Proceedings of the
Joint Oceanographic Assembly 1982
General Symposia

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Preface

The 1982 Joint Oceanographic Assembly was hosted by Canada on the recommendation of the Canadian National Committee for SCOR. This Committee extends its appreciation to all those who were concerned with the Assembly on a national or international basis.

The Assembly's success was the result of the efforts of many people who were involved in the preparation of the scientific program and the administrative arrangements. Although it would be difficult to single out all those who made substantial contributions, special thanks should be extended to Mr. G. N. Ewing, Chairman of the National Steering Committee, and Dr. W. Hay, Chairman of the Scientific Program Committee.

We are especially grateful to the National Research Council of Canada for its support of the Assembly and to the Scientific Information and Publications Branch of the Canadian Department of Fisheries and Oceans who arranged for the publication of the proceedings.

This publication contains a selection of edited papers from the General Session of the Assembly. We thank all the authors who have contributed to this volume.

Leo O’Quinn
Secretary
Canadian National Committee for SCOR
IN MEMORIAM

Commander Luiz Antonio de Carvalho Ferraz
21 February, 1940–11 August, 1982

Commander Ferraz, Brazilian Naval Officer, was a specialist in Hydrography. After following a course in the U.S. Naval Postgraduate School in Monterey, California, he obtained his B.Sc. degree in Oceanography in 1973–74, and in 1975 the same School awarded him his M.Sc. degree in Oceanography for his work on tidal and current prediction for the Amazon’s North Channel using a hydrodynamic numeric model. He wrote interesting papers on Physical Oceanography and represented Brazil at several international meetings, including SCOR and IOC. He was also a professor of Physical Oceanography in the Specialization Course on Hydrography for the Junior Brazilian Naval Officers.

At the time of his sudden passing, while attending the Joint Oceanographic Assembly held in Halifax, Nova Scotia, Canada, Commander Ferraz was acting as Chief of the Department of Geophysics of the Directorate of Hydrography and Navigation of the Brazilian Navy.

With his death at the early age of 42, the Brazilian Navy and the oceanographic community have lost a remarkable beloved scientist and coworker.
OPENING REMARKS

Welcoming Address

HONORABLE ROMÉO LÉBLANC

Minister of Fisheries and Oceans

Mr. Chairman, distinguished guests, ladies and gentlemen:

As Minister responsible for Fisheries and Oceans, it is both a pleasure and an honour for me to welcome you to the 1982 Joint Oceanographic Assembly. I am happy to know that more than 800 scientists from 36 countries have come to Canada on this occasion to discuss ocean science and the state of ocean research.

This is an opportunity for scientists from many countries to become personally acquainted with one another, and thereby contribute to the promotion of understanding and positive relations among the nations of the world.

I am particularly pleased that you have chosen to meet in Nova Scotia, where my own ancestors arrived by sea many years ago. Our first industry was fishing; our first settlements hugged the sea coast, and ships have challenged our northern waters for hundreds of years.

This city of Halifax is no stranger to the gathering of scientists and expeditions. It has hosted research and survey vessels from many countries, not the least of which was the famous British oceanographic research ship Challenger while she conducted a series of transects of the Gulf Stream in 1873.

At the present time, Canada is in the midst of a major expansion in its reliance on ocean resources and marine-based activities. In 1977, for example, Canada extended its coastal fisheries jurisdiction to 200 nautical miles, thus claiming full management control and responsibility for fish stocks within these limits. Since that time we have made determined efforts to rebuild fisheries stocks in our zones, and are having notable success with the enhancement of salmon on the Pacific Coast, and with many fish stocks on the Atlantic Coast as a result of strict conservation measures.

Offshore oil and gas explorations have been progressing for over a decade, and are showing promising results in such hostile environments as the Beaufort Sea, the Arctic Islands, the Labrador Sea, and the Grand Banks. Associated with these developments are requirements for transportation systems to bring hydrocarbons to market, and major projects are underway in industry and government to design shipping systems and production systems capable of operating in ice-covered and remote waters.

These offshore activities depend heavily on the practical application of scientific knowledge of the oceans and of the interrelationships between the marine environment and human activities.

Canada's oceanographic problems are in many ways a reflection of those facing the rest of the world. Our problems may be national in scope, yet the solutions may require global action. No single nation can solve all of these problems alone. We are in a period in which the nations of the world, impelled by a multitude of pressures, are, in fact, rediscovering the sea — reevaluating its importance and redefining its role in the affairs of mankind. The distinguished assembly gathered here is well

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1 Appointed Minister of Public Works on September 30, 1982.
aware of these changes — which are, for most of you, the reason for your professional interest.

Over the next two weeks you will be discussing the problems of the world's oceans. As a nonscientist, I will not presume to talk about the science itself, but as a politician I would like to take this opportunity of making some comments from my vantage point.

As I said earlier, man's activity is creating pressures on the oceans at an ever increasing rate. They take the form of conflicts between resource exploitation and conservation, between energy production and pollution, and between renewable and nonrenewable resources. Some of the symptoms are local, some are regional, and still others are international in scale. All of the problems require some sort of decision to be made, and these decisions will be made by people like myself, based on your advice and your knowledge.

Scientists and politicians work on different time scales. It is very seldom that the decisionmakers have the luxury of waiting until every possible fact and theory has been checked and double checked. We must make our decisions based upon the best advice available to us at the time. Governments look to the scientific community for advice, for guidance, and for advance warning of problems that will appear in the future. Political concerns are seldom purely scientific ones, but at the very least, we should have the benefit of the latest scientific knowledge. Thus your deliberations here and at other assemblies serve as a foundation upon which many decisions are made.

Today the seas are being used in ways never dreamed of a century ago. Irreparable damage to the ocean and ecological impairment from overfishing can result. During the first manned spaceflight around the moon, astronaut Jim Lovell radioed back that the earth looked like a fragile blue–green Christmas tree ornament. While there remains some debate about exactly how fragile the oceans are, we have reached the stage where management on a global scale is a necessity and no longer a luxury. This is no longer an academic exercise!

The interaction between man and his environment is now simply too large to ignore. It is easy enough to look at the effect that man has on the environment and to decry the despoiling of Nature. However, mankind does not deliberately set out to create global problems, oceanographic or otherwise. The pressures on the environment are a result of human society and they cannot be simply removed.

During the next two weeks you will be discussing many new and exciting issues in ocean science. In many instances the research may seem far removed from the political arena, but sooner or later the results of that research will likely impact on our day-to-day activities. Government looks to science to give it advance warning, and therefore an edge that will help us make the right decisions.

