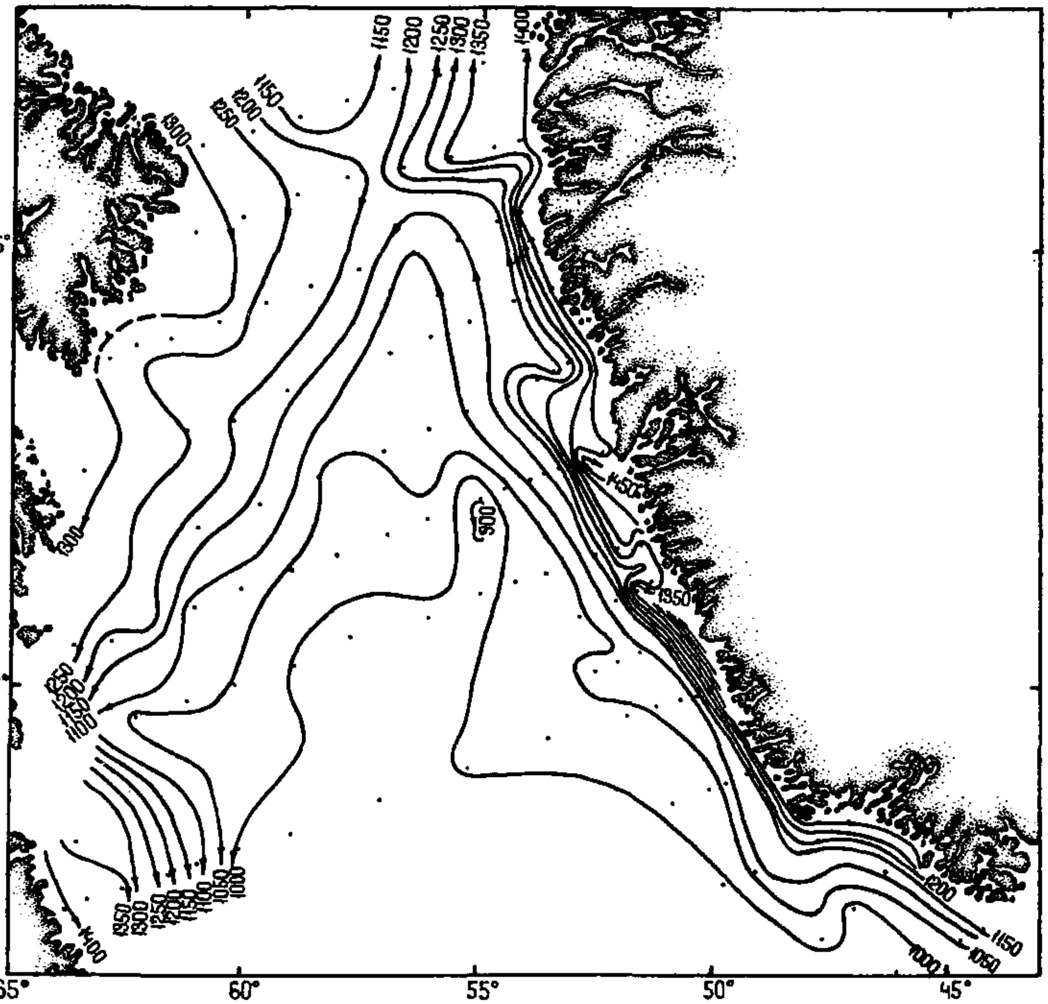

Fig. 4. The 0-500 db dynamic chart of Davis Strait drawn from data obtained by R/V *Topseda*, cruise 38, 19 August-15 September 1962.

Labrador Sea, respectively.

On the 1965 dynamic chart by Kollmeyer (1967) and on our chart (Fig. 4), as well as on current schemes drawn earlier, one can easily follow westward changes in the direction of the Baffin Land Current towards Hudson Bay. Changes in direction of currents take place not only near shore, but also considerably far from it, depending on the bottom relief.

Comparison of 0-200, 0-500, and 0-1,000 db dynamic charts shows that major details and peculiarities of water circulation in Davis Strait as revealed on the 0-500 db charts, can also be seen on the 0-200 and 0-1,000 db charts (Figs. 4 and 5). The same is true if we compare corresponding charts, drawn in other expeditions. It would seem that the type of horizontal circulation in Davis Strait changes insignificantly with depth.

Lee (1968) gave a rather detailed description of water circulation in Davis Strait in 1963 based on material from the three NORWESTLANT Surveys. It was interesting to compare the dynamic chart for June 1962 (R/V *Topseda*, cruise 37) and the chart drawn by Lee from NORWESTLANT 2 data for the period 1 May-18 June 1963. In both these years the circulation in June was the same type and can be characterized by an eddy on the Greenland-Canadian sill and an intensive water transport to the west by the Irminger component (across Davis Strait over a large area of the Strait which lies to the north of 61°N). A similar situation probably occurs in the northern part of the area when the centre of cyclonic circulation of the Labrador Sea is displaced to the southeast.

In autumn 1967, 1969, and 1970, observations were carried out only in the narrowest northern part of Davis Strait in the region of the Greenland-Canadian sill.

In September 1967 water circulation in Davis Strait was somewhat weakened (Fig. 6). Some poorly defined vortices with low velocities formed on the sill. Extensive eddying was recorded over the West Greenland shelf from Danas Bank to Tovqussâg Bank. Baffin Land Current was not broad and ran close to the shore.

The characteristic feature of the water circulation at the end of August and the beginning of September 1969 was the development of the Baffin Land Current. The waters of this Current reached 64°20′N in the central part of Davis Strait, and were then drawn into the vortex on the sill on their way back to Baffin Bay (Fig. 7). The West Greenland Current lost a considerable part of its waters reaching the latitude of Fyllas Bank (64°N); they flowed westward to the other coast of the Strait.

Features of the water circulation in Davis Strait in August 1970 were as follows: the northern extremity of the cyclonic vortex of the Labrador Sea was displaced to the west (Fig. 8). The area of the lowered sea level, i.e., the secondary cyclonic vortex with low current velocities was located in the central part of the Strait from 62°30′ to 65°30′N. Maximum velocities of the West Greenland Current were recorded on the western slope of Fyllas Bank; having passed it by, the broad fan-shaped

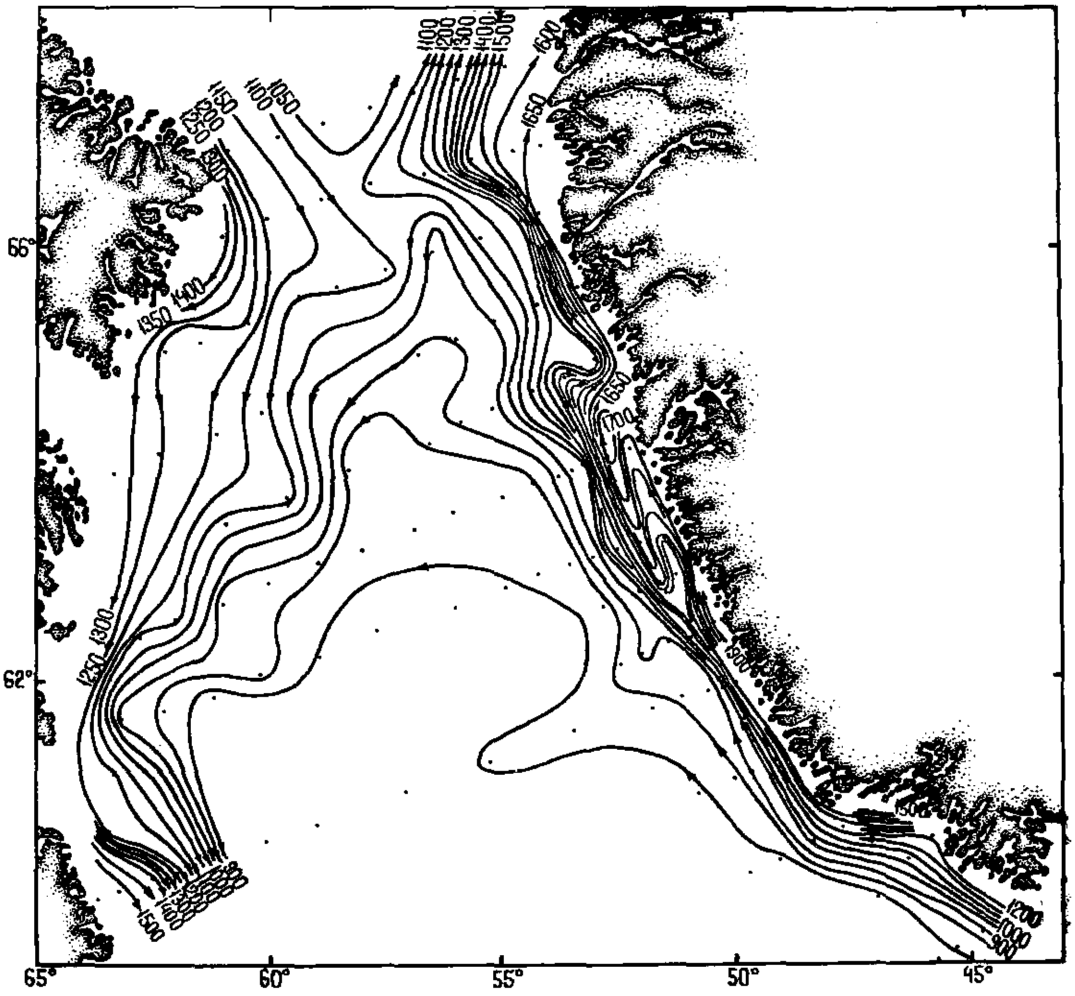

Fig. 5. The 0-1,000 db dynamic chart of Davis Strait drawn from data obtained by R/V *Topseda*, cruise 38, 19 August-15 September 1962.

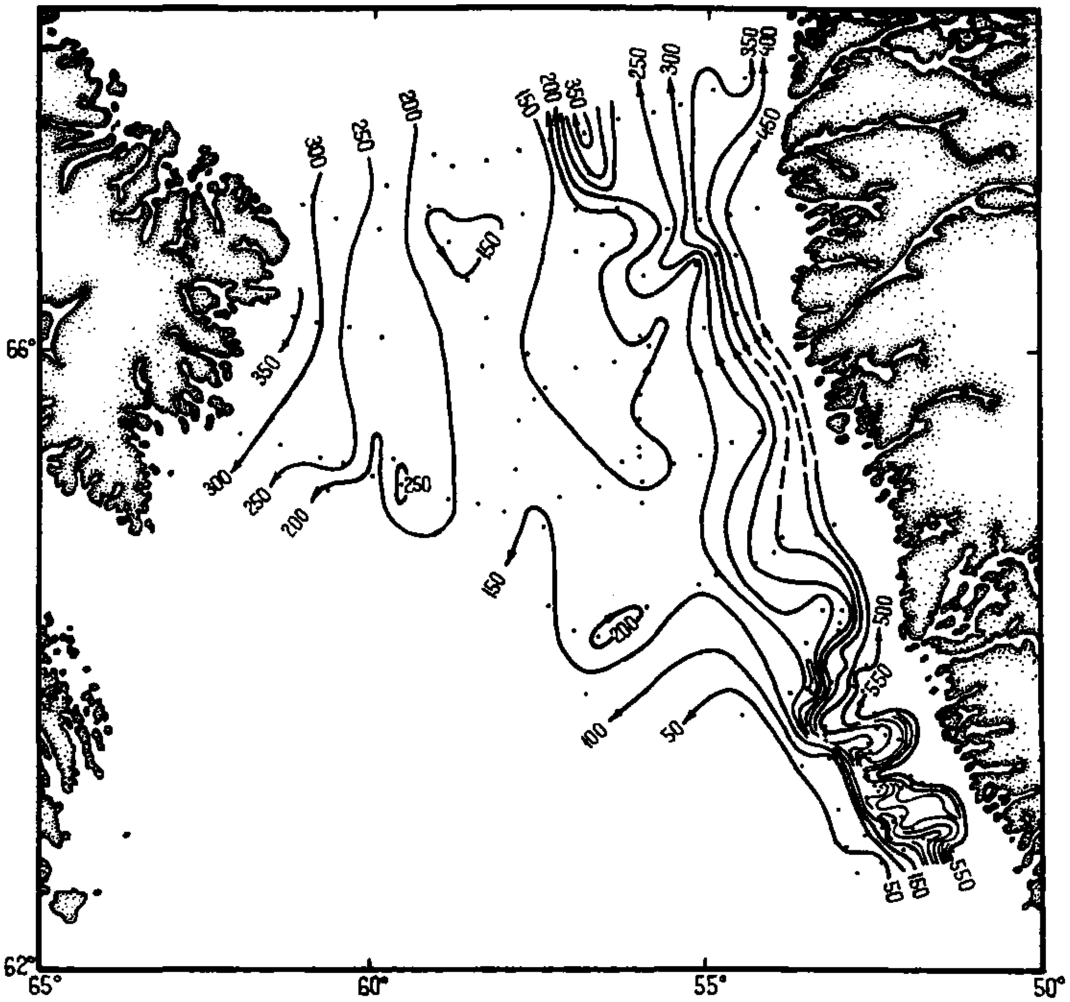

Fig. 6. The 0-500 db dynamic chart of Davis Strait drawn from data obtained by R/V *Novorossiisk*, cruise 22, 30 August-2 October 1967.

stream flows around Tovqussâg Bank from the West, and returns to the shelf only in the region of the Sukkertopen Bank.

An increase in velocity of the Baffin Land Current is registered in the area of the 500-m isobath from the latitude of Lady Franklin Island to the latitude of Loks Land Island.

Based on an analysis of the seven circulation fields of Davis Strait for the different years from 1962 to 1970, one can draw the following conclusions:

1. The general features of the scheme of the currents in the Strait being retained, the field of circulation undergoes year-to-year and evidently seasonal changes, which are revealed in the transformation of the scheme of circulation, in the formation of or disappearance of eddies and meanderings, in change of velocities and discharges of flows.

2. In Davis Strait two types of circulation are registered:

a) the deformation field type, when the West Greenland and Baffin Land Current flows are divided by the trench of the lowered sea level. This was observed in autumn 1962, in June 1963 (NORWESTLANT 3), and in autumn 1969 (Figs. 4, 7);

b) the cyclonic eddy type on the Greenland-Canadian sill, developing on the northern

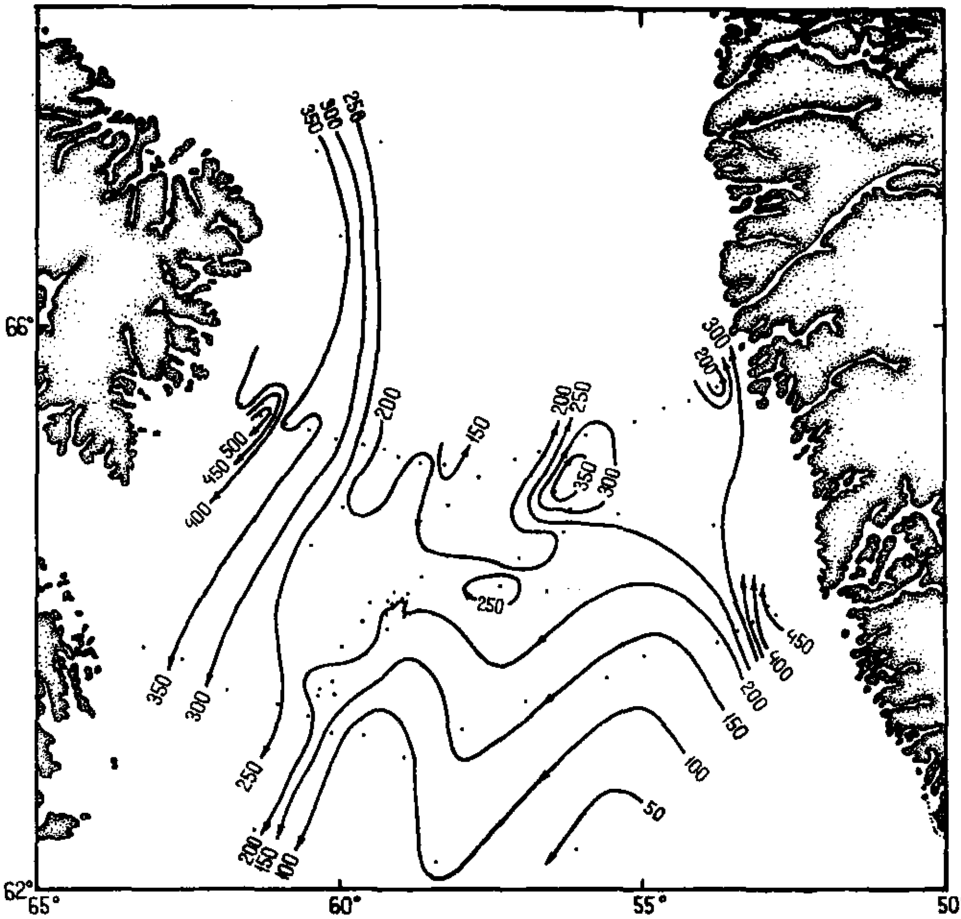

Fig. 7. The 0-500 db dynamic chart of Davis Strait drawn from data obtained by FRV *Perseus III*, cruise 2, 16 August-13 September 1969.

periphery of the cyclonic vortex of the Labrador Sea. The circulation of this kind was observed in summer 1962, May-June 1963 (NORWESTLANT 2), in autumn 1967, and 1970 (Figs. 3, 6, 8).

3. As a rule, the velocity of the West Greenland Current decreases after it has passed by Fyllas Bank, to the north of 64°N.

4. Great meanderings of the West Greenland Current in the area of the Tovqussâg-Sukkertopen Banks and the Store Hellefiske Bank are quasi-stationary.

The Labrador Current

Unfortunately, there was no data available with which to characterize the variability of the Labrador Current in its outflow at the northern extremity of the Labrador Peninsula.

Lines of flow of the Labrador Current are about parallel to the Labrador Peninsula coast (Smith and Soule, 1937). In other words, the Labrador Current is more "calm" and has not so many eddies, as compared with the West Greenland Current. The circulation field becomes more complicated in accordance with the bottom relief only when the current approaches the northern slope of the Hamilton Inlet Bank. The hydrographic conditions on the Newfoundland Banks and along the coasts of Nova Scotia and New England depend greatly on the Labrador Current.

Let us analyze the fluctuations of the Labrador Current over Hamilton Inlet Bank before it encroaches the Newfoundland Banks on section 8-A. The discharges

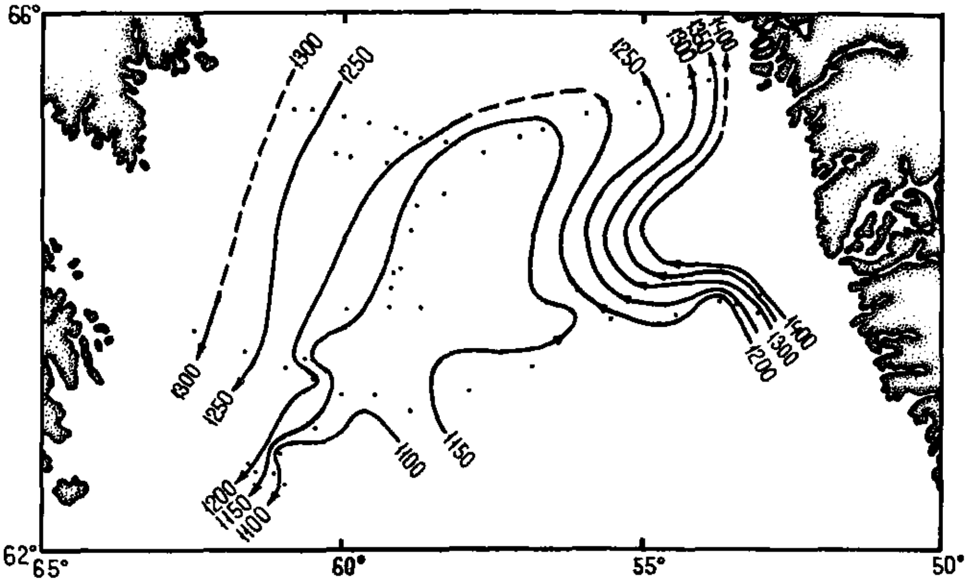

Fig. 8. The 0-500 db dynamic chart of Davis Strait drawn from data obtained by FRV *Perseus III*, cruise 5, 4-18 August 1970.

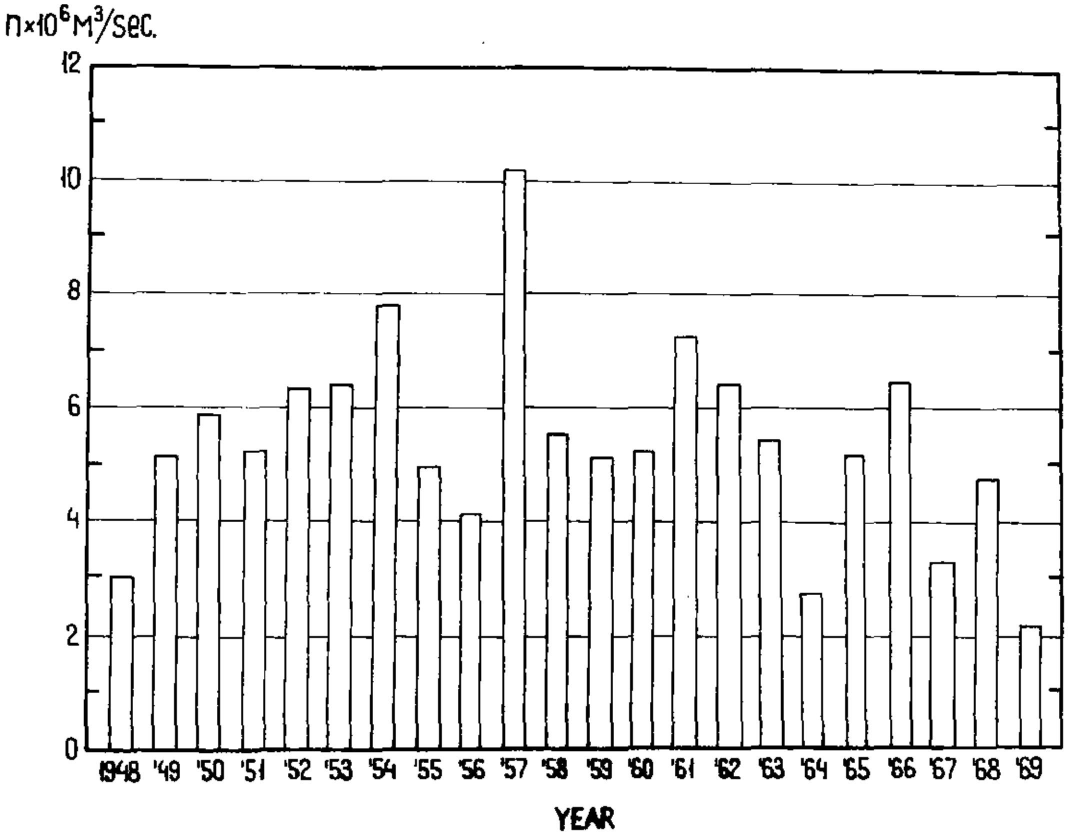

Fig. 9. Fluctuation in discharge of the Labrador Current through the Labrador-Cape Farewell section for the period 1949-69 (according to Dinsmore and Moynihan, 1969). The discharges for 1965 and 1969 are given in accordance with our calculations.

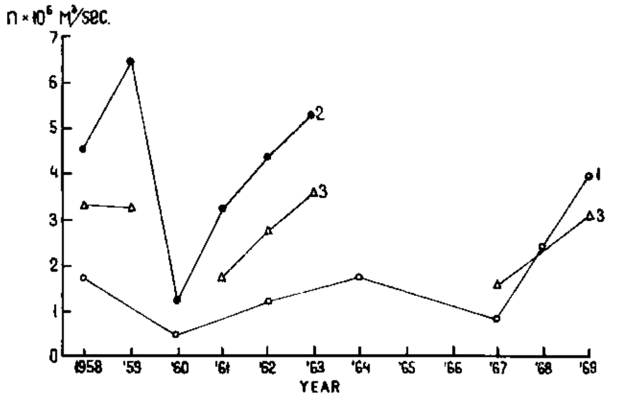

Fig. 10. Fluctuations in discharge of the main jet of the Labrador Current on section 6-A made along 47°N between the Grand Bank of Newfoundland and Flemish Cap Bank in March-April (1), on the section made along the meridian 50°15′W on the southern slope of the Grand Bank at the beginning of April (2) and at the end of May (3).

of the Labrador Current and the West Greenland Current on section 8-A from summer observations (July-August) of the International Ice Patrol are calculated by the dynamic method (Dinsmore and Moynihan, 1969). Discharge for 1965 was calculated by us from data of the International Ice Patrol; and for 1969 from observations of FRV *Perseus III*, cruise 2 (Fig. 9).

Mean discharge of the Labrador Current for 10 years during the period 1928-41 amounts to 4.04 × $10^6 m^3$/sec, and for the period 1948-68 (20 years) 5.51 × $10^6 m^3$/sec, i.e., it was greater 1.47 × $10^6 m^3$/sec.

Thus, during the post-war years, the intensity of water transport both by the West Greenland and Labrador Currents increased. Over the last 10 years, the intensity of water transport decreased. Only in 1961, 1962, and 1966 was summer discharge above the norm for the period 1948-68, while in the remaining years it was about the norm or considerably lower. No definite regularity in year to year fluctuations of the discharge of the Labrador Current has yet been revealed.

There is no doubt, however, that any increase of discharge of the Labrador Current leads to a fall in water temperature along its flow. It is especially evident from the case which took place in 1957, when the discharge exceeded the "norm" by nearly a factor of two. That year the minimum temperature was recorded in the Labrador Current (-1.70°C) and in the centre of the Labrador Sea, and the maximum quantity of icebergs (931) were observed on Grand Bank (Dinsmore and Moynihan, 1969) south of 48°N.

The data given in Fig. 10 are characteristic of the regime of the Labrador Current in the area of the Grand Bank of Newfoundland. The values for discharges on section 2-A along the meridian 50°15′W for 1958-63 are from the Bulletin of the International Ice Patrol, and for 1967 and 1969 the data are calculated from material obtained by Soviet vessels. Discharges on section 6-A along 47°N for March (and for 1968 and 1969 – in April) were also calculated by means of the dynamic method from data obtained by Soviet vessels. Unfortunately, the data were not collected systematically. There is an impression that fluctuations of discharges of the Labrador Current on the eastern and southern slope of the Grand Bank are different from those on the Hamilton Inlet Bank.

Part II. Subareas 4-5

Geostrophic Circulation Over the Scotian Shelf and Georges Bank in Winter and Summer Calculated from Many Years' Data

Let us consider the chart of horizontal geostrophic water circulation in this area for the winter and summer seasons as prepared from mean data over many years.

Averaging hydrographic characteristics over time scales makes it possible to single out the most typical long-term or large-scale temporal dynamical formations. Spatial averaging shades small-scale elements of circulation but retains those formations which take place in the given area quasi-stationary or during a rather prolonged period of time.

A total of 1,153 hydrographic stations occupied by AtlantNIRO, 374 in winter (January, February, March) and 779 in summer (July, August, September) in 1961-66 provided the main source of data.

Temperature and salinity were averaged according to the spherical trapezia with the measurements of 20′ in lat and 30′ in long. Such measurements of the "square" take into consideration rather well the great complicacy of the relief. Currents were calculated from the averaged estimations of temperature and salinity by the dynamic method (Zubov and Mamayev, 1956), based on the assumption of a geostrophic character of water movement.

In spite of a relatively irregular distribution of observations, the averaged values were considered to be associated with the middle of the seasons and of the "square". The 200db reference level was chosen in the supposition that such a choice would diminish the errors which are inevitable while choosing levels in the shallow water areas (Zubov and Mamayev, 1956). The calculations of dynamic heights were performed by the computer.

The results are presented in Figs. 11-16. On the charts the dynamic horizontal lines for the surface and the 50-m level are drawn with a contour interval of 25 dyn. mm and for the 100-m level a contour interval of 10dyn. mm.

Three water masses were singled out in the area under investigation: coastal, Gulf Stream and Labrador (Briantsev, 1963; 1970), the average vertical distribution of these water masses being represented as follows:

a) the 0-to 50-m layer is brackish coastal waters;

b) the 50-to 150-m layer is Labrador Current water;

c) the 120-to 150-m layer and to the bottom is bottom water of oceanic origin.

Thus, our charts represent geostrophic circulation on the surface and on the boundary between the Labrador and surface coastal water (50m), approximately, and also in the core of the water mass of the Labrador Current (100m). In some cases, water circulation on the 100-m level is affected by the encroachment of Gulf Stream waters (Briantsev, 1963).

From these charts one can single out a series of features characteristic of both the winter and summer; this is indicative of the quasi-stability of these circulation elements during the whole year.

At the surface, the northwestern periphery of the flow can be traced over the continental slope along the 200-m isobath. It is directed from the northeast to the southwest.

Pakhorukov (1961) noted a great transport of cold waters displaced in a south-westerly direction from the Gulf of St. Lawrence. Having met an obstacle, namely Sable Island, part of these waters move intensively to the northeast. This is seen clearly in Figs. 11 and 12.

The whole system of closed eddies with different directions of rotation is formed to the right of this flow, elements of anti-cyclonic eddies being predominant. In both seasons anti-cyclonic eddies were encountered also on Georges and Browns Banks, in the area of Emerald Bank and to the north of Sable Island. The waters, distributed partly in a south-westerly direction, move along the Nova Scotia coast and circle the Gulf of Maine in a counterclockwise direction, forming a cyclonic vortex, which is more developed in summer in the western part of East Channel.

The chart of geostrophic circulation on the surface for the summer season is presented in Fig. 12. It is in good conformity with the scheme offered by Bumpus (1960) and Bigelow (1927).

Here, we can also single out a cyclonic eddy in the Gulf of Maine and an anti-cyclonic vortex on Georges Bank.

Trites and Banks (1958) studied surface currents on the Scotian Shelf using 827 drift bottles which were released in August 1954. However, the scheme of surface circulation obtained by them does not agree with our scheme. The treatment of the data obtained by means of the "bottle post" has led them to the conclusion that there exists a well pronounced cyclonic vortex which covers nearly the whole of the Scotian Shelf, its centre being in the area of Sable Island. On our charts, a system of anti-cyclonic eddies is shown in this area of the shelf in winter and summer. In the Sable Island area, one can see the curve of the southwestern flow, which is distinctly pronounced in winter and can be taken as the periphery of the cyclonic vortex. In this case, however, its centre must be situated far more southerly than it is in the scheme of Trites and Banks, and it does not expand onto the Shelf. Only in summer (Fig. 12) can the branch of this flow be traced. It reaches the southern extremity of Nova Scotia, and then turns and flows along the coast to the northeast.

At the 50-m level, the above-mentioned eddy formations persist. At the entrance to the Bay of Fundy the anti-cyclonic rotation observed in summer, both on the surface and at the 50-m level, is also recorded in winter (Figs. 13 and 14).

At the 100-m level, anti-cyclonic eddies persist on Georges and Browns Banks. To the east, a cyclonic vortex is observed on Roseway and La Have Banks in winter. In summer it displaces further to the east into the area of Sambro and Emerald Banks.

In the area of Banquereau Bank and to the north of it, an anti-cyclonic eddy exists which is more developed in winter. As evident from Figs. 15 and 16,

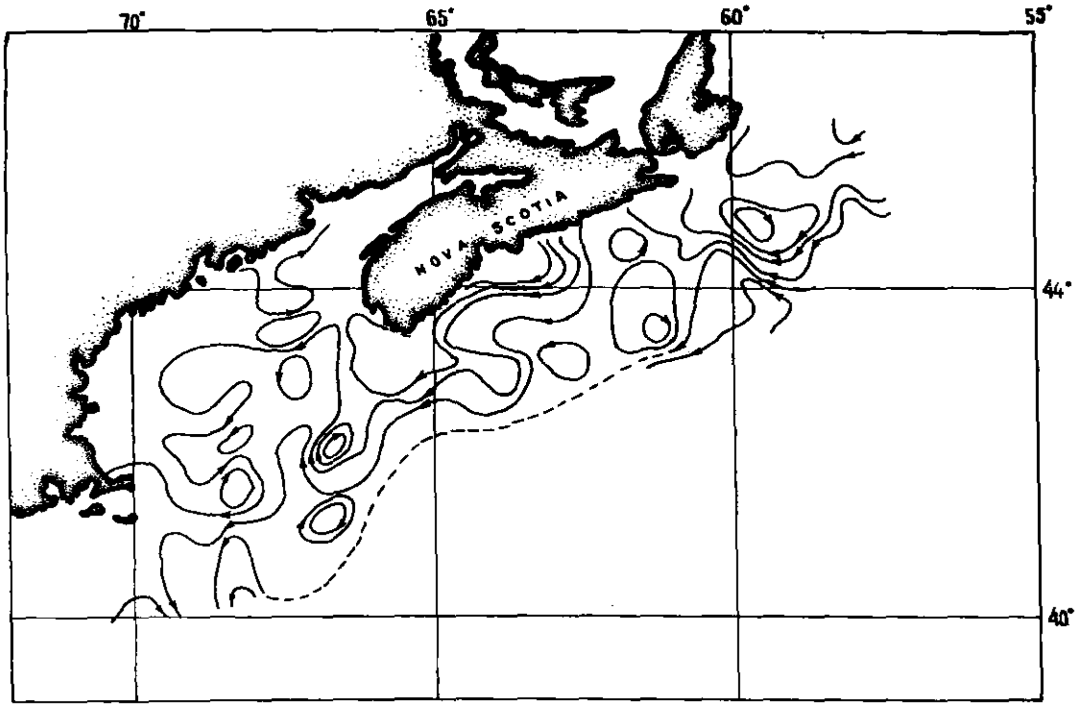

Fig. 11. Geostrophic circulation of surface waters in the area of the Scotian Shelf and Georges Bank in winter.

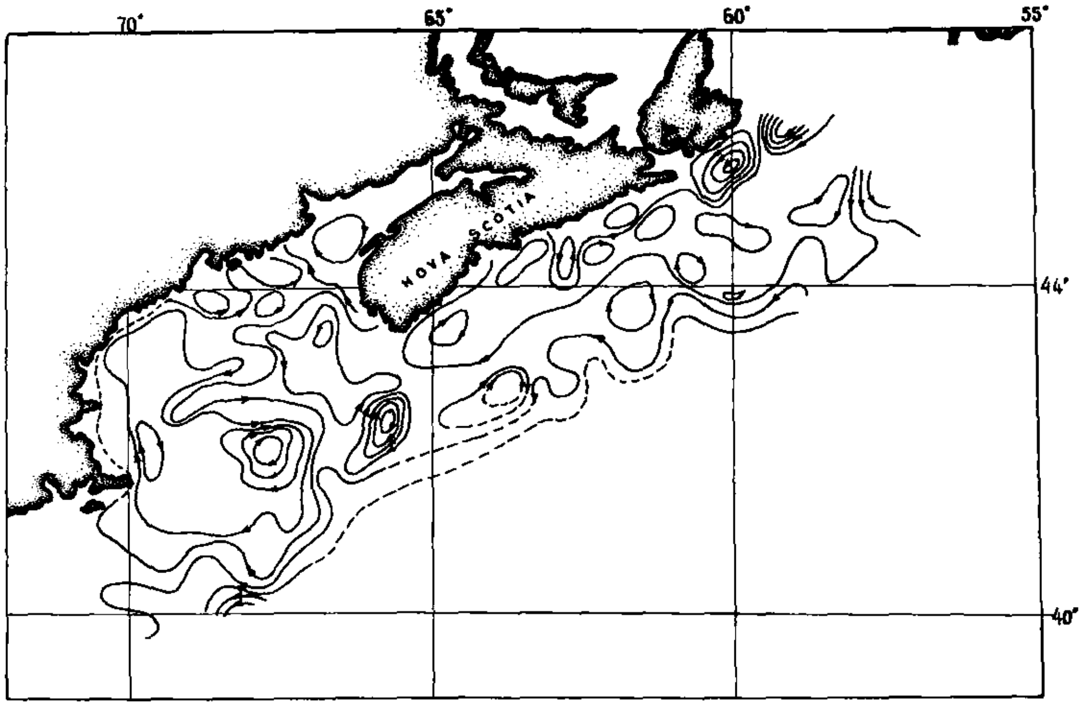

Fig. 12. Geostrophic circulation of surface waters in the area of the Scotian Shelf and Georges Bank in summer.

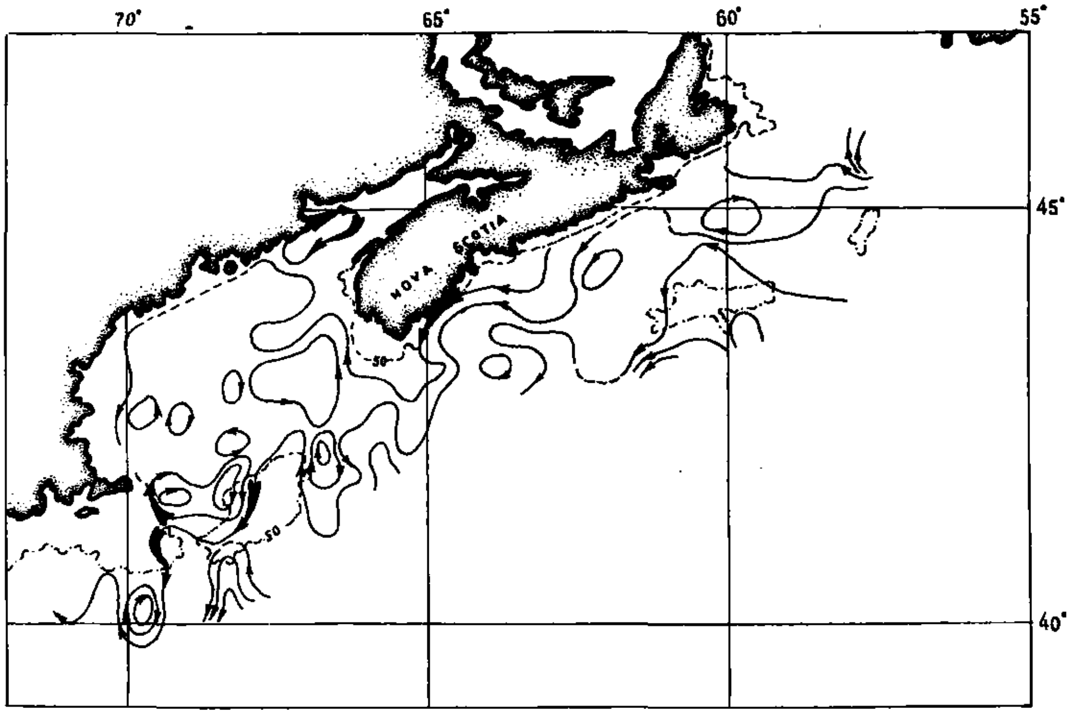

Fig. 13. Geostrophic circulation of waters at the 50-m level in the area of the Scotian Shelf and Georges Bank in winter. Here, and in other cases, bold type arrows show the supposed transport of water at shallower depths.

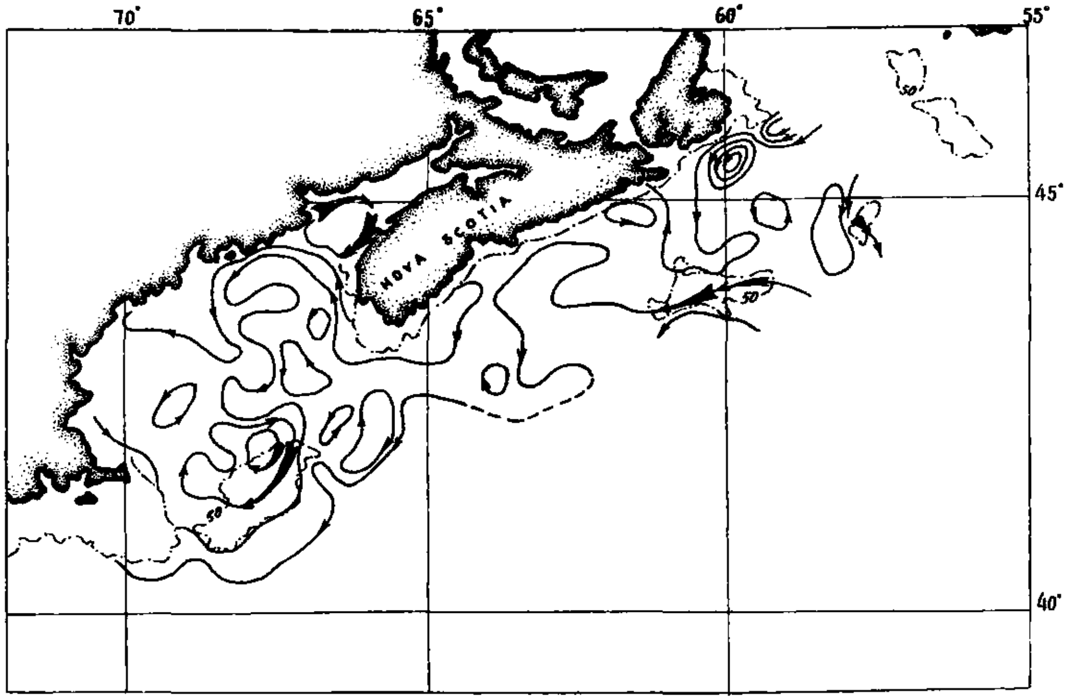

Fig. 14. Geostrophic circulation of waters at the 50-m level in the area of the Scotian Shelf and Georges Bank in summer.

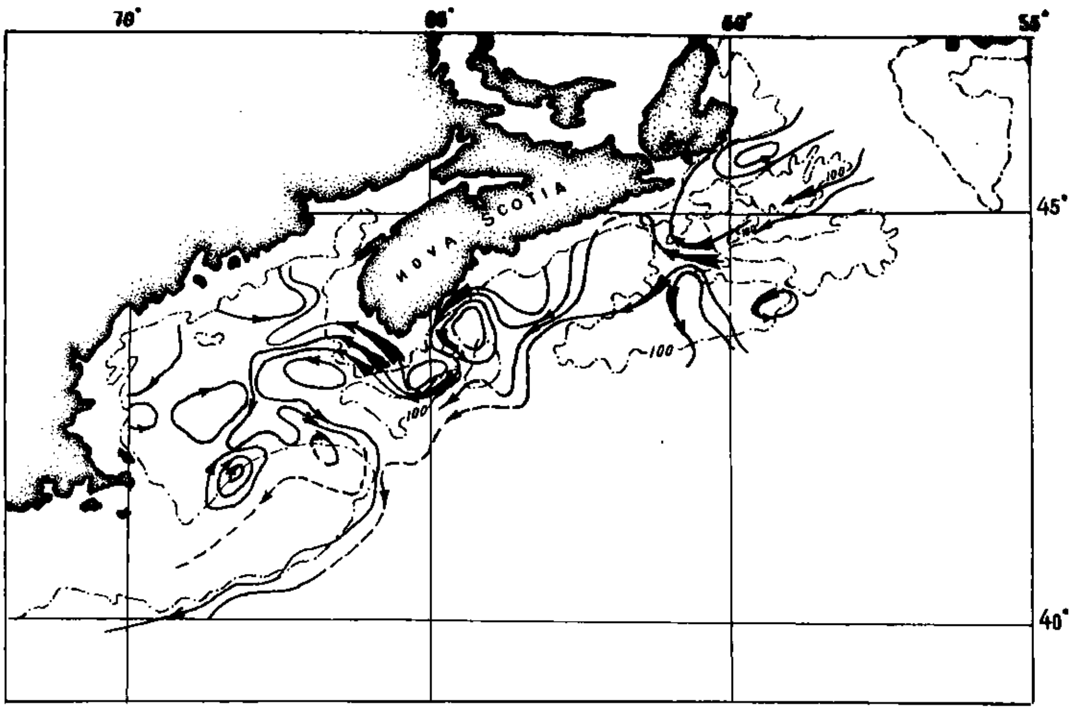

Fig. 15. Geostrophic circulation of waters at the 100-m level in the area of the Scotian Shelf and Georges Bank in winter.

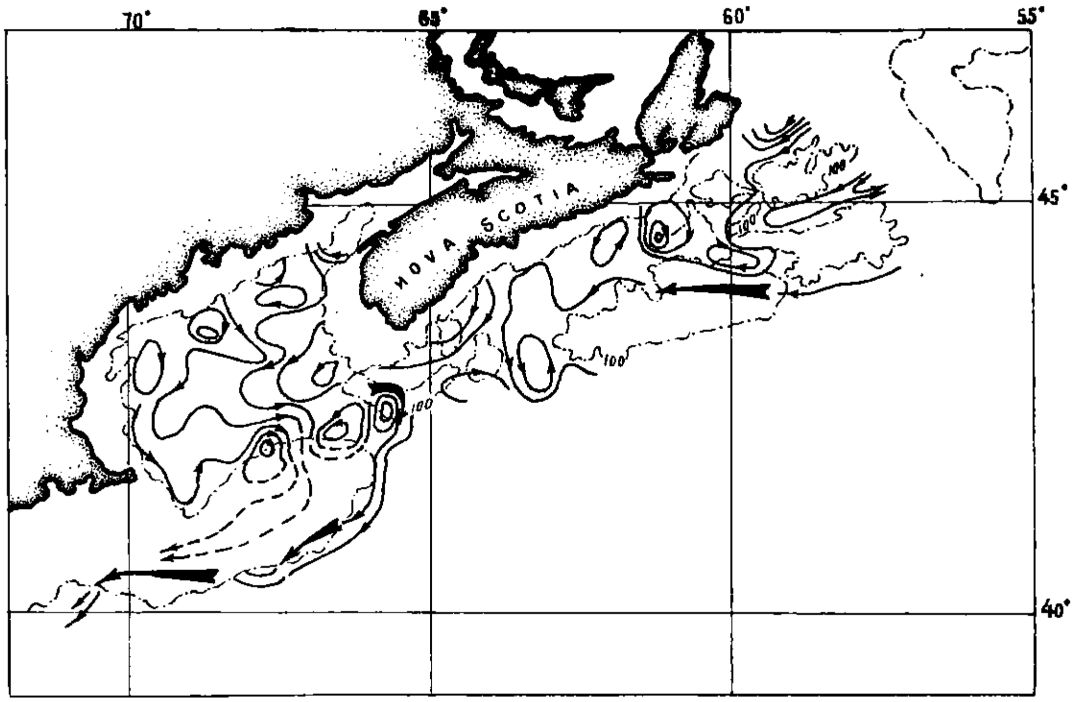

Fig. 16. Geostrophic circulation of waters at the 100-m level in the area of the Scotian Shelf and Georges Bank in summer.

the intrusion of waters at the 100-m level (and evidently also at great depths) is more intensive in winter. Hachey (1953) also pointed out this fact while speaking about the occurrence of the waters from the slope on the Scotian Shelf in the winter of 1949. This was the first case, when waters with such temperatures were observed on the Shelf. Usually, these waters move more northerly above the edge of the continental shelf. The geostrophic component of velocity of the surface currents, calculated by us, fluctuates within 0.1-0.6 knots. According to the data obtained by Trites and Banks, the velocity of the drift fluctuates between 0.11 and 0.15 knots. Our estimation of the maximum velocity seems to be closer to the actual one, especially as it is in good conformity with data obtained as a result of instrumental observations.