The predictive capability of ocean science depends upon our knowledge of the ocean and its processes. For example, with the possibility of a rise in Mean Sea Level due to the warming of the earth's atmosphere, a century of advance warning may not be sufficient. With 80% of the world's population living on or near the coasts, the political problems of relocation and coastal protection would be horrendous, and yet I understand that such a situation is in the realm of scientific possibility.

In the geoscience field, the delineation of marine resource potential of hydrocarbons, seabed minerals or even the exciting thermally active zones has an impact on national and world decisions far in advance of production. In the field of pollution, advances in marine chemistry will enable us to take action to prevent damage to the ocean from pollutants, hopefully before irreversible harm has been done.

The scientific aspects of all of these problems are obvious, but the results have to be sifted, compared, qualified, and quantified. The information fed to the decisionmaker in turn leads to another demand on the scientific community. In fact, a partnership must be struck between the social and economic considerations of the
politician, and the best available scientific judgments from you, the scientist. It is a fact of life that we often have to make difficult decisions, but it is a comfort when these decisions are made from a basis of solid scientific fact.

I am sure I am echoing the view of the majority when I say I see the value of assemblies such as this to lie primarily in their ability to allow scientists in many disciplines and from many countries to meet and to learn from each other. It is not only the individual scientific papers that are important, but also the free communication and overall exchange of ideas. These gatherings allow you to step back and gain a better view of the entire structure of global oceanography.

I hope you also gain a better understanding of my country and its people. I welcome you to Canada, on behalf of my government — and especially on behalf of our scientists who remind me very often that science and knowledge have no frontiers and speak only the language of understanding and hope.

Thank you!
I remember being told by a British marine biologist that ocean scientists would never speak one language, and never constitute a significant force in world science. This Halifax Assembly shows that he was quite absurdly wrong. It has brought together specialists from most of the disciplines of the sea. Halifax has a reputation in Canada for being a place where things fly apart with a loud bang, mainly because of our national propensity for bad jokes. Yet the Bedford Institute is built almost on the site of the celebrated 1917 explosion, and is a visible symbol of the rise of oceanography and its cousins to a place in the sun (or rather the photic layer). Perhaps the ocean scientists will be able to reverse the city’s totally undeserved reputation, and leave it remembered as the place where things come together.

To be asked to be your keynote speaker is a privilege, a pleasure, and a pain in the self-esteem. I count it a privilege to be allowed to preach to you before you get down to your real business. It’s a pleasure because there are many old friends in the Assembly, and I get a chance to catch up with the news. But it’s a pain because I am so inadequate. I know very little about most kinds of ocean science. I have some familiarity with climatology and with the role of the oceans as a modulator of the atmospheric climate. But on the whole I have taken the same view as my 4-year-old son did, when an athletic aunt of mine tried to get him into the English Channel at full tide for his first swim. “It’s too full,” he said, as he drew back from the waves. Often I say the same thing. And of my 215 Atlantic crossings, only one was on a ship.

The reason, surely, for my invitation to address you is the kinship that has grown up rapidly between the physical sciences of the ocean and those of the atmosphere, and perhaps to my status as a climatologist. Climate is one of the broadest ideas in circulation, and suddenly that circulation is wide. Everybody is interested. In this Assembly, I’d include everyone, from the geologists to the biologists. So it isn’t as silly as it seems to have a neighbour from the atmosphere start off your proceedings. Like the ocean sciences, those of the air have been utterly transformed in the past few decades by new technology, by the infusion and intermingling of ideas, and by the rising economic and political importance to all countries of good scientific understanding.

It is worth taking a few minutes to remind ourselves of the progress we have been making. I am old enough to be quite staggered by it. I was educated by Brunt and others on the basis of magistral books like Brunt’s own *Dynamical and Physical Meteorology*, Sverdrup, Johnson and Fleming’s *The Oceans*, and other notable volumes. They made it all seem cut and dried, as if nothing much else was left to be done. I was fascinated to see that Hank Stommel, in accepting this year’s Bowie award, made the same remark. Since these books were written, things have become much less certain. While I was an undergraduate, Carl-Gösta Rossby and Jack Bjerkenes were making the revolutionary changes in theory that were to transform postwar thinking, and which would finally bond together the atmospheric sciences with physical oceanography. Sverdrup’s *Oceanography for Meteorologists* appeared early in World War II to mark the linkage. We all know what has happened since, as the computer and the satellite have increased our capacity enormously. Prediction still eludes us on most time and space scales, but the empirical and theoretical scope of our combined sciences has increased out of all knowledge.

I note, for example, the extent to which we now know the characteristics of the polar ice, both at sea and on land. New sensors, new vehicles, and a willingness to spend money have given us an excellent understanding of the physics, chemistry, rheology, and geography of the sea ice, and of the continental glaciers. Remote sensing has removed the need for uncomfortable and often heroic field excursions.
Nansen, Sverdrup, the Soviet pioneers on the arctic pack, and the first penetrators of the antarctic and Greenland glaciers paved the way, but in recent times we have learned to rely heavily on sensing systems that require not courage but good delivery systems, good electronics, and good computers. These new methods have in no way negated the work of the explorers, but have immensely extended it — to the point where we know the underside of the oceanic pack-ice almost as well as we know the upper. Even as I wrote these words, the last issue of the *Journal of Geophysical Research* appeared, containing two excellent climatologies of the arctic and antarctic pack, both based on remote sensing techniques.

In comic contrast to our present sophistication, my first real research in postwar Canada was to help prove that Hudson Bay ever had a winter ice cover. At that time — 1946 — all competent authorities, notably the Hudson Bay Company post managers, the Oblate Fathers, and even the Inuit hunters, agreed that there was winter fast-ice along the shores, but that the central Bay stayed open. The *Arctic Pilot, Sailing Directions, Labrador and Hudson Bay*, and the *U.S. Ice Atlas of the Northern Hemisphere* all perpetuated the myth, which actually rested on the persistent separation of the central ice from the fast-ice by a lead. It took an aerological analysis by Burbidge, a climatological analysis by myself and Margaret Montgomery, and monthly R.C.A.F. reconnaissance flights across the central Bay to establish the existence of the annual Hudson Bay pack-ice cover, which has certainly never failed to form during the Bay’s 8000 postglacial winters.

One other personal reminiscence, this time about theory. My old teacher, Sydney William Wooldridge, said in 1948, during his inaugural address at Birkbeck College, that it was inconceivable that during our lifetimes the general equations of atmospheric motion would ever be integrated. At that very time Jule Charney was writing his paper on quasi-geostrophic motion, and John von Neumann was assembling the remarkable team at Princeton that was to make Wooldridge’s remarks look silly; the first integrations, as far as I know, were those on the ENIAC computer in 1950, only 2 yr after he spoke. Ever since then I have been cagey about predicting scientific progress, or the lack of it. And I am humiliated to think that while von Neumann, Charney, Eliassen, Eady, and others were laying the foundations of numerical weather prediction and climate simulation, my students and I were getting airsick and frozen trying to prove the presence of a pack-ice sheet whose existence should never for a moment have been questioned. It had to be there.

Ever since those earlier days we have watched a headlong race in the geophysical sciences between new theory and the flood of observational data made possible by the technological explosion. The theory of plate tectonics, for example, has radically altered the whole pattern of empirical research into the ocean floor; and seafloor technology has given that research some spectacular evidence — like the pictures of hydrothermal mineralization on the midocean ridges. Deep-sea drilling into ocean sediments has fundamentally changed our view of the evolution of Cenozoic and Quaternary climates. Isotope chemistry and the use of tracers have given us measures of the internal convective processes of the ocean which, when I was an undergraduate, we could only imagine. And in the atmosphere, the Charney–Eady–Eliassen theory of baroclinic instability was the inspiration for the successful Global Atmospheric Research Programme and its constituent experiments, notably FGGE. Today we can roughly match the demands of the huge atmospheric general circulation models with the flow of observations. The best way to design an empirical programme in geophysics is to be able to say, as we did about the Hudson Bay ice, that “it had to be there!” The had must come from theory.

It is likely, I’d say, that the next century will see a quite radical change of climate, in the ocean as well as in the atmosphere. It may be that we have little time left to study the contemporary climate. We know empirically that our industrial processes are altering the optical properties of the atmosphere. CO₂ is building up at about 4% per decade. Other infrared absorbers may be doing likewise. Several atmospheric
GCMs predict that such changes, if they continue, will raise surface temperatures substantially, and roll back poleward the snow and sea-ice limits. Some even predict the dispersion of the arctic pack-ice. Economic scenarios suggest that a doubling of the total burden of infrared absorbers is possible, even likely, in the 21st century. This possibility and its associated consequences are on your agenda, and I'll not try to scoop the speakers who will take them up. But I must point out that they are on the general scientific agenda because of the work of ocean scientists — the pioneer work of Roger Revelle, Hans Seuss, and Charles David Keeling, who monitored the CO₂ effect so effectively.

This hypothesis is yet another proof of the importance of the venerable theme of air–sea interaction. Thanks to a long line of meteorologists and oceanographers, most recently such figures as Budyko, Namias, and Wyrkki, we have seen such interactions as fundamental to the understanding of the earth's contemporary energy and moisture balance, and to intermediate and long-range weather forecasting. The events of the past 30 yr have shown that we must take an even more fundamental view. The climatic system includes both atmosphere and ocean in its domain. The heat transport achieved by the ocean circulation, and the enormous heat capacity of the waters below the permanent thermocline, are crucial parts of the system. Yet our models still do not include them adequately, or in some cases at all. Unfortunately our two media — water and air — insist on very different time scales of action, like science and politics. The so-called transient response of surface temperatures to CO₂ change, for example, is hard to calculate convincingly. The thermal coupling of the ocean and atmosphere turns out in some ways to be less tractable than the dynamical coupling, which, of course, is not independent. There is a huge job to be done here — well started at Princeton, at Boulder, at Bracknell, at Novo-Sibirsk, and elsewhere — but still mainly a labour for the future. So is the incorporation into our models of the Southern Oscillation or Walker circulation, currently the most visible of the known forms of air–sea interaction.

And may I let out a naive climatologist's cry of wonder at the mere existence of that bottom water, and of the evidence that has been gleaned as to its origins? To have a colossal reservoir and potential heat sink in the system that remains 10–12° K below mean global surface temperature is quite a trick — one that the heating and air conditioning engineers could well examine. So is the persistence over 10 million yr or more of the antarctic ice sheet. To have two asymmetric hemispheres is a marvellous opportunity for oceanographers and climatologists. North and south differ quite radically, yet are closely linked. Shortly before he died, Harald Sverdrup, who wasn't very patient with requests for after-dinner speeches, said in my presence (and that of several other people here today) that the arctic was up whereas the antarctic was down; and that the arctic was north of the antarctic. He'd be the first to say (but not after dinner) that the differences are more intriguing that that.

Of course the oceans don't figure in the climatic problem just in relation to heat and moisture. They are crucial to the carbon cycle itself, as are the bottom and shelf sediments. Bert Bolin has been preaching for years that the biogeochemical cycles are central to the physical sciences of nature, as well as to life. I agree with him. In the past few years we have been brought sharply up against the vital role of geochemistry in all these realms — not only in such things as isotope separation and the use of tracers, but in the actual physical functions of both atmosphere and ocean. Nowhere does Bolin's view fit the facts better than in the case of future CO₂ concentrations in the atmosphere. The two hard facts we have are that the 723 × 10⁹ tonnes of it now present (according to my latest bit of arithmetic) are increasing about 3 × 10⁹ tonnes per annum, and that this is about half the rate of release of fossil fuel carbon from chimneys. Where the rest of it goes, and how the system will behave in future depend critically on the geochemistry of the oceans and continental shelves — and, of course, on the planetary-scale circulation.
These and many other large questions will no doubt be discussed here and at many future Assemblies. I have a personal problem. I can hardly wait to hear the outcome of current controversies, especially as regards plate tectonics, climatic change, the stability of the antarctic ice and, for a change, what world population will be in 2050, if we avoid nuclear war. Unfortunately, at 63, I don’t expect to see the outcome myself. Some of you have been asking why I should have accepted appointment as the head of a religious foundation, Trinity College. I can now reveal that it is so that I can have powerful allies in my attempt to continue my journal subscriptions during the afterlife, whatever my then address.

I am, of course, a physical scientist, and I’m conscious of a tendency towards triumphalism in that capacity — of bragging about the great things the physical sciences have revealed about the natural world. But I have some biological training, and have tried to keep up with what the new biology is about. It is as revolutionary in content as the new physics of the first half of this century. Has ocean biology been as exciting as ocean and crustal physics and chemistry during the past three decades? I just don’t know, and thereby admit guilt, because I should have done my homework better. Certainly in the ecological arena, the role of the oceans has been heavily emphasized in recent times, or even dramatized. I expect to be educated immediately after this address.