Studies of schemes of water circulation in the area suggest a predominating influence of the Labrador Current waters on the formation of such a picture of circulation. The flow of these waters is driven by the Gulf Stream to the continental slope and forms a complicated picture of the above-mentioned eddy formation on the Shelf. In addition, from the data obtained in the volumetric statistical analysis by Briantsev and Barinov, Labrador waters occupy the 50- to 120-150-m layer and account for about 40% of the total volume of water on the Shelf.

Thus a heterogeneous picture of geostrophic water circulation in the area under investigation has been obtained; some non-typical details, complicated configuration of the lines of the current can be explained by the great heterogeneity of the relief and also by the fact that an insufficient volume of oceanographic data was obtained in some "squares", and by errors in averaging and the drawbacks of the method itself.

Conclusions

Thus, in winter and summer periods, water transport in the southwesterly direction is observed above the continental slope of the Atlantic Shelf of Canada. Its coastal periphery is situated somewhere in the area of the 200-m isobath. To the right of this flow a series of local small eddies rotating in both directions are observed on the continental shelf. Several such elements of circulation (the areas of Georges, Browns, Emerald Banks, and Sable Island) are recorded during both seasons. This fact points to the permanence of these processes during the year. In the deep part of the Gulf of Maine from the surface to the bottom, an anti-cyclonic vortex is observed; it is quite pronounced in both seasons, but is better developed in summer.

Our charts are in good conformity with the scheme of Bigelow (1927) for the area of the Gulf of Maine and Georges Bank, but do not agree with the scheme of water circulation on the Scotian Shelf offered by Trites and Banks (1958), which is based on material obtained by means of "bottle post".

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Discussion

Dr Campbell: *"Are these standard sections that you are occupying from Davis Strait southward along the east coast?"*

Mr Nikolaev: *"Yes, they are standard sections."*

Mr Lee: *"I am not surprised to see that the sections in the Flemish Cap area are showing different volume transports for the Labrador Current compared with the Hamilton Inlet section in any particular year. I suspect that things change according to the season of the year and these sections have been worked in different seasons. This is part of our trouble. We just cannot sample these standard sections often enough to understand how these currents change."*

Mr Nikolaev: *"Unfortunately, only sporadic observations were available to us and this is probably the reason final conclusions could not be made."*

Dr Campbell: *"Perhaps the ICNAF Environmental Subcommittee could consider the possibility of standardizing hydrographic reporting in the ICNAF research reports and setting up standard hydrographic stations and sections rather than having each country work independently in the areas. Then we could start filling in those gaps in our time series for sections along the Labrador coast."*

Contribution Number 8

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Variability in the Distribution and Abundance of the Plankton

By J. M. Colebrook[1]

Abstract

The Continuous Plankton Recorder has been used in a survey of the plankton of the North Atlantic and the North Sea since 1948. Data are available for the eastern area, to 19° W, for the whole of the period and for a steadily increasing area of the western part from the early sixties onwards.

These data have been used to derive charts of the geographical distribution of a large number of species, averaging the data available for the years 1958-68.

Analyses, by multivariate techniques, of the distributions of about 50 of the more abundant species have provided a classification of species on the basis of similarities and differences between their geographical distributions, a set of type distributions based on this classification and a set of principal components representing the major elements in the geographical distributions of the species.

The survey data can also be used to provide expressions of year to year fluctuations in the abundance of species. Virtually complete data sets are available for the years 1948-68 for each of a set of 12 area subdivisions of the North Sea and the Atlantic to 19° W.

Data for the zooplankton have been analysed in detail. These analyses demonstrate the existence of clear trends in the year to year fluctuations; the most pronounced being a fairly consistent downward trend in the abundance of a significant proportion of the more abundant species. There are also clear geographical patterns of relationship between annual fluctuations in abundance showing differentiation between northern and southern areas and between oceanic and shallow-water areas.

Relationships between species with regard to annual fluctuations of abundance appear to have a geographical basis.

Data for west of 19° W are available only for the last 6 to 8 years, such analyses as have been done suggest that there are relationships between species and between successive years, analogous to those found for the eastern waters, but the data are as yet inadequate to provide much useful information about the form of these relationships.

For the eastern area some consistent patterns have been detected for year to year changes in the timing of the seasonal cycles of a number of species.

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Introduction

All the work described here is based on the Continuous Plankton Recorder Survey of the North Atlantic and the North Sea (Glover, 1967) which is now part of the activities of the newly established Institute for Marine Environmental Research.

Continuous Plankton Recorders are towed regularly, at monthly intervals whenever possible, at a standard depth of 10 m, along a number of fixed routes using ships of opportunity. Figure 1 shows the routes currently in operation.

The recorder collects a continuous sample of the plankton on a band of bolting silk with 60 meshes to the inch. The silk bands are subsequently divided up into sections, each equivalent to a distance of 10 miles, and alternate samples are analysed for phytoplankton and zooplankton; identification being to species, genus or family depending on ease of identification.

Figure 2 shows a set of standard areas used in the routine assessment of annual and seasonal fluctuations in abundance and in each area is given the number of years for which data are available. In most of the eastern areas complete sequences of years from 1948 onwards are available. In most of the western areas, however, much shorter runs of years have been sampled.

The problems of sampling plankton and of processing and analysing plankton data are such that in order to demonstrate clear relationships between species and geographical patterns of annual fluctuations in abundance relatively long sequences of observations are necessary. Fairly clear patterns are emerging for the eastern areas but there are insufficient data from the western half of the North Atlantic for it to be possible to do much more than point to possible analogies with the better known eastern half.

Geographical Distributions

In order to set the scene for the work on annual fluctuations in abundance it is convenient to outline the biogeography of the plankton of the area.

A series of charts have been produced, based on a compilation of all the data for the years 1958-68, showing the geographical distributions of well over 200 species and genera.

The combination of data for a number of years permits the use of an area grid based on rectangles of 1° lat by 2° long. Figure 3 is an example of the charts that have been produced; it shows the distribution of the copepod *Calanus finmarchicus*. Superimposed on the chart is an indication of the major current systems. It is

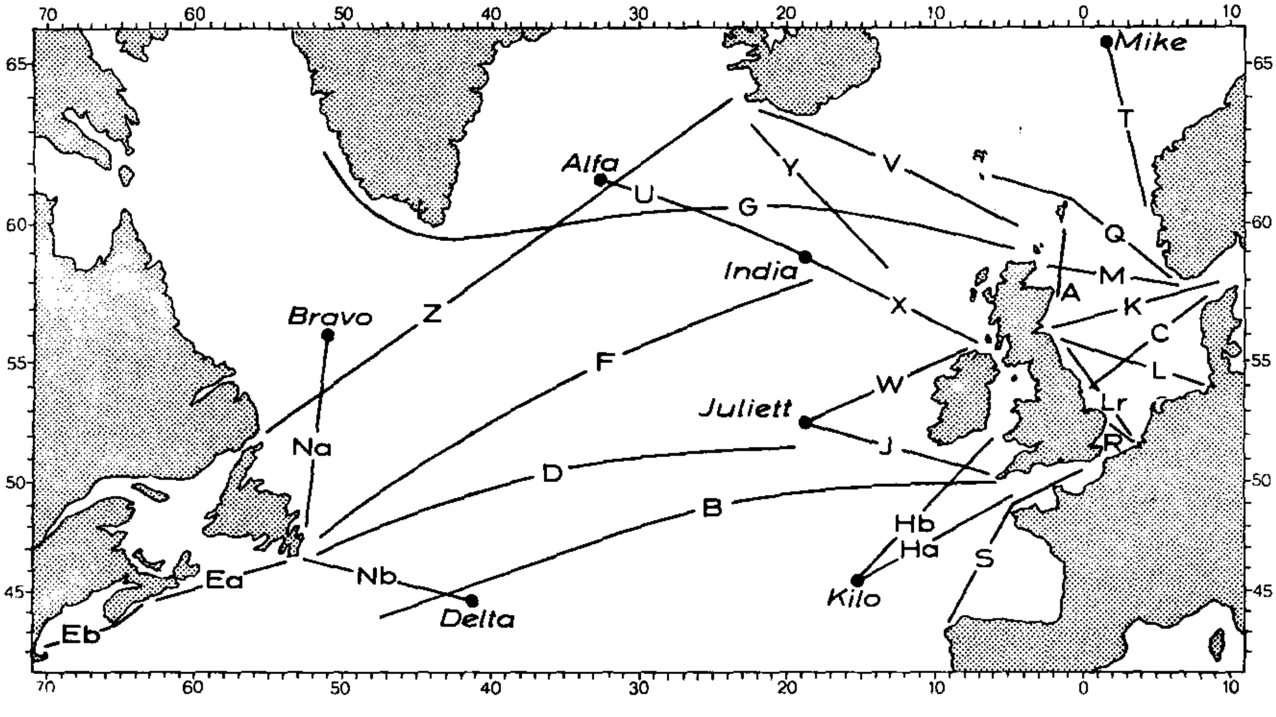


Fig. 1. Chart of the North Atlantic showing the routes along which Plankton Recorders are towed.

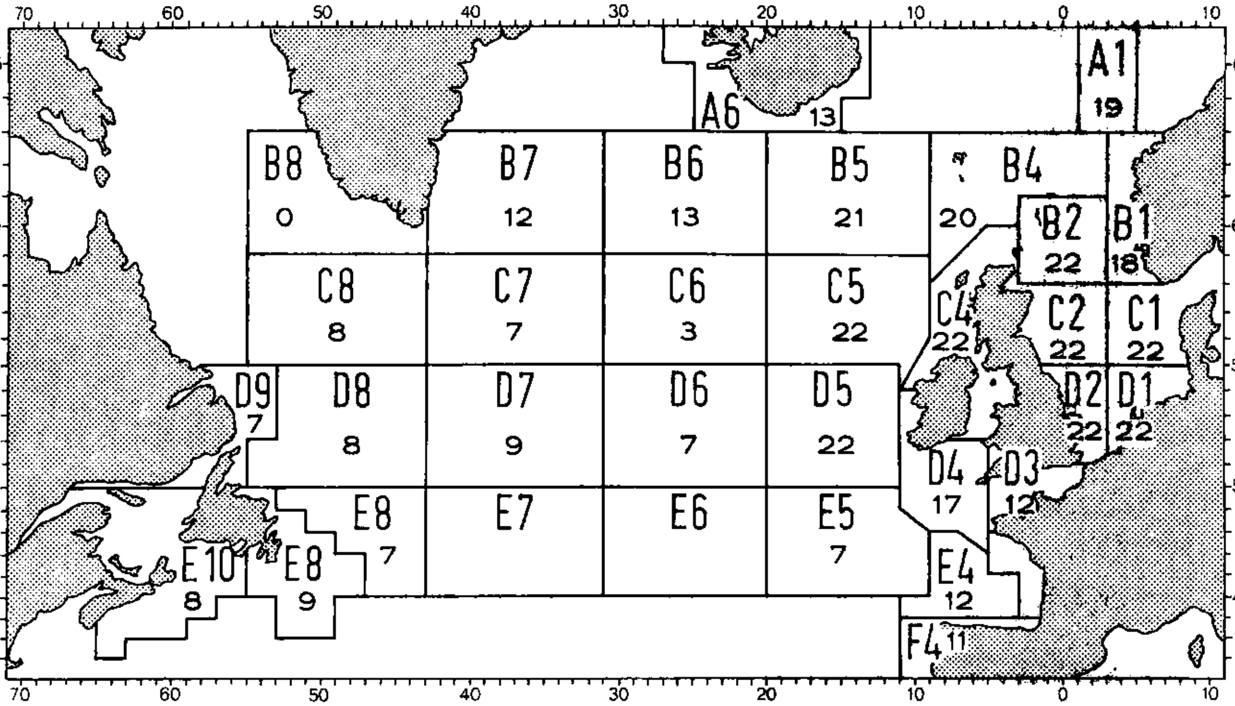


Fig. 2. Chart of the North Atlantic showing the standard area subdivision of the survey area. In each area is given the number of years for which the area has been adequately sampled.

Calanus finmarchicus

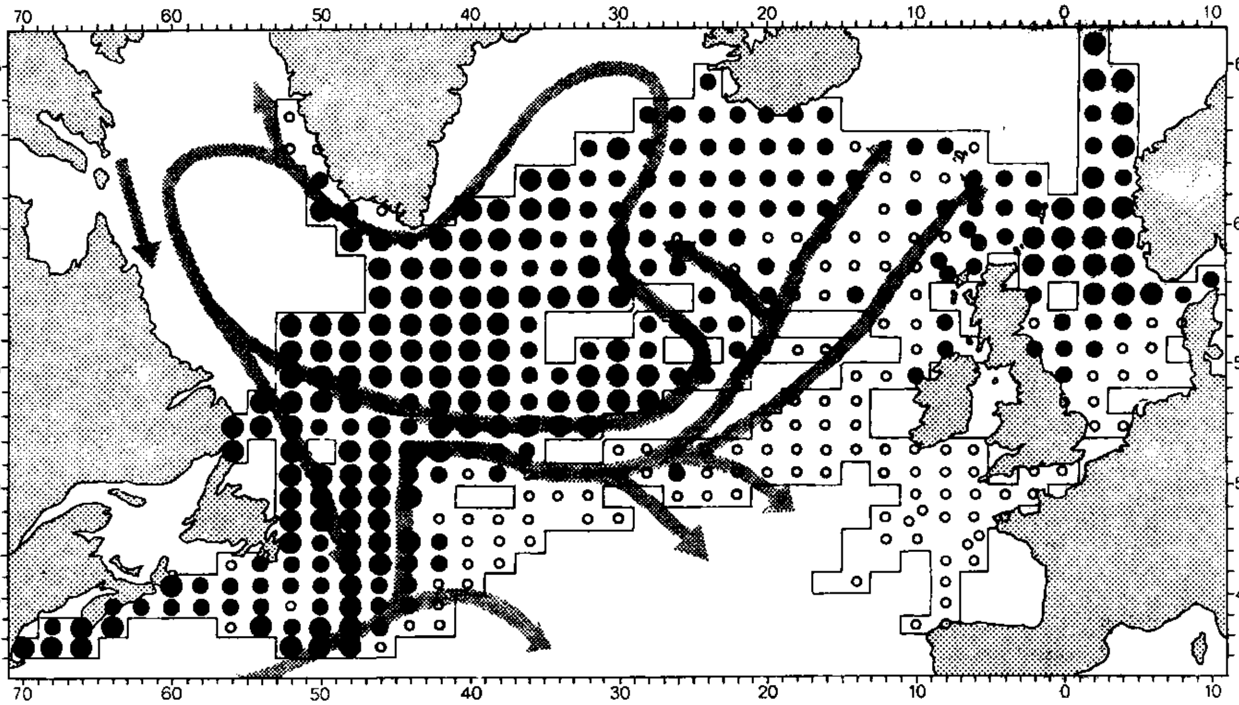


Fig. 3. Chart showing the geographical distribution of the copepod *Calanus finmarchicus*. For each rectangle, all the observations for the years 1958-68 were averaged. The lowest third of the means are represented by open circles, the middle third by small black circles and the highest third by large black circles. Superimposed on the chart is a system of arrows showing the major ocean current systems.

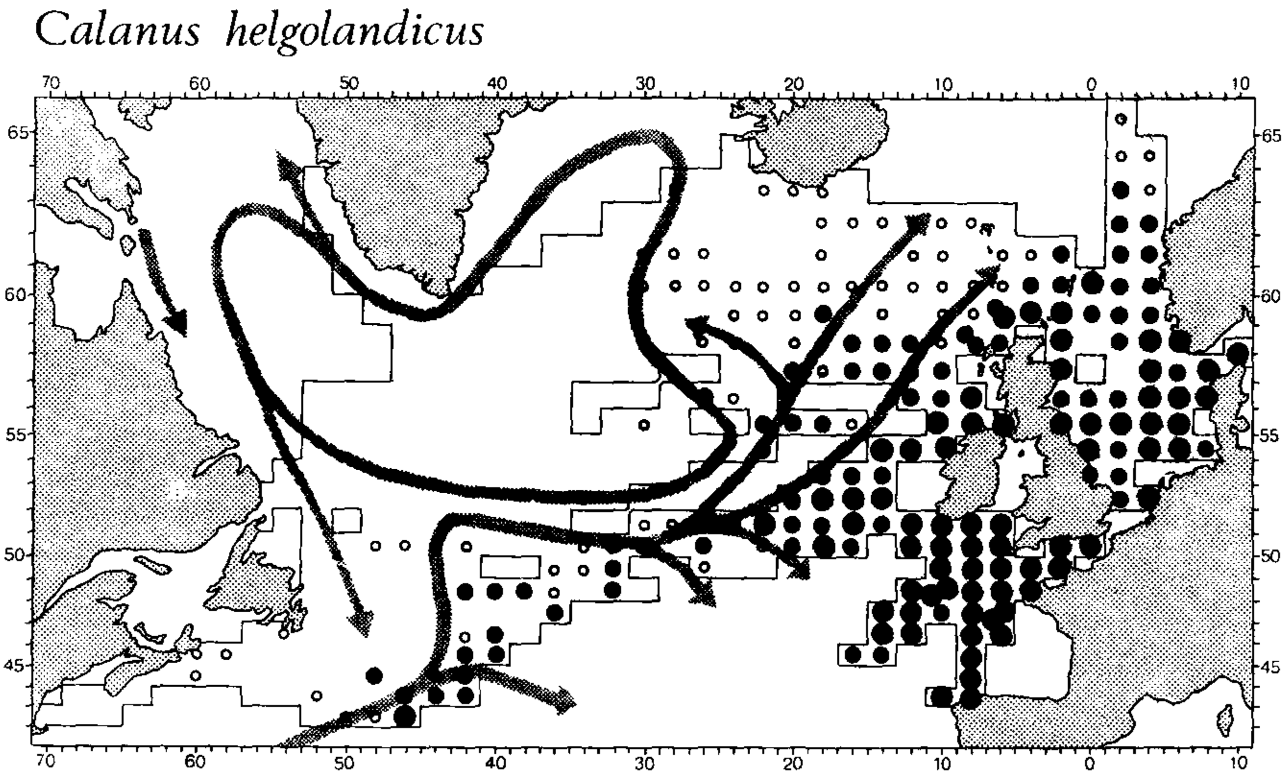

Calanus helgolandicus

Fig. 4. Chart showing the geographical distribution of the copepod *Calanus helgolandicus*. The format is the same as in Fig. 3.

clear that the main centre of distribution of this species is in the Irminger Sea-Labrador Gyral current system. In contrast to this, Fig. 4 shows the distribution of the closely related species *Calanus helgolandicus* which is largely restricted in its distribution to the warm waters just to the south of the main North Atlantic drift system.

These two charts illustrate very clearly one of the problems of dealing with plankton. The morphological difference between these two species would, at first sight, appear to be trivial, based on relatively slight differences in the configuration of the inner border of the coxopodite of the fifth swimming leg. In the Recorder Survey these species have been identified separately since only 1958, and the full specific status of these forms has been generally accepted only for the last 2 or 3 years, and yet ecologically they are very distinct.

Following the preparation of the set of distribution charts, the data for over fifty of the more abundant species of both zooplankton and phytoplankton were submitted to a principal components analysis. This has provided the basis for a classification of species with respect to their geographical distribution.

Principal components analysis is a multivariate technique which can be used to provide estimates of the major independent elements, in this case geographical patterns, in the data. The analysis also produces sets of coefficients known as vectors, which indicate the relationships between species.

The first three sets of coefficients, associated with the three dominant geographical patterns, were examined to see whether a classification of species could be derived. A looping and branching sequence of species was aimed at, rather than a cluster analysis or a dendrogram, as being intrinsically the more likely pattern of relationship between species in this context.

Figure 5 shows the final result of a trial and error ranking of the species for the first three vectors. In each case the species are in the same order and this has been manipulated to give the smoothest possible plots of the values of the coefficients. An interpretation of this figure in the form of a species sequence is given in Fig. 6.

A list of the full names of the species is given in Table 1.

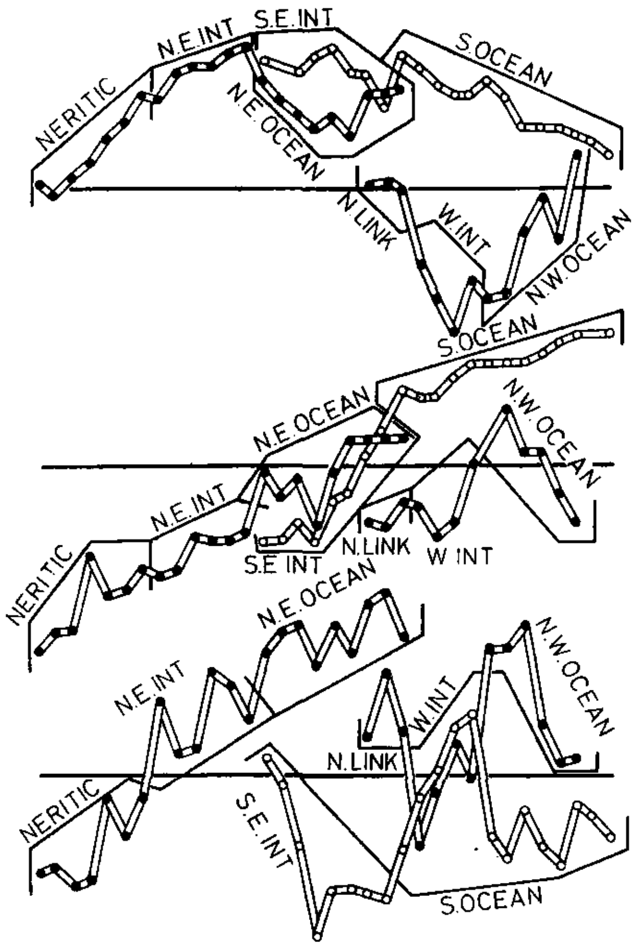


Fig. 5. Plots of the first three vectors of a Principal Components analysis of geographical distributions.

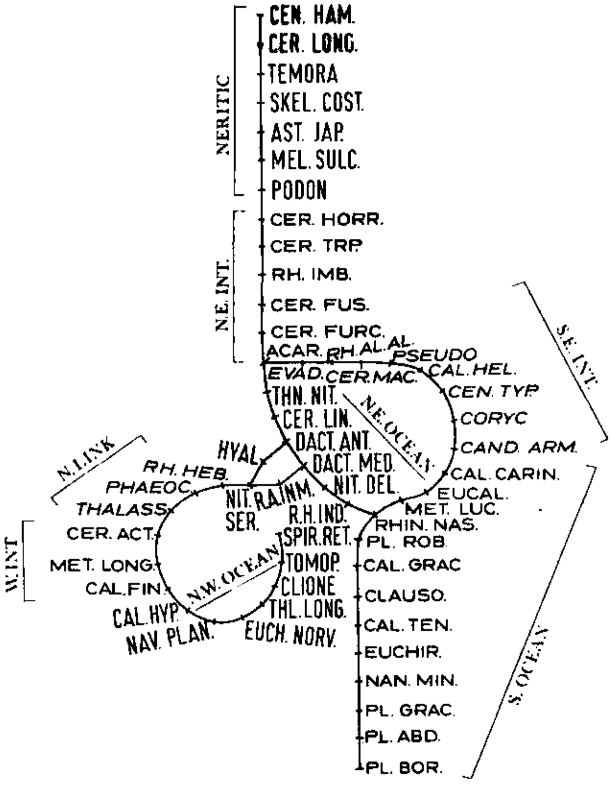


Fig. 6. An interpretation of the relationships shown in Fig. 5 in the form of a species sequence. The species names are given in full in Table 1.

TABLE 1. List of full species names for Fig. 5.

Centropages hamatus
Ceratium longipes
Temora longicornis
Skeletonema costatum
Asterionella japonica
Melosira sulcata
Podon spp.
Ceratium horridum
Ceratium tripos
Ceratium furca
Acartia clausii
Thalassionema nitzschioides
Ceratium lineatum
Dactyliosolen antarcticus
Dactyliosolen mediterraneus
Nitzschia delicatissima
Rhizosolenia alata indica
Evadne spp.
Rhizosolenia alata alata
Pseudocalanus elongatus
Calanus helgolandicus
Centropages typicus
Corycaeus spp.
Candacia armata
Calanus carinatus
Eucalanus elongatus
Metridia lucens
Rhincalanus nasutus
Pleuromamma robusta
Calanus gracilis
Clausocalanus spp.
Calanus tenuicornis
Euchirella rostrata
Nannocalanus minor
Pleuromamma gracilis
Pleuromamma abdominalis
Pleuromamma borealis
Hyalochaete spp.
Rhizosolenia semispina hebetata
Phaeoceros spp.
Thalassiosira spp.
Ceratium arcticum
Metridia longa
Calanus finmarchicus
Calanus hyperboreus
Navicula planamembranacea
Euchaeta norvegica
Thalassiothrix longisima
Clione spp.
Tomopteris spp.
Spiratella retroversa

The distribution charts of these species were arranged in the same sequence, and running through this, it was possible to detect discontinuities at a number of points in the sequence. These points are indicated in Figs. 5 and 6. The species have been divided into groups each of which has been given a name descriptive of the geographical distributions of the species making up the group. For each group, vector terms were estimated by taking within vector averages for the respective species. These were used as coefficients in calculating weighted means of the first three components. This resulted in the production of a series of distributions, one for each species group, which were independent of differences in abundance between species. For each species group a chart was prepared by ranking the highest quarter of the values and dividing this into three equal sections. The charts are given in Figs. 7 to 13 with open circles representing the lowest third of the observations, small black circles for the middle third, and large black circles for the highest third. These charts can be arranged in a sequence starting with a neritic distribution (Fig. 7) confined to shallow and coastal waters, on both sides of the Atlantic and off Iceland.

Figure 8 shows a type of distribution which is intermediate between neritic and oceanic, occurring in the shallow waters of the North Sea but also extending out beyond the continental shelf to the northwest of the British Isles. Figure 9 shows a northeast oceanic group occurring over deep water to the northwest and west of the British Isles.

There is a corresponding pair of intermediate and oceanic distributions with centres in the east and south. Figure 10 shows the intermediate group and Fig. 11 shows the oceanic group. This is centred in the warm oceanic waters and it contains a relatively large number of species.

There are two distribution patterns with centres in the western Atlantic. Firstly a western intermediate group, shown in Fig. 12. The members of which are *Calanus finmarchicus*, *Calanus hyperboreus*, *Metridia longa*, and *Ceratium arcticum*. Secondly, there is a northwest oceanic group shown in Fig. 13. The members of which are *Euchaeta norvegica*, *Clione limacina*, *Navicula planamembranacea*, and *Thalassiothrix longissima*. In addition to these species, there is reason to believe that there are at least some Euphausiids, Hyperiids, and Tintinnids also belonging to these western groups.

The main geographical patterns which form the basis of these distributions are shown in Figs. 14-17. These patterns are provided by the components produced by the Principal Components Analysis. Figure 14 shows the first component, indicating a clear differentiation between east and west with a fairly clear boundary

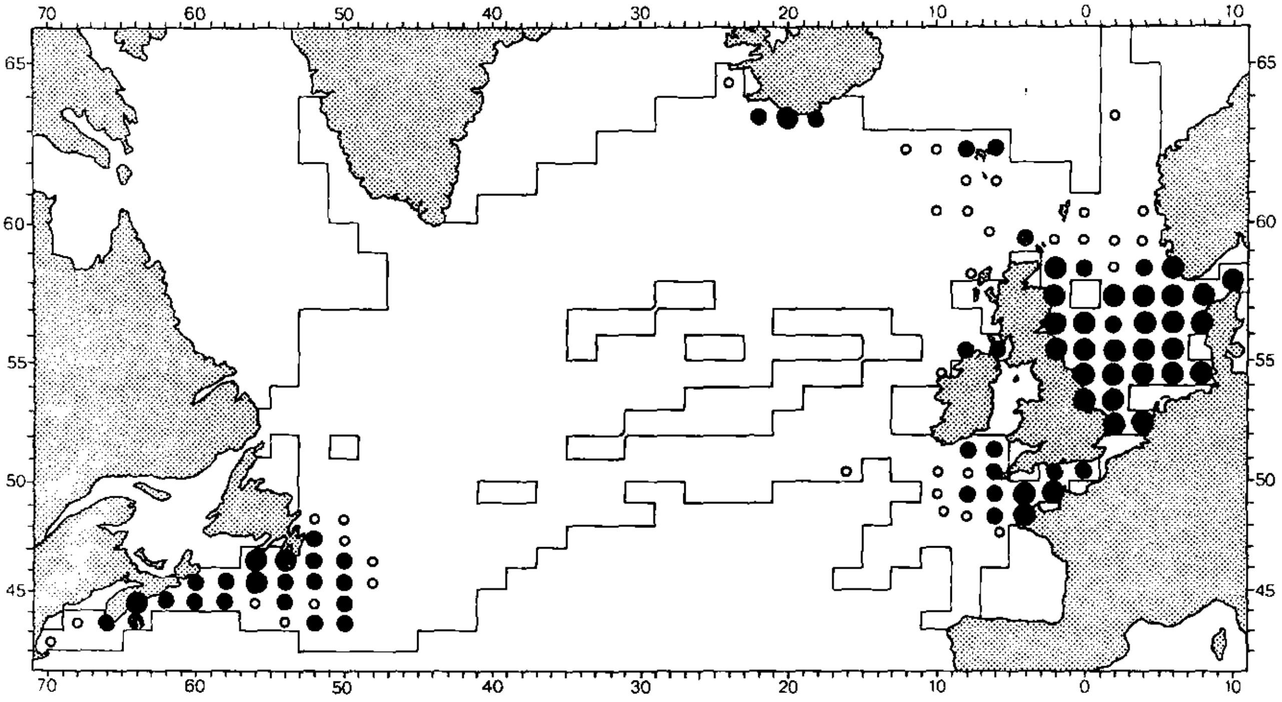


Fig. 7. Distribution type: Neritic.

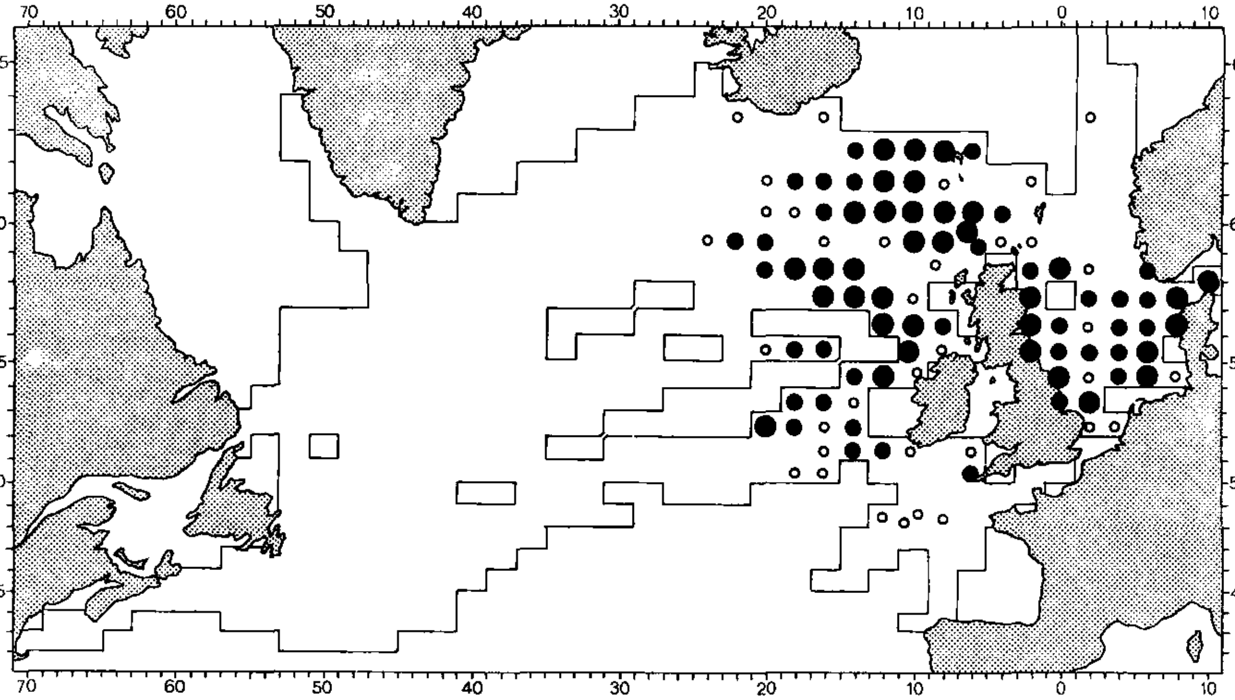


Fig. 8. Distribution type: Northeast Intermediate.

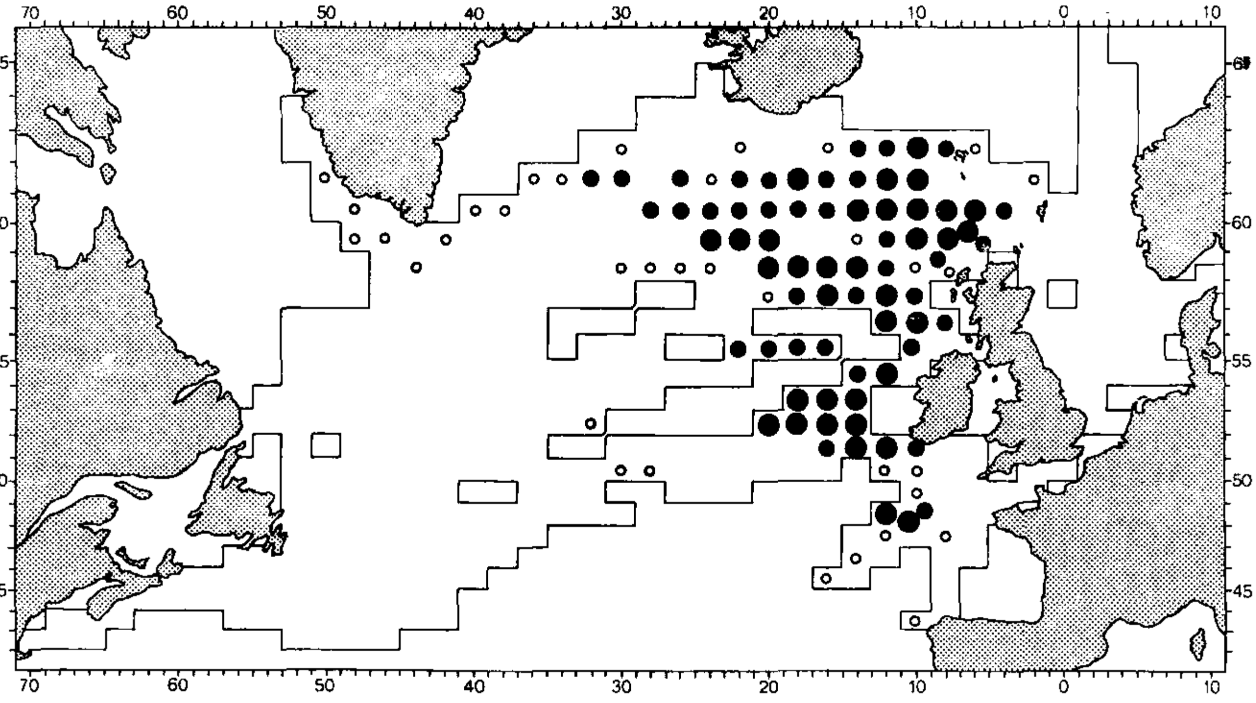


Fig. 9. Distribution type: Northeast Oceanic.

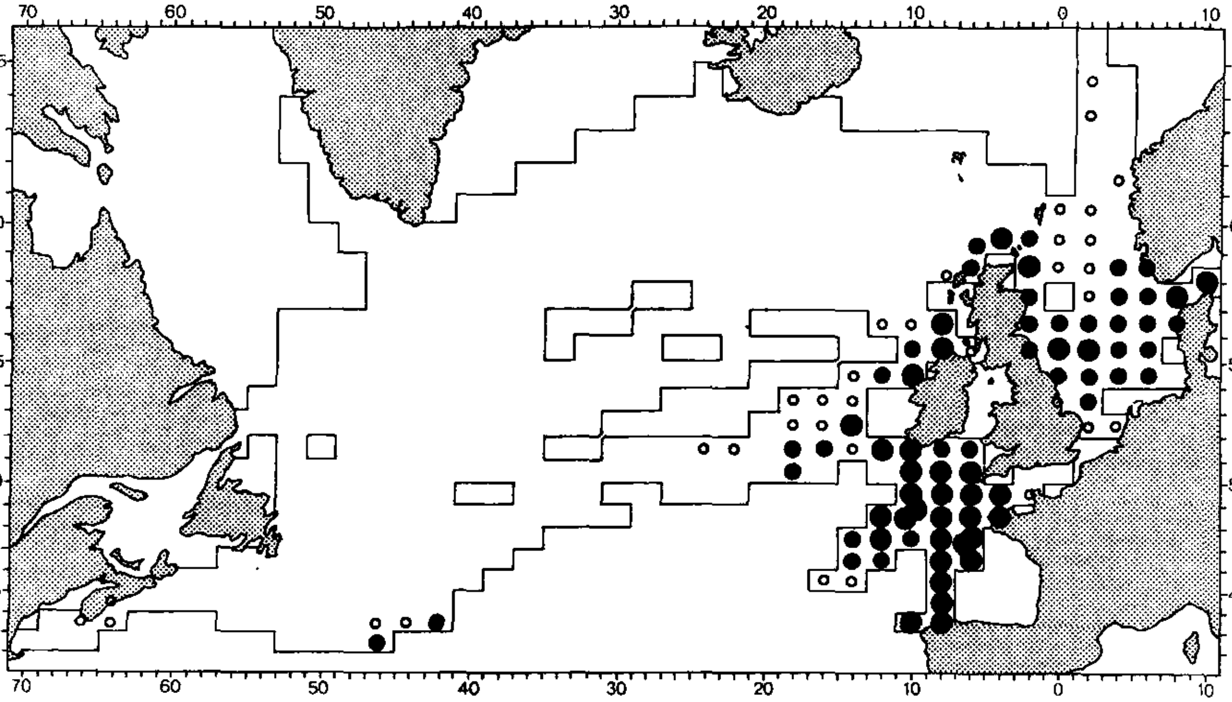


Fig. 10. Distribution type: Southeast Intermediate.

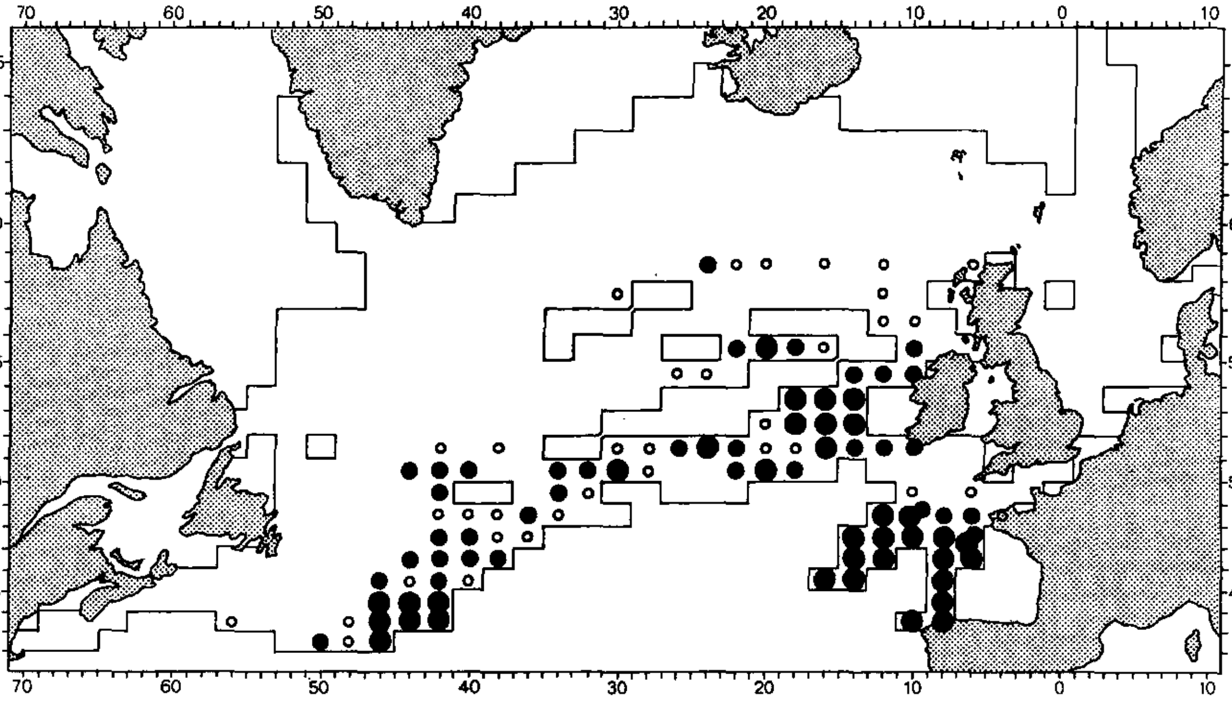


Fig. 11. Distribution type: Southern Oceanic.

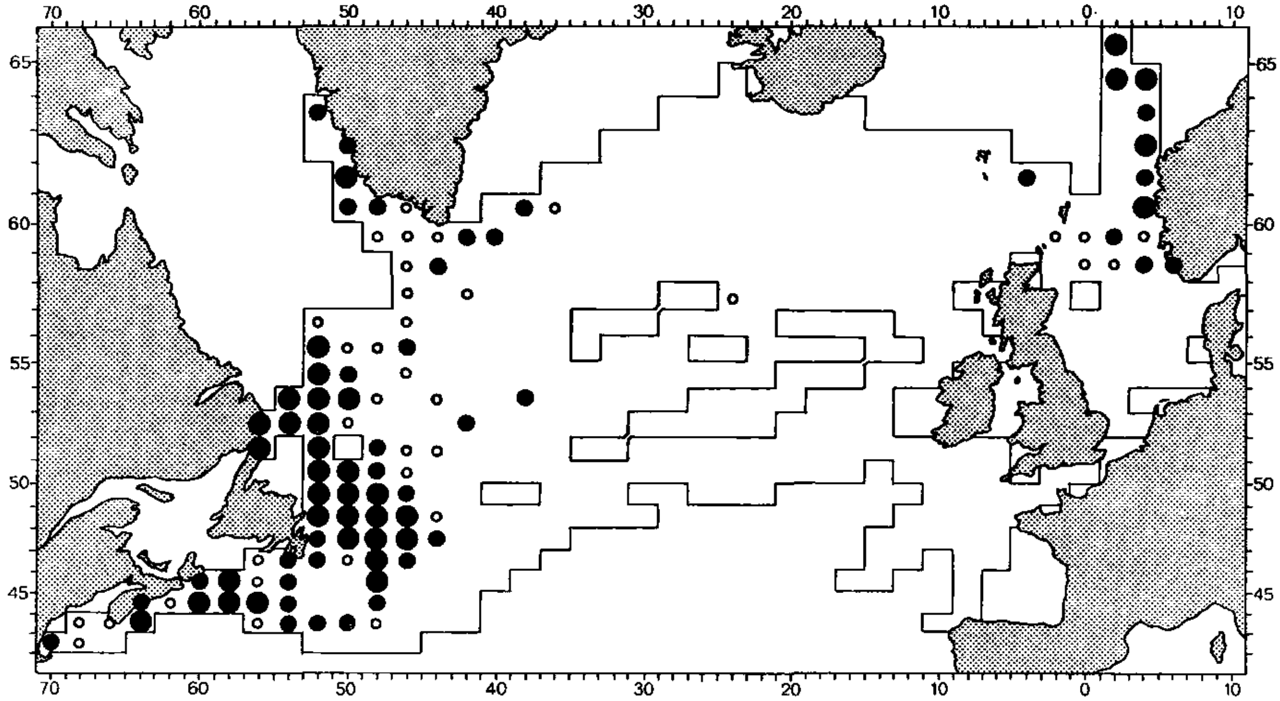


Fig. 12. Distribution type: Western Intermediate.