Let me wind down on my favourite theme, the necessary internationalism of science. There is science in Canada, but no Canadian science. Science is an activity of the mind in the ancient rational and empirical modes that have served mankind so well for ten millennia. It belongs to all or to none. It may be subverted to other purposes, and it may even allow us to destroy ourselves and much of life. With better luck it can contribute enormously to human welfare. Nevertheless it is at root discovery, the conquest of the unknown, the tearing aside of obscurity. It must ignore the parochialism of every other form of human activity. It is supposed to deal in universals, and actually gets pretty close, provided that we don’t blinker ourselves.

As we all know, science needs transnational organization and allegiances. This Assembly is a manifestation of the internationalism to which we aspire. Ocean and atmosphere are pervasive and mobile parts of the world environment. Recognizing this, and that no one can stop winds, tides, currents, and cycles from crossing national boundaries, we have set up bodies like those who organized this Assembly. We have created international disciplines of observation, communication, research, and information that are simply not matched in any other body of human activity. It has been a great achievement, though all of us know that it wasn’t put together without endless argument, strife, patience, and persistence. I go to meetings of ICSU and U.N. agencies with a sinking feeling, and yet with the certainty that we must at all costs keep scientific internationalism alive. It is the lifeblood of our kind of science. And so it is of the ecology of world ecosystems. The International Biological Programme and UNESCO’s Man in the Biosphere Programme speak of the same conviction, and of the same imperatives.

It has been a privilege, Mr Chairman, to be allowed to air such lofty views before this Assembly. I am not always so lucky. At the World Climate Conference in 1979, I was relegated to the role of describing what we meant by climate, climatic change, and climatic variability. The role of philosopher-kings was reserved for the Conference Chairman, Robert M. White of the U.S., and K. N. Fedorov of the USSR. They both did great jobs. Fedorov said, for example, that the vital conditions to adapt the world economy to new climatic conditions were “to prevent world conflict and establish a lasting peace...[and] to stop the arms race and promote disarmament,” which seem unanswerable views as close as we are to Pugwash, N.S. And White said that...“the time is at hand to view world affairs through a climatic prism...We will recognize the central role of climatic processes in the shaping of the world’s economic and environmental welfare, its political stability, and even world peace.” I can’t aspire to such eloquence, but if I had the words I would make
similar claims for the proceedings of this Assembly. All good wishes for a healthy
debate.

Mr. Chairman, I find that in my work as a climatologist, the modern literature of
the ocean scientists is compulsory if difficult reading for me. I hope that from time to
time most of you return the compliment, and read the atmospheric literature. I
predict that the problem of future climate, oceanic as well as atmospheric, will be the
amalgam that holds us together. It will also be the reason why political leaders will
not be able to forget our needs in money, equipment, and warm bodies. I hope that
during your stay in Canada the climate will behave itself. It it doesn’t, take comfort
from the fact that it is bad climate that makes our subjects hit the headlines.

I am delighted you have come to Halifax, which is one of Canada’s most stylish
and attractive cities, and to Dalhousie, one of the country’s really major universities.
I hope that your stay is pleasant and memorable, and that your journey here will be
helped along by memories. Have a good conference.
MAJOR ADVANCES IN OCEANOGRAPHY

Significance of Runoff to Paleoceanographic Conditions During the Mesozoic and Clues to Locate Sites of Ancient River Inputs

WILLIAM W. HAY

Joint Oceanographic Institutions, 2600 Virginia Ave., N.W., Washington, DC 20037, USA

The breakup of the supercontinent of Pangaea to produce the present continents and the Atlantic, Indian, and Arctic oceans with their marginal seas has had a pronounced effect on the world ocean. The uplift and rupture of the continent results in a specific sequence of events. Ideally these are development of a rift valley with lakes above sea level, subsidence of the floor of the rift valley below sea level, invasion of the ocean, and separation and subsidence of the continental blocks (Hay 1981). The rift valley lake phase may extract large amounts of organic carbon from the global system and incorporate it into the sediments, and the early narrow ocean phase may result in significant amounts of salt being removed from the ocean and deposited as thick layers of evaporites (Southam and Hay 1981). Topographic effects may result in juxtaposition of lacustrine sediments and evaporites in the rift complex (Hay et al. 1982a). After these initial phases of formation, conditions in the developing oceans became more like those in the modern ocean, but because the breakup of Pangaea occurred largely during the Mesozoic when sea level reached a maximum, there are significant differences in the nature of the vertical mixing processes.

Vertical mixing in the modern ocean is dominated by plumes of dense cold water formed in the polar regions. The plumes fill the ocean with cold water masses which constitute about 90% of the total volume of the ocean. The warm surface waters are but a thin layer on this great volume of oceanic deep water.

In the course of analyzing results of the Deep Sea Drilling Project, it has become evident that the ocean of the Mesozoic was very different from that of today in that the great mass of oceanic deep water was warm rather than cold. Oxygen isotope studies suggest that transition from the "Mesozoic Ocean" to the modern ocean took place between the end of the Eocene and the Middle Miocene (Savin 1977; Berger 1979). The complexity of facies patterns of Mesozoic sediments has been indicated by Thierstein (1979) who showed very different patterns of carbonate and organic carbon distribution in the north Atlantic, south Atlantic, Pacific, and Indian oceans. Arthur and Natland (1979) summarized information on the distribution of organic carbon-rich black shales in the Atlantic, and Tissot et al. (1979) documented the distribution of different types of organic matter in the north Atlantic. It is evident that knowledge of oceanic mixing processes of past geologic periods may be very important in understanding where petroleum source beds may be expected to occur.

The notion that the Mesozoic Ocean may have been filled by plumes of warm, salty water goes back to Chamberlin (1906), and salinity stratification as a cause of anoxic facies and other changes has been discussed in a variety of contexts by Ryan and Cita (1977), Gartner and Keany (1978), Kidd et al. (1978), Roth (1978), Thierstein and Berger (1978), Arthur and Natland (1979), Weissett (1981) and Brass et al. (1982). The complexity of facies suggests that there were many competing sources of bottom water in the Mesozoic Ocean.

Peterson (1979) and Peterson et al. (1981) have presented a more rigorous discussion of the formation of dense downwelling plumes in the ocean and the formation of warm saline bottom water in ancient oceans. Marginal seas in arid regions have been cited by Southam et al. (1980) and Brass et al. (1982) as the most likely sites of formation of dense saline waters, but, as Kauffman (1979) has noted, many marginal seas show evidence of having had a reduced salinity.

The formation of dense water that can sink and become the deep water of the ocean is a function of the evaporation/precipitation balance, temperature, and freshwater input from land. Evaluation of the role of the atmosphere in evaporation/precipitation balance and temperature is best handled by modeling efforts such as those currently underway by Eric Barron and others at the National Center for Atmospheric Research (NCAR) in Boulder, CO. Evaluation of the role of freshwater input requires that the nature of ancient river and possibly glacial inputs be examined.

As a hypothesis, it can be suggested that because of the nature of the salinity–temperature–density relationship, it is ultimately the sites and volumes of freshwater input from land to the ocean that determine whether the ocean basins will fill with cold dense water formed by cooling in the polar regions or warm salty dense water formed by evaporation in shallow seas in the arid zones. As shown in Fig. 1, for marine waters having a temperature approaching 0°C, density is almost unaffected by further cooling, and changes in density are in direct response to changes in salinity. As suggested by Brass et al. (1982), freshwater input can reduce surface salinity.

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1 Present address: Museum, University of Colorado, Boulder, CO 80309, USA.
and effectively prevent formation of dense water. This is why deep water formation in the northern hemisphere takes place in the Norwegian and Labrador seas rather than in the Arctic Ocean proper. If changing conditions cause fresh water to be introduced into a polar region where bottom waters are being formed, the surface waters will be freshened, made lighter, and will turn off the downwelling density plume at its source. This has in fact been noted as a phenomenon observed in the Labrador Sea, where, during the period 1962–67, unusually large inputs of relatively fresh water produced stable stratification and caused convection and deep water formation to cease (Rooth 1982). At present, the cold water density plumes of the Weddell and Norwegian seas are protected from freshwater input, and hence dominate the production of oceanic bottom waters. Oxygen isotopic evidence (Savin 1977; Saltzman et al. 1981, 1982) suggests that this situation has only existed in the late Cenozoic, and that the Mesozoic and early Cenozoic oceans were filled with warm salty bottom waters. To understand vertical mixing in the older ocean, it will be necessary to determine the regions or sites and relative volumes of freshwater input. The direct geologic evidence for freshwater input into the ocean is in the form of sediment, delivered by rivers or glaciers. Because river inputs are much more important than glacial inputs, it is critical to understand the principles that govern where modern rivers originate, how they drain the continents, and where they empty into the sea. In the future it will be possible to apply these principles to ancient paleogeographic configurations and evaluate the effects of ancient freshwater inputs to the ocean.

**Generalized Continental Topography**

Figure 2 is the NCAR topographic model of the earth using 5° square average elevations. There are only six "high areas" on the earth when seen in this form; two of these, Greenland and Antarctica, are icecaps, and one, Tibet, is the product of a continental collision involving a subcontinent and great continental mass. The great momentum of these two masses resulted in mountain building on an extraordinary scale. Two areas, western South America and the northern part of western North America, are related to subduction when the spreading ridge axis is close to a continental block and the material being subducted is a relatively warm, young

**CONTINENTAL BOUNDARY CONDITIONS**

![Map of continental boundary conditions](image)

**Fig. 2.** The 5° average elevation map used by the National Center for Atmospheric Research for global circulation modeling. Average elevation contours are in metres.
oceanic lithosphere. Two areas, southwestern North America and Africa, are related to incipient continental rifting. It will be noted that in the past 30 million years, the axis of uplift and rifting in the southwestern United States has moved eastward (Eaton 1979). In southern Africa, the axis of uplift is to the west of the southern part of the east African rift system, suggesting the possibility that the axis of mantle upwelling beneath Africa may be migrating westward.

From the generalized topography of the present-day earth, it may be assumed that the major features of continental topography in the past have been the result of only three factors: (1) subduction, (2) continental collisions, and (3) processes prior to and immediately following continental rifting. According to the reconstruction of the Pacific back to 74 million years ago, recently completed by Whitman (1981) as part of the global tectonic modeling effort at the University of Miami under the direction of C. G. A. Harrison, the subduction of warm young oceanic lithosphere has been affecting the western margin of the Americas for a long time, and can be readily taken into account. Continental collisions have been relatively rare during the past 220 million years, and none compare with that of the Indian subcontinent with Asia, which has been in progress since the mid-Cenozoic. For paleogeographic reconstruction of older geologic periods, the processes preceding rifting and shortly after the separation are of overwhelming importance and should dominate the palaeotopography. In the near future, it will be possible to prepare areally averaged elevation maps for each continent using the general estimates of broad regional uplift analogous to those of present-day Africa and the southwestern United States.

The areas of unusual elevation shown in Fig. 2 are the result of deep-seated mantle processes driving the lithospheric plates. These anomalies show clearly on the plot of hypsography of the continents presented by Hay and Southam (1977) and Hay et al. (1981) (see Fig. 3). If these anomalous areas were removed, the hypsography of the continents in the absence of plate tectonics could be determined. The resulting hypsographic curves could then be corrected for loss of sediment off the continental blocks and for sea-level change, to provide the theoretical elevation of each continent at each increment of geologic time. The elevation increments resulting from plate tectonic processes active at each time can be added back into the hypsographic curve using the western margin of the Americas as a model for elevation related to subduction of young warm crust, and Africa (Burke and Whiteman 1973) and the southwestern United States (Riecker 1979) as models for relating elevation to the rifting process. The maps generated by theory are in preparation, using the paleogeographic base maps of

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**Fig. 3.** Semi-log plot of present-day continental and estimated Pangaea (200 million years ago) hypsographic curves. Antarctica plotted from measurements of bedrock topography and adjusted for isostatic compensation after removal of ice (see Hay and Southam 1977 for discussion). The present anomalously uplifted area of Asia ($5 \times 10^6$ km$^2$) due to continental collision shows clearly. The $2 \times 10^6$ km$^2$ of South America which are unusually high at present are the Andes, resulting from subduction. The broad anomalous uplift of North America at present ($5 \times 10^6$) is the result of both subduction and incipient rifting, but has been a feature characteristic of much of the Cenozoic. The change from the hypsography of Pangaea 200 million years ago to that of the present continents has been gradual and affected the different continents in different ways according to their location with respect to spreading centers, subduction zones, and colliding continental fragments.
Barron et al. (1981). When completed, they will be compared with and modified by data from the geologic record, which may be useful in estimating regional slopes. Ancient shorelines are particularly useful in providing clues to uplift on a scale large enough to affect the hypsographic curve (Harrison et al. 1981).

**Drainage Divides**

Present global circulation models assume that runoff from rainfall on land enters the ocean at the same latitude at which precipitation occurred. Rivers may transport fresh water latitudinally. If moved only 5° N or S from its source, the effect is probably negligible, but if it is transported 10° N or S the effect is likely to be important. The Nile, which has its source in the tropics, is an excellent example of a river, with a relatively small discharge, exerting an important influence on the oceanography of the arid Mediterranean. The location of drainage divides which may force N or S flow of rivers is very important to paleoceanographic modeling. In the first approximation, it is not necessary to know the number of rivers or where the individual river mouths were; however, the total area and location of a basin draining to a particular sea or ocean is of great importance.