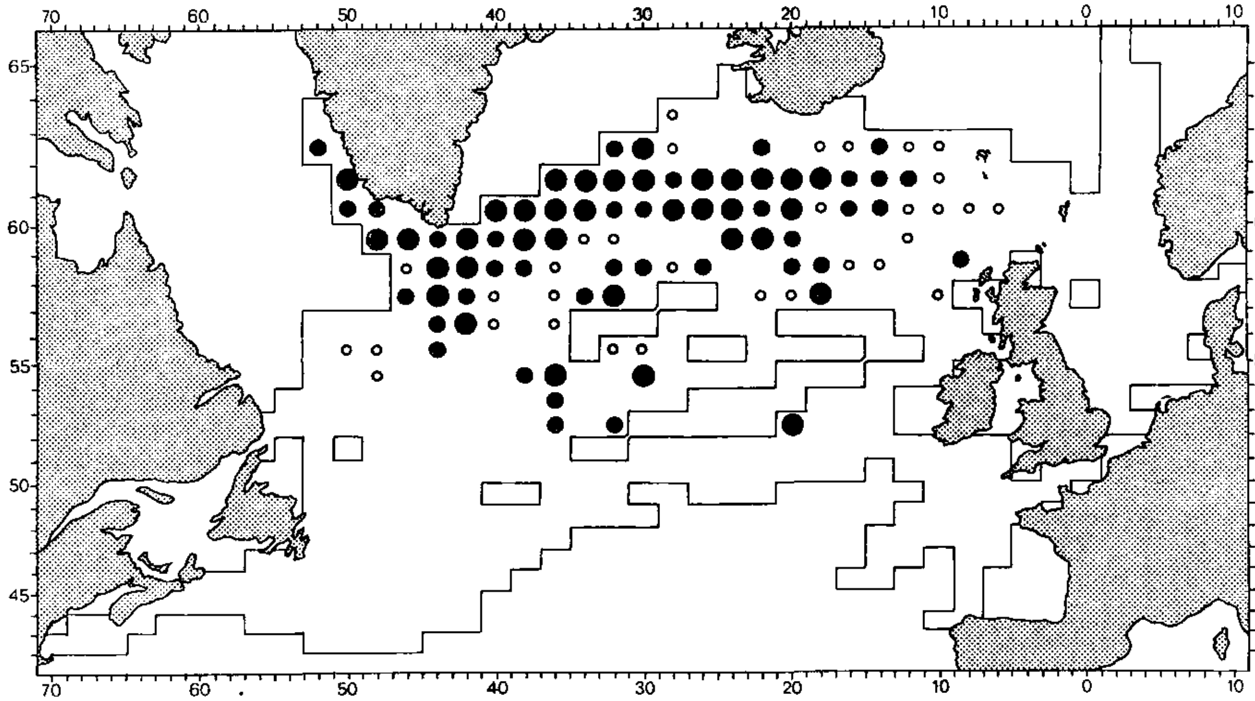


Fig. 13. Distribution type: Northwest Oceanic.

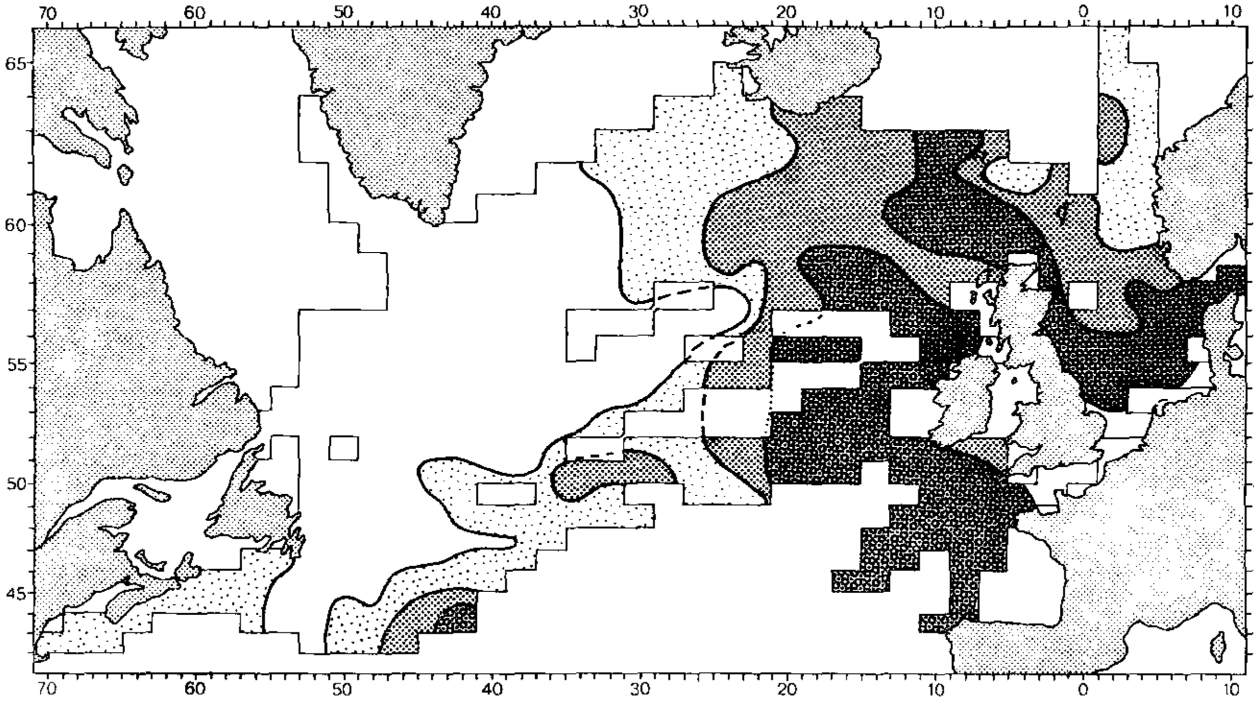


Fig. 14. Chart of the first component of geographical distribution.

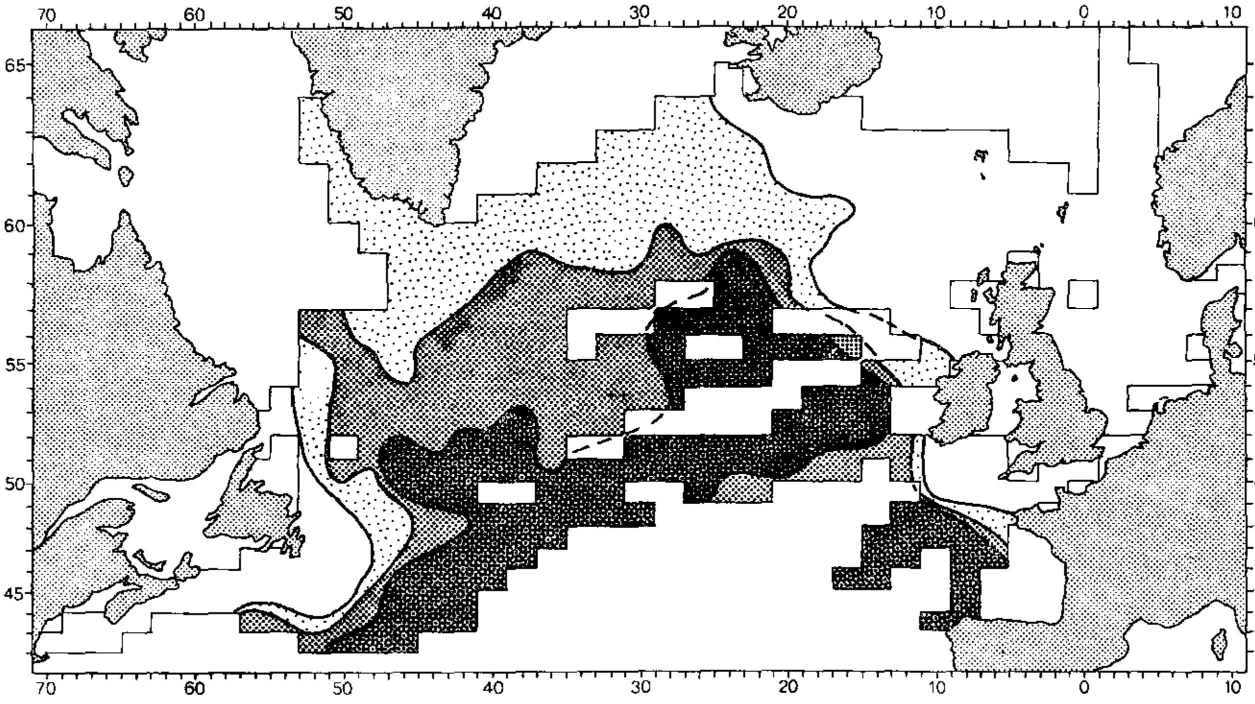


Fig. 15. Chart of the second component of geographical distribution.

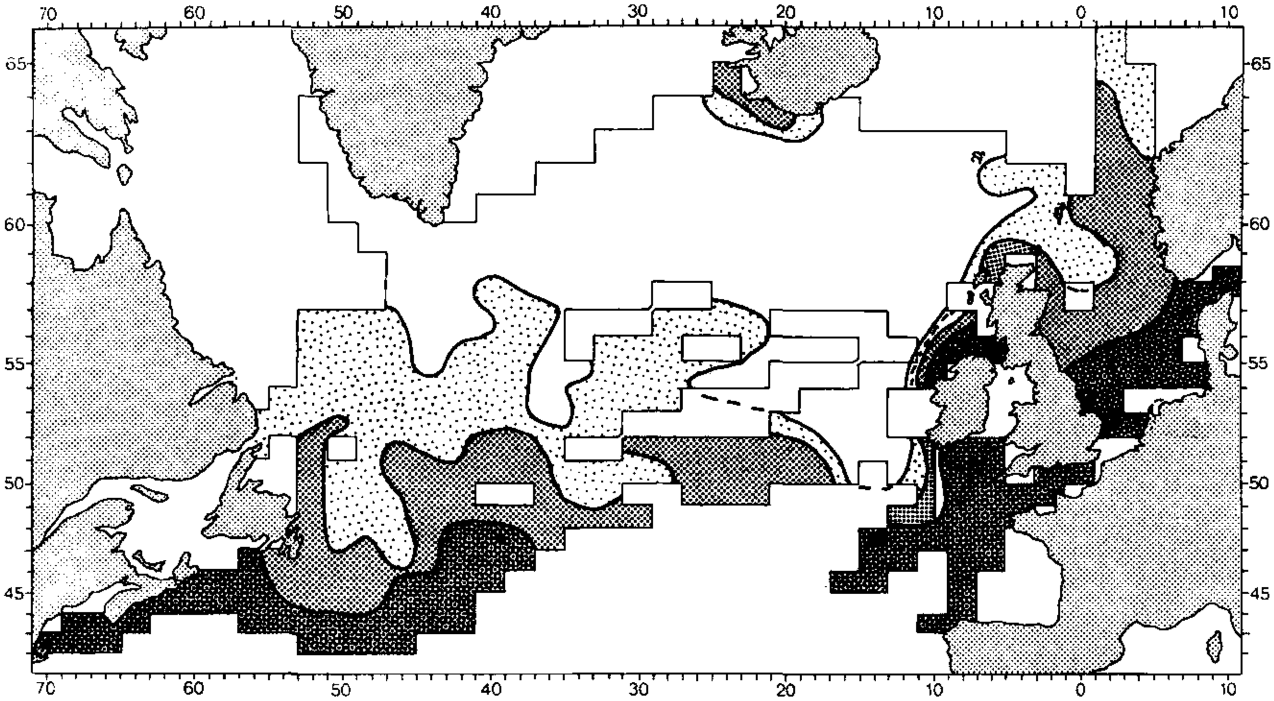


Fig. 16. Chart of the third component of geographical distribution.

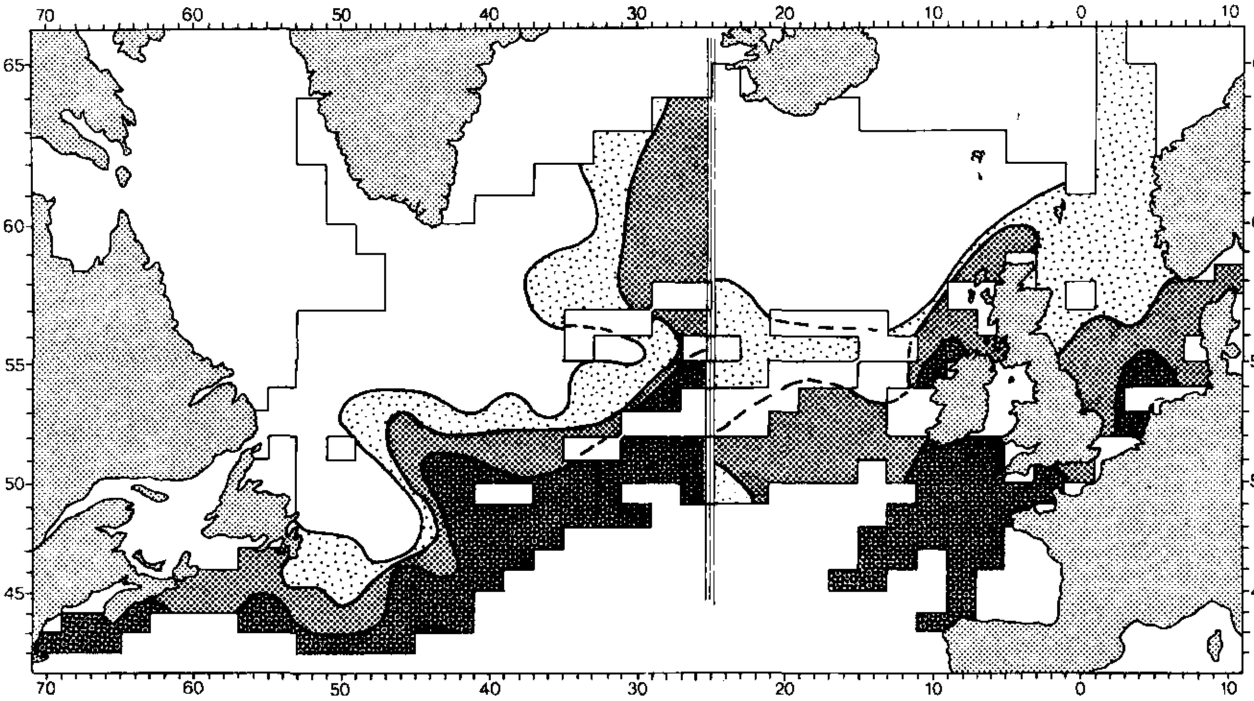


Fig. 17. Chart of the east and west area components corresponding to the third component.

running north and south at about 23°W. This pattern is not easy to interpret but it may conceivably be related to the timing of the seasonal temperature cycle.

The second component is shown in Fig. 15, indicating both north-south and oceanic-neritic differentiation and is very probably related to the North Atlantic drift system.

The third component is shown in Figure 16. This seems to be a composite pattern containing different elements in the east and west. Figure 17 shows charts of two components, based on separate analysis for the eastern and western areas, which when put together look very much like the third component for the whole area. For the eastern area analysis, the distribution is related to that of temperature, probably the distribution of surface temperature during the summer months. The chart for the west is the first component of the subarea analysis. There is no immediately obvious interpretation of this pattern.

This, very briefly, is the current state of knowledge of the zoogeography of the North Atlantic, as derived from the Continuous Plankton Recorder Survey. There are still many gaps to be filled in both with regard to classification of species and especially in the interpretation of the geographical patterns that have been found.

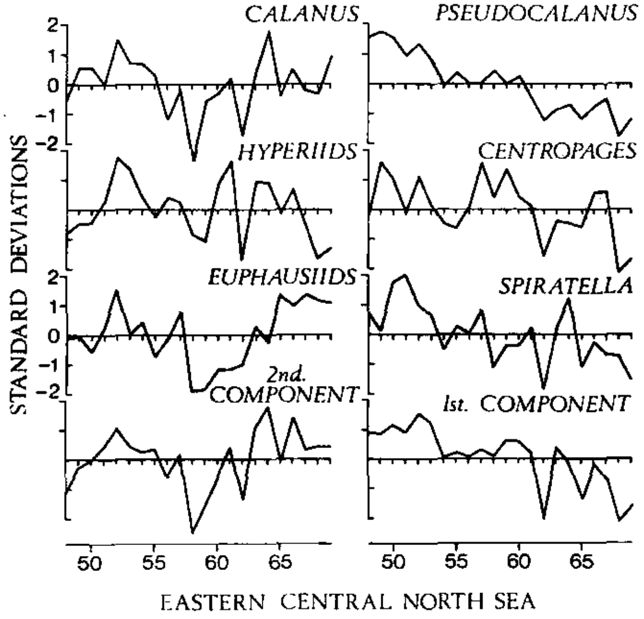


Fig. 18. **Graphs of annual fluctuations in abundance. Each graph is shown as a standardised variable about a mean of zero.**

HALF NORMAL PLOT, CORRELATIONS BETWEEN SPECIES: ANNUAL FLUCTUATIONS IN ABUNDANCE, EASTERN CENTRAL NORTH SEA

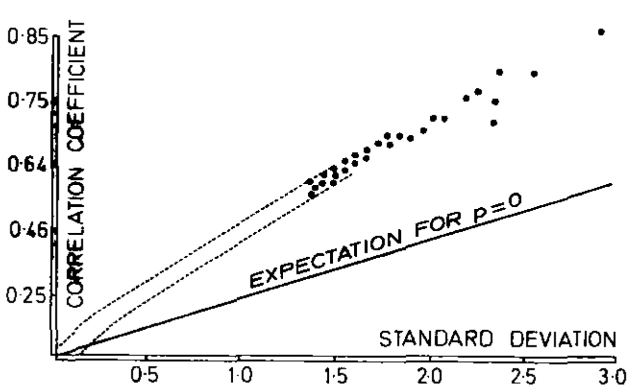


Fig. 19. **A half normal plot of the correlations between the annual fluctuations of 18 species in the eastern central North Sea.**

Annual Fluctuations in Abundance

The first point to make is that there is considerable coherence between the annual fluctuations in abundance of the species in any area. Figure 18 shows graphs of the annual fluctuations for six species or species groups in an area of the North Sea. The graphs within each column are clearly similar. This can be confirmed by principal components analysis: the two bottom graphs in Fig. 18 are the first and second components which are clearly similar to the graphs occurring above them. Data for a further 12 species are available for this area and most of them could be fitted into this figure in one of the two columns.

A method of illustrating the relationships between all the species is given in Fig. 19 which shows a half normal plot of the correlation coefficients between all possible pairs of species. If there were no relationships between the species the points should be along the line indicated in the figure. It can be seen however, that over the whole range of values and correlations fall well above this line, indicating the existence of relationships embracing just about all the species.

Using principal components analysis to extract the main elements in the annual fluctuation patterns, it is clear that there are considerable similarities between the annual fluctuations in different areas and also there are clear relationships between successive years.

To demonstrate these aspects, the results from 6 out of 12 possible areas have been selected, 3 from the

North Sea and 3 from the eastern Atlantic lying between 50° and 63°N. The first three components of each of these areas have been plotted. The set of first components are given in Fig. 20. Five out of the six areas show a clear downward linear trend in abundance, all six of the areas not shown in the figure also exhibit the same trend.

The existence of this marked common element over such a wide area covering warm and cold water oceanic areas as well as neritic waters is remarkable. Over and above the common trend there are similarities between the North Sea areas, C2 and D2, and also between the southern oceanic areas, C5 and D5. This differentiation between the oceanic and North Sea zones is supported by the other areas not shown in this figure.

Figure 21 shows the second components. These also show similarities within the oceanic and North Sea zones and all the areas except the southern North Sea (area D2) fit quadratic polynomials.

Figure 22 shows the third components. These show the same general pattern of relationships as the other components although the similarities are not so obvious. The components in the southern oceanic and North Sea areas show quite good fits to quartic polynomials.

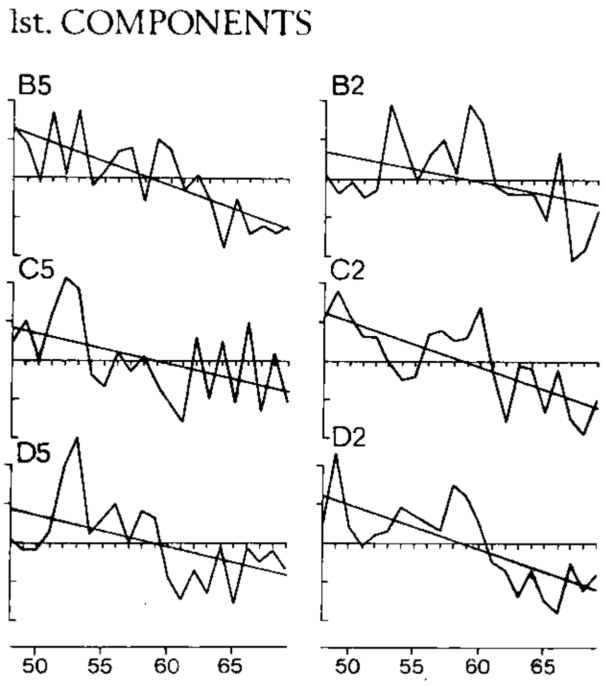


Fig. 20. Graphs of the first components of the annual fluctuations in abundance of the species occurring in six areas of the eastern North Atlantic. A chart of the areas is given in Fig. 2. Each graph is shown as a standardised variable about a mean of zero.

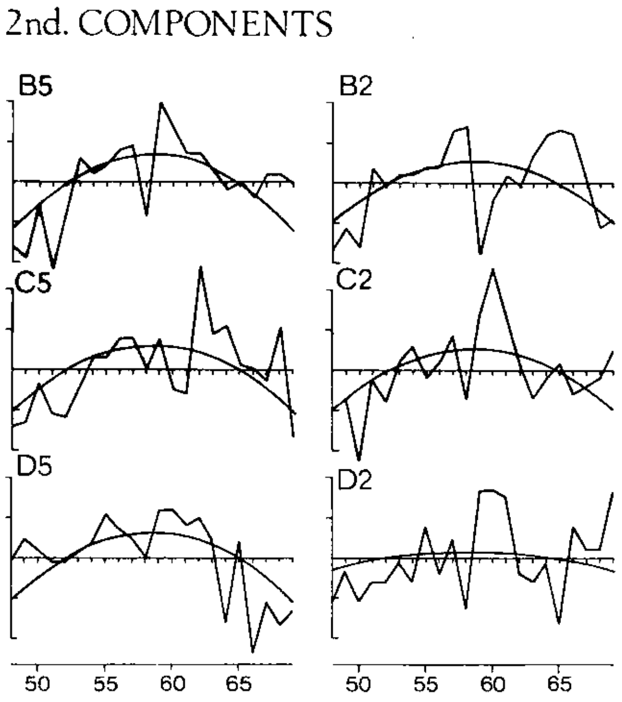


Fig. 21. Graphs of the second components of annual fluctuations in abundance in the same format as Fig. 20.

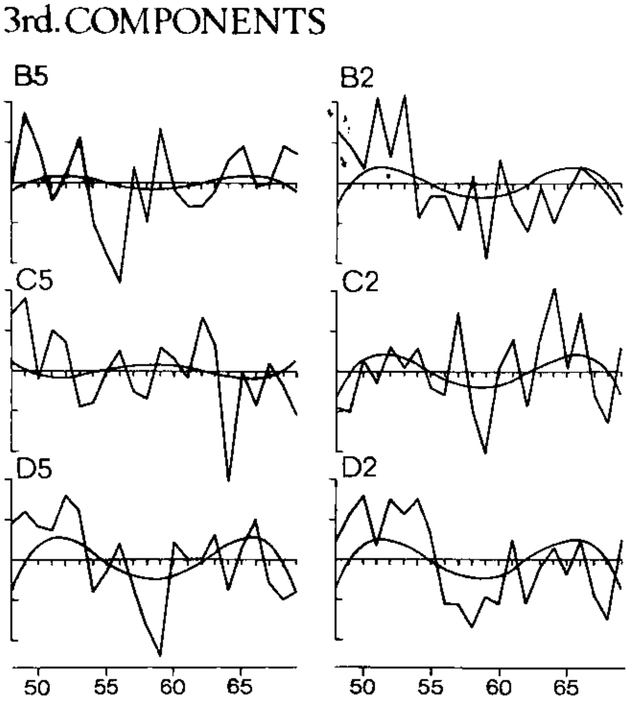


Fig. 22. Graphs of the third components of annual fluctuations in abundance in the same format as Fig. 20.

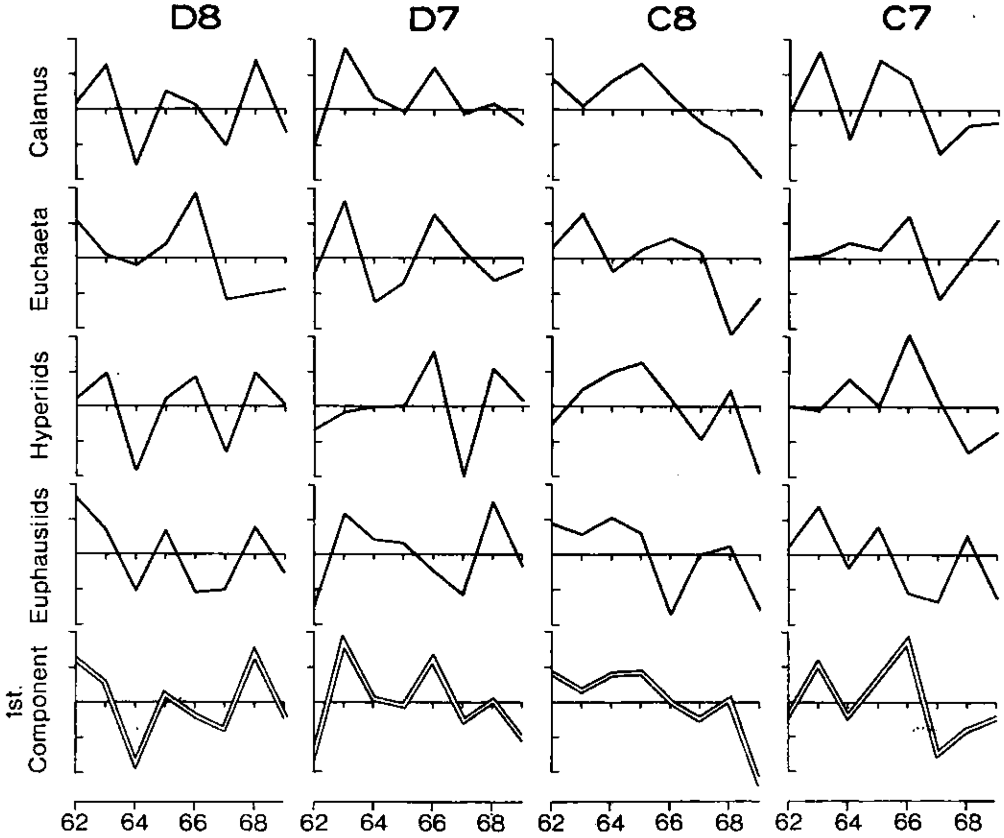


Fig. 23. Graphs of annual fluctuations in abundance for some western Atlantic areas. Each graph is shown as a standardised variable about a mean of zero. A chart showing the areas is given in Fig. 2.

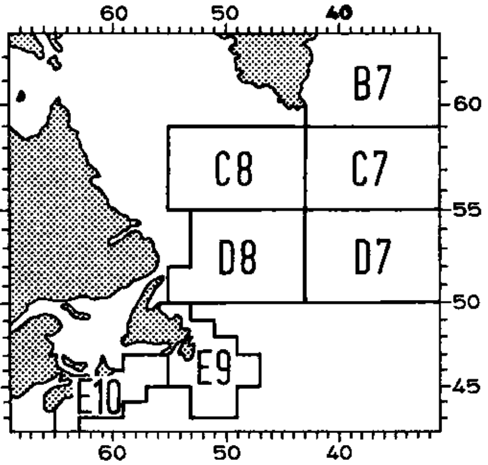
It is unwise to read too much into these fits to polynomials. This method of investigating the structure of the annual fluctuations was chosen because with a sequence of only 22 points the more conventional methods of time series analysis are difficult to apply. It cannot be claimed that there is any firm evidence for cyclical patterns of fluctuation. All that can be stated with any certainty is that there are relationships within sequences of years. It will obviously be interesting to see what happens in the next few years, particularly for the downward trend.

Very little progress has been made with interpreting these fluctuations in terms of relationships with environmental factors. The lack of progress is largely due to a lack of physical and chemical data on an equivalent scale.

Figure 23 contains a few examples of graphs of annual fluctuations in abundance for four areas of roughly 300 miles square in the western Atlantic (C7, C8, D7, and D8 in Fig. 2). The organisms are the copepods *Calanus finmarchicus* and *Euchaeta norvegica*, total Hyperiids and total Euphausiids. The bottom row of graphs show the first principal components in each area.

These graphs present, at first sight, a very different picture from the eastern area; there are some relationships between species, shown most clearly in the first column, but nothing like the common downward trend so obvious for the eastern areas. Figure 24 shows the correlation matrix for the first principal components for each of the areas shown in the chart. Nearly all the correlations are positive but only two indicate clear relationships.

This apparent lack of pattern is probably due almost entirely to the short run of years. The results look very much like those obtained from the first attempts at analysis of fluctuations in the North Sea when only 11 years data were available (Colebrook and Robinson, 1964).



	C7	C8	D7	D8	E9	E10
B7	3	2	2	$\bar{1}$	$\bar{2}$	0
C7		2	⑦	1	3	4
C8			3	2	3	0
D7				$\bar{1}$	$\bar{1}$	$\bar{2}$
D8					$\bar{2}$	⑦
E9						1

Fig. 24. The correlation matrix for the first principal components of annual fluctuations in abundance for the species occurring in each of the areas shown in the key chart. The correlations are given to one place of decimals multiplied by ten. A bar indicates a negative correlation.

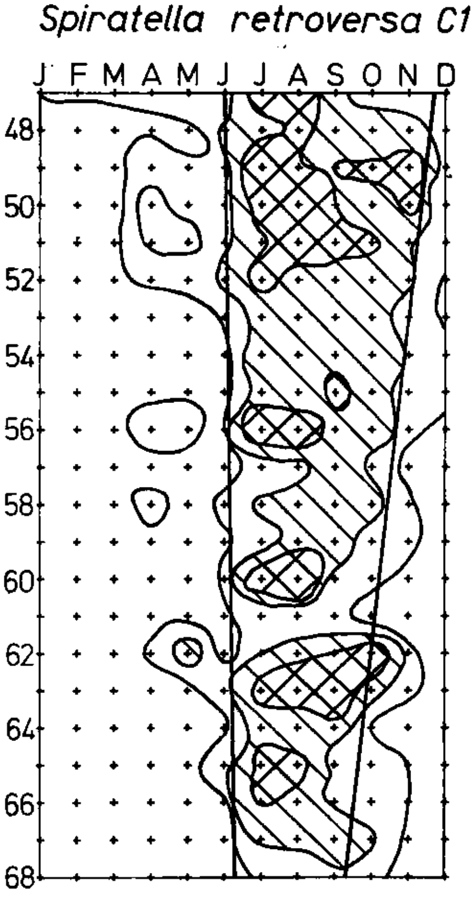


Fig. 25. A contour diagram of the annual and seasonal fluctuations in abundance of the thecosome *Spiratella retroversa* for the eastern central North Sea.

Annual Fluctuations in Timing

Diagrams demonstrating annual fluctuations in timing and season duration have recently been produced on a routine basis by computer methods. Some examples are given in Figs. 25-27, illustrating the main types of fluctuation that have been found. In Fig. 25 the thecosome *Spiratella retroversa* shows a clear downward trend in abundance and presumably associated with this is a marked progressive reduction in the duration of the seasonal cycle.

Figure 26 shows the results for the copepods *Temora longicornis* in the south western North Sea and *Acartia clausii* in the oceanic Atlantic west of Ireland. The seasonal cycle for *Temora* has become progressively later, amounting to a total of about 6 weeks over the 22 year period while in contrast to this, that for *Acartia* has got progressively earlier by 3-4 weeks. Figure 27 shows results for *Calanus*, in the western central North Sea (area C2) the timing of the spring increase has been remarkably stable and there is no change in season duration. In contrast to this, in the southern North Sea (area D2), the timing and season duration of *Calanus* were extremely variable. The difference between the two areas is probably due to differences in temperature regimes in the two areas. In the southern North Sea the water is vertically isothermal throughout the year while in the central North Sea there is a clear thermocline in the summer months.

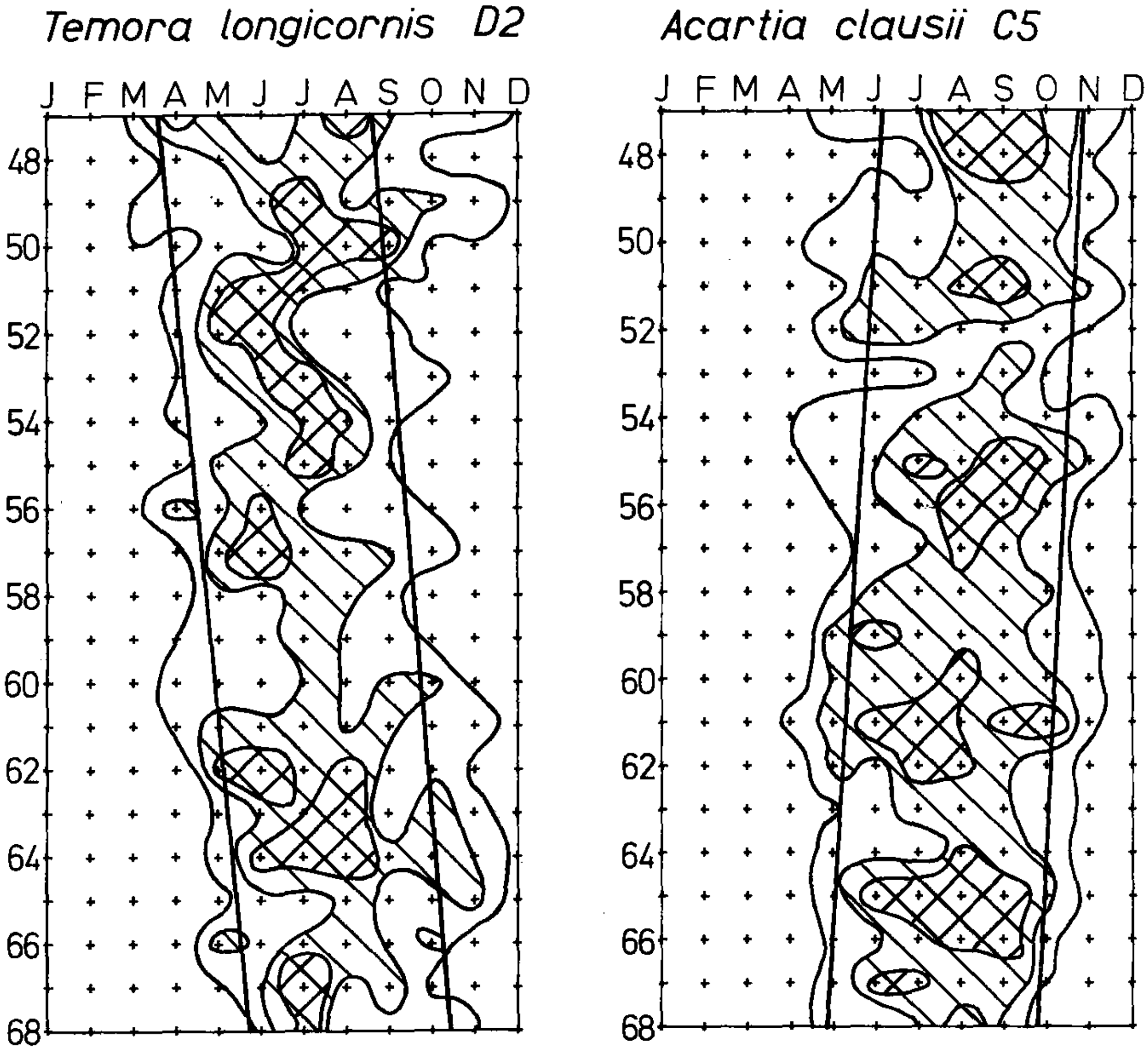


Fig. 26. Contour diagrams of the annual and seasonal fluctuations in abundance for the copepods *Temora longicornis* in the southwest North Sea and *Acartia clausii* in the Rockall area of the oceanic Atlantic.

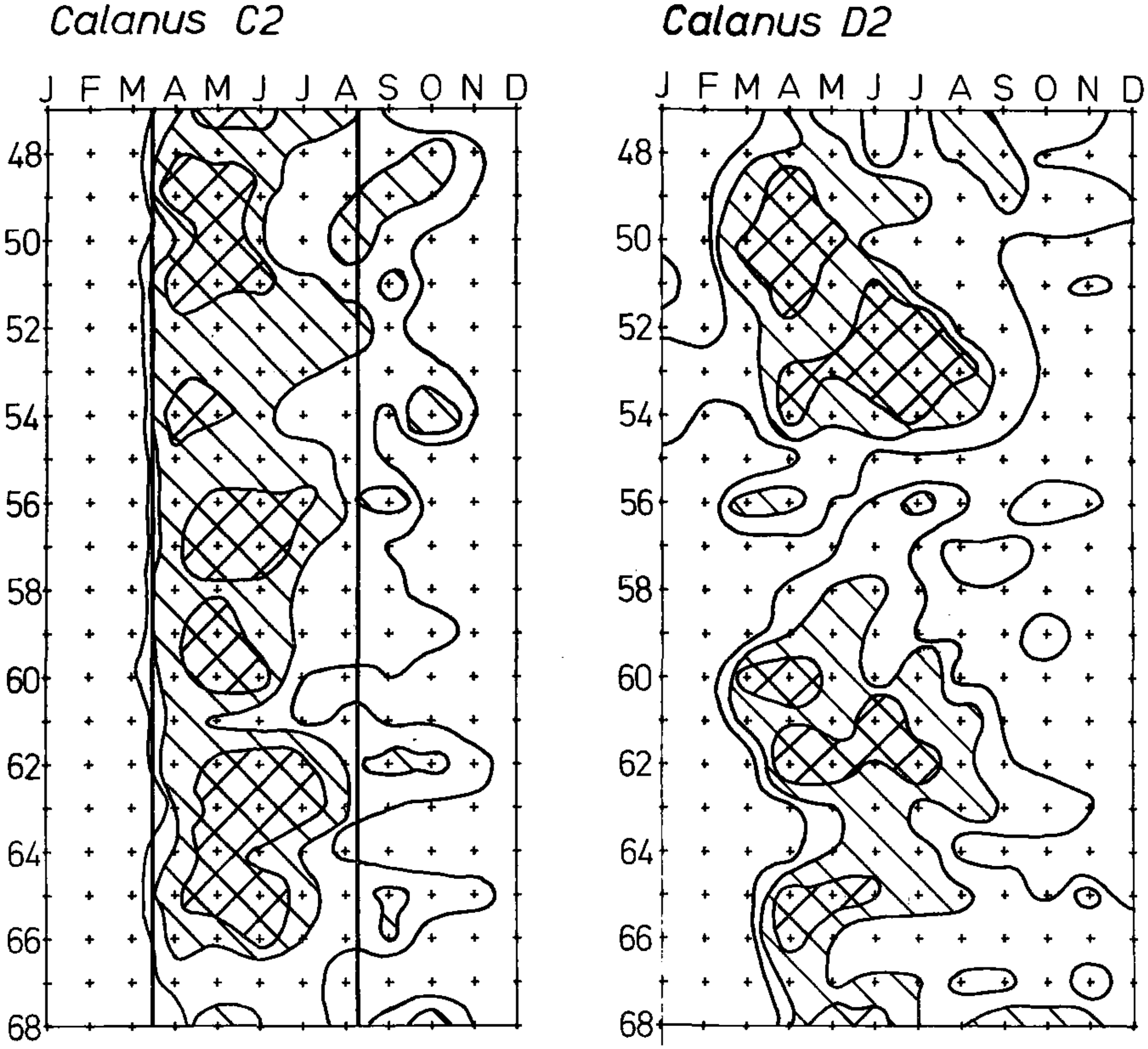


Fig. 27. Contour diagrams of the annual and seasonal fluctuations in abundance for the copepod *Calanus finmarchicus* in the western central and south western North Sea.

Conclusions

The results presented here illustrate the kinds of information that can be obtained from a long-term survey.

The nature of plankton populations lead to considerable difficulties in deriving descriptions of events and patterns of fluctuation. For the Continuous Plankton Recorder Survey these problems have, to a large extent, been solved and the processing and analysis of the survey data is quite successfully being reduced to a routine system.

As time goes by and more data are obtained, the quality of the descriptions can be expected to improve and further refinements in analysis should produce more informative statements about the patterns and relationships inherent in the system.

The next phase in this work must be to attempt, by empirical methods and by modelling, to interpret these descriptions of fluctuation in terms of the biology of the organisms and with respect to changes in their environment.

Acknowledgments

The author is very grateful to the captains and crews of the vessels which tow Plankton Recorders. Without their co-operation and support it would not be possible to run the survey.

The data presented in this paper are derived from the analysis of over 100,000 plankton samples. The skill and hard work of all those who have played a part in this mammoth task must be acknowledged.

The Continuous Plankton Recorder Survey is supported financially by the United Kingdom Natural Environment Research Council and by Contracts N62558-3612 and F61052-67C-0091 between the Office of Research, Department of the United States Navy, and the Scottish Marine Biological Association.

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COLEBROOK, J. M., and ROBINSON, G. A. 1964. Continuous plankton records: annual variations of abundance of plankton, 1948-1960. *Bull. mar. Ecol.*, 6: 52-69.

GLOVER, R. S. 1967. The continuous plankton recorder survey of the north Atlantic. *Symp. zool. Soc. Lond.*, 19: 189-210.

Discussion

Dr Grosslein: *"Would you care to comment on the numbers of fish eggs and larvae that you find on your recorder samples and whether or not you foresee any useful information about their distribution coming out of the recorder surveys."*

Dr Colebrook: *"The fish eggs and larvae are collected in the plankton recorder survey but are treated in a different manner from the rest of the data. They are all analyzed by one person as opposed to being a team effort. This means that the extent of the analysis is a bit more restricted. The numbers of young fish caught by the continuous plankton recorder are very few. They average about one young fish per sample. Nevertheless, we collect many samples, so that the answer to your question is yes, there is useful information on young fish and fish egg data. I do not have any of the information with me, however we have a number of clear relationships between numbers of young fish and the catch-per-unit-effort from fisheries."*

Dr Dickson: *"Do you think that the stability of the timing of major components of zooplankton production in the North Sea at the time when Robinson and yourself have shown a long-term delay in phytoplankton production in the North Sea has meant a decreased linking between the predator and the prey and in this case could it have been responsible for a decline in the abundance of zooplankton apart from any direct environmental change?"*

Dr Colebrook: *"No, I do not think so. The fluctuations in the timing of phytoplankton are there, but are quite small and barely detectable. The species which is most closely linked with the phytoplankton production cycle is Calanus. This is the main classical spring zooplankton, and yet Calanus is not one of those species which is showing a linear decline in abundance in the North Sea. I think that probably answers your question."*

Dr Dickson: *"Yes, it certainly does. But there was another one; it would be a bit unfair to go on to discuss phytoplankton, since you have not been talking about phytoplankton, but there is one thing that has confused me slightly. I should have expected the timing of the peak of phytoplankton production to be related to the rate of development of phytoplankton production in any given set of years. Would you say this is true?"*

Dr Colebrook: *"No, I do not think this is true. We have the feeling that this is an over-simplification. We feel that the key factor in the phytoplankton is the timing of the spring increase. What happens after this doesn't matter all that much."*

Dr Dickson: *"But does it not depend on what you mean by timing because Robinson, as I say, measures the timing of the center of gravity of the production "bump", in other works, the timing of peak primary production in the North Sea, and finds no delay from year to year. Yet when you take the change from year to year in the crossover time which you have done more recently, in other words, the time at which the standing crop rises above the mean for the whole production period Robinson found no delay and you found a delay. I would have thought that the two – the rate of development and the timing of the peak – would have been related."*

Dr Colebrook: *"I think this is really a function of the quality of the data to be quite honest. This form of chart with the month-year graphical presentation, as I pointed out, is getting very close to the limits of what we can usually expect of this data."*

Dr Dickson: *"And does it not alarm you, that if you should take one method of estimating timing, you find a delay and, yet with another method of timing, you find no delay?"*

Dr Colebrook: *"It doesn't alarm me all that much now."*

Dr Dickie: *"The question I wanted to ask you is whether, within the species groups as geographic groups, there are major differences in the size of the species concerned. I recall that, within this general period of decline you were talking about, Steele (J. H. Steele. 1965. Some problems in the study of marine resources. Spec. Publ. int. Comm. Northw. Atlant. Fish. No. 6, p. 463-476.), a few years ago, postulated that there has been a change in the species* *composition of the main groups such that the latter groups were composed of smaller animals. This might suggest that it would be interesting to look at the production rather than the biomass. Under such conditions, production may be maintained despite drops in overall density."*

Dr Colebrook: *"This particular work that Dr Dickie is talking about was, in fact, based on the results for a rather small area of the North Sea. Unfortunately, I do not think this relationship holds for the wider area. The composition of the geographical groups that I showed is really most peculiar. There does not seem to be any community structure as such with these groups. One of them consists entirely of phytoplankton species. Others consist predominantly of zooplankton species and so on. There certainly is no clear indication of any trophic relationship between the species within any group. It is a most confusing picture. With regard to the size of the species involved, one of the species which shows the decline in abundance most clearly is the dominant species of small copepod, Pseudocalanus elongatus, whereas Calanus and Euphausiids do not show this decline to anything like the same extent. The patterns one obtains are clear enough, but how one can interpret these fluctuations in terms of community structure within the plankton, is not at all clear."*

Dr K. Mann: *"I would like to ask a broad question, Mr Chairman. Parsons and Anderson have a fairly well-known program in the Canadian Pacific area in which they study the water chemistry, temperature conditions, and development of phytoplankton and so on from ships of opportunity – in other words, they put a technician on board who does routine determinations on a continuous flow of water through the vessel. They are interested in trying to understand the dynamics of phytoplankton production and, as far as I know, there is no program for plotting of distribution of consumer organisms as in the Atlantic. On the other hand, as far as I know, there is not in the North Atlantic any study of the dynamics of phytoplankton production. I would like to ask the speaker whether it might be a good idea to consider doing the kind of thing that Parsons and Anderson are doing in the Pacific in order to elucidate some of the mechanisms underlying the patterns of distribution of consumer organisms or alternatively, whether use of a more sophisticated towed body might serve the same purpose."*

Dr Colebrook: *"We are planning a more sophisticated towed body which we hope will undulate between 10 and 100 m. There are numerous problems but we are making some progress. In this towed body, hopefully, we will be mounting a number of sensors, in addition to a plankton sampler similar to the one we are using now. Again, hopefully, one of the sensors will be for chlorophyll. This will not give us production figures as such but it will give us a much clearer idea of fluctuations in the standing crop of phytoplankton at least. This is still very much a gleam in somebody's eye at the moment. There are a number of problems in measuring chlorophyll underway in situ but we hope that it can be solved. Meanwhile, we have just started a program of routine C_{14} observations from one of the eastern Atlantic weather ship stations. We are planning to extend it to more stations within the near future as staff and money become available."*

Dr Campbell: *"Dr Colebrook, I have just one question I would like to ask you more out of curiosity than anything else. Do you find particulate carbon matter on the silks of the plankton recorders after towing? I am wondering whether there is any occurrence of petroleum byproducts or residues on the silk screens."*

Dr Colebrook: *"The recorder silks have a fairly coarse mesh – 60 meshes to the inch. At the time of the Torrey Canyon incident we kept a close look-out for petroleum residues in the one plankton record which passed near the scene of the incident and, in fact, we did get them. But this is the only occasion on which we have knowingly picked up petroleum residues. There are often fat globules found lying on the silks but these are almost always excreted from copepods and it is not easy to differentiate between oil from a copepod and oil from any other source."*

Dr Hassan: *"This is just a short comment and it concerns the more sophisticated towed body referred to by Dr Colebrook. There is such a device in a rather advanced stage of development in the Bedford Institute. It has been successful for a number of initial tests. You might be interested to look at it."*

Dr Colebrook: *"Is this for chlorophyll?"*

Dr Hassan: *"No, this device is designed to undulate in the water and has fitted sensors, depending on what you want to measure."*

Dr Colebrook: *"I would be very interested in seeing it."*

Contribution Number 9

No Discussion

The Chemical Bases and Mechanisms of Spring Phytoplankton Blooming in the North and Northwest Atlantic

By V. S. Zlobin[1]

Abstract

In the northern Atlantic Ocean cyclic processes in the production of primary protein occur annually. Depending upon a number of environmental factors, particularly water temperature and illumination intensity, the maximum development of the microphyte biomass is observed in March-April in the western part of the area and in May-June in its eastern parts. In the central part of North Atlantic water mass, blooming of phytoplankton develops with time in a latitudinal direction. Oxygen and phosphates were taken as indicators of the microphyte vegetation. According to their concentrations during our observations, and their winter concentrations, one can determine the phytoplankton production if the constants of rates of reactions and temperature in the photosynthetic layer are known. Our observations in the Norwegian Sea enabled us to calculate the energy of activation according to Arrhenius $E_a = 28484 \pm 1232$ *cal. mol (n = 6), the coefficient of VantHoff* $\gamma_{10} = 2.308$ *for enzymatic reactions of the addition and splitting off the monophosphates and polyphosphates, and also constants which characterize the rate of consumption of phosphates by phytoplankton and the production of oxygen by phytoplankton. On the basis of these parameters, conditions for location of origination of blooms, and specific and quantitative composition of microphytes are analyzed, and the formula for the closest possible packing of cells and their biomass, depending on sizes and their electric charge are given. Calculated values are compared with those actually observed in nature.*

The paper deals with the peculiarities of oxygen production by phytoplankton at diurnal stations in Davis Strait, in the Labrador and North Atlantic currents. The paper also describes some mechanisms of the functioning of the organelles of the cells of green plants.