Figure 4 is a plot of the annual discharge versus drainage area for the 20 rivers having the largest discharge (data from Holland 1978). The flow of the 10 largest rivers is $1.13 \times 10^8$ cm³/yr, and represents 25% of the total input of fresh water into the ocean. The 20 largest rivers supply $1.41 \times 10^9$ cm³/yr, about 31% of the world total. One of the 20 largest rivers, the Volga (14th in rank) drains to the Caspian Sea and is not included in the total discharge to the ocean. Most of the largest rivers have drainage basins spanning 10° or more in latitude, and have the potential to be of oceanographic

![Diagram showing drainage basins and discharge sites of 29 important rivers.](image)

**Fig. 4.** Annual discharge as a function of drainage area for the 20 rivers having the largest discharge. COL = Columbia, DAN = Danube, GAN = Ganges, IND = Indus, IRR = Irrawaddy, MAC = Mackenzie, MEK = Mekong, NIG = Niger, PAR = Parana, PE = Pearl, STL = Saint Lawrence, VOL = Volga.

**Fig. 5.** The NCAR 5° average elevation map showing the drainage basins and discharge sites of 29 important rivers, AMA = Amazon, AMU = Amur, BRA = Bramaputra, COL = Columbia, CON = Congo, DAN = Danube, GAN = Ganges, IND = Indus, IRR = Irrawaddy, LEN = Lena, MAC = Mackenzie, MEK = Mekong, MIS = Mississippi, NEL = Nelson, NIG = Niger, NIL = Nile, ORA = Orange, ORL = Orinoco, PAR = Parana, STL = Saint Lawrence, TOC = Tocantins, VOL = Volga, YAN = Yangtze, YEL = Yellow, YUK = Yukon, ZAM = Zambezi.
significance. This potential is enhanced if the river enters a semiclosed marginal basin.

Figure 5 shows the NCAR 5° average elevation map with the drainage basins of 29 important rivers. Note that none of these rivers crosses any of the four major uplifts, but 24 of these rivers have their sources on one or another of the four uplifts. It is evident that rivers tend to radiate from the large elevated areas. This would become more apparent if some other major rivers were taken into account. In South America, the Magdalena, Orinoco, Amazon, and Parana form a radial pattern through almost 180° of an arc to the north, east, and south of the Andes. In North America, the Rio Grande, Mississippi, Nelson, Mackenzie, Yukon, Columbia, and Colorado form a radial drainage of the western uplift. The Yangtze, Pearl, Mekong, Brahmaputra, Ganges, Indus, Ob, and Yenisey form a radial drainage around the Tibetan plateau. In Africa, the Nile, Zambezi, Orange, and Congo radiate from the east African uplift. The Niger and the Volga are the only major rivers that do not conform to the pattern of radial drainage from an uplift, drawing their water from the tropical rainfall along the coast of west Africa, and from the northern humid zone, respectively.

The myriad smaller rivers carry two-thirds of the fresh water to the ocean. The location of drainage divides between individual lesser rivers is unimportant on a global scale, although it may be significant for local geologic development. Drainage divides that direct water to different ocean basins or marginal seas are of great significance to paleoceanography. Drainage divides, being sites of erosion rather than sedimentation, tend to leave only vague geologic evidence of their existence in the form of unconformities commonly covering broad areas, but geological information can be interpreted to locate ancient divides, using a variety of criteria such as those employed by Vinogradov (1967–69). Where the positive geological information is inadequate, it can be assumed that the major drainage divides follow the axes of regional uplifts and that the lesser divides radiate from the uplifts following geologic highs such as those between the failed rifts or aulacogens as discussed by Burke (1977). It can also be assumed that drainage basins conform to the generalities suggested by Strahler (1952), Leopold et al. (1964), Gregory and Walling (1973), and Buedel (1977).

It is evident that the river systems feeding the passive margins have evolved along with the margin, and that an originally diffuse drainage will tend to become organized with time, concentrating a significant fraction of the runoff into a few large rivers. The structure of present-day drainage systems entering the sea along passive margins needs to be investigated to determine whether there is a pattern of drainage development that can be related to the age of the margin. If such a relationship can be established, it will be a useful clue in evaluating river input along margins where little or no seismic stratigraphic evidence exists for deltaic complexes. It may be that the present-day distribution of rivers is anomalous for most of earth history, and is only characteristic of the later stages of continental breakup.

**Location of Sites of Freshwater Input from Land to the Ocean**

The location and nature of freshwater input to the ocean can be determined from the stratigraphic record of continental margins and shorelines on the continental shelf and in the continental interior. The freshwater input from the continents includes river water, glacial ice, and ground water; at present, rivers transport 90% of the fresh water to the ocean (Garrels and Mackenzie 1971). Rivers transported essentially all of the fresh water to the ocean in ice-free periods; hence the following discussion concerns rivers only. Although there is no single direct relation between the mass of detrital sediment transported and the volume of water discharge, the sediments can indicate whether the input was from a large river or from many smaller rivers.

At present, the largest rivers carry not only large volumes of water but also volumes of sediment large enough to oversupply the continental shelf so that a cone is formed on the continental rise. Lesser rivers may feed fans on the continental rise and may be represented in abyssal plain deposits. As shown in Fig. 6 (after Hay

![Fig. 6. Paleolatitude of deposition of the major lithologies encountered at DSDP Site 530 in the southeastern Angola basin (after Hay et al. 1982b).](image-url)
et al. 1982b), at Deep Sea Drilling Project (DSDP) Site 530 in the southeastern Angola basin, a thick sequence of turbidites indicates an abundant supply of sediment from the continent during the Late Cretaceous when the adjacent continent was in a humid zone. The supply of carbonate detritus increased markedly as the continent drifted northward into lower latitudes; however, the sediment supply stopped abruptly in the Eocene, corresponding to the time when the adjacent continent area entered the arid zone proper. At the time sediment was being supplied to the southeastern Angola basin, the region of the present-day Congo drainage basin was in the arid zone. The drift of continents and drainage divides across latitude is one of the most important mechanisms that cause the demise and origin of great rivers as drainage basins are deprived of or supplied with rainfall. For many of the continental margins of the world, there exist reconnaissance seismic stratigraphic surveys that indicate the site and age of ancient deltaic systems; however, the sites of input of sediment can often be determined from regional relationships even if the river did not produce a major delta.

In the absence of any other geologic evidence, it can be assumed that after subsidence has allowed a coastal plain to form, rivers will preferentially discharge on passive margins along the sites of failed third arms of the triradiate rifts that coalesced to define the site of continental rupture. Figure 7, after Hay (1981), suggests that many of the world’s major rivers may discharge into the oceans at such sites.

It is recognized that during the high stands of sea level, rivers may discharge into seas far inland from the present continental margins. The paleogeographic maps of Barron et al. (1981) provide a base for evaluating the areas and depths of these interior seas in particular climate zones. As Schlanger and Jenkyns (1976) and Jenkyns (1980) have observed, marginal seas are especially likely to be perturbed by freshwater input and may be important in regional paleoceanography.

Relative Volumes of Freshwater Input

Qualitative estimates of relative volumes of freshwater input at the present time can be achieved by knowledge of the size and climatology of the drainage basins.

In evaluating ancient paleogeographic configurations, several possible climatic scenarios should be considered. One scenario should be similar to the present day, with ice at both poles, quasi-symmetrical zonal circulation, and strong equator to pole temperature contrast. A second scenario would be for a Pleistocene Earth, with expanded glacial conditions in the northern hemisphere and appropriately altered zonal circulation. Another scenario should resemble Flohn’s (1982) model for the Late Miocene–Early Pliocene, with ice in the Antarctic, but an ice-free Arctic, with more asymmetrical zonal circulation producing intensified oceanic upwelling, and equator to pole temperature contrasts differing in the two hemispheres. A fourth scenario should resemble the model of Barron and Washington (1982) with warm poles and strong ocean–continent temperature contrast.

It is entirely possible that as the study of geologic evidence preserved in coastal plain and continental margin sediments proceeds, other independent methods of estimating river discharges based on characteristics of the sediments may be developed. Although the evidence for a river mouth lies in the sedimentary record, the relation of sediment yield to hypsography and climatology of a drainage basin is complex. Figure 8 is a modification of one of the diagrams of Langbein and Schumm (1958) with the abscissa modified to show runoff rather than effective precipitation at 10°C. It em-

![Fig. 8. Annual sediment yield from accumulation in reservoirs as a function of runoff (after data from Langbein and Schumm 1958).](image-url)
time when large volumes of sediment were retained in the interior of North America (Clark 1975) and they may be the result of generally drier climates. In the Mesozoic, much of poleward heat transport may have been accomplished by latent heat of evaporation and precipitation, implying overall wetter climates than today.

**Impact of Freshwater Input on Paleoclimate**

Oceans with cold and warm bottom waters behave in fundamentally different ways (Brass et al. 1982a). Downwelling cold water plumes are oxygen rich and cause the oceanic bottom waters to be well oxygenated, inhibiting the preservation of organic carbon in the bottom sediments. Downwelling plumes of warm salty water are oxygen poor, and cause the oceanic bottom waters to be more prone to become anoxic if there is a large supply of organic carbon. Production of warm saline bottom water in shallow marginal seas in arid regions is probably the cause of the anoxic conditions characteristic of many ocean basins during parts of the Cretaceous (Arthur and Natland 1979). There were many anoxic events during the Cretaceous but it is unlikely that they were synchronous in all basins because each basin that became anoxic would have been fed warm salty water by a particular ephemeral plume. If one can determine precisely where plumes of dense water formed, it would be of great significance in understanding paleoceanographic conditions. Locating sites of freshwater input is particularly important in determining where dense plumes did not form. Freshwater input inhibits the formation of both cold and warm plumes; it may cause local stratification of the surface waters and promote stagnation.

Freshwater input sites are also important because it is there that terrigenous organic matter is introduced into the ocean. Most of the organic matter that accumulates in marine sediments is in deltas (R. Berner, Yale University, New Haven, CT, personal communication). Most of the rest of the land-derived organic matter accumulates on the continental shelf, slope, and rise. Organic material of pelagic origin is most likely to accumulate in areas distant from rivers. Terrigenous organic matter is not likely to serve as a source for natural gas; pelagic organic matter is a source of petroleum as discussed by Tissot et al. (1980).

It seems intuitively obvious that a site for the deposition and preservation of organic carbon in the bottom sediment is where the deep waters are dense, warm oxygen-poor saline waters formed in marginal seas in the arid zone, and the shallow waters are freshened and stratified by river input which also delivers quantities of terrigenous organic materials and/or nutrients. A second likely site for the deposition and preservation of organic carbon in the bottom sediment is where the deep water and dense, warm, oxygen-poor saline waters are overlain by an active upwelling region from which pelagic organic matter rains down. Because of the prevailing winds, sites of the first type are located on the western margins of the ocean in the arid zone while sites of the second type will be on the eastern margins of the ocean in the arid zone. The detailed paleogeography of
the marginal seas, location of river mouths, and configura-
tion of the continental margin will determine the
specific sites of accumulation of organic carbon.

Summary and Conclusions

The formation of oceanic deep water is a function of
the local evaporation/precipitation balance, temperature,
and proximity of freshwater input from land. Lowering
the salinity of oceanic surface waters by runoff
from land may effectively prevent the formation of
dense downwelling plumes and promote stratification
of the water column. During the Mesozoic, the ocean
basins were filled with warm saline bottom waters formed
at many local sources in marginal seas in the arid zones.
The impact of freshwater inputs, likelihood of
stratified conditions, and tendency toward anoxia were
much greater than in the present ocean which is driven
by vigorous cold polar plumes.

Determination of the specific sites of freshwater input
during the Mesozoic must await detailed analysis of
paleogeographic and paleolithofacies maps, but poten-
tial sites of freshwater input can be deduced from
analysis of the factors governing river inputs today.

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References

sediments in the north and south Atlantic: the role of
salinity in stable stratification of early Cretaceous basins,
Deep drilling results in the Atlantic Ocean: continental
margins and paleoenvironment. Am. Geophys. Union.,
Maurice Ewing Series 3.

BARRON, E. J., C. G. A. HARRISON, J. L. SLOAN II, AND
W. W. HAY. 1981. Paleogeography, 180 million years go to
the present. Eoclegue Geol. Helv. 74: 443–470, 1 fig., 9 pls., 3
tables.

Cretaceous climate using realistic geography. Simulations
with the NCAR global circulation model. Geol. Soc. Am.,

1982. Cretaceous climate: a comparison of atmos-
pheric simulations with the geologic record. Pal-

BERGER, W. H. 1979. Impact of deep-sea drilling on pale-
B. F. Ryan [ed.], Deep drilling results in the Atlantic
Ocean: continental margins and paleoenvironment. Am.
Geophys. Union, Maurice Ewing Series 3.