The concluding part of the paper discusses practical aspects connected with the forecasting of biomass and future utilization of the phytoplankton biomass.

Introduction

The problem of the chemical basis of synthesis of primary protein and of the assessment of primary production by measuring uptake of elements of the biogenic cycle is not a new one. As early as the 1920's and especially in the 1930's Atkins (1926, 1928), Atkins and Harvey (1926), Harvey (1925, 1926, 1933), and Cooper (1933, 1935) developed and used in field conditions methods for the determination of primary production, based on seasonal changes in concentrations of phosphates and other nutritional salts.

Such an attempt was made for the Barents Sea by Kreps and Verzhbinskaya (1930) and by Voronkov (1941). However, Cooper (1958) suggested that calculations of primary production from the concentration of phosphates in the sea water could not be applied to the English Channel due to arbitrariness in specification of initial values in autumn, and due to horizontal irregularity in the distribution of phosphates. In our opinion, the potentialities of this method have not yet

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been exhausted. The papers by Riley (1941) and Harvey (1945) are particularly relevant.

Gran and Ruud (1926), Pütter (1924), Gaarder (1932), Gaarder and Gran (1927), Bruevich (1936), Vinberg and Yarovitsina (1939), Vinberg (1960), and other authors suggested a method of determining the primary production from oxygen content. In connection with this, Petterson (1938) studied underwater illumination with light from different angles of incidence. Marshall and Orr (1928) from the Plymouth Laboratory tried to examine some functional properties of phytoplankton cells at different depths. They found that in the daytime photosynthesis near the surface is sharply depressed. Maxima at 0.5 m were observed at 9 o'clock in the morning and at 6 o'clock in the evening; at depths of 4 and 6 m only one maximum was found in the middle of the day. Later, Jenkin (1937) showed that the photosynthesis is proportional to the total energy of radiation only above the threshold value, 0.03 $\text{cal.cm}^{-2}.\text{min.}^{-1}$; depression was marked above 0.16 $\text{cal.cm}^{-2}.\text{min.}^{-1}$. The compensation point was 0.022 $\text{cal.cm}^{-2}.\text{min.}^{-1}$ or 360 lux.

Based on the equation for the photosynthetic reaction, the threshold value for radiation, and the number of chlorophyll molecules which participate in formation of one molecule of glucose, Zlobin *et al.* (1970) calculated the depth of the photic layer (120-130 m) on the sections at 65°45'N and 67°30'N in the Norwegian Sea.

Improvement in techniques for study of mechanisms of nutrient uptake and photosynthesis in marine phytoplankton depended on the development of isotopic methods, particularly those using radiocarbon compounds (Steemann Nielsen, 1951, 1952, 1955, 1958; Sorokin and Kozlyaninov, 1957; Sorokin, 1958; Zeitzschel, 1969). In these papers one can see a relation between illumination and liberation of oxygen, and also find both evidence and some causal explanation for the existence of intrinsic rhythms in phytoplankton function.

In their experiments with the marine alga, *Gonyaulax polyedra*, Hastings and Sweeney (1957) discovered from pulses of bioluminescence that the alga mentioned has a rhythm which is relatively insensitive to changes of temperature; the coefficient γ_{10} in all the experiments was less than 1. However, under the influence of temperature a phase shift is possible (Sweeney and Hastings, 1964).

As known, the strongest stimulus for plants is light. Phase shifts of rhythm, for example in *Gonyaulax*, *Euglena* and in some other algae, take place if radiation occurs at night or if cells are irradiated by light of 475 and 650 mμ wavelength (Wilkins, 1964; Hastings and Sweeney, 1958, 1960). Hastings (1964) observed diurnal changes in content of chlorophyll and caratinoids. He considered these changes to be caused by the appearance of enzyme or co-enzyme that restricts the rate of the reaction in the dark. The analysis showed that pH increases during the period of daylight and decreases at night. Changes in pH are connected with the exogenous rhythm. Based on observations of stability of rhythmically active systems under changing physical and chemical parameters of the environment, Bünning (1936) and Ehret (1959) supposed that metabolism of nucleic acids is controlled by the biological clock.

Thus, there is an obvious dynamic interrelationship between uptake of phosphates (forming a part of nucleic acids) by phytoplankton and liberation of oxygen by phytoplankton in photosynthesis. Constant rates of reaction are external evidence of this relation. As is already known, in fermentative reactions, rate constants depend on concentrations of homogeneous catalysts-enzymes. In most cases catalytic action of enzyme is connected with the formation of an intermediate enzyme-substrate complex which later forms a reaction product either monomolecularly or with the participation of a molecule of another substrate (Emanuel and Knorre, 1969). If any reaction product

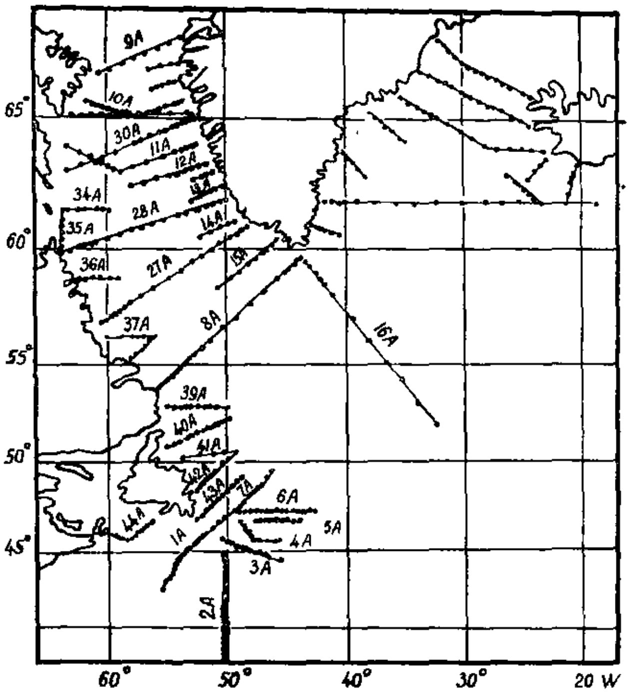

Fig. 1. Main hydrographic sections in the Northwest Atlantic.

influences the reaction catalytically, then the reaction is said to be autocatalytic.

We have tried to show that uptake of phosphates by cells of phytoplankton is an autocatalytic process and have obtained successful results in description and prediction of seasonal changes actually observed in the Norwegian Sea (Zlobin, 1969, 1970).

Remoteness of Davis Strait and Great Newfoundland Bank means that it is difficult to make monthly hydrochemical observations, and consequently, to predict the depletion of biogenic elements. The most complete data were collected in these areas in 1965 on board the PINRO research vessels *Topseda*, *Sevastopol*, *Novorossiysk* (Zlobin *et al.*, 1968) and in 1970 on board the R/V *Protsion* (Tables 1-6). Data given in these tables enable us to calculate some constants for Section 8-A (Fig. 1) at 0 m. Rate of depletion of phosphates during the intensive blooming of phytoplankton was 1.64×10^{-7} mg-At. l^{-1}.sec^{-1} and of regeneration in autumn was 3.48×10^{-8} mg-At. l^{-1}.sec^{-1}. Thus, the rate of oversaturation of water masses with oxygen in spring and summer was 4.74×10^{-6}%.sec^{-1}, and the loss in autumn 1.97×10^{-7}%.sec^{-1}. The rate of heating of surface water was (2.14×10^{-7})°C.sec^{-1}, and that of cooling was (7.18×10^{-7})°C.sec^{-1}.

Using average indices of phosphate loss on Section 8-A in 1965 and 1970 according to the formula given by Yatsimirsky (1967), the activation energy of intermediate complexes which are formed with uptake of phosphates was calculated (after G. Arrhenius).

$$lg\frac{0.0069}{0.0142} = \frac{3.50 \times Ea}{4.576 \times 274.71 \times 278.81} \qquad Ea = 31,325 \text{ cal. mole}^{-1}$$

This value is close to that determined for the Norwegian Sea, Ea = 28,484 ± 1,232 cal. mole^{-1} ($n = 6$), and to activation energy of monoanions which are formed in photosynthesis. Thus, for glucose-1-phosphate Ea = 30,000 cal. mole^{-1}, for glycero-1-phosphate Ea = 29,900 cal. mole^{-1}, and for pyrophosphates Ea ranges between 21,900-28,100 cal. mole^{-1} (Kosover, 1964). The coefficient of VantHoff calculated on the basis of field observations is equal to $\gamma_{10} = 2.308$.

The reaction rate for addition of phosphates to protein complexes may characterize the spring blooming of phytoplankton. For example, if one scales rates of loss of phosphate using γ_{10} to the temperature of 23° recorded by Pincemin (1969*a*) during a bloom of *Cochlodinium* sp., one can see that in 1970 the rate of loss was 7.92×10^{-7} mg-At.l^{-1}.sec^{-1} in Davis Strait and in the location of blooming of *Cochlodinium* sp., it was 7.5×10^{-7} mg-At.l^{-1}.sec^{-1}.

TABLE 1. Average and weighted average concentrations of phosphorus on some sections of Davis Strait and Great Newfoundland Bank in the spring-summer period.

Section:	8-A		6-A		7-A		8-A		8-A		28-A	
	3 February 1970		18-22 May 1970		16-18 May 1970		1-5 May 1970		11 December 1970		23-25 April 1970	
Depth (m)	n	Pμg-At.l^{-1} ± σ	n	Pμg-At.l^{-1} ± σ	n	Pμg-At.l^{-1} ± σ	*n*	Pμg-At.l^{-1} ± σ	n	Pμg-At.l^{-1} ± σ	n	Pμg-At.l^{-1} ± σ
0	2	0.94	27	0.22±0.10	15	0.27±0.12	14	0.81±0.07	5	0.24±0.14	8	0.73±0.04
10	2	0.92	27	0.21±0.09	15	0.28±0.12	14	0.81±0.07	5	0.26±0.13	8	0.73±0.04
20	2	0.90	27	0.23±0.10	15	0.29±0.13	14	0.82±0.07	5	0.38±0.20	8	0.80±0.12
30	–	–	27	0.29±0.12	15	0.39±0.18	14	0.84±0.07	5	0.56±0.14	8	0.79±0.07
50	2	0.99	27	0.55±0.17	15	0.63±0.18	14	0.91±0.07	5	0.75±0.11	8	0.84±0.05
100	1	0.84	22	0.92±0.12	11	0.90±0.11	14	0.97±0.07	5	0.90±0.19	8	0.91±0.10
200	2	1.04	17	1.12±0.09	11	1.08±0.16	14	1.07±0.09	5	1.00±0.10	8	1.02±0.12
300	2	1.04	14	1.11±0.10	7	1.11±0.13	14	1.08±0.09	5	1.04±0.11	8	1.02±0.19
400	2	1.00	10	1.10±0.09	6	1.16±0.06	14	1.11±0.07	5	1.09±0.13	8	1.10±0.09
500	2	1.04	10	1.03±0.16	5	1.15±0.06	14	1.14±0.08	5	1.08±0.10	8	1.12±0.08
600	2	1.08	10	1.10±0.04	4	1.14±0.05	13	1.12±0.07	4	1.08±0.10	8	1.11±0.10
800	1	1.07	8	1.12±0.06	5	1.15±0.06	12	1.13±0.06	4	1.08±0.10	8	1.17±0.08
1000	1	1.07	7	1.18±0.19	4	1.17±0.06	12	1.14±0.06	4	1.09±0.07	8	1.16±0.08
1200		–	5	1.16±0.06	4	1.21±0.08	11	1.15±0.07	4	1.09±0.11	8	1.16±0.09
1500		–	4	1.16±0.05	4	1.22±0.06	11	1.17±0.09	3	1.12±0.13	8	1.18±0.08
2000		–	4	1.20±0.07	3	1.28±0.09	10	1.20±0.08	3	1.21±0.10	8	1.23±0.07
0-20		0.93	–	0.22±0.10	–	0.28±0.12	–	0.81±0.07	–	0.28±0.16	–	0.78±0.07
0-50	-	0.92	–	0.31±0.12	–	0.38±0.15	–	0.84±0.07	–	0.47±0.15	–	0.83±0.07
0-100	-	0.93	–	0.52±0.13	–	0.57±0.15	–	0.89±0.07	–	0.65±0.15	–	0.90±0.09
0-200		0.93	-	0.77±0.12	–	0.78±0.14	–	0.96±0.07	–	0.80±0.15	–	1.13±0.13
200-500	--	1.03	–	1.10+0.10	–	1.13+0.10	–	1.10+0.08	–	1.06+0.11	–	1.15+0.09
500-1000	-	1.04	–	1.12+0.09	–	1.15+0.06	–	1.13+0.06	–	1.08+0.09	–	1.19+0.08
1000-2000	–	1.08	–	1.17+0.07	–	1.23+0.07	–	1.17+0.08	–	1.13+0.11	–	

TABLE 2. **Average and weighted-average indices of a relative saturation with oxygen (O_2) of Davis Strait and Great Newfoundland Bank water masses in the spring – summer period.**

Section:	8-A 3 February 1970		6-A 18-22 May 1970		7-A 16-18 May 1970		8-A 1-5 May 1970		8-A 11-12 June 1970	
Depth (m)	n	O_2%	n	O_2% ± σ	n	O_2% ± σ	n	O_2% ± σ	n	O_2% ± σ
0	2	93.6	27	110.4±3.2	15	111.9±4.5	14	100.8±2.9	5	117.2±3.6
10	2	95.2	27	109.2±4.6	15	110.7±4.1	14	100.3±2.9	5	115.6±3.7
20	2	95.6	27	108.2±3.5	15	110.9±4.2	14	100.2±2.8	5	111.6±7.9
30	–	–	27	106.5±4.0	15	107.1±5.5	14	99.3±4.2	5	106.3±6.7
50	2	95.8	27	100.2±4.1	15	99.8±5.1	14	98.2±4.4	5	97.3±2.9
100	1	92.9	24	86.4±6.3	10	90.6±3.0	14	96.0±4.3	5	95.8±4.5
200	2	93.2	16	84.1±6.8	10	86.2±3.6	14	93.7±3.9	5	94.8±2.0
300	2	92.4	14	85.7±4.7	7	90.4±5.1	14	92.6±4.2	5	93.0±2.4
400	2	92.6	10	87.9±2.1	6	88.6±2.6	14	92.0±3.1	5	90.6±2.3
500	2	91.8	10	90.0±2.8	5	90.9±0.8	14	90.0±2.3	5	90.9±1.0
600	2	92.1	10	90.2±1.4	5	89.8±0.6	13	90.3±1.6	4	92.1±0.9
800	–	–	8	90.4±0.6	5	90.6±0.5	12	90.0±1.7	4	90.4±0.9
1000	–	–	7	89.9±1.5	4	90.9±0.4	12	90.3±1.8	4	91.2±0.9
1200	–	–	5	88.8±0.9	4	89.8±0.5	10	90.2±1.7	4	91.3±0.5
1500	–	–	4	88.1±0.8	4	87.9±0.2	11	88.8±1.7	2	88.2
2000	–	–	4	86.1±0.5	3	86.9±0.1	8	87.9±1.3	2	87.0
0-20	–	94.4	–	109.2±3.9	–	111.1±4.2	–	100.4±2.9	–	115.0±4.7
0-50	–	95.0	–	106.5±3.9	–	107.6±4.8	–	99.6±3.6	–	108.5±5.2
0-100	–	95.4	–	99.9±4.6	–	101.4±4.4	–	98.4±4.0	–	102.5±4.5
0-200	–	94.5	–	92.6±5.6	–	94.9±3.8	–	96.6±4.0	–	98.9±3.9
200-500	–	92.6	–	86.9±3.9	–	89.2±3.3	–	92.2±3.5	–	92.2±1.9
500-1000	–	92.1	–	90.2±1.2	–	90.5±0.5	–	90.2±1.8	–	91.1±0.9
1000-2000	–	–	–	88.0±0.8	–	88.4±0.3	–	89.1±1.6	–	89.0

During the 'red tide', the concentration of cells of phytoplankton ranged from 2-6 million cells.l^{-1}. to 30 million cells.l^{-1}. and even to 80 million cells.l^{-1}. (Pincemin, 1969*b*). Under such packing the distance between cells is very small and unless cells possess an electric charge (Bogorov, 1967), they will inevitably form aggregates. The same thing occurs, for example, in old cultures and was observed in an intense bloom of *Trichodesmium* in Laccadives (Quasim, 1970).

Taking into consideration the Coulomb interaction forces, in a model where cells are located at the vertices of a cube, the following condition must apply:

$$D \geqslant R^2 \tag{1}$$

where R = the radius of a cell, D = the distance between cells. Then for the k^{th} and $(k + 1)^{th}$ cells, we can write:

$$D = \sqrt{(x_{k+1} - x_k)^2 + (y_{k+1} - y_k)^2 + (z_{k+1} - z_k)^2} \geqslant 2R^2 \tag{2}$$

To be conservative, we take $r = 10\mu$ *(Chaetoceros debilis)*. Then, in our model the cube cuts off 1/8 part of the volume of each cell, but as the cube has 8 vertices, each cube corresponds to one cell of phytoplankton. Accroding to the condition (1) the minimum side of the cube is:

$$a = r + 2r^2 + r = 2r\,(r + 1) \tag{3}$$

and if $r = 10\mu$, $a = 220\mu$, and the volume $v = a^3(220\mu)^3 = 10{,}648{,}000\mu^3$. Therefore, in 1 litre of sea water the maximum number of cells having $r = 10\mu$ is 93,914,400 and the biomass, amounting to 0.01-0.015% of the total mass of water, is equal to 0.1-0.15 $g.l^{-1}$.

When the size of cells increases, the maximum number of cells that can be contained in 1 litre of sea water decreases and it amounts to 7,538 cells.l^{-1} for *Rhizosolenia styliformis* (the volume of $452{,}154\mu^3$ and the nominal diameter is 100μ) and the biomass is 0.0034 $g.l^{-1}$ that is, 44 times lower than that for *Chaetoceros debilis*.

In phytoplankton blooms there occur specimens of different species, and their distribution in space is random. Let a ce-1 of an arbitrary form have volume 'v' and surface 'A', then D_c (diameter of a sphere with similar volume) can be obtained from the following equation.:

$$D_c = \frac{\sqrt[3]{6v}}{\pi}$$

Let us examine the sum total of cells having an identical form but different diameters,

$$D_{p,N} \text{ and } D_{c,p} = \frac{\Sigma D_p}{N}.$$

If the linear dimension of cells are random variates, then the law of the distribution of these values, and consequently that of their diameters, will be Gaussian.

If we define 'x' equal to deviation of diameters of

TABLE 3. Average and weighted average values of temperature (t°) on some sections of Davis Strait and Great Newfoundland Bank in the spring-summer period.

Section:	8-A 3 February 1970		6-A 18-22 May 1970		7-A 16-18 May 1970		8-A 1-5 May 1970		8-A 11-12 June 1970	
Depth (m)	n	t°	n	$t^\circ \pm \sigma$	n	$t^\circ \pm \sigma$	n	$t^\circ \pm \sigma$	n	$t^\circ \pm \sigma$
0	2	-1.12	27	5.47±1.82	15	2.60±1.48	14	1.34±1.78	5	2.08±1.60
10	2	-0.56	27	4.41±1.55	15	2.57±1.50	14	1.34±1.76	5	1.94±1.17
20	2	0.01	27	3.98±1.35	15	2.36±1.47	14	1.44±1.69	5	1.62±1.47
30	–	–	27	3.48±1.84	15	1.90±1.82	14	1.54±1.62	5	1.15±1.50
50	2	1.84	27	2.72±2.27	15	1.04±1.99	14	1.68±1.48	5	0.97±1.26
100	1	0.39	24	2.45±1.96	11	1.68±1.99	14	2.42±1.34	5	2.19±0.64
200	2	3.78	16	4.17±0.87	11	2.90±1.51	14	3.45±0.62	5	3.41±0.37
300	2	4.48	14	4.39±0.39	7	4.34±0.16	14	4.08±0.44	5	3.90±0.08
400	2	4.70	10	4.28±0.31	6	4.32±0.10	14	4.20±0.38	5	4.19±0.09
500	2	4.14	10	4.11±0.18	5	4.15±0.12	14	4.24±0.28	5	4.20±0.14
600	2	4.00	10	4.02±0.17	5	4.06±0.10	13	4.08±0.34	4	4.02±0.13
800	1	3.65	8	3.87±0.13	5	3.91±0.07	12	3.94±0.13	4	4.00±0.15
1000	1	3.33	7	3.75±0.11	4	3.78±0.11	12	3.76±0.11	4	3.79±0.16
1200	–	–	5	3.76±0.10	4	3.71±0.05	11	3.66±0.13	4	3.69±0.10
1500	–	–	4	3.61±0.08	4	3.63±0.08	11	3.63±0.13	3	3.68±0.02
2000	–	–	4	3.30±0.13	3	3.24±0.11	10	3.45±0.25	3	3.33±0.15
0-20	–	-0.84	–	4.57±1.57	–	2.52±1.49	–	1.36±1.75	–	1.90±1.35
0-50	–	-0.50	–	3.81±1.77	–	2.02±1.69	–	1.49±1.65	–	1.46±1.39
0-100	–	0.21	–	3.20±1.94	–	1.69±1.84	–	1.77±1.53	–	1.52±1.17
0-200	–	0.90	–	3.25±1.68	–	1.99±1.79	–	2.35±1.26	–	2.16±0.84
200-500	–	4.44	–	4.27±0.41	–	4.06±0.36	–	4.04±0.42	–	3.96±0.14
500-1000	–	4.28	–	3.92±0.14	–	3.95±0.09	–	3.98±0.20	–	3.98±0.14
1000-2000	–	3.66	–	3.58±0.10	–	3.57±0.08	–	3.61±0.16	–	3.61±0.09

TABLE 4. Average and weighted average concentrations of phosphorus (P) on some sections of Davis Strait and Great Newfoundland Bank in the autumn period.

Section:	2-A		6-A		7-A		8-A		8-A	
	16-21 October 1970		4-8 October 1970		20-23 September 1970		1-5 September 1970		25-30 October 1970	
Depth (m)	n	P ± σ	n	P ± σ	n	P ± σ	n	P ± σ	n	P ± σ
0	12	0.40±0.00	12	0.40±0.00	8	0.40±0.00	9	0.30±0.06	10	0.46±0.10
20	12	0.40±0.00	12	0.40±0.00	8	0.40±0.00	10	0.35±0.10	10	0.48±0.10
50	11	0.53±0.21	12	0.65±0.19	7	0.76±0.17	9	0.78±0.14	10	0.57±0.12
100	10	0.58±0.25	10	0.95±0.11	7	0.94±0.05	10	0.95±0.11	10	0.89±0.19
200	9	0.58±0.27	7	1.17±0.06	6	1.08±0.07	9	1.06±0.06	9	1.03±0.12
300	8	0.56±0.24	4	1.17±0.05	3	1.19±0.01	8	1.09±0.05	6	1.11±0.11
500	7	0.70±0.42	4	1.16±0.04	1	1.20	8	1.11±0.06	5	1.13±0.06
800	7	1.00±0.32	4	1.18±0.02	1	1.26	7	1.13±0.05	5	1.17±0.04
1000	7	1.36±0.12	4	1.18±0.02	–	–	6	1.15±0.03	5	1.18±0.02
0-50	–	0.45±0.12	–	0.47±0.06	–	0.51±0.10	–	0.45±0.10	–	0.50±0.11
0-100	–	0.50±0.29	–	0.64±0.10	–	0.68±0.11	–	0.67±0.11	–	0.62±0.13
0-200	–	0.54±0.41	–	0.85±0.09	–	0.84±0.11	–	0.84±0.10	–	0.79±0.14
200-500	–	0.61±0.61	–	1.17±0.05	–	1.18±0.13	–	1.09±0.06	–	1.10±0.04
500-100	–	0.98±0.62	–	1.17±0.02	–	–	–	1.14±0.12	–	1.16±0.11

TABLE 5. Average and weighted average indices of a relative saturation with oxygen (O_2) of Davis Strait and Great Newfoundland Bank water masses in the autumn period.

Section:	2-A		6-A		7-A		8-A		8-A	
	16-21 October 1970		4-8 October 1970		20-23 September 1970		1-5 September 1970		25-30 October 1970	
Depth (m)	n	O_2% ± σ	n	O_2% ± σ	n	O_2% ± σ	n	O_2% ± σ	n	O_2% ± σ
0	28	100.1± 2.7	27	101.3±0.9	14	101.7±2.1	23	100.7±1.7	23	99.8±1.8
20	28	99.4± 3.6	26	101.5±1.2	15	102.1±1.3	23	100.9±2.0	23	99.6±1.3
50	32	93.6± 7.8	27	93.8±7.3	12	93.3±9.1	22	95.3±4.4	23	97.1±2.6
100	29	83.6± 6.0	21	86.7±4.6	12	88.3±2.9	23	92.4±3.6	23	92.2±3.8
200	28	79.1±10.5	15	83.8±4.6	10	86.8±4.9	20	91.1±2.2	18	90.3±3.0
300	27	78.4±10.6	12	84.4±5.6	7	88.1±3.4	15	91.1±2.3	15	89.9±3.1
500	28	77.9±10.4	9	85.0±9.0	5	89.5±1.8	13	91.2±1.3	14	90.3±2.9
600	26	78.3±10.0	10	86.5±6.1	5	89.4±1.3	13	90.6±1.4	14	90.0±1.5
800	26	74.6± 9.6	5	87.5±3.1	3	89.8±1.9	13	90.2±1.6	13	89.4±1.2
1000	27	72.3±14.1	6	88.9±1.3	3	90.4±0.8	11	90.9±1.1	12	89.5±1.1
1200	26	77.9±10.0	5	88.1±1.0	4	89.0±1.2	12	89.6±1.4	12	89.5±1.4
1500	25	81.2± 5.2	5	87.9±0.8	4	88.3±0.9	11	88.0±1.1	12	89.1±3.4
2000	24	81.1± 3.1	4	86.8±0.6	3	85.7±0.9	9	86.7±0.5	10	87.2±3.8
0-20	–	99.8± 3.2	–	101.4±1.0	–	101.9±1.7	–	100.8±1.8	–	99.7±1.6
0-50	–	97.8± 4.7	–	99.1±3.0	–	99.4±3.8	–	99.2±2.7	–	98.9±1.8
0-100	–	93.2± 5.8	–	94.7±4.5	–	95.1±4.9	–	96.5±3.3	–	96.8±2.5
0-200	–	87.3± 7.0	–	90.0±4.5	–	91.3±4.4	–	94.1±3.1	–	94.0±2.9
200-500	–	78.4±10.5	–	84.5±6.9	–	88.7±3.1	–	91.1±2.0	–	90.1±3.0
500-1000	–	75.6±10.7	–	87.2±4.2	–	89.8±1.5	–	90.6±1.4	–	89.7±1.4
1000-2000	–	79.5± 6.8	–	87.8±0.9	–	88.0±1.0	–	88.4±1.0	–	88.8±2.8

TABLE 6. Average and weighted average values of temperature (t°) on some sections of Davis Strait and Great Newfoundland Bank in the autumn period.

Section:	2-A		6-A		7-A		8-A		8-A	
	16-21 October 1970		4-8 October 1970		20-25 September 1970		1-5 September 1970		25-30 October 1970	
Depth (m)	n	t° ± σ	n	t° ± σ	n	t° ± σ	n	t° ± σ	n	t° ± σ
0	36	17.36±6.25	27	10.05±2.15	15	8.02±1.68	23	5.92±2.28	23	2.57±1.98
20	36	16.83±6.81	27	9.97±2.45	15	7.85±2.54	23	5.24±2.84	23	2.76±1.89
50	36	14.04±9.43	27	3.61±4.68	13	2.70±1.74	22	1.85±2.67	23	2.74±1.99
100	30	13.29±9.41	22	2.93±3.42	12	1.30±1.96	23	1.93±2.29	23	2.12±1.78
200	29	12.17±7.04	15	4.56±2.72	10	3.15±1.49	19	3.35±1.69	18	3.48±1.14
300	28	11.95±6.22	13	4.88±2.26	7	4.12±0.52	15	4.47±0.38	15	4.34±0.67
500	28	10.79±5.87	9	4.68±0.92	6	4.16±0.13	14	4.41±0.37	14	4.47±0.48
600	27	10.40±5.56	10	4.44±0.64	5	4.16±0.08	13	4.20±0.48	14	4.40±0.51
800	27	8.41±4.31	7	4.17±0.31	3	4.15±0.12	13	3.95±0.25	13	4.20±0.44
1000	27	6.59±3.19	6	3.91±0.19	4	3.99±0.13	11	3.94±0.29	13	4.04±0.40
1200	27	5.40±2.59	5	3.89±0.23	4	3.90±0.20	12	3.88±0.25	12	3.90±0.26
1500	25	4.76±2.41	5	3.80±0.16	4	3.79±0.11	12	3.75±0.31	12	3.87±0.26
2000	25	4.22±1.56	4	3.61±0.15	3	3.24±0.14	11	3.33±0.22	11	3.62±0.32
0-20	–	17.10±6.53	–	10.01±2.30	–	7.94±2.11	–	5.58±2.56	–	2.66±1.94
0-50	–	16.10±7.48	–	8.08±3.06	–	6.34±2.13	–	4.36±2.68	–	2.72±1.94
0-100	–	14.88±8.45	–	5.67±3.55	–	4.17±1.98	–	3.12±2.58	–	2.57±1.91
0-200	–	13.80±8.34	–	4.71±3.31	–	3.20±1.86	–	2.88±2.28	–	2.69±1.69
200-500	–	11.60±6.24	–	4.76±1.89	–	4.01±0.55	–	4.26±0.60	–	4.24±0.68
500-1000	–	8.88±4.62	–	4.29±0.46	–	4.12±0.11	–	4.09±0.84	–	4.26±0.46
1000-2000	–	4.97±2.32	–	3.79±0.18	–	3.70±0.14	–	3.70±0.27	–	3.83±0.29

particles from their average $x = D_p - D_{c,p}$, then a number of particles with the deviation from x_o will be:

$$n(x > x_o) = \frac{Nh}{\sqrt{\pi}} \int_{x_o}^{\infty} e^{-h^2 x^2} dx = \frac{N}{2}[1 - \theta(hx_o)]$$

and

$$n(x < x_o) = \frac{Nh}{\sqrt{\pi}} \int_{-\infty}^{x_o} e^{-h^2 x^2} dx = \frac{N}{2}[1 + \theta(hx_o)]$$

where

$$\theta(hx_o) = \frac{2}{\sqrt{\pi}} \int_{0}^{hx_0} e^{-t^2} dx, \quad h = \frac{1}{\sigma\sqrt{2}}$$

where σ is the root-mean-square deviation. The number of cells, for which x lies between x and $x + \Delta x$, is:

$$dM = \rho \, \Phi_v \frac{\pi}{6} D_p^3 \frac{Nh}{\sqrt{\pi}} \quad e^{-h^2 x^2} \, dx$$

where $\Phi = \dfrac{(D_c)^3}{(D_p)^3}$

is the volume scaling coefficient.

For reasons mentioned above one can conclude that in predicting the predominant forms of phytoplankton, one should take into consideration the maximum possible ratio of their number to biomass. Each species of phytoplankton has a limit of abundance and biomass specified by the electric charge of a cell and its diameter; when a species reaches this limit, it cannot develop further and the destruction of a population begins. Maxima of the content of cells in waters of the Black Sea can be given as examples of occurrence of the abundance limit. So, the number of cells in *Leptocylindrus danicus* Cl. (Skolka *et al.*, 1970) in the Bosporus area was 14,136,000 cells.l^{-1}, and that in *Exuviaella cordata* (Steemann Nielsen, 1955) in the Mangalia Lake (Romania) was 107,540,000 cells.l^{-1}.

In fact, during phytoplankton blooming in the Northwest Atlantic, the number of cells is below maximum, and one can see the predominance of one or another species and the inhibitory influence of one species on another.

The average rate of saturation of waters with oxygen in Davis Strait and Great Newfoundland Bank area was mentioned. However, average indices do not show fine details of phytoplankton cell function. In this connection, on diurnal stations occupied in the Labrador and North Atlantic currents, we have made an attempt to determine the rhythm of oxygen production by green plants, and its dependence upon illumination; the index of illumination being the height of the sun above the horizon.

To this end data on the oxygen content and oversaturation value, $\Delta 0_2$ *ml.* l^{-1} ($\Delta 0_2 = 0_2$, obtained $0_{2\,t,s}$) were treated by harmonic analysis according to Fourier (Bezikovich, 1949; Whittaker and Robinson, 1933).

By Fourier's theorem any periodical fluctuation may be represented as a sum of harmonic sinusoidal variations, i.e., data on complicated fluctuations may be decomposed into sinusoidal terms. Thus, curves of fluctuations of 0_2, $\Delta 0_2$, t° and h_o during 24 hr were considered by us as superpositions of harmonic variations with multiple frequencies:

$$f(t) = \sum_{1}^{n} A_n \operatorname{Sin} (\omega_n \, t + \phi)$$

where A is an amplitude of fluctuations, ω is a circular frequency, and ϕ is an initial phase.

The fluctuations were represented as a spectral function, i.e., a complex of pairs of frequency values and the square of the corresponding amplitudes.

While correlating the spectra (Fig. 2) to find the

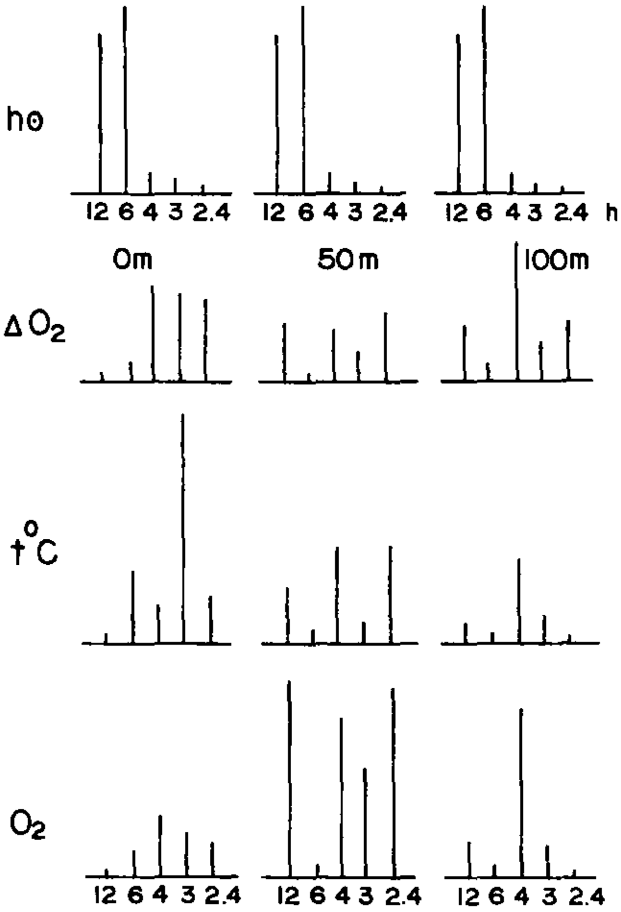

Fig. 2. Spectrogram of the sun heights (h_o), delta-oxygen ($\Delta 0_2$), temperature (t°C), and oxygen saturation (0^2) of water masses at the diurnal station – lat = 63°58′6N, long = 59°12′4W.

period of inherent rhythm in the system 'phytoplankton-oxygen', the value ΔO_2 is of special importance because in calculating this value the influence of temperature and salinity is eliminated. Fluctuations of period 2.4 hr can be considered as inherent frequencies in the biological system; the other lines are lost in the noise. So, periods of fluctuations of 3 and 4 hr correspond to temperature cycles and those of 12 and 6 hr to the 'solar oscillator'.

However, a more careful analysis enables us to show that increasing variance of biological oscillator fluctuations is observed at frequencies of 4 hr at 0 m, and of 12 hr at a depth of 50 m.

The relation between values studied (Figs. 3 and 4) can be represented in the form of closed curves. These curves are trajectories of any point with coordinates of $x = h_o$ and $y = \Delta O_2$. Such trajectories, which are named Lissajou figures, represent the variation of two mutually perpendicular vectors changing sinusoidally. Synchronous fluctuations of h_o and ΔO_2 have the form of an ellipse in the cartesian coordinates. The greater the difference in the frequency between the inherent fluctuation in the biological system and that of the solar oscillator, the greater the deformation of the ellipse.

Thus, Fig. 3A (from left to right) gives calculations for amplitudes with periods of 6 and 12 hr. As

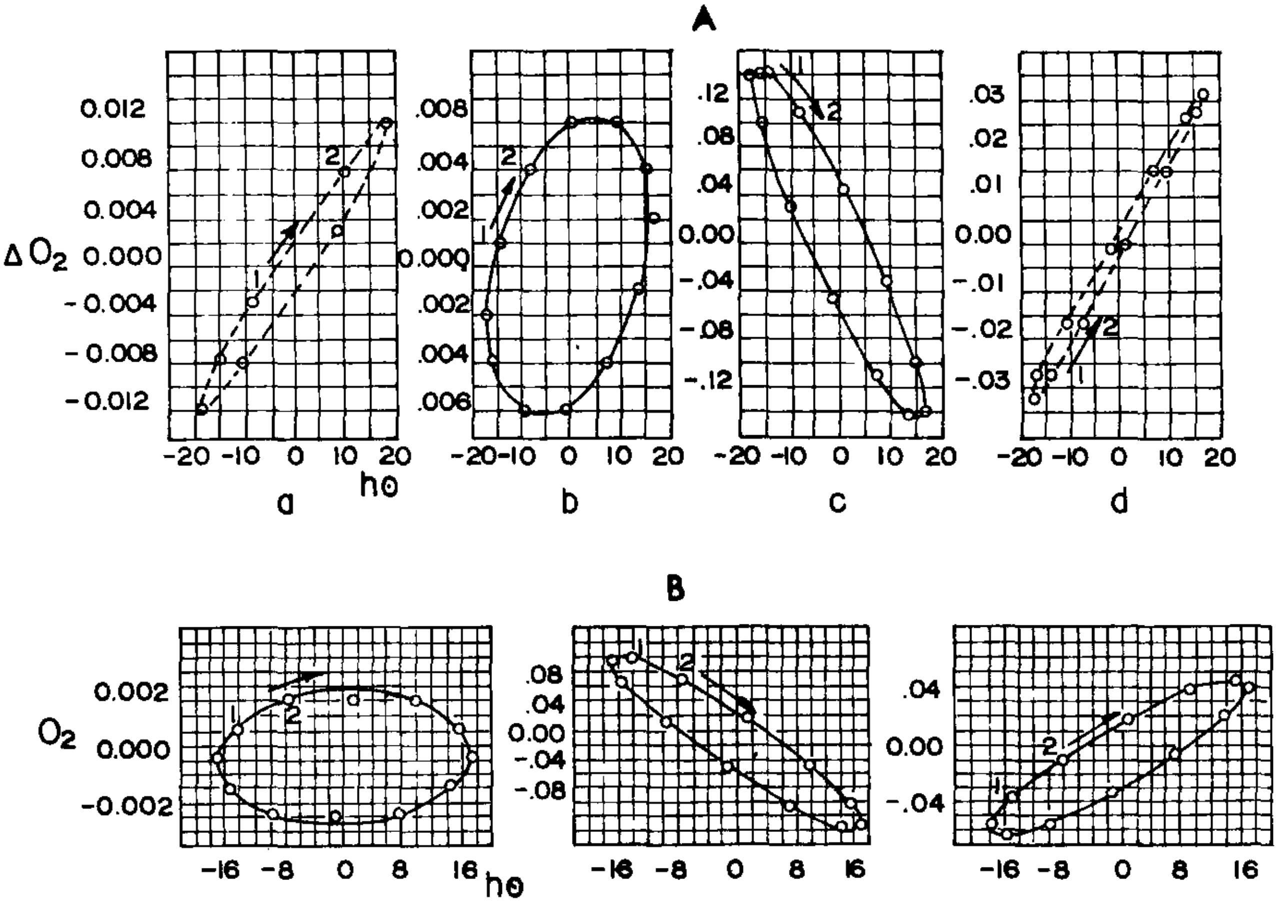

Fig. 3. Fluctuations in the functioning of the phytoplankton system.

A ΔO_2 and solar oscillation (h_O) at diurnal station (Fig. 2), a = 0 m, 6 h period; b = 0 m, 12 h period; c = 50 m, 12 h period; d = 100 m, 12 h period.

B. O_2 and solar oscillation (h_O), b = 0 m, 12 h period; c = 50 m, 12 h period; d = 100 m, 12 h period.

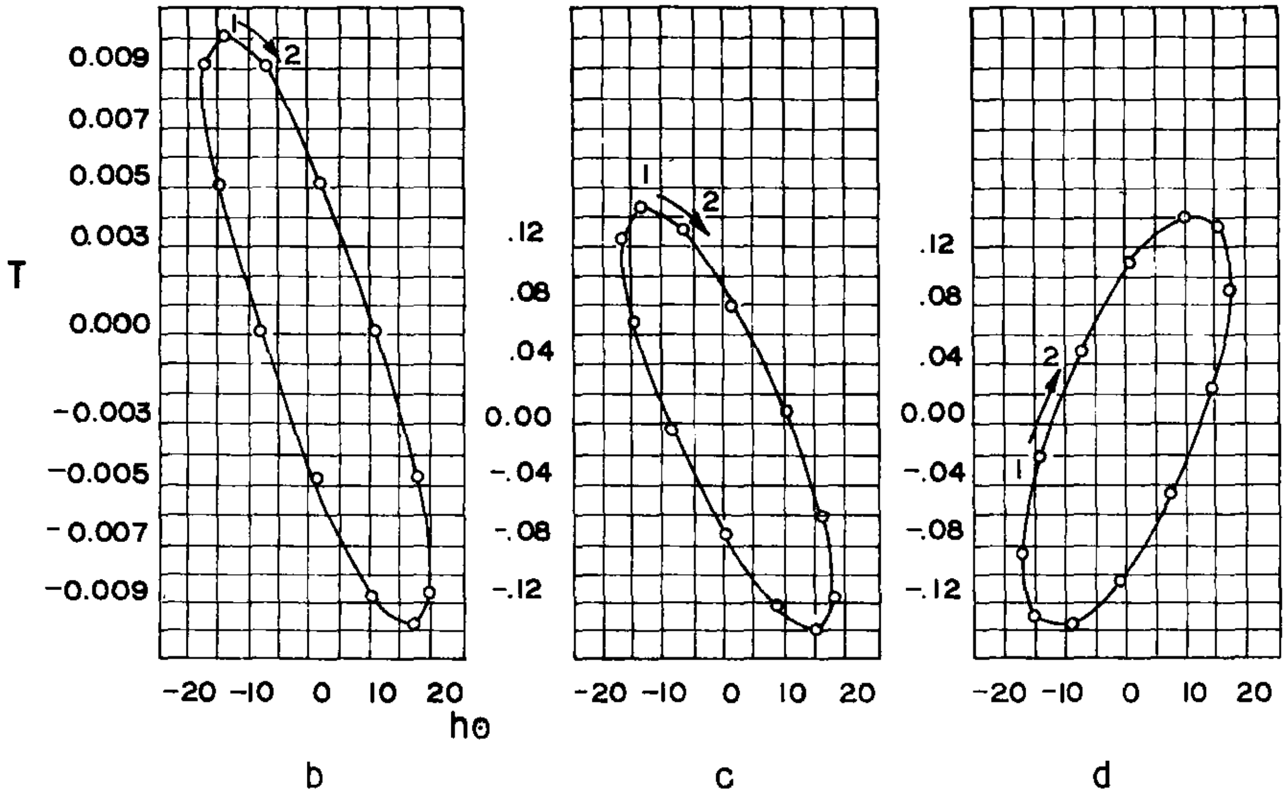

Fig. 4. Fluctuations in temperature (T) and solar oscillation (h_o), b = 0 m, 3-4 h period; c = 50 m, 3-4 h; d = 100 m, 3-4 h period.

TABLE 7. Phase shift and time when fluctuations on diurnal stations started.

	North Atlantic Current		[a]Davis Strait		Great Newfoundland Bank		Great Newfoundland Bank	
	St. 1 lat 49°50′N, long 29°31′W		St. 2 lat 63°58′N, long 59°12′W		St. 3 lat 53°30′N, long 54°00′W		St. 4 lat 52°45′N, long 53°00′W	
Depth (m)	Phase shift ϕ	Time fluctuations started	Phase shift ϕ	Time fluctuations started	Phase shift ϕ	Time fluctuations started	Phase shift ϕ	Time fluctuations started
0	53°06′	2346	-18°24′	1023	79°03′	1573	-25°54′	2273
	-35 30	0937	-76 00	1707	- 4 00	2327	9 18	2238
	-61 42	1511	-21 48	1645	4 36	069	-21 00	240
	-41 24	1776	- 4 24	1829	-28 18	489	-86 53	880
	-33 00	2120	-18 24	2223	-79 54	2367	-16 24	609
20	40°06′	0033	72°12′	418	-61°36′	111	54°18′	1738
	4 24	0671	-73 18	1689	32 00	2087	25 18	2131
	26 36	0923	-72 06	1981	59 24	2104	-56 12	475
	50 54	1160	-68 30	2257	-34 36	531	65 24	2264
	3 00	1880	-22 00	2247	59 06	106	25 24	331
50	- 3°36′	0324	-89°06′	1494	-67°57′	153	-68°00′	154
	45 00	0400	-40 12	1468	79 23	1770	25 00	2133
	10 06	1033	51 18	1158	-19 06	227	-18 54	226
	63 24	1077	16 42	1689	13 00	213	12 18	218
	-89 31	0097	65 00	1666	-62 54	920	62 54	080

[a]Station 2 (0, 50, and 100 m).

TABLE 8. Periods of the main harmonic and harmonic applitudes amounting to over 60% of the main one.

Station, date, and position	Main harmonic	0 m			20 m			50 m		
		ΔO_2	O_2	t	ΔO_2	O_2	t	ΔO_2	O_2	t
Station 1-7 July 1970	A = 100%	2.4	6	12	12	12	12	12	12	12
lat 49°50'3N	A > 90%	3	3, 2.4	–	–	–	–	–	–	6
long 29°31'W	A > 80%	–	–	–	4	4, 2.4	–	–	–	3
	A > 70%	–	–	–	–	3	–	–	–	–
	A > 60%	12	–	2.4	3	–	6	–	–	–
Station 2[a]-9 Aug. 1970	A = 100%	4	4	3	2.4	12	2.4	4	4	4
lat 63°58'N	A > 90%	3	–	–	–	2.4	–	–	–	–
long 59°12'W	A > 80%	2.4	–	–	12	4	4	–	–	–
	A > 70%	–	3	–	4	–	–	–	–	–
	A > 60%	–	–	–	–	–	–	–	–	–
Station 3-5 Sept. 1970	A = 100%	12	12	12	3	6	6	12	12	12
lat 53°30'N	A > 90%	–	–	–	–	–	–	–	2.4	–
long 54°00'W	A > 80%	3	–	–	–	–	–	–	–	2.4
	A > 70%	–	6	4	–	3	–	–	6	6
	A > 60%	2.4	–	–	–	–	2.4	–	–	4
Station 4-8 Sept. 1970	A = 100%	12	12	12	12	12	12	12	12	12
lat 52°45'N	A > 90%	–	–	–	2.4	–	6	–	–	–
long 53°00'W	A > 80%	–	–	–	–	–	–	–	–	–
	A > 70%	–	–	6	6	6	–	–	–	–
	A > 60%	–	–	–	–	–	4	–	–	–

[a] Station 2(0, 50, and 100 m).

synchronous changes of h_0 and ΔO_2 have the form of an ellipse, one can state that oxygen liberation takes place simultaneously with fluctuation of the solar oscillator. An analogous form of an ellipse can be seen in Fig. 3B for O_2 at a depth of 0 m and the optimum correspondence of fluctuations with predominance of the 3-4 hr rhythm of temperature (Fig. 4).

Tables 7 and 8 give data on phases of the fluctuations at different depths and on predominant frequencies of these variations.

Analysis of these data shows that fluctuations at depths of 0, 20, and 50 m differ from each other. In our opinion, this results from the decreasing period of the daylight at lower depths and adaptation of phytoplankton to the greater duration of the dark period. This adaptation of phytoplankton is expressed in the more efficient use of blue-green light per unit of phytoplankton, and of a different period of inherent change in the biological system.

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Contribution Number 10

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Food Chains and Fish Production[1]

By L. M. Dickie[2]

Abstract

Studies of food-chains in the sea have been undertaken in the course of efforts to predict the potential fisheries yields and to compare them with present yields. Over the past 10 years attempts at such prediction have become an increasingly popular exercise. The total range of the resulting estimates for the world's fisheries is high, although most of them cluster about a rather smaller range. However, this apparent agreement tends to obscure small but important differences in the basic data employed.

In attempting to examine the productivity of particular areas, such as the ICNAF Area, meaningful or useful estimates require somewhat greater precision than has been tolerated in global estimates. For this reason, basic concepts of trophic efficiencies are re-examined in the light of recent experimental data and theoretical developments. The results suggest that we have sufficient knowledge of the major mechanisms responsible for observed values of production efficiency to establish criteria useful in describing the natural biological production systems for purposes of prediction.

Recent data on productivity and food relations of North Atlantic fishes are reviewed. Ironically, many of the heavily fished regions of the Northwest Atlantic are still relatively poorly studied. However, existing data on fisheries together with knowledge of trophic production systems, makes possible an evaluation of the usefulness of various kinds of research programs for increasing the quality of yield predictions in the ICNAF region. Of particular importance will be critical tests of alternative hypotheses concerning the effects of changes in fisheries on the yields.

Introduction

During the past decade there has been an almost exponential growth in the number of papers aimed at predicting the upper limit of the world's fisheries catch from studies of the food chains of the sea. As might be expected, the range of estimates is rather wide. The highest is over 35 times larger than the lowest. This range is, however, not representative. As the decade passed, and the number of papers increased, the estimates themselves showed a strong convergence towards a range of 90 to 250 million metric tons; that is, to estimates of the upper limit which are only 1½ to 4 times the present catch level of about 60 million metric tons.

It is not my intention in this paper to review in detail, recent global estimates of fish catches. Their increasing frequency of appearance reflects the recognition that present rates of increase in catch levels cannot be long maintained without significant technological change. Of course, as Ricker (1969) points out, there is an already respectable tradition for this kind of concern, which has followed a remarkably set pattern. At least twice in the 1950's, estimated upper limits double the then existing levels of catch, were realized and exceeded. Similarly the most conservative estimate of the 1960's (Graham and Edwards, 1962) was for an approximate doubling, to about 60 million tons, a level which we now appear to have at least equalled. However, such historical precedent is hardly sufficient grounds for present complacency. What seems necessary at the moment is to establish the degree to which the successive estimates have been based on improvements in the theory and the basic data on marine food-chain production. In general there has been some improvement in both. However, we recognize that until our systems of both data collection and of yield estimation are based on explicitly known

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and experimentally testable relationships, we shall neither understand the reason for failures of past predictions, nor have a sufficient basis for improvement. Ironically, the Northwest Atlantic which is of most interest in the present context, although one of the most heavily fished regions of the world and one for which the fisheries statistics are possibly the best, is also among the least well-known from the point of view of specific information on the biological and oceanographic mechanisms involved in production processes. The purpose of this paper is to examine the basis for yield prediction with a veiw to identifying measurable relations which will warrant study if we are to make more reliable catch predictions for the Northwest Atlantic in the next few years.

Methods of Calculating the Potential Yield

The methods of catch prediction which have either seemed appropriate or been available to various authors have been classified by Schaeffer (1965) and by Gulland (1970). Their surveys suggest that the applications have not differed from one another very much in either basic data or general principles. On the contrary the marine production system is almost always described in very general terms as an ecosystem having a series of net energy transfers among components, and a series of outputs of energy or its equivalent in terms of materials. The various papers differ from one another primarily in the productivity weightings given to various geographical regions or trophic levels. A special class could possibly be made for estimates which deal only with the rates of yield of fishery products in relation to the estimated fish biomasses from which they are generated. However, these studies merit separate consideration primarily because fish catches may often be the only clearly relevant and accurate data available. We shall not provide them with special status in this review.

Possibly the most general approach has been that taken by Ricker (1969), who defines a "transfer efficiency" between trophic levels as

$$\text{T. E.} = \frac{P_n}{P_{n-1}} = E_n \times K_n \qquad \text{where}$$

T. E. is defined as the ratio of production (P) of a predator (trophic level n), to the production of its prey (trophic level n-1); E is an ecotrophic coefficient, defined as the fraction of the prey production which is eaten by the predator; and K is the efficiency with which the predator converts its food into growth or production. The problem of calculating or predicting the potential yield from an area of sea thus consists of four parts:

1) estimating the primary production,
2) arriving at values for T. E., either directly or by consideration of its components, E and K,
3) defining the appropriate trophic level or mixture of levels from which the yield is to be taken,
4) weighting the various numbers by appropriate geographic or zoographical regions.

As several authors have pointed out, the first part of this problem, that of estimating the primary production, is by no means solved. Generally speaking, however, the papers with which we are concerned do not discuss this problem in any detail. Primary production estimates are based on the best estimates of net C^{14} assimilation by phytoplankton, generally using *in situ* experimental and analytical techniques which are reasonably well standardized. These have to be adjusted for depth of the photosynthetic zone, and diurnal, seasonal and regional fluctuations. Understandably, the indices have a substantial variance, in addition to any possible weaknesses as measures of the total primary energy fixation. Earlier estimates of global marine primary production of the order of 1.3×10^{10} tons Carbon per year (Koblenz-Mishke 1965, Steeman Nielsen and Jensen 1957) have recently been revised upwards to about 2.0×10^{10} tons C/yr (Ryther 1969). But even now the precision of estimates is unlikely to be less than ± 50%, and the general level of estimates probably still underestimates the total (Ryther *et al*, 1970, Sutcliffe *et al*, 1970). More recent methods may well improve the precision significantly, or at least increase the degree of reliability with which different areas may be compared (Platt 1969). There are as yet no reliable estimates for the ICNAF area.

The papers with which we are concerned deal in some detail with the problems of estimating the transfer efficiency and trophic level. Thus Chapman (1965), the most optimistic of the recent predictors (> 2,000 million metric tons) based his opinions on rather large estimates of transfer efficiency and short foodchains. Schaeffer (1965) presented a range of calculations, based on the higher estimates of primary production, T. E. values between 10 and 20%, with three or four transfer steps postulated between primary production and yield. The possible range of total yields varies by a factor of 400, although in retrospect there seems to be no serious doubt that with present fishing methods the choices can be reasonably narrowed to a factor closer to 15, starting with the present level of catch of 60 million tons per year as the minimum and calculating yields "per unit primary production" to allow for likely readjustments upwards of the whole series. Within this range Schaeffer (1965) regarded 200 million tons as a conservative estimate of potential catch. Ricker (1969) accepted transfer efficiency values in the upper range, but after considering the catches to come from "ecological" levels made up of mixtures of trophic levels, found himself unable to assign low estimates for the number of energy conversions involved. His choices of values for these two factors, while different from Schaeffer's, approximately balanced them. Thus Ricker's low overall predicted yields appear to be largely a result of the fact that compared with Schaeffer he uses an initially lower primary production estimate.

Ryther (1969) supported the generally higher primary production levels, then weighted the primary production, efficiency values and transfer numbers roughly by zoogeographic regions. His estimate of 240 million metric tons of *production* he equates with Schaeffer's 200 million tons of *catch*; whereas his estimates of catch limits are, in fact nearer those of Ricker. The difference between them in accepted level of primary production is balanced by Ryther's relatively much more sevère classification of productive zones as proportions of the world's total sea surface.

Moiseev (1969) after a very detailed consideration of primary production by zoogeographic zones, arrives at about the same estimates of primary production as were used by Ryther and Schaeffer. This he compares with present world catch in the light of T. E. ratios derived from a variety of studies of production and yield in a number of intensivly studied, partly enclosed inland seas with different production systems. He concludes that transfer efficiencies must be generally low. Since the numbers of transfer levels he accepts appear to be much the same as those accepted by Ricker, on balance his estimates of low efficiencies lead him to even lower overall predicted catch limits than are given by Ricker, despite his upward adjustments of primary production. Moiseev's estimates of upper limits of world fish catch of about 90 million tons per year, based on food-chain productivity considerations, are slightly above estimates he arrives at by summing up the prospects for individual areas and fisheries, in the light of present catch compositions, fishing rates and histories of development.

In his second method of estimating overall world potential catch, Moiseev's weighting of individual regions is not discussed in detail, but appears not to differ remarkably from a very similar use of this method in the review compiled by Gulland (1970). Gulland refers to the food-chain types of calculations briefly, but prefers to rely on calculations based on the fisheries statistics themselves, qualified by his or his co-worker's general knowledge of areas. That is, in our present terminology Gulland's approach begins with concern for an accurate estimate of the ecotrophic coefficient, "E", for the final predator – man. He also examines briefly the relation of the present value of F to the value which would theoretically give maximum catch. Formally, the remainder of the trophic system is assumed to remain in much the same average state as at present. In actual fact, however, the method at least leans heavily on estimates of yield per unit area, which are compared with one another and with general information on hydrography and productivity to spot possible inconsistencies. Gulland's conclusion that the world oceans can support an approximate doubling of the yield of present types and kinds of species, seems slightly more optimistic than Moiseev's, but almost certainly would not be significantly different from it if quantitative error bounds were available for either one. Estimates of both Gulland and Moiseev are higher than the similar yield-per-unit-area type predictions made earlier by Graham and Edwards (1962). Differences arise largely because both of the more recent authors can permit a higher yield per unit area in heavily fished regions (based on experience in the 1960's) and higher estimates of the fraction of the total area of the sea which may be utilizable. Edwards (1968) found that catch per unit area in the Georges Bank area approximately doubled between 1957 and 1967 and that annual fisheries yield was approximately one-quarter of the average standing crop.

While each of the authors has considered various problems of catch prediction in some detail, it may be fairly concluded from this review, that except with Moiseev there has so far been relatively little new consideration of the mechanisms involved in the energy exchanges within the system. As a result, the reliability of various estimates seems to rest on a combined assessment of historical trend and personal judgement. Furthermore, at this most general level, our knowledge of the system seems to have offered little opportunity for creating and testing hypotheses (Steele, 1965). Moiseev again comes closest to it, including a first-order approach to a geochemical budget in his assessment, from which he concludes that very much higher than present catch rates may be unrealistic. However, the apparent agreement among the methods in this as in the other cases is clearly liable to errors arising from a combination of insufficiently detailed data, and serious uncertainties as to the nature of pathways and transfers being considered.

Any significant improvement in the present order of estimates will depend on increased knowledge of particular fishing situations. However, at this stage, it cannot be expected that energy exchanges among various trophic levels will be especially simple, or that the regularities which may underlie them will emerge easily. It is not necessarily a simple problem to decide what observations to make. For example none of the recent attempts at catch prediction come close to incorporating the kinds of information which Riley (1963) early showed may significantly affect the relation of fish catch to primary production. As a first step towards framing appropriate models for testing, we turn to the problem of making explicit the nature of the many observations and comparisons which seem possible and relevant.

Some Definitions and Comparisons

The basic theory on which fishing yields are predicted from food-chain production systems seems relatively well understood, although definitions and word uses are not always made sufficiently explicit to prevent misunderstandings. Kozlovsky (1968) summarizes definitions of some 20 "ecological" efficiencies which have been used in the literature. In the previous section we referred to Ricker's definition of "transfer efficiency" as the ratio of production in two successive trophic levels. Gulland (1970) refers to this ratio as the ecological efficiency although it is one which Kozlovsky does not include in his list. Slobodkin (1960), following the original definitions of Lindeman, suggested that the term ecological efficiency should be precisely defined as the fraction of food intake of a prey organism which is passed on as yield to its predators. Alternatively, Blackburn (1966) suggested that this "food-chain efficiency" (by which he apparently means ecological efficiency) is approximated by the ratios of biomasses in steady-state trophic systems. Other authors have employed the ratios of respirations, and so on. In this paper we will concern ouselves with only the first three: the ratios of production, biomass and food intakes or yields. There may be good empirical grounds for favouring one or the other of these measures in particular situations, and precise comparisons among them can yield useful information. It is therefore important to clarify the usages. The basis for an explicit comparison using terminology familiar in fisheries research is given by Paloheimo and Dickie, 1970.

We begin with the production (P_n) over a period of time by a predator of trophic level n. Evidently

$$P_n = K_n \times I_n$$

where I_n is the food intake of the predator and K_n is its food conversion efficiency (≈ gross growth efficiency). The average biomass, B_n, or "standing stock" of this predator, will be the production divided by the turnover rate. If the population is at equilibrium, the turnover is numerically the same as the total mortality rate (Z_n) whence we may write

$$B_n = \frac{P_n}{Z_n} = \frac{K_n I_n}{Z_n}$$

The yield from this predator to a higher predator, or to a fishery, is simply the fished fraction of the total deaths. If we consider that the total mortality is composed of fishing and natural parts, we may write:

$$Z_n = F_{n+1} + M_n$$

where F_{n+1} is evidently the mortality on population n, generated by its predator at trophic level $n + 1$. M_n is thus the probability of death for the animals in trophic level n, from all other sources of mortality. We may then write the yield to this top predator:

$$Y_{n+1} = \frac{F_{n+1}}{F_{n+1} + M_n} \times K_n I_n$$

It may be observed, that I_n is the yield to trophic level n from its depredations on level n-1. The equation in this form thus contains elements which are clearly dependent on conditions within three successive trophic levels, and must be considered in this context (Slobodkin, in press).

We are now in a position to write three efficiency definitions:

$$\frac{P_n}{P_{n-1}} = \frac{K_n I_n}{K_{n-1}\, I_{n-1}} = K_n \times \frac{F_n}{F_n + M_{n-1}} = K_n \frac{F_n}{Z_{n-1}} \quad (1)$$

$$\frac{B_n}{B_{n-1}} = \frac{P_n}{P_{n-1}} \times \frac{Z_{n-1}}{Z_n} \quad (2)$$

$$\frac{Y_{n+1}}{I_n} = \frac{F_{n+1}}{Z_n} \times K_n \quad (3)$$

From equation (1) it is clear that the ratio of productions in two successive trophic levels is the same as the ratio of their food intakes, corrected for the ratios of food conversion efficiencies in the two levels. However, observing the definition of the food intake of level n as yield from level n-1, the production ratio may be rewritten as the right side of equation (1). In this form the production ratio is identical with the definition used by Ricker (1969) for his transfer efficiency, the quantity which Gulland (1970) called the ecological efficiency.

The ratio of the biomasses (equation 2) is evidently the same as the ratio of productions corrected by the ratio of total mortality rates or turnover rates between the two populations. From equation (2), information on relative biomasses and productions may be used to give information on relative mortalities or turnover rates.

Equation (3) defines ecological efficiency in accordance with the usage of Slobodkin (1960). Using the conversion efficiency K_n by an exploited population as a reference point, it is defined as the product of this conversion efficiency and what Moiseev calls "the degree of utilization" of the exploited population's production by a predator (i.e. fraction caught). Slobodkin (1959, 1960) made use of this definition in experimental studies of animal populations and rates and types of predation. Paloheimo and Dickie (1970) point out that it is also of interest to fisheries biologists, and may be equivalent to yield curves in appropriately calculated models. However, it is not the same as the definition of ecological efficiency as used by Gulland. Evidently Ricker's and Gulland's efficiency measure (equation 1) is comparable with that of Slobodkin and Lindeman only if there are no differences among trophic levels. That is, equations (1) and (3) are the products of homologous relations, but taken over different intervals of the food-chain system. This distinction is not always made explicit in discussion, but may be important in empirical studies. In what follows we shall refer to the production ratio (equation 1) as the production efficiency, and reserve the term ecological efficiency to the usage of Slobodkin (equation 3).

The ecological and production efficiencies are both important parameters in the interpretation of fish production systems. Their ratio will of course provide a direct measure of the ratio of the ecotrophic coefficients of the predator to that of its prey. If the predators are fishing boats, the extent to which the ecotrophic coefficients may change relative to one another at different levels of predation is critical to long-term yield prediction. In fact, an important part of the difference in the predicted effects of fishing on yield between the simple food-chain calculations of Paloheimo and Dickie (1965), and the so-called "constant recruitment" models of fisheries developed by Beverton and Holt (1957), can be precisely defined and measured by reference to the ratios of these two ecotrophic coefficients. The remaining differences between food-chain and "constant recruitment" models are ascribable to changes in the gross conversion coefficients – K_n. These two coefficients would appear to be of sufficient importance to justify separate consideration.

The ecotrophic coefficient

The ecotrophic coefficient, E, in the form of its definition as $F/F + M$, has been recognized for many years as one of the central values in predicting yield changes due to changes in fishing effort. Where the top predator in the fish production system is taken as the fishery, information derived from studies of changes in yield in relation to fishing should, in theory, provide unique "experiments" for the testing of hypotheses about predation in natural populations. Conversely, explicit information on a variety of natural situations should prove enlightening for predicting the longer-term consequences of fishery exploitation and management. Up to the present, vagaries of sampling together with some confusion in terminology appear to have been playing a part in keeping the scientific efforts in these two areas separated into their "fisheries" and "natural population" components.

Slobodkin (1960) seems to have first suggested that ecological efficiencies are likely to be limited to a range of perhaps 0.05 to 0.15. Experience in the 10 years since has indicated that ecological efficiencies higher than about 0.10 may be rare. Since $F/F + M$ approaches 1.0 as an upper limit, equation (3) shows that ecological efficiency must always be smaller than the gross growth (conversion) efficiency, K, and approaches it asymptotically as the ecotrophic coefficient increases (Paloheimo and Dickie, 1970). There have been a number of studies of the value of K (see next section), which suggest that 0.20 may be an upper operating limit. If so, it appears that the ecotrophic coefficient in nature is unlikely to exceed a value of about 0.5. Interestingly enough the logistic population models of Schaefer (1954) suggest that for a resource limited population, "surplus production" is maximized when the population biomass is reduced by fishing (predation) to roughly half of its initial value i.e., at the inflection point of the logistic growth curve. This occurs, where the virgin total mortality is roughly doubled, or where $F = M$, at which point the ecotrophic coefficient is approximately 0.5.

However interesting such a conclusion may be, comparison of the natural and fishing situations needs to be made with considerable care. A consideration of equation (3) in the symbols of fisheries biology makes clear the special importance of clarity in this situation. In the language of fisheries the definition of the ecotrophic coefficient as $F/F + M$ is critically dependent on the applicability of the assumption that F and M represent independent probabilities of death due to fishing (or predation) and other causes. This problem does not appear to have been quantitatively studied in "natural" populations. Qualitatively it is an unlikely situation for most non-pelagic natural predation systems.

The extreme is the detritus feeder where the "predator" takes as food those organisms which have died or are about to die from other causes.

Since there have been very few studies of the population structures of "virgin" stocks the independence of mortality sources has received very little attention in most fisheries investigations. There are nevertheless a number of reports which suggest that total natural mortalities among unfished stocks may often appear to be rather high, of the order of 0.3 or higher, (Bakken, personal communication; Derzhavin, 1922; Dickie and McCracken, 1955; Kennedy, 1953; May, personal communication; Noskov and Zakharov, 1964; Ricker, 1958). These values are considerably higher than has generally been estimated for the natural mortality component of total mortality in exploited populations in the ICNAF areas (Beverton and Hodder, 1961), suggesting that significant fractions of fisheries catches may result from a lowered probability of death from other causes.

The consequences of interdependence of sources of mortality to the maximization of yield in relation to predation are potentially very important. This is as true in the "management" of exploited stocks as in the understanding of natural systems, so that Slobodkin (1960) was moved to offer an alternative efficiency measure to take them into account. This measure he called "population efficiency". Paloheimo and Dickie (1970) pointed out that it is equivalent to the gross conversion efficiency in simple population models. However, this conclusion holds only for a situation where F and M are independent. Where the various fishing and natural mortality risks are not additive, it would appear that optimization of fishing strategy will necessarily involve a modification of the formulation of the ecotrophic coefficient, keeping in mind the requirement for parameters which may be measured without ambiguity.

Equation (3) may be rewritten in a form which permits further examination of comparable interactions at various levels of the food-chain. We may recall that in the balanced stocks with which our entire set of relationships is constrained to deal $P_n = B_n (F_{n+1} + M_n)$. As is general practice in fisheries studies, we may also define $F = qf$, where f is some appropriate measure of the number of predators, grazers or fishing units, and q is a coefficient describing the probability of mortality of a prey organism due to a unit operation of the predator. From this, ecological efficiency may be redefined from equation (3) as

$$\frac{Y_{n+1}}{I_n} = (qf)_{n+1} \times \frac{B_n}{P_n} \times K_n \qquad (3a)$$

Here we have separated the ecotrophic coefficient into two parts which while clearly not independent, may nevertheless be independently measurable. Thus within a given trophic level the average annual total mortality rate of a particular exploited or grazed stock will be reflected in the ratio of the annual productivity to the average biomass. From one trophic level to another this ratio tends generally to increase with distance from primary production and to have a general relation with the sizes of organisms involved. Writing it in this fashion for a fish population over rather long time periods seems to imply no especial loss of generality since in most fisheries production models mortality is usually treated as constant with age. We return to further consideration of the P/B ratios below.

While the fishing mortality part of the ecotrophic coefficient may be defined in two parts (q and f) as above, one of the uses for equation (3a) in this form is to permit explorations of the regularity of food-chain relations among animals at different trophic levels. Since a change in the P/B ratio will almost certainly also mean a change in average size of organism involved, this in turn will usually mean a change in the average "livingspace" of an individual, in the sense of Strickler (1969). In such situations it is useful to distinguish the elementary "searching efficiency" of a predator per unit area from changes in grazing effectiveness due to the scale on which individual searching activities take place. We may therefore return to the elementary considerations of Baranov (1918) and write for the predatory rate of capture:

$$F = scf$$

where f is the density of predators in some area or water volume, and c may be defined as the elementary efficiency of predation by each predator within the volume it normally inhabits. It follows that s is a scaling factor, measuring the fraction of the total volume searched by each predator. This factor appears to have been best studied in birds, where the effective "living room" in our present context of annual time periods would be the feeding territory. Schoener (1968) shows that s appears to vary with $W^{1.4}$ where W is average body weight of the individual birds. Schoener found similar values for mammals, based on the data of Burt and Grossenheider (1964) for nine predatory mammals. A much lower value of the exponent (0.6) was obtained by McNab (1963) for a number of omnivorous and herbivorous small mammals. No comparable information is available for fishes.

The remaining term c is of considerable interest and Ivlev (1955); Brock and Riffenberg (1960); Paloheimo and Dickie (1964); and Parsons, LeBrasseur and

Fulton (1967) have suggested ways in which it is likely to be a function of the distribution and relative size of both predator and prey.

The growth coefficient

In most natural populations, the biomass of the young is an insignificant fraction of the total biomass, and the production of young at the "instant" of appearance may be considered an insignificant part of the whole population production. To the extent that this is true, the gross conversion or production efficiency, K_n, in the efficiency equations may be measured as the gross growth efficiency of the existing biomass.

There have been a number of studies of the relation of the gross growth efficiency of individual fish to the amount and characteristics of the food intake. Within the normal feeding range the logarithm of the growth efficiency decreases linearly with increased food (or predator size within a given food availability level), a relation which Paloheimo and Dickie (1965) termed the K-line. They used it in production models (Paloheimo and Dickie, 1970) to calculate production and ecological efficiency in relation to fishing rates where the food available to predators was a constant. Sushchenya (1970) found that the same basic equation could be used to describe the growth of a zooplankton organism.

More recently Kerr (1971 *a*, *b*, *c*) has re-examined the basic parameters controlling growth efficiency, and their utility in constructing growth and production curves in natural fish populations. His theoretical studies indicated that in an oligotrophic lake under the range of feeding situations normally found, growth prediction was virtually independent of the overall density of prey. On the other hand, it appeared that the size-composition of the prey resource was a prime determinant of fish growth.

These conclusions were tested on data derived from a series of trout populations (Kerr, 1971c). The results in one situation suggested that not only did size-composition of the food population explain a change in the growth rate with high fidelity, but that this change in turn accounted for an overall change in total fish production from the lake. The results confirmed an earlier conclusion from a different data series (Kerr and Martin, 1970) that an increase in growth efficiency, due to increase in average food particle size, was sufficient to completely compensate for an increase in food-chain length in production systems. That is, catch and apparently production per unit area in several similar simple natural fish-producing systems was constant despite differences in food-chain length. However, in the shorter food chains, where the fish food consisted of small plankton, the fish were slow growing and numerous, hence the yield was of smaller sizes than in situations where small forage fishes were interposed between the zooplankton and the exploited stock.

Kerr's findings suggest that there must be strong interactions of the ecotrophic and gross growth efficiency components of the production efficiencies as they are defined in equation (1). To explore these relations in the food chain, this suggests that it is appropriate to rewrite equation (1), by analogy with equation (3a) in the form:

$$\frac{P_n}{P_{n-1}} = \frac{(KI)_n}{(KI)_{n-1}} = K_n \times qf_n \times \frac{B_{n-1}}{P_{n-1}} \qquad \text{(1a)}$$

where in Kerr's studies the production P_n is the trout production. The ratio $(B/P)_{n-1}$ reflects the turnover time and growth efficiency of the forage fish. Kerr's results indicate first, that with the change in the size of the forage animals, the trout production increased. In this formulation his conclusion that this resulted from an increase in the K_n for trout is consistent with the fact that the fishery yield also increased by about the same fraction. Comparison of equations (1) and (3) indicates that they are both proportional to K_n.

It cannot be deduced from this much information whether the production and ecological efficiencies remained constant. However, an increase in average size of the forage animals seems likely to be accompanied by an increase in their (B/P) ratio. If, as has often been suggested, the production ratios tend to be constant, an increase in the size of forage animals, since it is accompanied by an increase in B/P, must also represent a significant decrease in the ratio $(qf)_n/P_{n-1}$. That is, there must have been a decrease in the fraction of the forage fish production which was passed on to the predator. Conversely, it would appear that in general if during its evolution, a prey organism could in any way increase its size, it may by this device achieve a "protection" for the fraction of its own population production which is taken by its predators. That is, under the influence of predation, size increase must have a strong selective advantage. A corollary is that in the evolution of ecosystems there will be a tendency towards the development of larger sizes within the food-chains.

Kerr and Martin's (1970) other finding, that the interposition of an extra step in the food chain led to approximately the same overall production of the top predator, but with an increase in the average sizes of the predators, is consistent with this conclusion. Evidently there is a strong interaction of the parameters of both the production and ecological efficiencies. Information of the details of this interaction would thus appear to provide powerful tools in the analysis and understanding of production and yield in ecosystems. The growth efficiency K, plays an important role in this understanding.

A further definition problem – the trophic level

Before attempting to deduce the implications of the foregoing for improved predictions of Northwest Atlantic fish catches, it is necessary to consider an additional practical difficulty. Conceptually the calculation of production or catch from a food chain system is relatively straightforward, as indicated above **(p.202)**. However, as has been repeatedly pointed out since Harvey (Steele, 1965), the actual food chain system supporting and competing with any given feeder is extremely complex. Added to this is the fact that almost any species of interest will not only consume a variety of organisms at any time, but will show complex patterns of diurnal and seasonal change, and changes with its own size and age. Thus while one may write

$$P_n = P_o \times \overline{K} \times E^n$$

it is a practical impossibility to measure E and *n* independently.

In considering any trophic system in nature, it is the rare situation, such as among some groups of pteropods (Conover 1970), to find that any predator is totally dependent on a single prey. The pteropod exception is, in fact, instructive as Conover and Lalli (personal communication) have found that in areas where the prey pteropods are small, the predator species also remains small and exhibits neotenic reproduction. In situations or years when conditions appear to favour prey growth, the predators increase in size and undergo "normal" morphogenesis, sexual maturation and reproduction. It might be inferred that such extreme food specialization is functionally related to the unusual ranges of physiological and anatomical adaptations in the predator.

Except for such situations, the concept of an animal at one trophic level feeding on animals or plants at another can probably only be clearly defined in the case of herbivores. Beyond this, the approach to a trophic structure consists of a series of untestable approximations. Hence, an investigator's decision to deal with any particular groups of animals as representative of any two trophic levels, will, under conditions of equations (1) to (3), more or less arbitrarily set the average levels of the ecological or production efficiency. On the other hand the expectation that any particular value, such as 10%, may characterize the ecological efficiency, introduces an additional hazard that this fore-knowledge may unwittingly influence the definition and observations of trophic level. Such problems make comparisons among the results of different investigations and investigators doubly difficult.

This problem has been understandingly reviewed by Petipa (1967). She traced the development of attempts at generalization from the initial morphological or "life-form" species-group classifications, through attempts at including physiological and ecological adaptations in some types of life-history patterns where the same species may occur in different classes, towards the larger functional groupings which depend more directly on patterns of feeding. There are difficulties with each, their resolution often depending on the willingness or ability of the investigator to gather and utilize the potentially infinite array of data. Ultimately the decision must be made on the basis of the amount of detail necessary to a given objective. The approach adopted by Moiseev (1969) of defining rather broad groupings such as zooplankton, zoobenthos, and bacteria is the most generally adopted. Until ecology has advanced to the stage where it is possible to generalize about such assemblages, it is still not possible to compare results from place to place with assurance, although recent attempts to standardize the definitions by the careful definition of sampling methods should improve their repeatability.

At present it appears that we still require considerable detail on the structuring and physiology of the biomasses of trophic assemblages within the limits of available sampling apparatus. Various biochemical indicators linked with intermediary and cellular metabolism are showing promise of the sorts of generalizations on which phytoplankton biologists have depended for many years (Sutcliffe, 1970). From the foregoing review it appears that animal size, linked with a knowledge of physiological and ecological parameters underlying size-composition changes is a powerful tool which is already available, but perhaps still underutilized except by the fisheries biologists.

The Elements of Synthesis – The Production-Biomass Ratio

From equations (1a) and (3a) it is clear that one of the most critical factors for improved understanding of production is the turnover rate within any defined trophic grouping. As has been indicated, in a balanced population the turnover rate may be defined as the production-biomass ratio. That is

$$\frac{P}{B} = (F + M) = G$$

where the average total mortality rate (F + M) is equal to the average growth rate of the biomass (G). In fisheries research, major efforts have been devoted to the estimation of F + M and G from size and age data since a fishery may change the initial "balanced" condition towards a new equilibrium, and long-term relative yield is therefore strongly dependent on the age-specific differences of G − (F + M) and its relation to age-specific F. However, as is apparent in fisheries literature, these calculations are plagued by the vagaries of sampling and the natural heterogeneity of population distributions. Furthermore the data as used give estimates only of relative abundance and catch whereas countries are interested in absolute catch and fishermen in catch per unit effort. For this reason, research efforts are increasingly devoted to the direct measurement of total biomass and production change (cf. Edwards 1968).

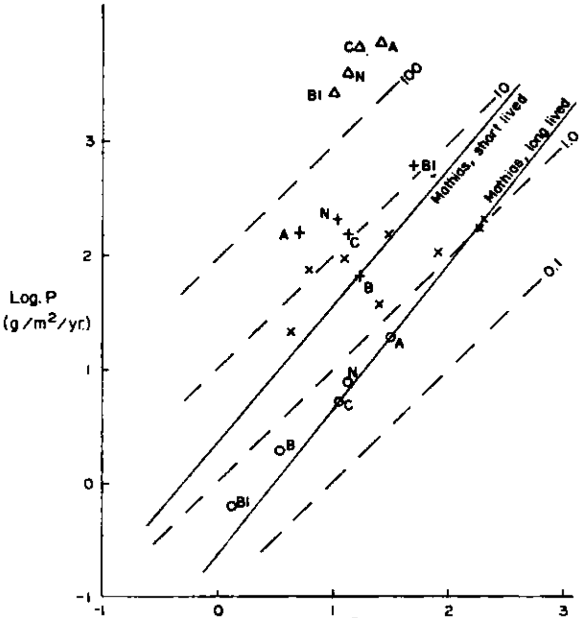

Fig. 1. Production (P) Biomass (B) relationships. The solid lines are the equations calculated by Mathias (vide Mann 1969). Dashed lines are construction lines drawn at log intervals of constant P/B.

△ – Moiseev – phytoplankton
\+ – Moiseev – zooplankton
○ – Moiseev – fishes
× – McNeill and Lawton, data not used by Mathias (Teal, 1957; Tilley, 1968)
A – Azov; B – Baltic; Bl – Black;
C – Caspian; N – North

The same tendency is clear in ecology generally as in the past decade field methods of study and data collection have improved. The present state of the art of direct measurement of production and biomass has been fully reviewed by Mann (1969). An alternative method, based on the similarity of growth mechanisms in all animals (Zaika and Makarova, 1971), has been proposed by Mathews and Mead (1971). Of particular interest in the present context is Mann's summarization of the study by Mathias who found that over a wide variety of species and habitats there is distinct regularity in the relation between P and B. I have plotted Mathias' equations in Fig. 1[3] where the animals are sorted into those with a life-span of more than one year, and those with a life-span of one year or less. Mathias found the results to fall along two more or less parallel lines, separated by about 1 log-interval of the P/B ratio. A similar relationship was recently reported by McNeill and Lawton (1970). They plotted their data in the form of a relation between production and respiration. However, their respiration estimates were reported to be derived mostly from data on respiration of individuals and estimated biomasses. We have therefore consulted as many of their references as were available to us, and replotted those data which were not originally included by Mathias. These additional data are plotted in Fig. 1 with the Mathias' curves. Biomass and production values derived for several partly enclosed seas by Moiseev (1969) were also plotted on Fig. 1. In general they too confirm Mathias's plots, but extend the series to include the smaller plankton. (Some of the variation in the points as I have plotted them undoubtedly arises from my uncertainty in some cases about conversion of data from wet weights to equivalent kilocalories.)

Two features of Fig. 1 seem especially worthy of note. The first is that for the short and long-lived animals and the planktonic organisms the slopes of the relation of log P and log B appear to be parallel and significantly greater than 1.0. The second is that the estimates of biomass per unit area from various areas and trophic levels seem constrained within, at most, a variation of two orders of magnitude. This is in spite of the likely differences in sampling methods for different areas and species. The production values range over more than four orders of magnitude, giving rise to P/B ratios which fall in a range of from about 1.0 to 300.

The problem of comparing turnover rates has been considered in general terms by Waters (1969). Noting that, as Mann (1969) points out, production and biomass can be measured from data on numbers and weights over the life-time of a cohort, Waters adopts the life-time as the basic time unit for his study. He then examines the effects of various combinations of mortality patterns and growth rates on the P/B ratio. He concludes that almost all values must fall within a

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[3] It should be noted that in Mathias' equation for long-lived animals the intercept of the logarithmic relationship was mistakenly reported as -1.683. The correct value, which is closer to -0.400 will be incorporated in the published version and is used in Fig. 1.

narrow range of from about 2.5 to about 4.5, with a strong tendency towards the latter value. He verifies that the average results are unaffected by even large internal vagaries in the shapes of the mortality and growth functions. More important effects result from differences in the ratio of the final to the initial biomass. This result is of course to be expected from any calculation dependent on unweighted averages of initial and final states over a given time period. Since observations in nature are especially likely to involve uncertainty in the assignment of the "end" values, the reliability of the estimates will thus depend strongly on the accuracy of this determination. For example the initial value may be a very high fraction of the biomass in bacterial and phytoplankton populations and the end points difficult to define. Extrapolations of the Allen curves in marine organisms from zooplankton to fish should provide accuracy sufficient for most purposes. Recognizing these difficulties Waters concludes that starting with a given weight, the life-time turnover of cohorts of animals in most natural populations is likely to lie close to an average value of about 4.5 and to exhibit a narrow total range. The important implication from Waters' study is that if we could plot the data from Fig. 1 in terms of life-time of cohorts, the various equations must become a single equation or line passing through a value of about 4.5 for life-time P/B, which will also be an approximate upper limit of the data points.

Put in another way, Waters (1969) study leads to the conclusion that life-time production of a cohort provides a common time scaling for the turnover relationship in natural production systems. An understanding of the mechanisms upon which this scaling depends, both within a given trophic level, and between trophic levels would provide the basis for an objective analysis and prediction of the production.

Scaling the production-biomass relations

The first problem is that of understanding the basis for the array of points along the slope of the generalized P/B curves drawn by Mathias. As pointed out above, P/B ratios calculated from life-time yields will undoubtedly be strongly subject to errors in observations at the end regions, or to inconsistencies in definitions of the life-span. However, for the practical purpose of the prediction of yield, it is not so much the life-time production, as the production over particular periods of the life-history which is of interest. That is, the calculations of interest are the integrals over specified ages or sizes. This differentiation by size is of general interest in predator-prey relationships in nature, since predators and fisheries alike are strongly selective for size in their exploitation. Changes in this pattern, or induced changes in age-specific production due to natural environmental factors, would be expected to give rise to a variety of production and biomass values when production is measured in solar-time units.

This problem was considered by Paloheimo and Dickie (1965) in their attempts to generalize growth equations for fish in terms of the complex possible changes in body size, food intake and physical conditions. They identified four parameters influencing production by individuals, two of which appeared to be of primary biological significance. These they termed α, the general level of metabolism of an organism, and b, a coefficient denoting the rate of which K the gross growth efficiency decreases with feeding rate under various feeding conditions.

Both of these parameters have been the subject of special study. Thus it is known that α changes with a variety of feeding and temperature conditions (Paloheimo and Dickie, 1966). However, the land-mark study by Winberg (1956) established that except under unusual circumstances metabolism tends in nature towards a value approximately twice the standard or basal level usually termed the routine metabolic level. Over a wide variety of animal sizes, the total routine metabolic level changes with body weight to the power γ, and with body weight information given, α indexes departure from this trend. If weights are expressed in units of α, it is evident that the time scales of production curves may be expressed with reference to a standard metabolic or physiological turnover rate. Such a treatment is implicit in Waters' generalized model.

The second parameter, b, has been most recently and fully studied by Kerr (1971*b*). He found that in natural systems it reflected changing availability of food to a predator being especially sensitive to changes in food particle size and distribution related to the relative sizes of predator and prey. These aspects of the predator-prey relationship suggest that b is essentially a space-scaling index. Scaling of rations in terms of b, permits the construction of growth curves on a common spatial scale, related to the ordinary range of feeding activities of the predator.

Observing these properties, Paloheimo and Dickie (1965), proposed a scaling of growth equations in terms of α and b. In these terms the time scale for the production was found to be measured in units of

$$\frac{1}{\alpha^{1/\gamma}\, b^{1/\gamma - 1}} .$$

In this form it will be observed that as values for α and b increase, time as measured in solar units becomes shorter. That is, the producers go through their period of high production and reach their final sizes more quickly. It is clear, however, that in translating from the scaled form to production in solar time units, the two values do not have equal effects. Variations in the numerical value of α have a much greater effect than comparable changes in the value of b. It is equally clear from a study of data, that the possible range of real values of α is considerably smaller than the range of values of b.

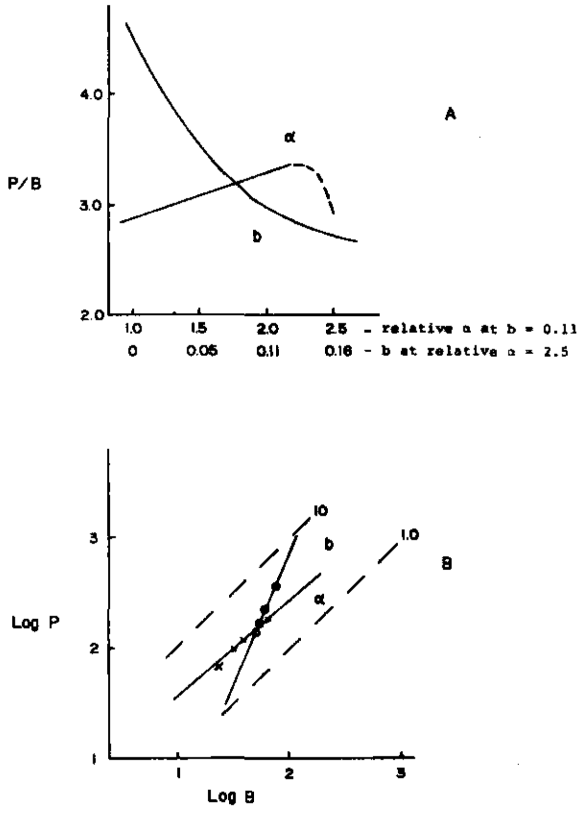

Fig. 2. Production-Biomass relations for populations scaled in life-time units. Data from Paloheimo and Dickie 1970.

We conclude that there is considerable knowledge of mechanisms underlying the variation in P/B relationships at a given trophic level, and that changes in the P/B relations will be reflected in the size-composition of the biomass. A model, scaled in terms of α and b, which may be compared with the steady-state system described by Waters, was calculated by Paloheimo and Dickie (1970). Their figure 3 shows calculated values of mean P and B expressed explicitly in terms of α and b as independent variables. From their data I have derived Fig. 2. It shows in Fig. 2a that over a representative range of values of the two parameters, increasing α and b has opposite effects on the P/B ratio per unit time. It decreases with b but increases with α over the range of routine metabolism, except at high values, close to the level of "active" metabolism, where it decreases again. Of more interest here, however, is Fig. 2b plotted in the manner of Mathias. Here it appears that changing α tends to generate a log P/B line which has a slope very near 1.0. However, the spatial scaling term b, which has a larger overall influence, generates a relationship which is steeper than 1.0 and is reminiscent of the slopes found empirically by Mathias. Since both α and b are measurable in terms of body sizes of the organisms making up the producing biomass, it would appear possible to predict shifts along the P/B line per unit time at a given trophic level from observations of the average size-composition of the populations. That is, size would appear to be an effective device for "descaling" of P/B relations within a given trophic level.

From the foregoing it also appears that, for a given biomass, change in size can be used to scale P/B lines along the production or vertical axis, provided there is a basis for relating body weight to the expected turnover time.

There are a number of possibile approaches to this second problem. One of them would be the gradual accumulation of empirical information on annual P/B ratios for animals of different generation times or sizes in much the same way as Mathias or McNeill and Lawton have done. This is almost certain to be a necessary long-term approach. A first-order approximation to a solution is, however, available in the work of Bonner (1965) who perhaps more than anyone else, has made an appreciation of the significance of body size in biological relationships. We have therefore adapted the information on body length and generation time given in his book (Fig. 1). Most of the data used by Bonner came from animals which reach maximum size at maturity, whereas marine poikilotherms grow throughout their lives, reaching maturity at about one-third or less of their final age. We have therefore assumed that, for larger animals at least, his "generation" times are only about one-third of the life-times in which we are interested. We have also added a general length-weight relationship to convert from length to weight. The resulting equation; relating average maximum weight (W in grams live weight) and expected life time or turnover time (t in years) is

$$\log W = -0.45 + 2.7 \log t$$

$$\text{or} \quad \log t = +0.17 + 0.37 \log W.$$

The inverse of the turnover time in years is, of course the turnover rate or P/B in terms of annual production. That is, Bonner's relationship, or a similar improved relationship for a variety of marine animals, gives an index of the change in the P/B relationship with a change in weight of the animals concerned, or since we are dealing with the vertical scaling of Fig. 1, the change in annual P/B for a relative change in sizes of animals making up a given biomass. For example, from the modified Bonner equation we may derive a table of average weight and turnover times as follows:

Average W (gms)	Turnover Times = B/P (years)	Turnover Rate = P/B (per year)
.001	.11	9
.01	.27	3.7
.1	.63	1.6
1	1.5	.67
10	3.5	.29
100	8.1	.12
1000	19.0	.05

That is, a decrease in average body size by an order of magnitude indicates, on the average, a slightly more than doubled turnover rate.

While it is clear that the relationships between body-size and turnover-times given here are subject to errors, it is equally clear that further observations of production and biomass relationships in natural populations can overcome the deficiencies. The present data serve only to illustrate that knowledge of the combination may lead to a practical methodology for predicting yield potential in specific areas.

Within a trophic assemblage, defined as an observed size-composition of animals taken with a particular sampling gear, a difference in the size-composition indicates different positions on the types of curves drawn by Mathias. Evidently, from Fig. 1, the larger biomasses are generally composed of the smaller more productive animals.

An increase in the rate of fishing on any stock will lead to a decrease in the size-composition of the stock which in its turn reflects a change in the P/B ratio. As a first approximation any such change may be regarded as a shift along the vertical axis of the relationships shown. Relationships of this sort derived from Bonner indicate the degree to which any such shift is accompanied by a relative change in generation time. The increase in production or of yield is, of course, associated with the decrease in the average length of time required for the production.

It is this principle which appears applicable to the calculation of the yield potential of an area of sea. That is, observations of the sizes and densities of fishes available for exploitation provide a reference point on the P/B curves. Observations of the stomach contents of the fish may, however, be used to establish an average size, hence, average production time for the prey actually consumed, and so on. If trophic exchanges, which themselves appear strongly dependent on relative predator-prey sizes, are approximately equal, the summed observations provide a direct estimate of total transmission time, as well as an estimate of the number of steps between any chosen trophic level and any higher level. The potential yield would, of course, depend on the acceptable sizes of fish in the catch and the acceptable level of catch per unit of effort. If the calculations can be carried to the phytoplankton, this should provide a substantial base for area-to-area comparisons. Information on these earlier stages of food-chains would be essential to an understanding of the system which would permit confident extrapolations. It is also essential to our understanding of effects of changes in environmental conditions. With such data the approach offers the possibility of objective measurement of the turnover times and associated productions, independent of the judgements involved in *a priori* selection of the trophic assemblages. Equations 1 and 1a suggest that the results provide estimates of conversion efficiencies and rates of uptake which may be checked by experiments.

Evidently there is a need for refinement, but this too appears to be practical. For example, while the predator-prey size relations may be especially difficult to determine for the very small animals, the rapid turnover times associated with these animals do not add significantly to the total turnover time involved. The essential information from them is the number of levels involved. Satisfactory estimates of turnover times can probably be made from observations of the first two or three "links" in the chain. A second, more important refinement, and one which will undoubtedly be necessary for predictions of sufficient accuracy for fisheries purposes, involves an estimate of biomass. The data of Moiseev, plotted on Fig. 1, indicate that there is high stability in the average biomass of different trophic levels in the Caspian, North and Azov Seas, although in the latter case the zooplankton abundance appeared to be relatively low.

Recently, Sheldon, Prakash and Sutcliffe (1972) have found that for various widely separated ocean areas, the concentration of different sizes of particles per unit volume is remarkably constant over 6 logarithmic intervals of particle size. They suggest that some of the variations in biomass which have been reported may rather reflect the sampling methods and size classifications which have been used. If their hypotheses are confirmed by further experience, the type of size and time scaling suggested here could lead to highly satisfactory precision in yield predictions. Presumably this assurance can only come from further refinements in the measurement of biomass in natural surroundings.

It seems reasonable to conclude that further study of the time scales of production/biomass ratios in relation to sizes of organisms could lead to significantly increased reliability of estimates of marine production. The yield which can be realized from natural production is a separate problem which is briefly considered in the next section.

Production in the Northwest Atlantic

The ICNAF area in the Northwest Atlantic is well known as one of the most heavily fished regions of the world. In Table 1 we show calculations of landings per unit area (tons/km^2/yr) based on the most recently published statistics. In making the calculations we have used recent charts to re-measure each of the designated Subareas within the ICNAF region. For comparison, with other world fishing areas, we present in Table 1 data provided by Gulland (1970) for the Northeast Atlantic and the Northwest Pacific.

The data show that the overall production per unit area is almost the same in the three regions. The two

TABLE 1. Comparative estimates of annual landings per unit area from northern seas.

ICNAF, 1966-68, Fish landings – tons/km^2/year

	Area	Pelagics			Demersal					
Subarea	000 km^2	1966	1967	1968	Avg.	1966	1967	1968	Avg.	Overall average
1 (B, C, D, E)	87	–	–	–	0	3.06	3.91	3.52	3.50	3.5
2	105	–	–	–	0	3.50	3.11	4.51	3.71	3.7
3 (K, L, N, O)	290	0.01	0.01	0.01	0.01	1.89	2.87	2.75	2.50	2.5
3 (P_s)	52	0.42	1.49	2.01	1.30	1.74	2.04	2.18	1.99	3.3
4 (R, S, T, V_n) $3P_n$	159	0.32	0.47	1.10	0.63	1.55	1.72	1.74	1.67	2.3
4 (V_s, W)	105	0.05	0.04	0.21	0.10	1.52	0.99	1.57	1.36	1.5
4X	55	3.51	3.54	4.20	3.76	2.14	1.99	1.64	1.92	5.7
5Z	70	2.03	3.27	5.65	3.65	7.15	5.40	3.73	5.43	9.1
Average 3, 4, 5	731		0.9				2.4			3.2
Overall	923		0.7				2.6			3.3

ICES,[a] 1968, Fish landings – tons/km^2/year

Subarea	Area 000 km^2	Pelagics	Demersal	Total
NE Arctic (I, II)	1,300	1.0	1.1	2.1
North Sea (IV, IIIa)	600	3.3	2.3	5.6
Overall	1,900	1.7	1.5	3.2

Northwest Pacific[b], Fish landings – tons/km^2/year (estimated for 1966)

Subarea	Area 000 km^2	Pelagics	Demersal	Total
Sea of Okhotsh	580	0.7	1.0	1.7
Sea of Japan	200	2.3[c]	6.0[d]	9.3
Yellow & East China Seas	950	0.6	1.9	2.5
China South Coast	280	0.6	2.5	3.1
Gulf of Tong King	200	0.5	1.3	1.8
Overall	2210	0.8	2.1	2.9

[a]Data from Gulland (1970), Table B3; [b]Data from Gulland (1970) Tables D3, 15, and 16; [c]Includes squid; [d]Includes Alaska pollock and sand lance.

sides of the Atlantic each yield approximately 3.2 tons/km^2/yr and the Northwest Pacific 2.9 tons/km^2/yr. Since the latter calculations are based on an older set of data (1966) it is doubtful if the Pacific yields are actually different from those in the Atlantic. The tables indicate that major differences occur from north to south within regions, and that there are also large differences in the division between pelagic and demersal components of the catch within and between regions.

At the present time there are no food-chain data available which would permit us to make direct deductions about the differences in food-chain productivity within and between the various regions. In the ICNAF area in particular, we do not yet have estimates of the annual primary production by regions. Nor do we have sufficient information on the size-distribution of foods taken at successive trophic levels, although there is a gratifying increase in the information on dietary habits of the principle commercial species. Except for the estimates of Edwards (1968) data on production and biomass in the trophic system are also missing. He estimates that in the Georges Bank area the ratio of annual fish catch to average total standing biomass of fish is approximately 1:4. Since the fishery exploits only the larger sizes the ratio of production to exploited biomass is likely of the order of 1:2 or less. This is, of the same order as deduced by Moiseev for several heavily fished European areas. Despite deficiencies, these data on yield per unit area permit some preliminary interpretations which provide a background for judging the needs for future research efforts.

Differences in the annual yield per unit area among regions are the combined results of several factors. Underlying all other effects are the differences in basic biological productivity. Added to this are several important fishery factors. For example, the intent of fishing interests is to obtain the highest possible yield per unit fishing vessel operation. This yield will be judged in terms of the market acceptability of the various products which can be realized, resulting from a combination of market or product development and fishing technology, related to the sizes and densities or availabilities of the fish that can be taken. Such factors affect the distribution of fishing effort on various grounds. That is, present yield densities result from a complex interaction of biological, economic and social factors, all of which undergo gradual related changes with time.

Without detailed independent information on biological, economic and social factors for different areas it is clearly impossible to determine the causes of the tabled differences. However, the fact that densities of overall yield in the various major fishing regions of the world are about the same, should not be especially surprising. A significant part of world fishing, especially in the offshore and more northern regions, is carried out by large mobile fishing fleets. The economic managements controlling these fleets are certain to disperse fishing effort in such a way, that over a period of time returns per unit effort from the various available world areas will tend to approach similar values. These considerations are likely to be predominant factors determining levels of yield for northern seas, far removed from markets and ports. In these regions, it appears that maximum present yields are of the order of 2.5 to 3.5 tons/km^2/year for demersal species. Since in such cases it is reasonable to conclude that economic factors have become more or less balanced, the data give no reason to suppose that biological productivities in these regions are very different from one another.

In the ICNAF area, a possible exception to this generalization lies in Subarea 2, the Labrador coast. The history of large trawler fishing there is relatively short. In such a situation, the stocks of fish, primarily long-lived cod and redfish, must consist of a considerably larger range of sizes than will characterize a more nearly equilibrium condition. It may therefore be expected that if the technology of fishing and the total fishing effort does not change significantly, the yield per unit area will eventually drop. This may not happen quickly, since there is still some likelihood that new grounds and stocks are being uncovered. If the fleet fishing the banks is the same as that exploiting the Greenland area, the yield per area may level off to about the same level as in Subarea 1. Otherwise, a drop to about the level of yields from the Grand Banks is to be anticipated.

The tables show a substantial difference among regions in the relative proportions of pelagic and demersal species, as well as in the absolute yields of pelagics per unit area. These are of considerable interest ecologically, since the food-chains leading to pelagic species are generally considered to be shorter than those supporting demersal fisheries. However, meaningful comparison of pelagic yields among the southern regions, where they are generally higher, is difficult to make. In the ICNAF area the pelagic fisheries, mostly on herring, are relatively new so that it is doubtful if the data up to 1968 represent an equilibrium condition, relative to the Northeast Atlantic and Northwest Pacific. These pelagic yields in the ICNAF area may reasonably be expected to decline with present fishing methods and distributions. The North Sea pelagic fisheries have a yield comparable to that in the Northwest Atlantic, but this includes species such as sand lance (0.3 tons/km^2/yr) which are not yet exploited in the Northwest Atlantic, and the pelagic landings data in the Northwest Pacific are also difficult to interpret since they include squids, but exclude some 0.5 tons/km^2/yr of sand lance (which are included with the demersal landings).

Low landings of pelagics in the northern parts of the ICNAF area do not necessarily indicate low pelagic production. They indicate rather, that the species which may be available, mostly capelin, are apparently not yet considered sufficiently marketable to warrant a fishery development.

On balance it would appear likely that pelagic fish stocks have considerably higher production than do demersal stocks. However, as must be clear from the foregoing review, (see equation 3a) higher productivity associated with shorter food-chains inevitably means a smaller average biomass. To realize the potential harvest therefore requires a generally more efficient fishing effort. We return to this point below, in connection with the demersal fisheries.

The demersal fisheries of the northern hemisphere have a rather longer history than do the pelagic fisheries. Comparisons among Subareas and between regions therefore seem more likely to provide meaningful results. The most striking feature of the table is the high yield per unit area in those parts of each region near major concentrations of human populations. In the ICNAF region, Subareas 5Z and 4X share this characteristic with the North Sea and the Sea of Japan.

Table 1 shows yields from the Georges Bank area of about 5.0 tons/km^2/yr of demersal fish out of a total of about 9.0 tons/km^2/yr. Comparable yields in the Sea of Japan are 6.0 from a total of about 9.0. North Sea yields are lower, about 2.5 if the sand lance are included, out of a total of 5.5. It is particularly unfortunate that we are not in a position to identify the natural and fishing effects contributing to these differences. For example, Moiseev (1969) concluded that fish yields in the Azov Sea, prior to regulation of the Don River, were of the order of 8.2 tons/km^2/yr, or comparable to the ICNAF and Sea of Japan estimates. He suggested that this difference between the Azov and North Sea yields may be due to the comparably higher primary production in the Don estuary. This would suggest that primary production in the Georges Bank and Sea of Japan areas might also be substantially higher than in the North Sea. However, this is not borne out by the preliminary information available. Earlier analyses by Riley (1944) showed a rate of primary production on Georges Bank which was not appreciably different from subsequent estimates for the North Sea (Steele, 1965). Estimates given by Fukuda for the Sea of Japan (Gulland, 1970) are the same. Until further information is available, it would appear that differences in primary production cannot be invoked to explain regional differences in fish yields.

On the other hand, it seems likely that fishing effects, operating on the food chains can explain the yield differences between regions, as well as differences between Subareas within regions. In the foregoing review we have stressed the importance of the effects on yields or ecological efficiences of changes in food-chain length, as these are reflected in changes in the P/B ratios. That is, if we rewrite equation (3a) in the form

$$Y = (qf) \times (KI)_n \times (B/P)_n$$

it can be seen that yield (Y) is proportional to the product of the fishing mortality (qf), the food conversion (K) and food intake (I) of the exploited species n, and the inverse of its P/B ratio – $(B/P)_n$. As fishing increases, the quantity (B/P) decreases. To maintain a constant yield therefore requires that the product $(qf) \times (KI)_n$ increase sufficiently to offset (B/P). Paloheimo and Dickie (1965, 1970) argued that with increased fishing, the decreasing size of fish would lead to an appreciable increase in both K and I, and Steele (1965) showed evidence for an effect of this sort on the North Sea haddock and herring landings. In total, however, the effects do not appear capable of explaining the entire difference between regions. In fact, it would appear that a substantial proportion of any such effect must already be included in the data.

We are left to consider possible changes in the effectiveness of fishing, qf, which, following Baranov, we earlier suggested should be written in the form csf, where c is the coefficient of fishing efficiency per unit area, s a scaling factor describing the relative effective area covered by a unit operation of the gear, and f is the number of gear operations. In this context, it would appear especially significant that the high yields per unit area in the northern hemisphere are all in areas where the major part of the fishery is pursued by small boats, operating on short trips of the order of one to 10 days long from home ports. Catches generally consist of a mixture of species, some of relatively small size.

With small boats, the number of fishing units increases; that is, the number of fishing units f, is increased. Within a given area of operation this increase is undoubtedly associated with a sacrifice in the area s, covered per unit operation, although relatively large engine sizes are undoubtedly a device to compensate for this. However, it is also likely that the elementary efficiency of operation is larger for a series of small vessels, than for a comparable area swept for a large vessel. This effect must be larger, as the average density of fish decreases if for no other reason than that small craft will have a greater capacity to search out and make use of small local fish concentrations. This effect is comparable to the increased "grazing" efficiency which

Paloheimo and Dickie (1965) found to characterize the foraging activities of smaller predators. That is, we are implying that a number of small fishing units is capable of more efficient fishing than a comparable tonnage of large vessels, and can thus better sustain yields with high P/B ratios.

This hypothesis, while reasonable to the author, has not been specifically verified for fisheries, since most research on fishing effort has been devoted to studies of the operational and economic success of the very large mobile trawlers. If it is verified however, it suggests that with present technology the yields of northern and offshore areas cannot be expected to reach levels which characterize the southern areas. To accomplish this would require major changes in fishing technology or fishing economics which would permit fisheries to take advantage of generally higher production rates at a necessarily reduced level of catch per unit effort, expressed in terms of present-day fishing units.

A corollary to this conclusion is that future maximization of fishing yields is likely to increasingly favour the deployment of larger numbers of small and intermediate sized fishing units. Such a conclusion has sufficiently important implications for future fisheries planning and management to justify specific fishing effort studies in relation to the production-biomass ratios of the exploited populations. The strategy chosen would be expected to have a bearing on the predicted possible sustained levels of fish yield from a given sea area.

Discussion and Conclusions

The foregoing review has been designed to exhibit some of the principle features of trophic or food-chain production systems that can be deduced from work in aquatic ecology and fisheries over the past ten years. On balance, it does not seem that the task of predicting the potential fish production from an area of the sea has been simplified. That is, recent work has not revealed any unsuspected short-cut methods which would allow significantly improved predictions from general information. Clearly, there is no substitute for real data on natural systems. However; it does appear that our new knowledge offers the kinds of insight into ecological relations from which it should be possible to understand the value of particular sets of observations and experiments, and upon which we may eventually build an improved methodology of prediction.

Perhaps the most important advances which have been made during the decade concern the new generalizations about the relations of production and biomass in natural populations. The theoreticians among us have long recognized the interdependence of these quantities. However, it has not been until the development of improved methodology of direct measurement over the past decade, associated with better calorimetry, respirometry and growth measurements and improved sampling techniques, that it has been possible to develop the kinds of relationships reported by Mathias and by McNeill and Lawton for animal populations in their natural habitats.

In this review, we have compared the observed P/B relations with population models based on energetics considerations. As developed by Paloheimo and Dickie (1970), these models rest on two critical features. One is that the growth production is responsive to feeding conditions which are empirically measurable in terms of food abundance and food distribution. The second is that the population system as a whole is limited by the resource energy available. The fact that P/B curves deduced from these models appear to predict the relations found in natural populations, constitutes a kind of preliminary test of the hypothesis that resource limitation is a valid base from which to interpret and predict population effects within trophic levels. However, logarithmic plots are not very sensitive instruments for analyzing and predicting yield variations on the scale that are of importance to fisheries. Other tests of this hypothesis, including further study of its applicability to exploited fish populations are needed and will undoubtedly emerge in the next decade. The results to date strengthen the prospect that the bioenergetics approach will be most useful in this development.

Our recently increased appreciation of the unity of growth processes in organisms, (Bonner, 1965, Zaika and Makarova, 1971), coupled with the new information on P/B relations, emphasizes the significance of body size in interpreting energy transfers in the trophic system, both within and between trophic levels. Of particular value to the understanding of productivity in a body of water is the implication that we may be able to make fuller use of the size relations and biomasses quantities which are relatively immediately measurable, as powerful tools in the tracing of energy pathways from primary production to fish. While these findings indicate that we may be able to overcome the obstacle of unsatisfactory definitions of successive trophic levels, the data we have at present are obviously inadequate for the immediate task of yield prediction.

The review indicates the central importance of the Production-Biomass relations in understanding ecological and production efficiencies in relation to fishing pressures. There is, for example, little doubt that although the terms relating them in the equations may be independently measurable, they are clearly interdependent functionally. This fact leads to speculations about effects which are likely to turn up in our models and may need consideration in real systems. In the foregoing, we wrote equation (3a) as

$$\frac{P_n}{P_{n-x}} = qf_n \times K_n \times \frac{B_{n-x}}{P_{n-x}}$$

where x was 1.0, i.e. the prey population. If however, we write the equation for a four-step food-chain, we have

$$\frac{P_n}{P_{n-3}} = \frac{(qf_n \times K_n \times B_{n-1}) \times (qf_{n-2} \times K_{n-2} \times B_{n-3})}{(qf_{n-1} \times K_{n-1} \times B_{n-2}) \times (qf_{n-3} \times K_{n-3} \times B_{n-4})}$$

We earlier noted that if the production ratio in two successive trophic levels tends to be stable, then for any change in body size in level n-1 there will be a tendency for an inverse correlation between qf_n and K_{n-1}. In this formulation it would therefore appear that a high $qf_n \times K_n$ is inversely correlated with $qf_{n-1} \times K_{n-1}$. That is one would expect the terms within the numerator or denominator to be positively correlated, while the numerator and denominator are inversely correlated. The implication is that production efficiency may tend to be higher for odd-numbered food-chains than it is for even-numbered chains (Smith, 1969). The even-numbered steps in the food-chain will have a tendency to exhibit high production and the odd-numbered steps low production. If this effect were present in nature, we could only conclude that an objective measurement of food-chain length is of considerable practical importance in fisheries where the pre-adult and adult fish may have effectively different food-chain lengths. Fishing alters the proportions of the various links which are present.

The foregoing arguments were developed from our consideration of an organism as a prey organism. However, within a trophic system an organism is both predator and prey. As a predator, an organism at any trophic level is faced with competition, which must generally exert a selection pressure to ensure its production by remaining small. That is depredation leads to development of larger body size, while competition works in the opposite direction. The net result of these two forces must be strongly dependent on environmental circumstances. Ultimately its outcome will depend on conditions at the base of the food-chain, in which the size-compositions, abundances and distributions of the phytoplankton must play an important role. This fact has been well-recognized by Parsons, LeBrasseur and Fulton (1967) and Parsons and LeBrasseur (1970). From such work, one might expect that natural systems may be characterized by some kind of "optimium" average relation of predator-prey sizes, but that this may be rather different in different geographic regions. It is the demonstrated importance of such features which makes prediction of potential yield dependent on specific size and abundance information in the area of interest.

At first sight the foregoing considerations seem to be of interest only to the biologists. However, their implications would appear to bring the fisheries administrator and manager face-to-face with a very fundamental question, – one that cannot long go unanswered. In the past, fisheries administration has focussed attention on the question of how to maximize the yield from individual species. This will undoubtedly always be an important consideration, because fishing and processings costs and market values tend to differ among species. However, the findings we have reviewed here are only part of the rapidly growing realization that our present exploitation is not simply affecting species. What we are dealing with is the effects of fishery exploitation (and increasingly of pollution) on the properties of ecosystems. Fishing effects are now sufficient to significantly alter the structure of these systems, by shortening or lengthening food-chains and altering the relative importances of predatory and competitive relations within them. These changes affect the average relations between production and biomass in a manner that we understand better than ever before. But they also affect their stability, a factor which has special importance in the economics of fishing and in the effectiveness of regulatory measures.

There is, of course, no *a priori* reason why an economic or regulatory system necessarily has to have a complete correspondence with a natural system. They overlap. However, the consequences of various different definitions must soon become a major problem to be solved jointly by biologists, economists and administrators.

Finally, we must conclude that the statistical information that we have for the ICNAF fisheries would appear to be an invaluable background for the further study of the problems of yield prediction and regulation. It is clear, however, that for the ICNAF area the scientific information on which improved long-range yield prediction may be based, or from which we can predict consequences of ecosystem interactions, simply does not now exist. If this is a requirement for intelligent planning and management, and I believe it is, then the governments and international Commissions concerned will have to set about obtaining it by deliberate support of the research which can provide it. One fact emerges very clearly from the ICNAF catch records: By 1968 the ICNAF area was, per unit area, the largest producer of demersal fishes in the Northern hemisphere. It is likely that this reflects an equally unprecedented rate of exploitation. In the future, it is clear that we cannot look to other regions of the world for precedents in the matter of judging the prospects for increasing the fishery yields. What happens in the Northwest Atlantic may well be setting the precedents.

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Discussion

Dr Cole: *"I am not sure that one can comment very usefully on a paper like this without a great deal of thought. I had the suspicion once or twice while I was listening to Dr Dickie that really he was masking in scientific language a lot of truths that we have realized for a long time. For example, I think he was telling us that if we are fortunate enough to be able to eat plankton then we could get a lot more of it from the sea than we can of fish and I think that's something that has been known for some time. What I would like to see are the biological considerations in this paper married up to a series of economic appreciations and I think that was the sort of thing that Dr Dickie himself was saying towards the conclusion of his paper. I would like, however, to take issue with him in his Table 1 where, in the statement that the southern parts of the ICNAF area are more productive in landings per unit area than any other part of the world. It seems that he is not really making fair comparisons.*

"If you look at the area, for example, of ICNAF Division 5Z, which gives you a nice overall average of 9.1, and compare it with the North Sea, 5Z is 70,000 sq km and the North Sea is 600,000 sq km. Now, what I have done and frankly, very quickly and no doubt inaccurately, is to calculate the weighted mean for 3Ps to 5Z put together which comes to 441,000 sq km and the North Sea which is 600,000 sq km. So we are getting to the same sort of general size of area and I find that the ICNAF group comes out at slightly less than the 4.0 whereas the North Sea is 5.6, and the other remaining part of the ICNAF area – Subareas 1, 2 and the rest of 3 – comes out at about 2.8. I am merely making this comparison to make a further point about the North Sea in which I am very interested and which I can legitimately make because Dr Dickie mentioned the effects of pollution. I have been very much concerned with trying to measure the effects of pollution on fish stocks and I am looking rather critically at the North Sea catches because the North Sea, I think, is as is demonstrated here, one of the major fishing areas of the world and yet it is also one of the most highly populated areas, and has probably a great deal more pollution per unit exploited area than any other part of the world, except perhaps some small areas in Japan that might call for first place in polluted regions. If, however, you look at the gross catches of the North Sea, just in a very simple way to start with, you find that they are in fact at record levels and have been so during the last 5 years; the gross catches are higher than ever recorded before and you have some 60 or 70 years of history to consider. Then somebody says, of course, you have cheated because you have exploited a lot a sand eels, Norway pout and other things for fish meal that you didn't exploit before. That's perfectly true so let's take these out and then they say, of course the Norwegians caught half a million tons of mackerel that you didn't know were there, so let's take them out as well. When you finally come down to cod, haddock, and plaice which we have been exploiting heavily for generations, you still find that the catches are at the moment at record level and also that the catch-per-unit effort has gone up in recent years. So you are in some difficulty in trying to prove that pollution in this area has damaged the fish stocks. But, of course, this is really only half of the story and one might finish in the sort of way that Bruce Halstead favours by saying that you may get plenty of fish but none of it will be good to eat because it all contains quantities of mercury, copper, zinc, pesticides, and God knows what."

Dr Dickie: *"Thank you very much, Dr Cole. Actually you have been making my point for me. I wish that, when I was preparing Table 1, I had had a much longer series of data readily available. In the case of the Georges Bank area, there have been tremendous changes in the past few years. You will notice on the last line of the diagram or the last line of the first part of the Table there has been a very marked drop in the total demersal production in the Georges Bank area from over 7 to less than 4 tons per sq km per year in the 3 years for which I had published data. So there is no sign that the very high productive situation that we have here is actually an equilibrium one. On the other hand, Graham and Edwards (H. W. Graham and R. L. Edwards. 1962. In: Fish in Nutrition, edited by E. Heen and R. Kreuzer, London, Fishing News (Books) Ltd., 1962) produced figures on this in the early 1960's. Edwards (R. L. Edwards. 1968. In: The future of the fishing industry of the United States, edited by DeWitt Gilbert, Seattle, University of Washington Publications in Fisheries, N.S. No. 4, 1968) again produced estimates in 1968. Both agree quite well with the mean of the estimates that we have available for the Georges Bank area. I conclude that, although we have fluctuations above the mean, in the small Georges Bank area we are dealing with an area which has a fairly long tradition of high production – probably of the order of that which has also been known for the North Sea for a long time. The important conclusion is that we cannot see any signs of overall long-term downward fishery trends as a response to fishing and indeed, on the basis of the arguments I was trying to advance, I would not have expected to see any. But, in addition, I believe that the explanation for the high continuous productivity in the Georges Bank* *and North Sea areas, compared with other regions is more likely to be found in the differences in the elementary fishing effectivenesses of the fleets rather than in the basic difference in productivity.*

"We badly need information on the basic productivity for the ICNAF area, information comparable with your very good data for the North Sea. This is necessary before we can hope to understand the extent to which any of the productivity differences that show up could be ascribable to fishing and what to other causes. From the kind of evidence we have from the ICNAF area at the moment, I do not see how we can pretend to detect any kind of biological overfishing. For the same reasons we are in a poor position to decide whether pollution may have a long-term deleterious effect on the potential yield from any of our sea areas."

Mr Hennemuth: *"You have in fact covered a couple of points I was going to bring up and that is the business of dynamic versus static models. This one looks very static. This, of course, is not really the case in nature. The accuracy of the production rate as measured by catches on Georges Bank does depend upon where you are on the yield curves implied in your equations. The other thing is that your A (scale factor in the fishing effort relation) is, I would guess, an economic factor. If I understand, what you mean by S, it is not true for Georges Bank. The large vessels which you say are uneconomic compared to the small ones have been the ones which have increased the catch, and which have given these rather large production figures.*

"In terms of your model, I do not quite see the importance of this scale factor economically, it may be very important as you say, but not in the system of production in relation to biomass. Also, I think that obviously some second order terms are important here – I think, both F and K are dependent upon the B/P ratios – I do not know to what extent – and you probably just can not throw in second order effects on the end of your equation and expect to gain much. The interrelation of terms in your equation may have something to do with the environment. Perhaps K is about the only one on which the environment might have a significant effect in these equations. You did remark that you thought K was rather constant. On what basis do you say this?"

Dr Dickie: *"Perhaps I will deal with your last point first. There is evidence, which I tried to review in the paper, showing that K is, in fact, capable of considerable change, probably through at least a two-fold range. Furthermore there is a likelihood that K is inversely correlated with the B/P ratio. So that, in a sense, when we start fishing smaller and smaller animals such as the sand eel for example, one would expect B/P to become rather small in the sense that the production of sand eels tends to be very high for any given standing biomass at any one time – for any small animal this must be the case. Correspondingly we have an increase in K. However, the yield differences that we are looking at among different areas are substantial. The question is then, "What other parts of our equations are being affected?" I would be very surprised if the effects that we are seeing here are not similar to those which Kerr and Martin (S. R. Kerr and N. V. Martin. 1970. In: Marine Food Chains, edited by J. H. Steele, Edinburgh, Oliver and Boyd, 1970) postulated to account for a maintenance of yield with differences in food chain length in their paper presented at the ICES/FAO/ICNAF/UNESCO Food-Chain Symposium, July, 1968. He found that planktonic feeders and fish feeders have a marked difference in grazing efficiency. That is changes in grazing efficiency go along with the change in the food system itself. The same must be true when the predators are fishing vessels. According to the equation areas of high yield with low B/P ratios must then involve the fishing efficiency in the compensatory mechanism. With many small vessels the F component in the term FSC goes up. S must certainly go down, almost in proportion but probably not quite. The question is whether C changes in a manner similar to the grazing coefficients for small fish. I do not think our data are sufficient to test a hypothesis about this which would explain the kinds of differences between areas, but everything points to important effects involving them. What I was trying to say is that, in the face of such evidence, we have a responsibility to do more explorations or experiments with the parameters of fishing efficiency itself.*

"I know I have not really answered your question except to say that we know that K can go up, but so far the evidence looks as though this is not enough to compensate for the decrease in B/P, hence fishing efficiency must be involved. In conclusion I agree that it is a terribly static model I have put up here. However, I can see no real barrier to turning it into a thoroughly dynamic model as this appears necessary to describe reality and suggest testable hypotheses."

Contribution Number 11

Contributors to the Discussion

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Year-Class Success in Some North Atlantic Stocks of Cod and Haddock

By Wilfred Templeman[1]

Abstract

Data on year-class success for the past 30 years of most of the main stocks of cod and haddock in the North Atlantic have been reviewed. There appear to be area relationships for year-class success of these species, between success of haddock on Georges and Browns Banks and of year-class success either of cod or haddock or both on Sable Island Bank, St. Pierre Bank and Grand Bank 1 year later or sometimes in the same year. Success in the Icelandic, Greenland and Norwegian-Spitzbergen-Barents Sea areas may occur in the same year as but usually 1 year later than good year-classes in the Sable Island to southern Grand Bank area or 1 or 2 years later than haddock success on Georges and Browns banks. The pattern was most definite and regular in the first 16 years of the period. In recent years there appears to have been a shortening of the period of relationship between the western and eastern areas.

Some of the factors in year-class success appear to be: temperature, higher temperatures being favourable for cod toward the northern and lower temperatures toward the southern limits, but temperature in itself may be of secondary rather than of direct importance; drifts of larvae in currents to unfavourable situations; mixtures of water by winds and currents, the resulting high plankton abundance being favourable; variations in predation and in competition with other fish species; levels of spawning stocks, which when very low require unusually good environmental relationships for year-class success and when high may reduce larval survival by density dependent factors. Food size, abundance, and quality must affect growth and hence mortality of the fish larvae through greater predation on smaller, weaker and slower-moving larvae. To take best advantage of available food, fish must spawn and larvae must hatch at the time of production of the appropriate size, quality and quantity of food in the plankton bloom. The spawning time of cod and haddock, which usually spawn in the early part of the year, is determined mainly by light and temperature conditions in the deeper water, the hatching by water temperatures in the upper layers, and the plankton bloom by light and temperature and other conditions in the upper layers.

It is evident for stocks which have for many centuries existed in abundance, that natural selection must on the average have brought the time and location of spawning into adequate relationship with the plankton blooms and with currents which distribute the larvae to suitable nursery grounds. With the changes introduced by heavy fishing, the small fish remaining will have shallower depth, lesser area and spawning time, and often different egg and larval size relationships than the larger fish which have disappeared, or the average of the former spawning population. It will therefore again be necessary for natural selection to act to bring spawning and hatching times on the average into a successful relationship with the plankton bloom. This can only occur by a greater than average mortality of the progeny of fish which are not attuned to the plankton bloom and, since the process must be slow, a considerable period of lower average abundance may be experienced.

On occasion many other environmental factors may dominate so as to control year-class success, imposing their control on the basic or average pattern.

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ICNAF SPEC. PUBL. NO. 8

Introduction

I shall review only year-class strengths of most of the main stocks of cod and haddock in the North Atlantic. Many of these stocks have been studied intensively for a long period. Much information on herring is available from the eastern Atlantic and Northeast Arctic but for the western Atlantic herring little comparative information on year-class strengths is available and for recent years this is confused by apparent differences in age reading.

I have been unable or at least unwilling to confine myself to the past decade as much of the information for the last part of the decade is often imperfect from research vessel studies of pre-recruits and the most complete information from virtual population estimates often does not extend beyond the previous decade. Also, there may be only one or two successful year-classes and often no very successful year-classes in a stock of fish during a particular decade. Because the recent decade has been one of rapidly declining stocks of mature fish, a longer period is also needed for comparison and perspective.

The period of 1941-70 has been chosen. For this period there have been intensive studies of a number of stocks, some of the earlier errors due to reading of scales instead of otoliths in cod and for the greater ages in haddock have disappeared as the use of otoliths for ageing these fish became more common, and more length frequency and age information has been available from research vessel surveys for the young fish of pre-recruit sizes.

The data used for selecting the good year-classes are occasionally relatively quantitative as for the Northeast Arctic stocks, for Georges Bank, 4W, and North Sea haddock and West Greenland cod. In other cases they have been qualitative from impressions of relative year-class strength in successive years and may therefore be only the largest among small year-classes. For other stocks data have been partly qualitative and partly quantitative.

Because information is often a year or more late before it is published, I cannot always be certain that statements made on year-classes since about 1963-65 are completely accurate but they are the best that can be made with the information available to me.

Materials and Methods

In composing the list of successful year-classes (Fig. 1) for cod in Subareas 4 and 5, information on year-class success has been obtained from: Wise and Jensen (MS, 1960), Martin and Kohler (1965), Paloheimo and Kohler (1968), Garrod (MS, 1968) and Halliday (MS, 1971). For information on year-class success in cod in Subareas 2 and 3, the following are important references: May (1959, 1965), Fleming (1965), Williamson (MS, 1965), Pinhorn (1969; MS, 1971*a*; MS, 1971*b*), Bulatova (1970), Konstantinov (1970), and ICNAF Research Reports of various countries from Redbook, Part II (1963-70). Knowledge of year-class success in cod of Greenland, Iceland, and the Northeast Arctic has been obtained from: Hermann, Hansen and Horsted (1965), Templeman (1965*a*), Garrod (MS, 1968), Jónsson (1969), Meyer (1969), Samarvov (1969), Schumacher (1970; MS, 1970), ICES (MS, 1970; MS, 1971*a*; MS, 1971*b*), Horsted (MS, 1971), ICNAF Research Reports of various countries from Redbook, Part II (1965, 1970), Meyer and Lenz (MS, 1971), and from Smidt (MS, 1971).

Information on year-class success for haddock was obtained from: Kohler (1958), Saville (1959), Hennemuth, Grosslein and McCracken (1964), Martin and Kohler (1965), Templeman (1965*a*), Hodder (1966), Jones and Jermyn (1969), McCracken (1968), Graham (1969), Meyer (1969), ICES (1969*a*; 1969*b*; MS, 1970; MS, 1971*a*; MS, 1971*b*; MS, 1971c), Grosslein and Hennemuth (MS, 1970), Halliday (1970; MS, 1970), Hodder, Chaulk and Cluett (MS, 1970).

From the above material an attempt has been made in Fig. 1 to identify the successful, very successful and exceptionally successful year-classes of cod and haddock of the main stocks and areas during the past 30 years. For a few of these stocks and areas comparative numerical values for comparison of year-class success are available for all or part of the period: for 4X cod in Halliday (MS, 1971), for the southern Gulf of St. Lawrence (4T) cod in Paloheimo and Kohler (1968), for Labrador (2J) cod in Pinhorn (MS, 1971*a*); for West Greenland cod in Schumacher (1970; MS, 1970) and Horsted (MS, 1971); for Icelandic cod in ICES (MS, 1971*a*); for Arcto-Norwegian cod in ICES (MS, 1970; MS, 1971*b*); for Georges Bank haddock in Grosslein and Hennemuth (MS, 1970); for Nova Scotian Shelf (Subarea 4W) haddock from McCracken (1968) and Halliday (1970; MS, 1970); for Icelandic haddock in ICES (MS, 1971*a*); for North Sea haddock from Saville (1959) and ICES (1971c); and for Arcto-Norwegian haddock in ICES (MS, 1970; MS, 1971*b*). For the remaining areas and stocks more general information has been obtained from abundance of year-classes in commercial catches and in catches from research vessels including young-fish surveys.

The year-class information (Fig. 1) is not usually available in the literature after 1968-69 and much of the most recent information is speculative or may even be

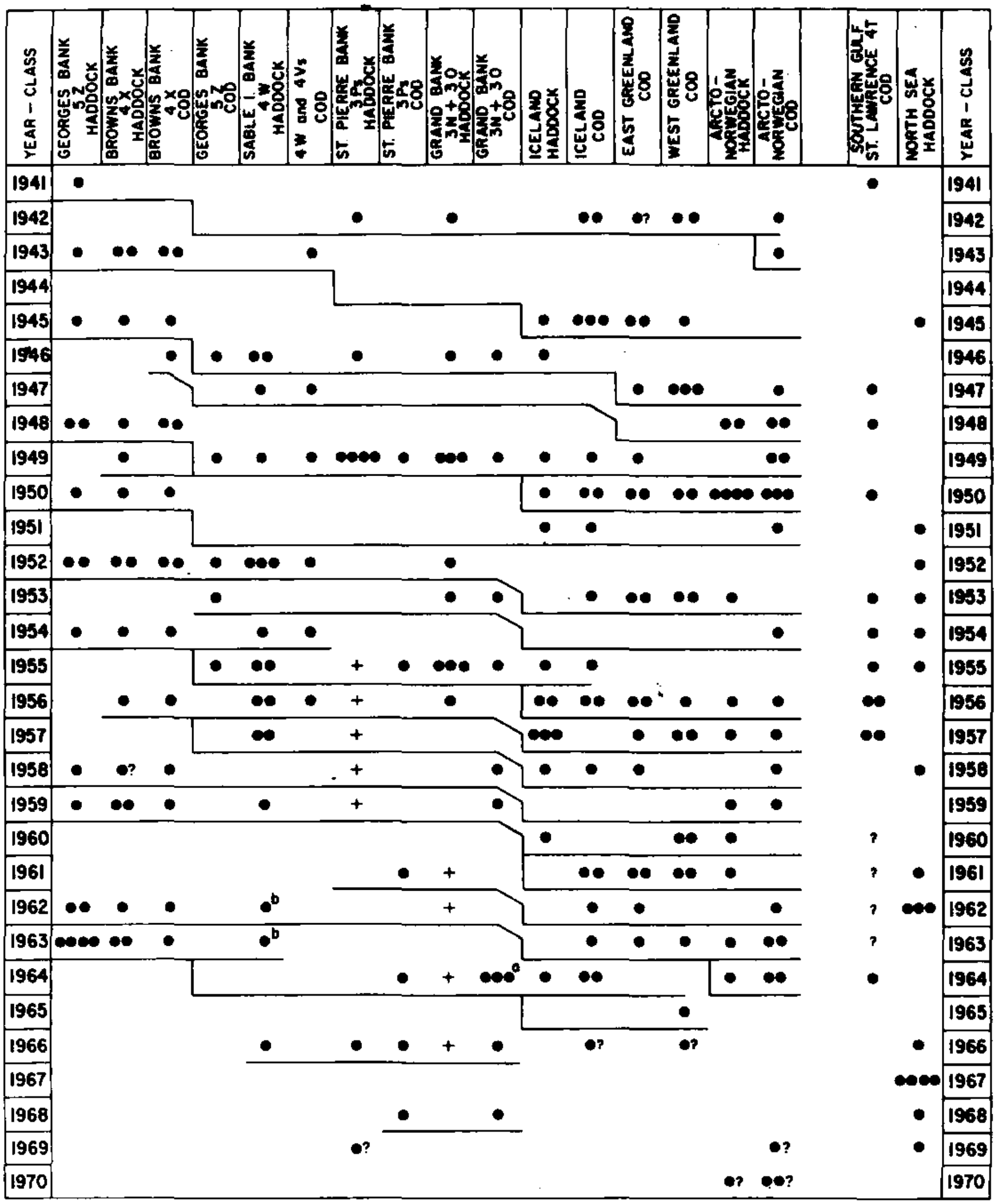

Fig. 1. Successful year-classes of cod and haddock in various areas of the North Atlantic.

Notes regarding Fig. 1

[a](1964) 3N***, 3O*

[b](1962, 1963) There is some doubt regarding the size of these year-classes. They were not large year-classes as adults but were apparently seriously reduced by the sharply increased amount of fishing for 2- to 4-year-old fish in 1965-66.

+The best year-classes for the period but of only minor commercial volume.

?Relative strength of year-class not available or information so recent that size of year-class is doubtful

The successful year-classes recorded for 4X are from the seaward part of 4X to the coast, not including the Bay of Fundy.

The number of circles indicates approximately the relative abundance of good year-classes within a stock or area. They do not necessarily have the same relative weight in different areas, especially for those for which quantitative information is not available.

lacking since definite information is usually not available in the literature.

Year-class Success of Cod and Haddock in the North Atlantic from Georges Bank to the Northeast Arctic

Relation of year-class success between areas and stocks

Year-class success of haddock on Georges Bank (Division 5Z of ICNAF) usually occurs in the same year as year-class success for haddock and cod of Browns Bank (Division 4X) (Fig. 1). Year-class success of Georges Bank cod on the other hand usually occurs 1 year after year-class success of haddock on Georges Bank and of cod on Georges and Browns banks and in the same year as year-class successes of cod and haddock from Sable Island Bank (4W) to St. Pierre Bank (3Ps) and the southern Grand Bank (3N and 3O).

Sometimes there is year-class success for Georges Bank cod and for cod and haddock in parts of the area from Sable Island Bank to the southern Grand Bank in the same year as year-class success of haddock on Georges and Browns banks and of cod on Browns Bank. This has occurred much oftener after 1951 than earlier in the 30-year period. Also, there is occasionally a combination of both, where in two consecutive years good year-classes on the more northern banks mentioned above and of cod on Georges Bank occur both in the same year and the year after good year-classes of haddock on Georges Bank and of haddock and cod on Browns Bank.

Passing northward and eastward from the southern Grand Bank, successful year-classes in Iceland, Greenland and the Norwegian and Spitzbergen-Barents Sea areas normally occur in the same year as or one year after good year-classes in the Sable Island to southern Grand Bank area or of cod on Georges Bank, or 1-2 years after a good year-class of haddock on Georges Bank or of haddock and of cod on Browns Bank. The pattern is clearer in the first half of the 30-year period because since 1955 there have been no very large and since 1956 no moderately large year-classes of haddock on the Grand Bank and only the 1966 and doubtfully 1969 of very modest strength on St. Pierre Bank. Since 1963 there have been no good year-classes of haddock on Georges or Browns banks, or on Sable Island Bank with the possible exception of 1966. Also, since 1964 there is no evidence of successful year-classes of Icelandic cod and haddock except possibly for cod in 1966 and since 1965 and possibly 1966 of West Greenland cod. For Arcto-Norwegian cod no good year-classes were produced in 1965-68 and for Arcto-Norwegian haddock none between 1965-69, but there is preliminary evidence for the former of a good year-class of cod in 1969 and a better one in 1970, with the possibility of a good haddock year-class also in 1970.

Because young fish surveys are now carried out at least yearly in most areas and because young pre-recruit haddock are from their bottom-feeding habits closely related to bottom and readily taken in bottom trawls with suitable codend mesh, the indicated scarcity of good year-classes of haddock in recent years over the whole area will almost certainly be reflected in poor year-classes in the recruited stage. Young cod surveys in the Northeast Arctic have been so intensive and have indicated former good year-classes so well that some confidence can be placed in the predictions of poor year-classes of recruits for the next few years followed by considerably better year-classes. Young cod are, however, more difficult to catch than haddock as they do not maintain the same close relationship to the bottom, being farther off the bottom in the early years than later and hence are more difficult to sample in bottom trawls especially as one-year-olds. Also in the eastern Newfoundland area a great part of the zero and 1- and 2-year-old cod of the Labrador-Newfoundland stock of 3K and 3L (Templeman, 1962) are near the coast and not available to ocean trawler surveys. (See Bulatova's, 1970, very small catches per hour's trawling of especially 1- but also 2-year-old cod in 3K, 3L.)

Although the typical pattern is as described above there are some years when good to excellent year-classes occur in many areas and stocks and other years and periods when year-class success occurs in less areas and in less years.

Information over the most recent 6 years is very incomplete but enough information is available to indicate that, after 1963-64, year-class success of cod and haddock stocks in many areas of the North Atlantic has been much below average, or highly variable.

Gulf of St. Lawrence Cod and North Sea Haddock. The pattern of year-class success of Gulf of St. Lawrence cod does not show any close relationship to year-class success in other parts of the North American area, and good year-classes of North Sea haddock are not closely related to the pattern of good year-classes in the Greenland-Iceland and Norwegian-Barents Sea areas. A very great year-class of North Sea haddock appeared in 1967, a year for which no other large year-class is yet recorded for any of the stocks of cod and haddock listed in Fig. 1, but there may be a good 1967 year-class in 2J.

Labrador-Newfoundland Cod. In the northern parts of the Labrador-Newfoundland area in 2J, 3K and 3L, there is usually not much indication of dominant year-classes in the literature. In the ICNAF research reports of member countries (1963-70), year-classes of cod from commercial catches for the period 1951-64 in 2J and 1955-62 in 3K and 3L showed little dominance, each year-class passing through the fishery as part of the pattern of usually three to four strong year-classes in any one year with not a single year during this period being indicated as a failure or a poor year-class. Individual authors, however, recorded some good or poor year-classes for the period under consideration. May (1959) found 1943 to be a very poor year-class in Subarea 2. In Pinhorn's (MS, 1971*a*) virtual populations assessment for 2J it is indicated that 1957, 1962, and 1963 were better than average year-classes, although the story for these last two is incomplete. Recent research vessel surveys in 2J indicate that 1967 may be a successful year-class.

Fleming (1965) found a strong 1947 year-class, predominant in the deepwater cod fishery off Bonavista in 3L from 1956 to 1961. Bulatova (1970) from special dragging for young cod in years 1959-66 found 3- and 4-year-old cod in 3K and 3L most abundant for year-classes 1963, 1964, and 1966 but catches were also fairly high for year-classes 1961, 1962, and 1965.

Flemish Cap Cod. For Flemish Cap (3M) Konstantinov (1970) from age readings of commercial or research vessel catches indicates good year-classes in 1949, 1950, 1953, 1954, 1957, and 1958.

Period when year-class success is mainly determined

Parrish (1950) found that the contribution of a brood of North Sea haddock to the commercial stock is already determined in the first year of life and probably even before it becomes demersal.

Saville (1956) for Faroe haddock in the 4 years 1950-53 showed that the abundance of pelagic larvae in June provided a reliable index of the abundance of the year-class in its first year of benthic life.

Saville (1959) concluded for the North Sea haddock that control of year-class success is determined in the planktonic stage.

Maslov (MS, 1956, quoted from Wiborg, 1957) found a positive correlation between the number of 0-group cod and the size of the corresponding year-classes in the commercial catches.

Grosslein (MS, 1969) has shown that relative year-class strength of haddock on Georges Bank is determined within the first 6 months of life.

Ponomarenko (MS, 1970) studied the survival in nature of the Barents Sea cod of the same year-classes, comparing the numbers of bottom stages of the 0-group with those of the 2-group obtained in bottom hauls of a trawl with a fine-meshed codend. The numbers of the 0-group and of the 2-group (obtained by multiplying the coefficient of survival in Ponomarenko's table 3 by the average catch of 0-group per hour's trawling) in October-January for the year-classes of 1948-64 both indicated generally but not always and often not closely the relative levels of poor, successful and very successful year-classes as judged by the virtual population assessments of 3-year-old recruits from these year-classes in ICES (MS, 1970; MS, 1971*b*). The catches of the 2-group do not appear to have much better agreement than those of the 0-group with the calculated numbers of 3-year-old recruits to the fishery (Fig. 2). Ponomarenko, however, concluded from these data and from his studies of food, nutritional condition and temperature that survival of cod fry from the 0-group to the 2-group was most closely related to variations in nutritional condition of the bottom stages of the 0-group. Averaging the abundance of the 0-group and the 2-group provides a much closer relationship to the recruit data than either separately. The yearly levels for the 3-year-old recruits are relatively lower for the highest 3 years and higher at the lower parts of the curve than the yearly averages of the 0- and 2-group fish. This could mean that there is a relatively higher level of predation with abundant populations of demersal young and a relatively lower level with lesser populations as a result of less growth and more concentration of the individuals of the abundant year-classes.

Area relationships

Tåning (1931) noted a certain coincidence in the years when dominant year-classes of cod were produced at Norway, Iceland, and West Greenland, except that not all of the dominant year-classes in the other two areas were also dominant at West Greenland.

Hennemuth *et al.* (1964) noted that certain year-classes of haddock appeared strong in all Divisions 5Z to 4W and suggested that factors common to the entire area may control year-class success.

Templeman (1965*a*) showed that in the period 1942-58 for which information is most complete, there were years or two or three adjacent years when successful year-classes of cod, haddock, and herring were produced in Greenland, Iceland, and the Norwegian-Barents Sea area and that there were intervening years or groups of years when successful year-classes were relatively scarce. The more scanty data for cod and herring for previous years back to 1912 also indicated that certain years or adjacent years were more favourable than others for the production of good year-classes

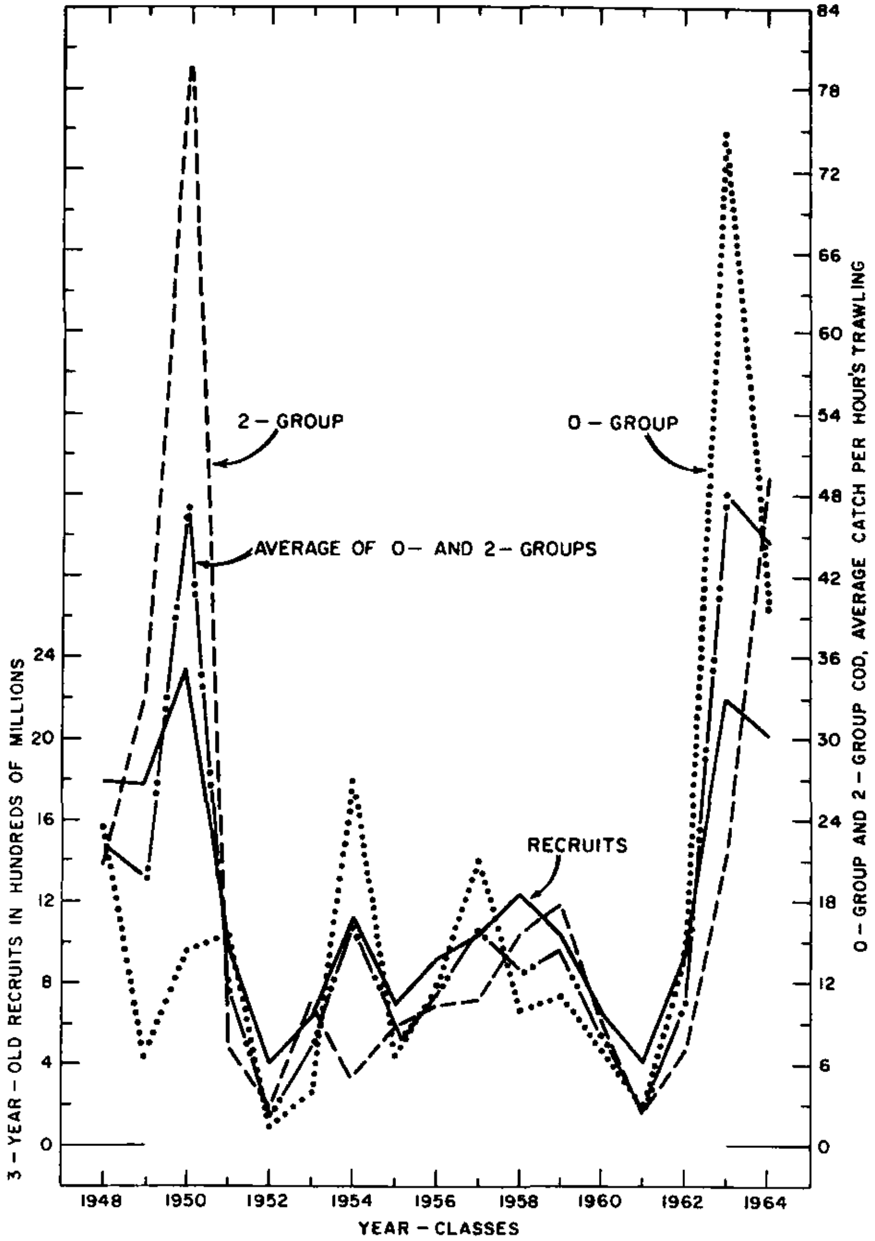

Fig. 2. Comparison of average catches per hour's bottom trawling of 0-group and 2-group cod of the Barents Sea by Soviet research vessels for year-classes 1948-64 (Ponomarenko, MS, 1970, table 3) with absolute number of 3-year-old cod of these year-classes recruiting to the fishery (ICES, MS, 1970, table 7; ICES, MS, 1971b, table 10).

throughout this region. A relationship was also shown between the years of good year-classes of haddock on the Grand Bank and those of good year-classes in the above areas. It was suggested that a hydrological or meteorological sequence of favourable situations was likely in these areas from the southern Grand Bank to Iceland, Greenland, and the Barents Sea. The time required for surface drifts agreed approximately with that of succession of good year-classes.

Martin and Kohler (1965) pointed out that strong year-classes of cod and haddock often occurred in the same year over adjacent or wide areas of the western Atlantic.

Water temperature and spawning period

The success of year-class survival of the northern West Greenland stock of cod is positively correlated with the mean temperature in the upper 45m on Fyllas Bank for the 11 years between 1924 and 1950 for which this temperature information is available (Hermann, Hansen, and Horsted, 1965). Predictions by these authors from mean temperatures on Fyllas Bank for good year-classes (of the northern stock) in 1957, 1960, and 1961, were correct. The 1961 year-class in south western Greenland presumably received contributions from East Greenland. The prediction for fairly good year-classes in 1953 and 1954 was correct for 1953 but 1954 was a poor year-class in spite of intermediate temperatures. The predictions for small year-classes in 1956, 1959, and 1963 were correct except for 1963 which is probably a moderately good year-class in the northern stock but presumably also received contributions from East Greenland. The 1956 year-class was well represented in the West Greenland catch but was a dominant year-class only in the southern divisions and was apparently of East Greenland origin.

Kislyakov (1961) found a high correlation between year-class success of Arcto-Norwegian cod and the mean and surface temperatures of the water mass on the spawning grounds west of Norway. Higher than usual temperatures produced the more successful year-classes. Gulland (1965) using his own numerical data on year-class success and Kislyakov's temperature data found a lesser correlation. Gulland, also, found a slight relationship but no significant correlation between mean temperatures in the 0-200m zone at PINRO stations along the Kola meridian and year-class strengths of Arcto-Norwegian cod for the years 1924-56, and suggested that Kislyakov's relationship between temperature and year-class success may be fortuitous.

Martin and Kohler (1965) showed for the period 1921-62 a negative correlation between annual mean sea surface temperatures at St. Andrews, New Brunswick and Subarea 5 cod landings 4 years later. I have continued the comparison of Martin and Kohler for cod of Subarea 5 to the years 1959-65 for temperatures and 1963-69 for landings, and with the lowered temperatures of the recent period cod catches have increased so that the correlation still holds, but fishing pressure in the area has also greatly increased during the recent period.

Martin and Kohler (1965) found a similar negative correlation for the period 1946-62 for the cod landings at ages 3-7 from the eastern part of Division 4X and sea-surface temperatures at St. Andrews during the spawning year. In the southern Gulf of St. Lawrence (4T), of the five dominant year-classes of cod which occurred between 1937 and 1957, four were spawned within the 8 years for which mean daily surface temperatures from May to October at Entry Island in the southern Gulf were below the average annual temperature of 11.9°C. However, Paloheimo and Kohler (1968) for the cod of the southern Gulf of St. Lawrence found no noticeable correlation between yearly average surface water temperature (Entry Island, April-October) and strength of year-class at age 6 for the 1943-59 year-classes.

Haddock spawning on Georges Bank was more than a month later in 1940 and 1941 than in 1953, 1955, and 1956 and spawning in 1968 was intermediate in time. Also March and April bottom temperatures on Georges Bank were lower (average 3.1° and 3.3°C) in 1940, 1941 than in the other years: 1953 (5.8°C), 1955 (4.8°C), 1956 (3.9°C) and 1968 (3.5°C) (Marak and Livingstone, 1970). Haddock year-class success in these years was approximately in the ratio of 1940 (27), 1941 (14), 1953 (8), 1955 (10), 1956 (9), and 1968 (3) (Grosslein and Hennemuth, MS, 1970). Thus the coldest years and latest spawning were somewhat but not greatly more successful than the highest temperatures and the earliest spawning, showing that some year-class success can be achieved over a range of temperatures and a considerable span of spawning time.

Winds, current drifts, and mixed water

Carruthers, Lawford, Veley, and Parrish (1951) correlated the success of haddock year-classes in the North Sea 1920-49 and a wind function. Saville (1959) applied the same wind function to the success of year-class strengths of North Sea haddock 1916-56 and showed that although the agreement between wind and year-class strengths was very good until 1937, an analysis of the data for 1938-57 showed no correlation between haddock year-class size and the wind function of Carruthers *et al.* (1951). Saville states that the effects of wind in relation to spawning and drifts of eggs and larvae should be March-June rather than the February-May of Carruthers *et al.* since no haddock spawning occurs in February. This new comparison, however, was not made.

Rae (1957) correlated the apparent easterly movements of the copepod, *Metridia lucens* in the northern North Sea in November-January with the mean residual wind in the same area during the same period and noted also that for the 32 years for which data were available no above average year-classes of North Sea haddock occurred during the 9 years when the residual southwest component of the wind in these 3 months averaged more than 250 miles per day. Fourteen above average and nine below average year-classes occurred when mean residual southwest winds were below this value. Because the months used are before spawning begins, it is suggested by Rae that larval survival is better in the mixed Atlantic and North Sea water held in the Great Northern North Sea Eddy, and that with strong southwest winds in November-January this eddy will be displaced northward or northwestward so that at spawning time less water of Atlantic origin will be present over the main spawning grounds of the North Sea haddock.

Saville (1959) from a study of the distribution of haddock eggs and larvae in the North Sea in each of the years 1952-57 concluded that drift of eggs and larvae was too slight to be of major importance in controlling year-class size. There was some relation between spawning locality and year-class strength, more easterly spawning being unfavourable. Over the period 1936-57, haddock year-class strength was closely correlated with the incidence of mixed oceanic-coastal (Atlantic-North Sea) conditions over the spawning area during the spawning season. There is a suggestion that unusually large proportions of either oceanic or neritic elements in the mixture are unfavourable to year-class success.

Hill and Lee (1958) presentated data indicating a correlation between good year-classes of Arcto-Norwegian cod and a strong southerly wind causing increased northerly water transport during the time of the drift of cod larvae from the Lofoten spawning grounds to the Bear Island area. Gulland (1965) for a greater series of years and more precise estimates of year-class strength found the correlation to be not significant.

Because large quantities of cod eggs and larvae from West Greenland are diverted westward into the Labrador Sea and are probably lost from the West Greenland cod stock the onshore wind component was compared with year-class success but the correlation was not significant (*Hermann et al.*, 1965).

Chase (1955) found that haddock brood strength and northwest wind components on Georges Bank had a significant negative correlation for the period 1928-51. Grosslein and Hennemuth (MS, 1970), however, say that in recent years the predictive accuracy of Chase's index has been low.

Colton and Temple (1961) concluded that under average conditions most fish eggs and larvae are carried away from Georges Bank and are lost in the easterly-flowing slope-water current, that only under exceptional hydrographic conditions are large numbers of eggs and larvae retained on the bank or in nearby coastal areas, and that wind conditions over the bank are very important in deciding these results.

Plankton and water mixing

In relation to Saville's (1959) conclusion that haddock year-class strength in the North Sea was closely related to the incidence of mixed oceanic-neritic conditions over the spawning area, many authors have demonstrated for the North Sea area that mixed oceanic-neritic water is apparently more favourable to plankton development than either separately (Russell, 1939; Fraser, 1937, 1952, 1961; Wilson, 1951; Wilson and Armstrong, 1952, 1954; Rae, 1957). The work of Williamson (1961) on the plankton of the Scottish herring grounds of the northwestern North Sea, 1949-59, indicated the importance, for plankton production, of vertical mixing rather than the strength of the Atlantic inflow.

Corlett (1965) for Arcto-Norwegian cod in the period 1958-63, correlated dry weight of summer plankton and the strength of the southerly wind (which are themselves correlated) in the spawning year and the subsequent catch per effort of 5- and 6-year-old cod in the Bear Island-Spitzbergen area. The prediction of a very strong 1960 year-class from the highest level of southerly wind in 1960 was not borne out. Gulland (1965) using relative numerical values of year-class strengths calculated differently from Corlett's, found no significant correlation between Corlett's weights of summer plankton and year-class strengths.

Abundance of spawning stock

Parrish (1950) for North Sea haddock, Saville (1956) for Faroe haddock in the years 1950-53, and Saville (1959) for North Sea haddock concluded that variations in egg number did not control year-class size.

Gulland (1965) for Arcto-Norwegian cod, found no significant relation between the total numbers of eggs laid in different years and the total number of recruits at 4 years of age but did find a significant negative correlation between the number of eggs and the number of recruits per one million eggs.

Garrod (1967) shows a dome-shaped relationship between weight of spawning stock and year-class strength in Arcto-Norwegian cod for the period 1937-60, with best recruitment at intermediate levels of spawning stock.

Garrod (MS, 1968) compared stock and recruitment relationships in the Arcto-Norwegian cod stock and found that since the early 1950's recruitment has been on the average proportional to the size of the spawning stock.

The North-East Arctic Fisheries Working Group (ICES, MS, 1969c) found that the replacement rate of the Arcto-Norwegian cod stock has declined in recent years and concluded that there is a stock recruitment relationship in this stock which reduces the probability of rich year-classes being spawned at high levels of fishing mortality (i.e. low spawning stock), and that at the present level of fishing mortality and conditions determining density independent mortality of eggs and larvae, the depleted spawning stock is unable to replace itself.

Paloheimo and Kohler (1968), for the cod stock of the southern Gulf of St. Lawrence, concluded that there was an inverse relationship between stock density and recruitment.

Herrington (1941), using size, number, and weight relationships of Georges Bank haddock for 1914-40, found a dome-shaped recruitment curve with the best production of recruits from intermediate levels of adults. However, for the period 1931-61 in which the data for Georges Bank haddock are better and year-class information is available, Grosslein and Hennemuth (MS, 1970) found no clear relation between spawning stock and recruitment but the observations were over a limited range of stock abundance, only 2-3 times for stock. These authors note a marked downward trend in spawning stocks since the mid-fifties with usually poor year-classes except for 1962 and 1963 which were large, the 1963 year-class being several times larger than any other on record since ageing began in the early 1930's. The year-classes since (and including) 1965 have been the poorest on record. The large 1962 and very large 1963 year-classes did not arrest the downward trend of the spawning stock since so many haddock of these year-classes were caught before they had reached sexual maturity. The authors say that these data strongly suggest that the probability of good recruitment is reduced at such low stock levels.

General

Wiborg (1957) found some indications that year-class success of Arcto-Norwegian cod in the period 1948-56 was related to: a long spawning period, a late hatching or late spawning, strong northward transport of the eggs and larvae from the spawning area, and a northward displacement of the spawning centre.

There must often be a relation between year-class success and other major events in an area such as the great increase or reduction in stock of a competing fish. One of the best examples of this is the well-known reduction in quantity of the Pacific sardine and the increase in numbers of the anchovy. For example, the southern Gulf of St. Lawrence cod stock had an unusual succession of good year-classes of cod from 1953 to 1957, three good year-classes in 1953-55 and two larger year-classes in 1956-57. During this period there was a great epidemic of *Icthyosporidium hoferi* among the herring of the Gulf, in 1954-56 (Sindermann, 1958) and with no earlier observations on the incidence of the disease, causing the death of great quantities of adult herring, very likely at least half the population. The great mortalities were first observed among the spring spawners. At the same time the older cod grew considerably faster from 1953 to 1956, presumably due to the ready availability of large quantities of weakened and dead herring (Kohler, 1964). Thus, there was a combination of more cod eggs than there would have otherwise been, eggs of better quality because of the excellent condition of nourishment of the adult female cod, and a lack of herring to prey on the eggs and larvae of cod, on copepods such as *Calanus*, the young stages of which supply the early larval food of cod, and on euphausiids which form so much of the food of later stages of young cod.

Also, in the latter part of the 1950's two large year-classes of herring were produced in the southern Gulf, a very successful 1958 year-class of autumn spawners and a successful 1959 year-class of spring spawners (Hodder, MS, 1971). These two strong year-classes of herring still persist and have provided most of the large winter fishery in recent years on the western part of the south coast of Newfoundland and presumably also much of the summer fishery of the southwestern Gulf of St. Lawrence.

Discussion and conclusions

There is general agreement among fisheries biologists, both for the stocks under discussion here and for stocks of other fish species, that the main factors determining year-class success occur early in life, for demersal fish mainly in the planktonic phase before settling toward the bottom if settling is in a favourable depth and nursery area.

It is also apparent that, in the North Atlantic, year-class success in adjacent or even distant stocks of several species have patterns of relationship too consistent to be due to chance, so that favourable conditions for survival can apply over wide areas in the same or in succeeding years depending on the distance and the circulation pattern.

Over the whole area of the North Atlantic it is apparent that abundant cod populations occur where moderately low temperatures provide a general favourable environment and slightly higher temperatures are available for spawning. Apart from any inherent favourability for the cod, lower temperatures may also provide an abundance of plankton food and some reduction in at least the variety of predatory fish species. Cod become scarce to absent as temperatures become higher to the south or lower to the north.

When temperatures are generally quite low, as for the northern West Greenland stock of cod, increases in temperature in the hatching year are usually favourable to year-class success.

Where temperatures are high enough to be generally unfavourable to the development of great cod populations as at the southern end of the cod range in the western Atlantic it is likely that lower rather than higher temperatures in the hatching year are usually favourable for year-class survival. So little information however, is available on relative year-class strength of cod in the southern part of the Northwest Atlantic that detailed correlations cannot be made.

For the Arcto-Norwegian cod stock it is likely that there is some positive relationship between higher than usual upper water temperatures and year-class success but the correlation between the two may be less than for the West Greenland stock.

After indicating the evidence for these temperature and year-class correlations toward higher temperatures in the north and lower temperatures in the south, there are results which are still difficult to understand with the information available and which indicate that more than temperature is involved. Cod spawning off Labrador is very extensive and principally in March-early April when temperatures in the upper layers over the spawning area are usually below 0°C (0°C to - 1.8°C – Postolaky, 1968, and author's personal observations), and the area is often covered with ice. In spite of these conditions, the year-classes of this stock are typically successful and usually are not highly variable. Thus low temperature may be more harmful in relation to later larval growth, than during the immediate hatching and early larval period.

It must, however, be of great significance that drift from the spawning grounds of the great cod stocks takes the cod larvae into areas where there is a large supply of cold water: the Arcto-Norwegian larvae northward to Spitzbergen-Bear Island and the Barents Sea, the Icelandic cod larvae to the north coast of Iceland, to East, Southeast and Southwest Greenland, the Labrador-Northeast Newfoundland cod larvae with the colder division of the Labrador Current toward the coastal waters, the East Grand Bank cod larvae along the path of the eastern cold water branch of the Labrador Current, the larvae from the West Newfoundland cod stock northward to the Strait of Belle Isle and northwestward to the north shore of the Gulf, and the larvae from the Gaspé cod stock southeastward over the Magdalen Shallows where the bottom waters are mainly cold.

Gulland (1953) pointed out the statistical dangers of such retrospective correlations as wind direction and year-class success where the selection of the wind factor is a matter of choice. The test of such a correlation is in its fit in future years and the results of such projections have been discouraging. This is not to say that water movement as generated or affected by wind is not a factor but merely that the particular wind factors correlated with year-class success have not stood the test of time, and that wind is only one of the factors.

In the NORWESTLANT Surveys of 1963, the principal invertebrate predators of cod eggs and larvae were usually much more numerous over deep than over shallow water (Bainbridge and Corlett, 1968); hence the location of production and the drift of the eggs and larvae are very important with regard to predation as well as plankton production.

Upwelling and mixtures of water due to wind, and the accompanying or other improvement in plankton production are very likely also to favour the growth and survival of the fish larvae in the plankton.

The temperature variations on Fyllas Bank in June, where successful year-classes of cod are correlated with higher temperatures, are the consequence of the degree of mixing of the colder East Greenland and the warmer Irminger Current. These increased temperatures are likely to influence the timing and intensity of spring spawning of *Calanus finmarchicus*, which in this area has one major spawning period occurring in the spring, and the younger stages of which formed almost the sole food of cod larvae at West Greenland (Bainbridge and MacKay, 1968). The success in survival of the cod larvae off West Greenland may therefore depend on the degree of synchronization of the timing of spring spawning of the cod and the beginning of active feeding by the cod larvae, with the abundance of the early stages of *Calanus*. Also Marshall and Orr (1964) have shown that egg production by *Calanus* is related to the supply of phytoplankton, and there are large variations in phytoplankton production in the Greenland area (Gillbricht, 1968). From year to year there are large differences in the seasonal abundance and the time of appearance of the stages of *Calanus* (Glover and Robinson, 1968) and also there are large area differences in this timing (Bainbridge and Corlett, 1968). Bainbridge and McKay (1968) found cod larvae of West Greenland to have less food in their stomachs than cod larvae of the same size in Faxa Bay, Iceland and that this difference was probably due to differences in the availability of food in the plankton. The water mixing, of which the increased temperature over Fyllas Bank is an indication, may therefore provide the temperature and chemical conditions to set off a chain of phytoplankton and *Calanus* production in time with cod larval growth and development from the spawning of the cod at the higher temperatures in the adjacent deeper water, and, as Bainbridge and McKay (1968) have suggested, the correlation of these relationships may explain the success of cod year-classes at West Greenland.

For the relationship of stock and recruitment, there is evidence for different stocks in the papers quoted, all the way from no correlation between stock and recruitment to an inverse relationship between stock and recruitment. In all heavily-fished stocks the spawning stock and especially the number of eggs produced have evidently declined greatly.

Cushing (1969*b*) has demonstrated the relatively fixed spawning dates of a number of northern fish stocks and (Cushing, 1967) has related the spawning and early larval feeding times of autumn-, winter-, and spring-spawning herring of the Northeast Atlantic to three corresponding periods of phytoplankton production cycle in the same areas.

There must be some level where a spawning stock will not on the average produce as many recruits to the fishery as were formerly produced by a larger spawning stock and still less, if the fishing intensity is increasing or younger ages are increasingly fished, replace the weight of spawning stock or the number of eggs produced. Especially is the latter true of stocks such as the Arcto-Norwegian cod with sexual maturity delayed to a late age. It is not surprising for this intensively fished and intensively researched stock that evidence is accumulating that the low level of the spawning stock is reducing the number of recruits, producing more highly variable year-classes, and that the few large year-classes produced do not greatly increase the spawning stock.

When density dependent mortality is of great importance, the value of a drift over the area and at the time of spawning, of water bearing quantities of suitable food is evident. The speed and direction of the drift must be important in spreading the eggs and larvae throughout the plankton over a wide area and in bringing them, at the time of settling, to a suitable nursery ground.

Haddock stocks of the western Atlantic have had relatively poor survival since 1963. The last highly successful year-class of Grand Bank haddock was 1955 with a modest success in 1956. This stock is probably at not more than 5% of its former abundance. The great reduction in standing stock and potential spawning stock occurred in 1960-62. In the period 1942-56 there were good to moderate year-classes every 2-4 years. Thus, when spawning fish were relatively abundant in 1957-62 there should have been one or two good year-classes; consequently unfavourable factors must have been acting in this period. Since 1962, in addition to the other factors, the spawning stock of Grand Bank haddock has been at a very low level, and especially favourable conditions will be needed to restore the stock to its former abundance.

The haddock population of Georges Bank was also greatly reduced by the heavy fishing in 1965-66 and the lack of good year-classes in recent years. The stock is now at such a low ebb that it is logical to believe that extremely favourable conditions will be needed to restore it. However, the stock was large enough for at least 4 or 5 years after 1963 that other factors than low spawning stock must have operated to reduce the survival of young.

Within the recent period of lack of year-class success of haddock on Georges and Browns Banks (and the Grand Bank) a very great 1967 year-class of haddock appeared in the North Sea following an unusually successful 1962 year-class. Cushing and Harris (MS, 1970) remark that conditions during the recent decade in the North Sea have become colder and that this coincided with rich year-classes of haddock and cod. This appears, however, to be a general remark, with no detailed comparison of temperature and year-class success. The great decline in haddock landings from the North Sea from the early 1930's to the early 1960's (a warm period) was mainly due to a sharp decline in landings from the southern North Sea (Jones, 1970).

In addition to the factors already discussed, many other factors probably affect larval survival and thus year-class success. Most of these have been mentioned recently in reviews by Hempel (1963, 1965), Templeman (1965*b*), Cushing (1966, 1969*a*), Jones, MS, 1970), and individually or collectively by many earlier authors but, at least for the stocks I have been considering, through lack of knowledge of variations in these factors there have usually been no attempts to correlate them for a long period with year-class success. Some of these factors are:

1) Food limitations, size, quantity or quality, accompanied by death by starvation or selective predation of the slower growing and weakened larvae (O'Connell and Raymond, 1970; Ponomarenko, MS, 1970);

2) Size of spawning fish, older and larger fish often produce larger eggs, and thus produce larger larvae with larger yolk reserves;

3) Variations in quality of eggs which may vary with changes in nutrition and in size and age of parent fish;

4) Occupation of different areas and depths by mature fish of different ages, thus extending the spawning area;

5) Changes in the location of the spawning area;

6) Usually earlier spawning by older fish, thus allowing a larger number of age groups of spawners to extend the spawning season considerably and populate different and greater sections of the current drift. (In Labrador, however, the larger cod spawn 1-2 months later but these have probably migrated from farther south than the smaller cod. The effect of lengthening the spawning season is the same.);

7) For fishes with demersal eggs, heavy spawning in a limited area with too great a thickness of the egg layer may be unfavourable;

8) Extension of spawning and variation in size of larvae, both produced by having a large range of spawning sizes and ages, may allow at least some of the young to have a better chance of matching the time of production of food of suitable size and characteristics in the plankton bloom and may allow variations in the size and influence of predators;

9) Variations in the abundance of predators on the eggs and larvae. These predators will sometimes be competitors, and sometimes be food fishes or invertebrates the adult populations of which may increase in number when stocks of the larger commerical fish which feed on them are greatly reduced;

10) Changes in current patterns;

11) Injuries to pelagic eggs by wave action (Rollefsen, 1930);

12) Death from high temperature (Colton, 1959), or from low temperature;

13) Variations in light intensity. If upper layers become unfavourable, larvae may be driven deeper and encounter a light intensity too low for adequate feeding (Einsele, 1965).

Summary picture of probable main factors in year-class success

Thus, although the complete picture of the reasons for year-class success is not available, some of the more likely possibilities are emerging: For cod and haddock it is evident that moderately low temperatures are favourable to the production and existence of large populations. Low temperature toward the north and high temperature toward the south must limit year-class success. Food size, abundance, and quality must affect growth and hence mortality of the fish larvae through greater predation on smaller, weaker and slower moving larvae (as described by Jones, MS, 1970). To take advantage of abundant food, fish must spawn and larvae must hatch out and feed at the time of production of the appropriate size, quality and quantity of food in the plankton bloom. The quantity and composition and timing of the plankton bloom may well be determined largely by water of suitable nutriment, temperature, and plant and animal species content produced by horizontal or vertical water movement and mixing and also temperature and light. It is apparent for populations which, like the Newfoundland cod, have existed in large numbers for centuries, that natural selection must on the average have matched hatching and early development with the suitable periods of the plankton blooms. The abundant egg supply and the length of life under virgin or moderate fishing conditions apparently have on the average allowed replacement of the small to moderate mortality.

The timing of spring plankton blooms must be closely related to temperature and light conditions in the photic layer above. In cold areas of the Northwest Atlantic, winter depths of pre-spawning cod and haddock depend on the severity of the winter conditions in the water and they are driven deeper by a thickening of the overlying cold layer. Since ripening of the ovarian eggs and thus the timing of spawning are presumably affected by light and temperature these variations in winter depths should produce variations in spawning time with relation to the plankton bloom which will be affected by heating from above. Thus, although under average conditions natural selection will have successfully matched spawning periods and plankton blooms, in any 1 year or for a period of years the matching may be lost.

Under very heavy fishing where the spawning fish are very much reduced in numbers and size, the available eggs, also the spawning area, spawning period, and range of recently hatched larval size and quality are reduced since, apart from abundance, large and small fish have different depth and area and spawning time and often different egg and larval size relationships. It will therefore again be necessary for natural selection to act to bring spawning and hatching times and locations into on the average a successful relationship with the plankton blooms. This can only occur by a greater than average mortality of the progeny of fish which are not attuned to the plankton bloom and if the process is slow a considerable period of low abundance may be experienced. Especially may this be the case when, because of the effective reduction in the number of age groups in the spawning stock in the heavily fished population, there is a much smaller number of years when heavy egg production and long spawning period are available to act as insurance against the sometimes long periods of years when unfavourable factors tend to prevent the appearance of successful year-classes.

Granted that variations in larval numbers and in food and predators must affect larval survival in a water mass, the mass can still drift so that an abundant year group of larvae of demersal fishes must settle on unfavourable nursery grounds or on grounds so unusual as to be lost to the parent stock, or may drift away from the banks and shelf over very deep water or water too warm or too cold so that the larvae may be destroyed.

It is not likely at the present time for these heavily fished stocks of demersal fishes, that early larval survival will often be affected very unfavourably by larval density although, when there is excellent larval survival, the later larval or early bottom stages are likely to be so affected. Generally, however, it appears that year-class size is approximately established by the time that the fry have established their bottom relationship on a suitable nursery ground.

It is apparent that fish such as the Grand Bank haddock, living near the extremes of their range and with great variation in year-class strength and many poor to very poor year-classes even under moderate fishing, are likely to be very readily affected by some climatic change or other disturbance such as great reduction of the spawning stock and that fish situated centrally in regions of great abundance of the stock and under more average climatic conditions are likely to have more resilience to the effects of heavy fishing.

On occasion some of the additional factors listed may dominate so as to control year-class success, imposing their control on the basic or average pattern.

Finally, as a dessert to this exercise one could not do better than read the short but excellent summary of the results of the Stock and Recruitment Symposium held at Aarhus, Denmark, 7-10 July 1970 (Parrish, MS, 1971).

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Discussion

Mr Garrod: *"I would just like to add one further piece of information on Fig. 2 of your paper concerning some recent information that Dr Dickson and I are preparing for publication, to the effect that the peaks of good year-classes in the Northeast Arctic cod stocks correlate very well with periods of increase in the North Atlantic Drift. In 1968 we used this as a basis for a look forward which led us to expect an improvement in recruitment somewhere about 1970-71. This, in fact, has come to pass. In that area, anyway, I think the fluctuation in hydrographic conditions will probably have wider significance beyond the cod, inasmuch as the Atlanto-Scandian herring seems to fit very closely with the same mechanism. On the other hand, this is not clear for haddock: sometimes the haddock year-classes follow the cod and herring, and at other times it works in quite different ways. I suspect if we could get the fishery data up to a sufficient level of precision, we should find this kind of environmental year-class correlation was probably much more obvious than it seems to be at the present time. I do not think necessarily that the same mechanism is going to apply to all the major stocks in the North Atlantic, but then I think this is obvious from your previous comments."*

Dr Templeman: *"Of course this fits very well with the relationship between success for the southern Grand Bank area and even Georges Bank cod and haddock stocks and 1 or 2 years later for the Arcto-Norwegian stocks. The time required for surface drifts across the Atlantic are not out of order for a 2-year period. It's of the nature of 1½ or 2."*

Mr Garrod: *"I would agree with you. I think the major problem is getting the fishery data up to an adequate level of precision to see really what is happening in the fish stocks. The very strong year-classes always stand out, of course, but when you are looking for a good correlation you also need accurate estimates of year-classes where the variation has been much smaller and for that you need another level of precision."*

Dr Templeman: *"It is obvious you are getting two things here. You are getting a little higher temperature which is often favourable toward the north. I presume there is a greater volume inflow which will take the cod larvae further northward and northeastward and there is some water mixing, I presume, which will be good for plankton development."*

Mr Garrod: *"I believe, if you think about it long enough, you will always be able to postulate a biological mechanism to suit the particular set of circumstances you find in this occurrence. Intuitively, I would expect an influx of warm water into that area just to give an overall improvement in the environment for young fish and leave it at that – not knowing quite what the mechanism is."*

Panel Discussion

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Panel Discussion

Moderator – Mr A. J. Lee

Mr Lee: *"Ladies and Gentlemen, the final session of this symposium is devoted to a general discussion of the papers.*

"I do not consider that there is any point in summarizing what has been said in the papers. They covered a wide range of subjects. It is satisfying to learn how our knowledge of the whole of the marine environment is developing; in what Dr Loncarevic called the fish bowl with the water in the bowl, the air over the top of it and the fish swimming in the water in the bowl. However, I do feel that our knowledge of the physical and biological environment for the fish, as far as observational time series goes, is still extremely limited. It seems that we are relying entirely on temperature information, largely for the sea surface or for the air above it, and on wind data. To some extent in the ICNAF area we do have some indication of a time series of zooplankton data. We have time series of fish data but very few records of salinities, currents and phytoplankton from the area. There is little knowledge of what goes on below the sea surface.

"I think that it would be more productive to examine one or two general questions. We should note that the purpose of this symposium is to examine the effects of changes in the environment over the last decade upon the abundance of the fish stocks in the ICNAF area. Yesterday a paper by Dr Templeman reviewed the many factors that might influence year-class strengths. Looking at his data of different year-class strengths for various species and geographic areas throughout the North Atlantic, one finds very few good year-classes in the last decade, except in the North Sea. One wonders, in fact, if this is an effect of the environment or an effect of the exploitation of stock to a low spawning potential, or a combination of these factors. This is one thing that we should try to clear up here. Can we see from the data presented, any effects of the changes in the environment upon the abundance of the fish stocks themselves?"

Dr Templeman: *"I reported that the Grand Bank haddock stock had not had a really good year-class since 1955. This large stock was destroyed by 1963, largely due to fishing in 1961 and 1962. Countries at that time were exploiting haddock on the Grand Bank of Newfoundland rather heavily and with the appearance of the Russian fleet, the fishery doubled very quickly. Normally, there was a good year-class every 2, 3, and occasionally 4 years. Often there would be a good year-class followed by a smaller one, twice in succession which is common to some of the fisheries. However, between 1957 and 1962 when there was a moderately large and sometimes quite large spawning stock, there should have been several good year-classes before that stock was reduced to, say, 5% of what it was. On Georges Bank, the last two good year-classes occurred in 1962 and 1963. Formerly, there was a good year-class every second year. A large spawning stock existed for 4 or 5 years after 1963 but there were no good year-classes.*

The lowering temperatures in the area might have had something to do with this. However, it is hard to pinpoint whether or not temperatures are responsible. Both the Grand Bank and Georges Bank spawning stocks are now quite low and it may take an entirely different environment to produce a large year-class."

Dr Meyer: *"We have observed significant changes for the Greenland cod during the last 25 years. These changes might be connected with environmental conditions. To begin with, cod fished off Greenland belong to two different stocks, the West Greenland and the East Greenland stocks. Cod of West Greenland origin spawn off West Greenland between about 62° and 65°N. The feeding areas of the immature and mature fish are the banks from 62°N to north of Disko, mainly in ICNAF Divisions 1C to 1A. The second stock originates off East Greenland, mostly in the area from Bille Bank to Dohrn Bank. Most of the larvae are transported by the East Greenland and Irminger Current to South and Southwest Greenland. The mature cod after spawning also move to South and Southwest Greenland. When returning to spawn at East Greenland, often a considerable number of the East Greenland cod move farther on to the Icelandic spawning grounds where they remain. Until 1960 strong West Greenland year-classes were produced with great regularity, i.e. the 1942, 1947, 1950, 1953, 1957, and 1960 year-classes, with the 1947 year-class being by far the strongest. However, in the 1960's, recruitment to the West Greenland stock decreased considerably. Probably only the 1965 and 1966 year-classes are of average commercial importance. On the other hand, more and more year-classes of East Greenland origin were produced. The first strong East Greenland year-class was born in 1945 and lasted 11 years. The next good East Greenland year-class was that of 1956, but then many average to very strong East Greenland year-classes were produced in rapid succession in 1958, 1961, 1962, 1963, and 1964, of* *which the 1961 year-class especially, but also the 1962 and 1963 year-classes strengthened the Iceland spawning stock in 1968, 1969, 1970, and 1971.*

"The decrease in West Greenland year-classes and increase in East Greenland year-classes, which, in my opinion, can be related to recent hydrographic changes in the Greenland area – as shown by Dickson and by Rodewald, and also by the hydrographic investigations of Hermann (F. Hermann. In: ICNAF Redbooks 1967, Part IV; 1969, Part III; 1971, Part III), Blindheim (J. Blindheim. In: ICNAF Redbook 1967, Part IV), Lenz (J. Messtorff and W. Lenz. In: ICNAF Redbook 1970, Part III), and Burmakin (V. V. Burmakin. In: ICNAF Redbooks 1968, Part III; 1969, Part III) and Kudlo (V. V. Burmakin and B. P. Kudlo. In: ICNAF Redbook 1971, Part III) – affected the recent fisheries in the following ways:

1) there was a proportional decrease in fishing activity on the northern banks off West Greenland and a corresponding increase off South and Southwest Greenland (ICNAF Div. 1E and 1F); and

2) there was a considerable increase in catch-per-unit effort in the cod fishery off East Greenland.

This is true especially for the German fishery which, for many years, has taken by far the highest percentage of the total catch in Greenland waters. The percentage of the German catches taken in Div. 1E and 1F increased gradually from 27% in 1967 to 77% in 1970, despite the fishery on the southern grounds during these years being much more hampered by ice than in the more northerly Div. 1D and 1C. The increase in year-classes of cod of East Greenland origin also changed the fishery off East Greenland, where mainly redfish were caught. In 1970 for the first time more cod than redfish were landed from East Greenland and in 1971 the cod will probably be by far the main species caught. This increase in East Greenland year-classes also increased the quantity of cod emigrating to Iceland, and thus, had a great influence on the Iceland spawning stock and the Iceland cod fishery."

Mr Horsted: *"Dr Meyer has said most of what can be said about Greenland cod year-classes. They do originate from two different regions, but I am not sure that our early knowledge of the origin of the year-classes is as good as it has been since 1945 when a year-class of East Greenland origin was identified for the first time. Since then we have discovered an inflow of cod of young age-groups to West Greenland regions from East Greenland, which has directed our attention to this phenomenon. It seems that as soon as one is aware of a phenomenon, there is a tendency to observe that phenomenon more frequently. Probably, one could also say that it is not just a question of year-classes being good or poor year-classes, because our present distinction is merely a relative distinction, and when the stock is fished down to a level where there are not many year-classes occurring, you will again have the phenomenon that one or two of them will have a tendency, in terms of percentage occurrence, to be more predominant than at a time when there is a great overlap of several year-classes or age-groups. Nevertheless, I do think that the year-classes pointed out by Dr Meyer are not only relatively good year-classes but, in terms of actual numbers, also good year-classes. We have made some progress in recent years in estimating absolute year-class abundance.*

"We should also note that environmental conditions have influenced not only the actual occurrence and determination of good or poor year-classes but have also influenced the accessibility of fish to fisheries. The last few years' conditions as reported by Captain Dinsmore and Dr Rodewald must surely have had a great influence on the accessibility of cod in Greenland waters. First of all, ice has limited fishing activity in some of the regions where cod are concentrated at spawning time and where the trawlers could have made their best catch-per-day. This has resulted in less effort in these waters which, in turn, has influenced the size and age composition of stock and future catch, because, if we had had good accessibility in these years, the future stock would not have contained the same relatively high proportion of cod with a high weight."

Mr Garrod: *"I agree with the comments that Dr Meyer and Mr Horsted have made with regard to the situation of the West Greenland fishery. There are some points I would like to take up with Dr Meyer concerning the accuracy of our estimates of changes in the East Greenland cod stock, but the point I want to make here is that for very few of these stocks do we have any real information at the moment as to the strength of year-classes since about 1964 or 1965, i.e., for the last 5 years of the decade which have been so prominent in our discussion of current trends in the environment. The information we do have has come primarily from USSR young fish surveys in the Newfoundland and the Labrador area and I have the impression that for those cod stocks there has been no obvious deterioration in the year-class strengths. Yet in those areas the temperature trends have been substantially the same in terms of cooling that we have heard spoken about in relation to West Greenland. One must be very careful before drawing a general conclusion from this type of environmental data."*

Dr Rodwald: *"Concerning the remarks of Dr Meyer on the opposite development of West Greenland and East Greenland year-classes of cod the following tentative explanation could be given. The tendency of the atmospheric circulation during this recent period was in the direction of strengthened easterly to northeasterly winds around South Greenland. These anomaly winds would mean expansion of the warmer than normal Irminger water against the stronger and colder than normal Polar Current near the Southeast Greenland coast. The boundary between Atlantic and Polar water masses, more accentuated than normal, would be pressed westward. With the cold water confined to a near coastal belt, an amelioration of conditions over the East Greenland shelf would result. However, the Polar water of the West Greenland current being colder than normal, would eventually broaden westward in the uppermost layer under the influence of anomalous easterly winds. Thus, a deterioration of conditions for cod eggs and larvae would take place in the West Greenland spawning areas."*

Dr Dickson: *"I think we must look under the surface as well. I agree with Dr Rodewald but, while there has been a cooling of the narrow Arctic current rounding Cape Farewell and a spreading of it across the surface from the West Greenland coast by the easterly winds which he mentions, Hermann (F. Hermann. In: ICNAF Redbook 1967, Part IV) has shown quite clearly that at depth on the banks off West Greenland there has been a considerable warming until the mid-1960's. There has also been a warming at depth to the mid-1960's in the Irminger Sea. As regards the temperature relations of cod, it is known that the year-class strength goes up when we get warming conditions at the poleward limits of range (as in this case) and vice-versa. As Dr Rodewald and I pointed out yesterday, at present there is a cooling condition in the Irminger Sea and Dr Meyer says it is also cooling off West Greenland. So looking at recent events, in the general sense, can we now say that conditions are worsening for the cod at West Greenland and East Greenland compared with what they were up to the mid-1960's? I think if we can say that, then we are saying something and giving some sort of prediction which may be useful."*

Mr Horsted: *"Dr Dickson mentioned that Hermann has shown a warming tendency to the mid-1960's at West Greenland in the sub-surface waters. This is quite true, but cod have a very strong tendency to spawn in deep water. However, the critical period comes after spawning when the eggs rise to the surface where they hatch. So what is important for the cod larvae and eggs is not the sub-surface water but the surface water."*

Dr Meyer: *"Dr Rodewald mentioned the additional northeasterly wind on the East Greenland side, caused by the positive pressure anomalies over Greenland. This additional wind must have increased the velocity of the East Greenland current in recent years. This means that more water of lower salinity is now transported to the West Greenland side and that the difference in salinity between the Polar and Atlantic components of the West Greenland Current has increased. As a result, as Mr Blindheim pointed out some years ago, during the winter owing to more stratification there is less convection, less heat exchange and more cooling of the surface layers, resulting in a later plankton bloom and consequently, deterioration of the conditions for the survival of the cod larvae.*

"Now, I have a theory but I cannot prove it. This theory could help in understanding the history of the fishery off West Greenland during the last 150 years in the light of environmental changes. The recent cod fishery on the West Greenland banks started in 1925 as a consequence of the warming of the northern parts of the North Atlantic. Before this period a good cod fishery was known to have existed around 1820 and from 1845 to 1850. During the cold period from 1850 to about 1910 there were no cod on the banks – the first two cod were taken on Fyllas Bank in 1921! However, there was a paying fishery off West Greenland for halibut. Is it not interesting that during the cold period only the more southern species, the halibut, could exist in Greenland waters while the more boreal cod could not!

"One explanation for this interesting fact could be the different specific gravity of the eggs of these two species. Both species spawn at great depths in the warm Atlantic water. The eggs of the halibut are heavier and remain where they are spawned, while the cod eggs rise to the surface where the resulting larvae have no chance to survive when it is too cold and when the plankton bloom is too late. If, as a result of further deterioration of the climate – as predicted by Johnsen, Dansgaard, Clausen and Landway (S. J. Johnsen, W. Dansgaard, H. B. Clausen, and C. C. Landway. In: Nature, 227, No. 5257, 1970) – the Greenland cod stock dies out, as happened after 1850, the halibut could survive and increase (especially in the absence of cod which with other species feeds on young halibut) and a fishery for halibut could again be possible!"

Dr Rodewald: *"Figure 30 of my paper shows a trend curve which is representative of the 0 to 50-m layer of springtime Polar water at West Greenland. The curve is derived from Norwegian data received from Mr J. Blindheim. The* *cooling trend of the surface layer should be very important when – according to Mr Horsted and Dr Meyer – the surface waters are the environment for the eggs and larvae of cod. The trend may reflect a deterioration of the survival potential, as it shows a decline of 2°C in the surface temperature for April alone during the last decade. Certainly, the trend derived by smoothing over several years, is not decisive for each single year-class; there may be an intervening warmer year, more favourable for cod reproduction. But the recent failure in frequency of good West Greenland year-classes could be explained by that cooling trend."*

Dr Dickson: *"Then, up to the mid-1960's, the warmer conditions at depth off West Greenland would have improved the conditions for adult fish, perhaps enabling them to colonize the banks further to the north, but the spreading of cold surface water offshore by easterly winds provided no improvement in conditions for their eggs. In the most recent years, on the other hand, the reason for the warming at depth off West Greenland, as we see it, has been removed in that the increased cyclonic airflow over the eastern Atlantic to the Irminger Sea – which we think boosted the Irminger Current in the years up to about 1965 – has changed, whereas the reasons for the boosting of the cold East Greenland Current still remain. Therefore, cooling in the surface layers can be expected to continue. We can also now expect some cooling in the deeper layers. I do not know what total effect this would have on the cod stock at West Greenland."*

Mr Garrod: *"I have a quick question for my own clarification to bring information from other stocks to bear on this general question of the influence of temperature upon recruitment to fish stocks. How does the average surface temperature at West Greenland compare with that at Labrador in the area of drift from the Labrador cod spawning grounds? Is the West Greenland surface temperature lower than that at Labrador? My reason for asking is that we have an apparent failure of recruitment at West Greenland but so far as I understand it, we have, if anything, an improving recruitment at Labrador: the difference in temperature between the two areas might, therefore, have an instructive bearing on the various factors involved here."*

Dr Templeman: *"I have no new information. Because of the regular success of the year-classes at Labrador, where cod spawn mainly in March and early April at surface temperatures between 0° and -1.7° and because ice regularly covers the spawning area, so that temperature cannot rise very fast during March-April, I concluded that temperature must only be an indicator of some mixing and lack of mixing of waters and of variations in synchronization of plankton development and spawning. Of course, the Greenland larvae drift northward and the Labrador larvae drift southward toward the Newfoundland coast but mostly the current turns toward the coast. There might be some difference because of the two directions of drift. Actually, surface temperatures are so low off Labrador at spawning time and surface salinities generally very high – mostly 32°/oo at the surface – that I can hardly believe the temperature itself is responsible, rather than being only an indicator factor, unless the northward drift with possibly declining temperatures on one hand and the southward drift with rising temperatures on the other hand has some influence."*

Dr Smidt: *"Adding to what Dr Meyer said about the halibut in Greenland, it is generally accepted that the stock declined because of overfishing in the 1920's. The halibut fishery was very important in Greenland up to 1930. By then large Norwegian and British factory ships had destroyed the halibut stock. The Greenlanders' fishery for halibut had declined over several years and the canning industry in Holsteinsborg was forced to cease operations in 1935. Then the prawn grounds at Holsteinsborg were discovered and a prawn industry was started. However, that is another story. It may also be that the halibut stock in West Greenland was mainly recruited from areas outside the West Greenland area. Another species, Brosme brosme, has occurred more frequently in recent years in the fishery at great depths west of Fyllas Bank at West Greenland.*

"Also, in sampling for cod eggs about 17% were found to be Brosme eggs. However, Brosme larvae have never been taken in West Greenland waters. Therefore, the stock of Brosme must be recruited from other areas due to the stronger influx of deep warm water in the 1960's. Perhaps this was the case with the halibut in former times."

Mr Garrod: *"Mr Chairman, I think what Dr Templeman said was important in relation to this general problem. As I understood him, he would agree that the cod stock at Labrador is producing what, in a historical sense, is a perfectly satisfactory level of year-class strength under temperature and salinity conditions which are probably less favourable than those at West Greenland. This suggests, as you yourself said, that temperature itself is not necessarily the determining factor in this particular set of circumstances. I find this quite encouraging, especially in relation to something that Dr Meyer said to me in conversation earlier today. He said that in 1963 the NORWESTLANT Survey found rather few cod larvae off East Greenland, yet the 1963 year-class is turning out to be a particularly good* *year-class in that area. It may be that if Labrador cod stock is capable of producing good year-classes in low temperature water under ice cover, perhaps the same sort of thing may be happening at East Greenland. Indeed, recent research on the relationship between cod at Iceland and at Greenland carried out under the auspices of ICES suggests there is quite a big population of cod at East Greenland which is largely inaccessible to the commercial trawlers, possibly because a substantial part of the population is under the ice. That relates to older fish rather than the juveniles. But the point I wanted to make is that, if the Labrador stock can produce strong recruitment at lower temperatures than those at West Greenland where recruitment appears to be poor, then the influence of climatic trend upon the fisheries becomes more difficult to interpret. The real mechanism relating the environment to the fisheries is further down the food chain, in the primary or secondary production where the zooplankton are being produced."*

Mr Lee: *"I was going to say when Mr Garrod first raised this question of the Labrador stock, that Dr Rodewald's map of the anomaly wind for the last 5 years shows the same situation at Labrador as at East Greenland, mainly, that the winds have been such that the cold water will have been confined much more to the coast. What you said about the Labrador stock holding up the analogy seems sound in that recent East Greenland year-classes have been good.*

"You have now raised another point. Will it ever be possible to say what factors govern the year-class strength? Yesterday Dr Templeman dampened our prospects. I wonder if he would like to discuss this question further."

Dr Templeman: *"A single factor may be a very good indicator and cover a number of other factors. For example, this inrushing of warmer North Atlantic current into the Norwegian Sea could imply quite a chain of events: higher temperature, greater flow; for the larvae – greater mixing of different water masses, more plankton, greater larval drift, etc. In West Greenland the recent paper by Hermann, Hansen and Horsted (F. Hermann, P. Hansen, and Sv. Aa. Horsted. In: ICNAF Special Publication No. 6, 1965) makes predictions. They knew the temperatures in certain years but the year-classes were not yet evident so they predicted year-class strength for a number of years ahead on the basis of temperature. Some of these predictions were correct but a number were not. So it is obvious that either the full information on temperature is not available, the temperatures are not being taken in the proper place, or temperature is just an indicator which sometimes is suitable for prediction and may be suitable on the average, but not always."*

Dr Løken: *"I am not very knowledgeable about this question of productivity in the ocean but it seems to me from the discussion that we really have to ask some very fundamental questions: What are the important factors that influence productivity? Is it the number of spawning fish? Is it the number of eggs that each of them spawn? Is it the temperature of the surface water or of the deeper water where the spawning occurs? Is it the temperature at the spawning place and at the spawning time or are the temperature conditions which the eggs are subsequently exposed to more important? What is the significance of the availability of food for the larvae, both in terms of quantity and quality? Perhaps we should look at key trace elements. We have seen productivity correlated with average temperatures for five years, for two years or for one year. This may be completely irrelevant. What we should have is perhaps the average temperature for the month when the spawning actually takes place.*

"I think it is very important to ask these fundamental questions so that we can go to the fishing grounds and measure the parameters that are important. I have a suspicion that we have vast amounts of data, on surface temperature and salinity, for example, from stations all over the North Atlantic and we might fall into a trap by making detailed analyses and developing extensive theories on the bases of data which happens to be available but which may not be relevant. I urge, therefore, that we should examine closely the various factors influencing marine productivity and make sure that we obtain measurements of the key parameters."

Dr Hassan: *"It is quite possible that some of the questions raised depend on variations on a very small scale. For example, where does the spawning take place or where do the eggs exist in relation to temperature? We may find that, for example, we have used temperature data from a weather ship in the Atlantic. Does this temperature really relate to where the eggs or larvae or whatever phenomenon we are looking at, exist? So we should start by analyzing the fundamental information that we have and ask questions, and, in many instances, we may find as Dr Løken has suggested that the factors we are looking at, although they look relevant, may not be relevant in a particular issue."*

Dr Cole: *"On this question of survival of larvae and the relative strength of year-classes, I am of the opinion that although there are many factors, as listed by Dr Templeman, which may affect the year-class strength a small* *number of them are of major importance. Some of them are quite critical and I am reminded of the work of Dr Cushing (D. Cushing. In: J. Cons. 33, 1969), which Dr Templeman also mentioned, showing that fish have the habit of spawning from year to year at very much the same time of year. There is a very small deviation from this standard time of spawning. This belief is based on not very much evidence but there is a certain amount of information from the shellfish field, where it is easier to gather evidence than it is in the deep sea in regard to fish. Here it is the presence, at the critical stages in larval development, of the right kind of food in sufficient abundance that is perhaps the most critical factor of all. Temperature, salinity, currents and many other things as well may affect this but I think in the end you come down to the question: Do the larvae have at this critical time sufficient of the right kind of food to eat? If they do, then the chances are that you will get a very high survival and a good year-class.*

"As Dr Templeman also mentioned, this is not a thing that is easy to get evidence about without an enormous amount of work. We have tried to delve into the situation much more closely in the southern North Sea where we have a convenient stock of plaice that has been well studied. Its spawning location is well known and is not far from the laboratory. But we find that, in practice, in order to examine the situation critically and thoroughly during a single season, we must follow the production of eggs, the spread of eggs and larvae, obtain the hydrographic data, examine the plankton and follow the survival of larvae. For this it is necessary to mount a succession of cruises of the order of half a dozen trips of 10 or 12 days duration. If one tried to do the same thing in a spawning area that was more extensive and less favourably located for study, then an appreciably greater amount of effort would have to be expended."

Dr Grosslein: *"Further to Dr Cole's comment, many will remember our proposal some time ago to select an area in the northwestern Atlantic that was reasonably circumscribed, within which we might be able to muster enough ship time to cover the spawning area and season of at least one species. Perhaps we should consider an intensive investigation on one of the major cod fisheries in the more northern areas of ICNAF. However, if we are ever going to understand the mechanisms that really control survival, we shall have to pool our resources for the type of intensive study outlined by Dr Cole. I would suggest that we should concentrate on a particularly important stock until we begin to get some answers. This would involve more than observing temperature. We should look at almost the whole water column, especially the upper layers where the eggs and larvae are at the mercy of the currents and other phenomena. There would be a considerable effort necessary to sort and analyze plankton samples quantitatively. We should be considering this kind of investigation, whether it be off Greenland, off Labrador or on the Grand Banks."*

Dr Dickson: *"I have a grouse which I should like to direct at the biologists for a change! An example will explain what I am getting at. At Iceland we know that the herring are in decline. We know from Cushing's work (D. Cushing. In: J. Cons. 33: 1969) that stocks of fish spawn at set times and cannot adapt their spawning time to rapid changes in their environment. We know from the work of Robinson (G. Robinson. In: ICES CM 1969/L: 16 [mimeographed]) with the continous plankton recorder that there has been a delay in the timing of the spring phytoplankton outburst over the past 20 years amounting to approximately one month in that region, which is a considerable change, with presumably some effect on the zooplankton. We then say, here is an environmental trend which should have had an effect on the fish stocks in this area, especially the spring spawning fish stocks. It is not hard to find evidence of decline in spring spawning fish stocks in the area, notably the herring, but when a fisheries hydrographer asks, Can you take the effect of fishing effort out of this fluctuation and let us see the residual variation?, they tell us that sampling procedures have not been rigorous enough in many cases to determine what the residual variation due to the environment may be. Now, in the case of the Barents Sea cod, Mr Garrod has been able to take out what he thinks is the effect of fishing effort on the stock and to hand me, as a fisheries hydrographer, the residual unexplained variation. Until I get this residual variation, I am not going to have much idea about fitting it to any particular environmental trend which I may know about. So my point is this: If you can as biologists, take away the effect of fishing from the jumble of variations from year to year, leaving the hydrographers with a clear residual variation which may be due to environment, then I think we would be a lot further forward in linking the environment with the year-to-year fluctuations of fish stocks."*

Mr Hennemuth: *"I have a few points! The first has to do with deciding which factors are important and developing a scheme to measure the effects of these factors on survival and production. This approach generally fails because we have not first erected the correct conceptual models.*

"I have been particularly interested in the probablistic nature of the system which we seldom seem to take into account. For example, we go out and observe a temperature change and if we do not see a change in something else,

we think we either have failed or have confirmed some hypothesis. In fact, with any change in any particular factor or cell of the system, the probability of the effect it is going to have on another cell depends on what state the whole system is in at the time. We must have some idea of this.

"My second point is that the effect of the factors which we are trying to measure feeds back on the particular sampling or measuring gear that we are using. In other words, the changes in the environment (temperature, etc.) may certainly affect the availability of fish to the gear that we are using. We tend to standardize gear over a long series of time and this may have the effect of ruling out the detection of the changes we wish to measure in the first place. If we do not know this, we probably will not be able to observe the kinds of things that we should and, what is more troublesome, will not know it. Another thing is that these long-term trends in conditions correlate with a number of things. There has been an obvious trend in the environment over the last 10 to 15 years and there has also been an obvious trend in fishing effort, for example, and these long-term correlations are probably mostly spurious. Dr Templeman said that there was a big 1963 year-class of Georges Bank haddock. Indeed, there was and also a reasonably big 1962 year-class and a not-all-that-bad 1964 year-class. This should have resulted in a big spawning stock shortly thereafter. Yet no great year-classes were produced and scarcely any year-classes at all. I think, in this case, the earlier 1960 and 1961 year-classes in the spawning stock were rather small and the spawning stock which produced the 1962 and 1963 year-classes was moderate to small. The fishery was so intense in 1965 and 1966 that very few of the large 1963 year-class got into the major part of the spawning population which consists of 4-, 5-, or 6-year-olds. Furthermore, they may have been in the population for a number of years but grew very slowly. Fish of the 1963 year-class grew slowly and were rather small as 3- or 4-year-olds at first maturity. This may have had an effect on their spawning efficiency. All these things lead one to believe that simple or generalized observations do not contribute much."

Mr Lee: *"I think your point about the gear which one uses may throw some light upon a mystery of the NORWESTLANT Survey. We found quantities of cod eggs but no larvae. Now we learn there is a good 1963 year-class at East Greenland. If you look at the NORWESTLANT report you will find that our colleagues at Aberdeen decided to go beyond the instructions to sample between 0 and 50 m with the 2-m stramin-net. Instead, they went at times to 100 m. You will also find that they caught more larvae there. Did we, in fact, miss the larvae because we stopped sampling at 50m? It certainly is odd that many eggs should appear on the first survey and that scarcely any larvae should appear in the second and third surveys, yet there is a good year-class."*

Dr Meyer: *"Large quantities of larvae of the 1963 cod year-class were found at the close of the NORWESTLANT Surveys off Southwest Iceland. Possibly in 1963 there was great inflow of Iceland cod larvae to East Greenland. Another explanation could be that our sampling for larvae in that year stopped too early."*

Mr Lee: *"May I now turn the discussion towards other topics that were covered by our speakers. I hope that the Chairman of the ICNAF Standing Committee on Research and Statistics (STACRES) Dr A. S. Bogdanov, and the Chairman of its Environmental Subcommittee, Dr N. J. Campbell, will forgive me if, at this juncture, I raise some points that will be discussed during the upcoming ICNAF Annual Meeting. A few years ago the Intergovernmental Oceanographic Commission (IOC), at the request of the Secretary General of the United Nations, drew up a long-term and expanded program of ocean research. A group called the Group of Experts on Long-Term Scientific Policy and Planning was set up under a former ICNAF colleague, Dr C. E. Lucas. It was to develop programs of research on an international basis. One of the Group's findings is as follows: One of the most important aims of the expanded program is to obtain more complete and more precise estimates of the potentials of the living resources in the world oceans. We felt that the approach must, at this stage, be essentially regional. Direct global studies of primary and secondary production are not proposed now because methodology and knowledge of these fields are not sufficiently developed to permit meaningful results to be obtained at that level. It is hoped, however, that general studies of marine productivity, comprising all levels from the enrichment processes to the fishery resources, undertaken in different types of enrichment conditions in the ocean, will lead to a better understanding of the complex processes involved and to better methods of quantitative measurements and hence, towards more direct studies of global production. Meanwhile, it is expected that fishery surveys and assessments undertaken on national and regional bases will steadily improve our knowledge of the world's resources and marine organisms. We were aware of the recent issue by FAO of The Fish Resources of the Ocean, which contains a review of all available knowledge on the subject and we propose that this document should be revised at suitable intervals and thus form an up-to-date record of global resource evaluation.*

"Now, this bears on what Dr Dickie was talking about yesterday. There is a clear obligation on ICNAF and similar bodies to undertake this kind of study in the future. Perhaps Dr Dickie would say a few more words about this general field of work and the direction it might take."

Dr **Dickie**: *"This is certainly not out of context with the discussion we have just been having. When we get to the stage where now we have 30 years of data on fish catches, where we have been attempting to get a long series of information on year-class strengths, where we have an equally long series of environmental data on a broad scale and where we still cannot see in these series meaningful trends or relationships, we are faced with several unarguable conclusions. Some of these you have already mentioned. That is, there is really very little hope that we are going to solve the problems of useful prediction, by relying exclusively on very broad-scale types of study. There seems to be no substitute at the moment for detailed data on particular areas of particular times. Having said this, I should admit that we biologists are always telling the hydrographers that We didn't really expect year-class strength to depend directly on temperature, and since temperature is only an indication of something, could you please tell us what it is so that we could measure the effect better or more directly. The hydrographer, I think, would be perfectly justified in replying But you haven't told us what it is in the way of fish production parameters that you would have us relate our observations to. We need real data on regional systems. However, we have to accompany these with the type of study implied by Mr Hennemuth. That is, in a very real sense the conceptual models on which we are trying to build relationships between fish abundance and environmental factors obviously just are not fine enough.*

"How to get more accurate conceptual models as a basis for prediction is something I think we always hope will be accomplished sometime in the near future as a result of someone waving a magic wand. Dr G. A. Riley (G. A. Riley. In: The Seas. Interscience Publishers, N.Y. 1963) in 1963, in considering problems of food chains and production in the sea, tried to show the degree of predictability which appears possible between major environmental variables and parameters which affect primary production, as well as those which may be used to predict secondary prediction. He also discussed the extent to which this same kind of knowledge might apply to the higher stages of the food chain and result in predictability for fish catches. I think that the evidence we have, together with the work Dr Riley and others have done, gives us some confidence that we can make useful predictions if we apply the research effort and get the actual data for regions. With good estimates of certain physical parameters we can estimate the primary production. From this we can go to some kind of predictive model of zooplankton. But we still have the major problem of linking the secondary production to the fish production. In ecological studies generally what we've had to do in the past and we still do is try to tackle this final problem from both ends. In the ICNAF area, we have missed out badly the bottom end of the production chain. Here, we are seriously deficient in data on primary and secondary production to relate to hydrographic trends. I think this is one of the chief reasons for not being able to interpret the very extensive time series of environmental variables presented at this symposium.

"In the ICNAF area, it might be said that we have in fact erred too much on the "direct approach" side of trying to predict fish production. But, as a result, we have good fisheries statistics. In our past assessment efforts we have tried, with reasonable success, to make something out of these. However, we are faced more and more, throughout the world, with a need to estimate fish production potentials. To accomplish this, fisheries statistics, even if not enough, are certainly a requirement. But we have not gone far enough in the direction of developing from them certain necessary parameters, one of which describes the relation between the total fish yield and the amount and kind of fishing effort, somewhat along the lines suggested in my paper and the other gives an elementary estimate of the actual biomass of fish.

"I would suggest that, if we are to make further progress within the next decade in predicting productivity from a regional body of water such as the ICNAF area, we must, first, make a more determined effort to actually and directly measure abundance of our fish stocks, i.e., obtain detailed area density estimates, and, information on fish sizes and, second, make a more decided effort towards direct measurement of the basic production parameters. From a combination of these, I believe we have some reason to hope that we can make sense out of the information on the physical environment and climatic change now available."

Dr V. **Yakovlev**: *"Due to the grand scale of present-day fishing operations the viewpoint prevailing in the past about unlimited resources of the ocean is proving to be wrong. However, if we take a retrospective look, we notice that over the centuries colossal changes in fish abundance have occurred without any significnat influence of fishing effort. That is, these changes were caused exclusively by environmental factors.*

"The present discussion confirms the significant role played by abiotic factors in the dynamics of the abundance of the commercial fisheries. However, our knowledge is far from perfect. Sometimes, we are unable to answer even the simplest questions. For example, is the temperature lower or higher in an area as compared with the previous year? If the water is warmer, then to what extent has it affected year-class abundance and the fisheries? In any case, future generations will still have much work to do on this problem, but we all look forward to eventual success.

"First, it is probably necessary to obtain agreement between the biologists' and geographers' viewpoints. It seems that our future research must take two general directions. The first direction should be to combine our efforts to conduct a complex of investigations, including hydrographical, biological, and hydrobiological studies at all trophic levels, in order to obtain the models which have been discussed here today. The second direction should be to provide experts with all the data required for evaluating commercial fishery resources and for forecasting fishery conditions. I am sure we all would agree that joint efforts within the ICNAF framework would yield positive results in the years to come."

Dr K. Mann: *"It may be useful to extrapolate from our experience in small scale investigations in this area. For a number of years the physical oceanographers went into a particular area – we chose St. Margaret's Bay for intensive study – and simply made a general study of processes as they saw them. The biologists did the same kind of thing, again, trying to understand the general system from their point of view. The hope was that a synthesis would emerge, but after a number of years when the two groups sat down to compare results, they found that the physical people had not been asking the questions to which the biologists needed an answer, and the physical people felt that the biologists were not matching their effort by monitoring biological parameters with sufficient intensity. It has proven to be a much more fruitful approach for the biologists to state very specifically what are the hypotheses on which they are working, what are the physical parameters which they need to marry to their biological data in order to test these hypotheses, and for the physical people, in their turn, to ask the biologists hard questions about how they are going to use the data which are produced at great trouble and expense. I wonder if the time isn't ripe for this kind of exercise to take place on a larger scale, for instance, on the scale of the ICNAF area."*

Dr Colebrook: *"Regarding the work of the International Biological Program (IBP), one or two projects on systems modeling are underway in the United States. These are concerned with terrestrial eco-systems but the organizational and administrative approach could well provide quite useful guidelines to a similar approach to the study of a particular fish stock. The organizational basis of one project was described at a conference held in the United Kingdom 2 or 3 weeks ago and one of the points made was that one professional scientist in ten employed in the project spends his entire time talking to the other nine finding out what they are doing. The figure of 10% seems a bit high but I was assured it was not at all unreasonable."*

Mr Lee: *"I feel certain that a director of a marine laboratory might have a difficult time explaining to his establishment officer that one man in ten should be employed purely for the purpose of walking around the laboratory holding conversations."*

Dr Dickie: *"In a sense this is following up something that both Mr Hennemuth and Dr Mann have said, and that Dr Grosslein has also referred to. Certainly, there is a need for a better set of specific models if we are going to start dealing with this environmental-biological interaction. I think that we are approaching a concensus. If we are going to make progress in the near future, we must conduct our studies in manageable areas in which we can mount a fair amount of coordinated research effort. But we are also approaching a stage where we can no longer do our research as a kind of background explanation, hoping to turn up something useful. It is becoming increasingly clear that we have all the kinds of data we can expect. Now we have the more specific task of taking these data and forming hypotheses which we will test as in a set of scientific laboratory experiments. Only in the natural environment, because there we cannot manipulate the way we would like to, do our experiments have to take the form of comparisons between critically selected areas. Even on the basis of the kinds of information now available, I think we can be more clever at setting up hypotheses and experimental situations than we have been in the past. For example, looking at the data in the tables and figures presented by Dr Templeman for cod and haddock, one is struck by the fact that, here we are trying to look at the effect of environment on year-class success. We are talking principally about cod because it is one of the commercially most valuable and interesting species in the ICNAF area. Yet if we look at the cod fisheries and try to measure changes in year-class success by looking at changes in the catch of different year-classes, we find that we are dealing with one of the two species of the area which shows the least fluctuation. In a sense, we have allowed ourselves the least chance of success by trying to test a general hypothesis* *about environment-production interaction, using as one variable something that is especially difficult to measure.*

"There are many reasons for so much ecological, especially fisheries, research having been done in this awkward way. But it is showing itself to be a very expensive, time-consuming and unproductive method, at a time when it is becoming increasingly important to derive a critical test for a hypothesis we have all had in the back of our minds for a long time. That is, Does increased fishing really deleteriously affect the natural biological productivity of the food-chains systems, hence its potential usefulness to us? If we hope to test this hypothesis we must be very careful not to fall into the cod-environment trap. That is, we have to be very careful to distinguish between testable and untestable situations. In this case, there seem to be at least two alternatives, one which may be testable and one not. On the one side, evidence is growing that, if we deal in terms of total biological production, we can see relations between it and certain of the environmental parameters. We may even relate components of the variation to certain of the in-between food chain parameters, and perhaps even come out with some generalizations which could be the basis for real prediction. However, as in the cod-temperature case, the situation in an international commission such as ICNAF is that people are not interested in total production. They are interested only in what they can catch, and often only in specific species. This means we have to face the difficult initial problem of picking individual species on which we can give answers. There may be many species for which we can say little. In fact, I would risk saying that we will be able to make predictions of total biological production for a natural system, perhaps useful, even up to a first order economic use, within 10 years. But I do not believe that we are going to be so fortunate when it comes to predictions for individual species. I would even venture the hypothesis that we shall never be in a position to do this for individual species in a system because of the basically probablistic and systems nature of the phenomena we are dealing with. So we may never be in a position to predict 5 years in advance, the yield of individual species from, say, egg and larvae or environmental information. Of course, short-term predictions will still be requested. What we have to be clear about, is whether or not these require a different approach. For individual species predictions I would expect that the best we can do is start looking at the abundance of a year-class 1 or 2 years before we would expect to start fishing. This is the way that the Assessments Subcommittee of ICNAF has approached the problem in recent years, and it looks to be a very good one. In between these two extremes of overall long-term prediction, and short-term species predictions, there is a large gap which we may be able to fill in a variety of ways in our future research."

Mr Lee: *"Could I raise a point that I think stems from Dr Colebrook's paper and from what you say? It was certainly raised by Dr Mann this morning. Dr Colebrook's paper gave us the start of a time series of the zooplankton standing crop in the Northwest Atlantic, and Dr Mann asked – Is this the right approach? Should we be doing this sort of biological monitoring or should we really be looking at the whole system? I wonder if we could discuss this a bit further. Should we be only towing plankton recorders and observing the catches, or should we really be testing a hypothesis, as Dr Dickie was saying?"*

Dr Colebrook: *"I believe that we should be trying to do some of both. The systems-model approach may yield results in about 10 years time. I am not sure that we have 10 years to spare until this system starts producing answers. Meanwhile, I am sure that we must continue monitoring the main events in the system and carry on with our rather empirical methods in order to produce some answers to the kind of problems that arise now or in the next year or the year after."*

Dr Dickson: *"I think Mr Garrod might agree that we have been testing a hypothesis in the Barents Sea. We have a good indication as to which part of the fluctuation in cod year-class strength is due to environment. We did forecast and we were successful in one particular year. The testing of the hypothesis comes from the joint Russian, Norwegian and British egg and larval surveys of the Barents Sea which have been carried out in the summers from 1965 onwards. Without these we would not have been able to find out whether we were right in 1970 in predicting a high survival of eggs and larvae. But I think the fact that we were right does mean that we were actively testing a hypothesis."*

Dr Mann: *"I would like to ask Dr Colebrook for amplification of his comments about continuing to muddle along with the methods that we have available now because of some impending crisis, implied or real, and ask what contribution he thinks the present approach to zooplankton distribution is making to an understanding of the events that are occurring in the fisheries of the North Atlantic. Also, whether he thinks that supplementing this information by organizing as quickly as possible a study of the dynamics of nutrients and phytoplankton production in the North*

Sea by some such method as putting technicians on ships of opportunity, would significantly increase the kind of contribution that we can make without, as he says, waiting 10 years for some kind of comprehensive systems theory."

Dr Colebrook: *"With regard to our direct contribution to the fisheries biology in the North Atlantic this is a very good question. I am not sure that we are measuring the right things. The question of the timing of events has already been raised. I think our laboratory was at least partially responsible for recognizing the importance of this. However, we are well aware how difficult it is to measure the timing of events from one year to the next. Supplying information to the fisheries is only one of the objectives of the Continuous Plankton Recorder survey. Another objective is to provide a data base from a relatively unpolluted area to help in making assessments, in the future, of what happens if and when pollution begins to have some impact. This is sort of a long-term investment. We would like to replace the Continuous Plankton Recorder with a black box that would measure something simple but just as useful. We are hoping to do something along these lines using our undulating oceanographic recorder. As I pointed out, we hope to achieve chlorophyll estimates and particle counting. If these provide short-cut measurements to events in the eco-system that seem to be useful and to provide valuable information, we will be happy to stop the counting of the thousands of animals that takes so much of our time at the moment."*

Dr Mann: *"More specifically, since you are putting your hopes on this mechanization of chlorophyll and particle counting observations, I would be interested to know how it squares up with your statement that we do not have 10 years to wait. What kind of timetable do you expect this kind of development to run to? Might it not be better tactics to think in terms of manually operated observations which, given a little financial backing, could be put into operation in a matter of months, if necessary?"*

Dr Colebrook: *"We will continue our present program until such time as we are certain it can be replaced by something better. With regard to immediate implementation of a ship of opportunity program to measure productivity and total biomass, our Institute would be very happy to have someone else do it because we have no experience in or expertize for such a program."*

Dr Dickie: *"Mr. Chairman, returning to the form in which you originally put this question, my recollection is that you were asking whether we should seriously consider giving up time-series studies in favour of some kind of short-cut which would involve the testing of a specific hypothesis. I don't have that much experience in fisheries but I have enough now to know that we have to view these alternatives in the light of the experience that our society has a habit of changing the kinds of questions it asks from time to time. When we started out a few years ago the fisheries question uppermost in the minds of society was: Are the cod stocks going to be damaged by fishing? The next thing we knew the question sounded something like: If the cod stocks aren't available in some years, is this because cod, haddock, and redfish interact in some way that is explicable? Of course the next thing on which information was wanted was: What other species might be available somewhere? Now everyone is asking: To what extent will the basic productivity in the fisheries system that we are dealing with be affected by pollution? Faced with this shifting pattern of new demands, even before there is information about the first one, the researcher has no substitute for detailed regional studies, i.e. for real data on particular localities through time. This is basic. Nor is there any substitute for attempting to look at a total system in as much detail as possible. This does not mean that we cannot or should not continue for the next 10 years to a certain extent as we have been for the past 30-odd years: exploring and hoping that something will occasionally turn up which will give unexpected and useful answers. Nevertheless, this whole systems approach is also an absolutely essential one.*

"I would like to add a comment with respect to plankton recorder surveys. I think there are many, myself included, who were once doubting Thomases with respect to the usefulness of this kind of work. I think that you could have found far fewer supporters for this type of exercise 10 years ago than you would now. Merely seeing a general pattern emerge from this exercise, as it has, is something that we are all grateful for. But now especially we cannot rest on our laurels. We are beginning through these data to see more clearly, what must be done. I personally hope that the data on species and relative density will be turned as quickly as possible into estimates of the kind of thing that we are all asking about, that is, production measurements. In the long run, it is information on production in the system as opposed to abundances, or availabilities, that we need to have."

Dr Meyer: *"Mr Chairman, I was very impressed by the papers given by Dr Dickson and Dr Rodewald showing the development of the climate and the environmental conditions during the last 10 years. ICNAF is an organization to* *benefit fishermen and I guess a fisherman now would ask the scientists of this symposium a very pertinent question: What will happen during the coming 10 years? If the downward trend continues it must have great consequences for the future fishery, for the reproduction of the fish stocks, and for the areas in which the fishery has to be carried out. If we have a further decrease in temperatures we will again lose the big northern feeding areas, which were populated by the important commercial fish stocks during the warming period of this century. Off Labrador and Greenland and in other northern fishing areas, increased amounts of ice hampered the fishery more and more in recent years. Will this trend continue? How much truth lies in the curve of climatic oscillations published by Johnsen, Dansgaard, Calusen, and Landway (S. J. Johnsen, W. Dansgaard, H. B. Clausen, and C. C. Landway. In: Nature 227, No 5257, 1970), which was shown yesterday by Dr Rodewald."*

Dr Dickson: *"The biggest word (I don't know if you saw it the same size as I did) that Dr Meyer used was the word if. If the climatic trend continues. I think this is all we can say. We can never forecast in the light of what has happened before, what is going to happen to the climate in the future. This has been fairly well demonstrated by the meteorologists and they take it as axiomatic. We can only say that if the climatic trend continues, this or that will happen to the environment, and when we get a closer knowledge as to the effects of environment on fish stocks, we will be able to say what will happen, if the trend continues, to these as well. But I think you are on very shaky ground when you start predicting what the climate is going to do in the light of what it has done just recently. The strengthening of the westerlies went on from the 18th century to the mid-1920's, but they have weakened very quickly since then. So I don't know what the climate is going to do next. I can only say that if the present trend continues and if you continue to get, for example, a boosting of northerlies over the Norwegian-Greenland Sea, there will be X increase in the icing hazard to fishing vessels (an important point which isn't raised often enough), there will be X change in the cloud cover with X change in phytoplankton production as a result. But then say Can you give me the environmental part of the year-to-year fluctuations in stock size?, and we are back to square one! Perhaps Dr Rodewald has something to add."*

Dr Rodewald: *"Concerning the difficult problem of climatic forecasting, we now have some additional different aids for gaining an outlook. One is from the North Greenland ice sheet. Past climatic variations are reflected by oxygen-18 variations in the ice, and the analysis of a very long ice core by Johnsen, et al. His collaborators have shown that climatic periods of 78 and 181 years dominated these variations through the last 800 years. A synthesis of these two periods approaches the oxygen-18 step curve to about 85%. By extrapolation of the combined curve, the future climatic development can be foreshadowed (with an 85% probability) as a continued cooling through the next one or two decades. The last peak of warming which was about 1940 was rather high, because the maxima of the two periods were coinciding. But now the two periods are going out of phase which points to the possibility of generally colder conditions even for some centuries (as in the "Little Ice Age", 1430-1850).*

"However, the difficulty with meteorological periods is that they hold for a certain time and then, for some unknown reason, suddenly disappear. Normally, if you have a three to four time recurrence of a period, and you think it is wonderful for making a prediction, you are just at the point of disappearance of this (short-term or long-term) oscillation. The same Greenlandic ice core that showed those well-marked variations between 1200 A.D. and the present reflected much more uniform climatic conditions during the previous warmer centuries (ca. 500-1100 A.D.). The recently dominant periods of 78 and 181 years were obscured during these former times, and they could die away once more in the future. It is this uncertainty about the persistence of periods that makes every extrapolation of periods risky.

"Prof. H. C. Willett of the Massachusetts Institute of Technology published a climatic outlook 20 years ago, before the present cooling trend. His prediction was based on the longer solar cycle of about 80 years, and on a sunspot-climate relationship for active and inactive sunspot periods. His climatic prognosis, made from quite a different approach than Dansgaard's recent ice-based attempt, was also that of a cooling trend for the northern hemisphere, and of an especially marked decrease of temperature in the Greenland-Spitsbergen-Iceland area.

"Willett's extended forecast has proven successful to the present, in spite of the fact that the first solar part of it was a complete failure. He had expected a late and very low sunspot maximum (not before 1962, and with a sunspot number near 50). What really happened was an early maximum (1957) with the highest mean sunspot number ever recorded (228). The explanation of this strange contradiction could be that the solar constant, as an indicator of the radiative force of the sun, has its maximum value with sunspot numbers of ca. 80-100, and that it decreases with lower as well as with higher sunspot numbers as suggested by Kondratyev (K. Ya. Kondratyev. In: WMO Bulletin

XX, No. 2, April 1971). In any case we have at least these two climatic outlooks, Dansgaard's and Willett's, which favour a further cooling trend. According to Professor M. I. Budyko of the University of Leningrad (M. I. Budyko. In: Izvestÿa Akad. Nauk SSR, Ser Geogr., No. 5, 1968), the recent cooling trend could be attributed to a decrease in solar radiation after 1940.

"On the other hand, one has to be aware of the fact that a man-made component is being introduced into the climatic development of modern times. The burning of coal and oil in enormous quantities is increasing from year to year and is increasing the amount of CO_2 in the atmosphere. As the atmospheric CO_2 has a greenhouse effect, its increase should cause a warming trend. Now, we had this secular warming during the first half of the 20th century, and it seemed to be obvious to many scientists to explain it by the increase of CO_2 in the atmosphere. Professor Bergeron of Uppsala (T. Bergeron. In: Reports of the Meteorological Institute of the University of Uppsala, No. 19, 1970) expects the CO_2 content to increase from 290 ppm in 1870 to at least 370 ppm by the end of the present century with a consequent global warming of ca. 0.5°C from 1960 to the year 2000. That would be the same amount as we had through the secular warming mentioned above. Thus this approach to a climatic outlook gives a result that is contrary to the cooling one.

"Furthermore, we must take into consideration the amount of energy which is being produced by the industrial activities in the world. Compared to the solar energy available in the radiative balance, the man-made energy – ultimately transformed into heat – is still very small, ca. one part per 5,000. But the rate of increase of the human production of energy is about 4% per year. If this rate of increase continues through future years and decades, the heat produced by man will be one part per 100 (1%) – compared with the available energy coming from the sun - in ca. 100 years. It can be shown by simple calculations that the amount of heat in our atmosphere which is artifically produced by man would equal the amount coming from the sun in ca. 200 years, if the present rate of increase of energy production continues. Such calculations were made by Professor M. I. Budyko (University of Leningrad) 10 years ago, and they seem to indicate that in the long run no Little Ice Age is to be expected, but rather the reverse.

"As accessories to our industrial activities, air pollution is increasing. Many think that this would produce a cooling trend in the atmosphere by diminishing the input of solar radiation. But what we really observe is that our big cities and industrial centres are becoming warmer and warmer, compared with the rural areas which have only a minor air pollution. As this artificial air pollution is mainly in the lower troposphere, its effect seems to be different from the natural pollution caused by volcanic dust blown up to stratospheric heights. The real heat production by combustion processes, augmented perhaps by the heating effect of CO_2 and by the heat retention exerted by smog particles appears to be more effective here than the reduction of solar radiation.

"Thus, it is really difficult for a climatologist to say what may go on in the future. Some outlooks favour a cooling trend, and some a warming trend and the outcome is in doubt. I could say, Qui vivra, verra, but I should also say that, at present, we have this cooling trend which we should watch carefully, because a certain feed-back mechanism may cause it to continue and intensify (a surplus of Polar ice and snow causing an increase of the albedo, thereby an additional cooling, etc.).

"Dr Loken was certainly right in saying that averaging environmental data over 5 years or so may be completely irrelevant for biological purposes. To understand the development of a single year-class of fish, we must make detailed observations, we must look at a particular year, a particular month, and perhaps at those few weeks or even days to observe the fate of the larvae. Averaging would be a foolish thing in this situation. But this is only one side of the problem. The other is to follow up the more general trends, their strengthenings, standstills, or reversals, their interrelations and interconnections. For this purpose, we need running averages, spatial means, a smoothing of shorter variations. In addition to knowing the details of environmental factors we must also know the net result of pentades and decades for large regions. To see the results of running 5-year periods for all the nine North Atlantic Ocean Weather Stations is essential for climatic balance.

"I showed you four maps of interdecadal winter cooling in the European sector of the Arctic. They show a spectacular climatic event, unprecedented in our century: 40 winter months (December - March, one decade) 1961-70 were, on the average, 5°C colder than in the preceding decade 1951-60 in the area of Franz-Josef-Land. The downward trend cannot continue at such a rate for another 10 to 20 years, otherwise one concludes quite improbable values. But such a climatic phenomenon might have after-effects which we should try to detect in time.

"I showed you the downward trend of sea temperature in the spring in the Polar Current off West Greenland. But now the Norwegian hydrographic station from which we had data over a 11-year period has been discontinued. We do not have the station's data for April 1970 and 1971. Consequently I will not be able to extend my trend curve over another running 3-year or 5-year period. I think it is a pity that such a station, after having gained the status of a standard station through 11 years of operation, could not be occupied regularly each year. Certainly there can be sound reasons for discontinuence, but, in such a case, if it were announced to the oceanographic world, some other interested country might undertake to continue occupying such a station. A trend could then be followed year by year as with the Ocean Weather Stations. These OWS are in the open Atlantic and it would be of great value to have some standard stations also on the fishing grounds, on the Banks, off Greenland, in the Barents Sea, etc. Such hydrographic stations occupied regularly each year would be an important counterpart to meteorological stations – important for detecting, watching, and eventually foreshadowing climatic trends near the environment of the fish."

Dr Løken: *"I would like to make a few comments about the interpretation of the Dansgaard-Johnsen curve. In interpreting the curve it is very important to note the assumptions they are making. These are very fundamental and if they were changed, results would have been very different. The last paper in in which the curve is presented, unfortunately does not give any details about the number of analyses which were made or in other words the number of data points which they have. If one looks at the previous paper where they discuss the much longer curve it turns out that they have approximately a 1,400-m long core and they have analyzed 1,600 samples, but from only 218 points in the core. That curve extends over more than 10,000 years which means an average of about two samples a century. If this is the typical sampling frequency, it is very difficult to pick out a cycle of about 80 years so we must look mainly at the longer cycles. In spite of this their study is extremely interesting and is the type which will provide many more valuable results in the future."*

Mr Lee: *"I must now draw this discussion to a close and ask Dr Campbell, our Convener, to say a few words."*

Dr Campbell: *"I do not wish to prolong this session but I do want to say that the organization of this symposium with its excellent review papers and discussion seems to have some advantages and we might organize in the same way to look at environmental problems in the future. I know that we have ranged over a wide field of environmental problems but it would appear from the way the discussion has gone today that we are now converging on the more critical and specific problems and plans must be made to deal with them. I would really like to do nothing more at this time than thank you all for your participation and contributions and hope that you will continue to support our effort to come to grips with the environmental problems in the North Atlantic. This Symposium is adjourned."*