BRASS, G. W., E. SALTZMAN, J. L. SLOAN II, J. R. SOUTHAM,
Ocean circulation, plate tectonics, and climate. NRC
Geophysics Study Committee. Climate in earth history,

Warm saline bottom water in the ancient ocean. Nature
(Lond.) 296: 620–623.

Berlin, vii + 304 p.

Rev. Earth Planet. Sci. 5: 371–396.

BURKE, K., AND A. J. WHITTEM. 1973. Uplift, rifting and
Runconm [ed.] Implications of continental drift to the
earth sciences. Academic Press, London. 2

CHAMBERLIN, T. C. 1906. On a possible reversal of deep-sea
circulation and its influence on geologic climates. J. Geol.
14: 363–373.

CLARK, J. 1975. Controls of sedimentation and provenance of
sediments in the Oligocene of the central Rocky Moun-
tains, p. 95–177. In B. F. Curtis [ed.] Cenozoic history of
the southern Rocky Mountains. Geol. Soc. Am. Mem.
144.

WORSLEY. 1977. Estimates of Cenozoic oceanic sedimenta-

DAVIES, T. A., AND T. R. WORSLEY. 1981. Paleoenviromen-
tial implications of oceanic carbonate sedimentation rates,
Winterer [ed.] The Deep Sea Drilling Project: a decade of

EATON, G. P. 1979. A plate-tectonic model for late Cenozoic
crustal spreading in the western United States, p. 32. In R.
E. Riecker [ed.] Rio Grande rift: tectonics and magma-

FLOHN, H. 1982. Major climatic events associated with pro-

GARRELS, R. M., AND F. T. MACKENZIE. 1971. Evolution of
sedimentary rocks. W. W. Norton, New York, NY.
xvi+397 p.

event: A geologic problem with an oceanographic solution.
Geology (Boulder) 6: 708–712.

form and process, a geomorphological approach. Wiley,

HARRISON, C. G. A., G. W. BRASS, E. SALTZMAN, J. SLOAN
II, J. SOUTHAM, AND J. M. WHITMAN. 1981. Sea level
variation, global sedimentation rates and the hypsographic

HAY, W. W. 1981. Sedimentological and geochemical trends
resulting from the breakup of Pangaea. Oceanol. Acta 4
(Suppl.): 135–147.

HAY, W. W., E. J. BARRON, J. F. BEHENSky, AND J. L. SLOAN
II. 1982a. Triassic-Liasic paleoclimatology and sedimenta-
tion in proto-Atlantic rifts. Palaeogr. Palaeoclim. Pal-

HAY, W. W., E. J. BARRON, J. L. SLOAN II, AND J. R.
SOUTHAM. 1981. Continental drift and the global pattern

HAY, W. W., J. C. SIBUET, E. J. BARRON, R. E. BOYCE, S.
BRASSELL, W. E. DEAN, A. Y. HUCE, B. H. KEATING, C.
MCNULTY, P. A. MEYERS, M. NOHARA, R. E.
SCHALLREUTER, J. C. STEINMETZ, D. STOW, AND H.
STRADNER. 1982b. Sedimentation and accumulation of
organic carbon in the Angola Basin and on Walvis Ridge.
Preliminary results of Deep Sea Drilling Project Leg 75.

HAY, W. W., AND J. R. SOUTHAM. 1977. Modulation of
marine sedimentation by the continental shelves, p. 569–604.
In N. R. Anderson and A. Malahoff (eds.) The Fate of Fossil
Fuel CO2 in the Oceans. Mar. Sci. (NY), Plenum Press,


A Chemical Revolution: Water Mass Analysis in Physical Oceanography

CLAES H. G. ROOTH

Cooperative Institute for Marine and Atmospheric Studies, University of Miami, 4600 Rickenbacker Causeway, Miami, FL 33149, USA

Abstract

In the past two decades, research in physical oceanography, particularly in the United States, has been heavily dominated by the development and applications of observation techniques that focus on local processes. Only satellite mapping of sea surface topography, and perhaps the currently developing methods of acoustic tomography offer some prospect for large-scale motion field description on the basis of direct physical measurements. Meanwhile, substantial advances have been made in techniques of chemical measurement. Improvement in the precision of the classical chemical hydrographic determinations of salinity and nutrient distributions is complemented by access to a broad suite of transient trace substances such as radioactive compounds, and anthropogenic ones not previously encountered in nature. These developments have suddenly come into focus with the emergence of the climate problem as a central scientific concern, and the need to produce holistic models that show how the entire ocean system functions in cooperation with the atmosphere. An important test for such models will be the question of how they handle water mass production and modification. The biggest challenge facing us now is the development of adequate diagnostic methods. The inverse problem of what we can deduce about large-scale ocean dynamics from water mass tracer data is as of yet only partly understood. Some significant advances have been made, however, and the field seems ripe for rapid development.

Chemosynthesis at Deep-Sea Hydrothermal Vents

HOLGER W. JANNASCH

Woods Hole Oceanographic Institution, Woods Hole, MA 02543, USA

Abstract

The primary production of organic carbon by chemosynthetic sulfur-oxidizing bacteria has been proposed to provide the base of the food chain for the extensive populations of animals found at hydrothermal vents at depths of 2600 m. The oxidation of reduced inorganic compounds (such as H₂S, S²⁻, S₂O₅²⁻, NH₄⁺, NO₂⁻, Fe²⁺ and possibly Mn²⁺) as the source of energy for chemosynthesis is equivalent to the role of light in photosynthesis. Epifluorescence microscopy and biomass determinations demonstrated substantial bacterial densities in the emitted vent waters. Multilayered mats of unicellular bacteria were observed, often encased in heavy Mn/Fe deposits, as well as assemblages of Leucothrix/Thiothrix-like filaments and others resembling trichomes of apochlorotic cyanobacteria. Masses of Beggiatoa filaments were found on artificial surfaces deposited near the vents for 10 mo. To date, species of the genera Thiomicrospira, Thiothrix, and Hyphomonas have been isolated and studied in detail. Furthermore, a pure culture of an anaerobically chemosynthetic, extremely thermophilic, methanogenic bacterium was recently obtained as well as a number of “Type I” methylotrophic bacteria oxidizing methane and methylamine. The gills of bivalves, collected from areas intermittently flushed with H₂S-containing vent water and oxygenated ambient seawater, contained masses of bacteria showing high activities of sulfur metabolism and Calvin–Benson cycle enzymes. Likewise the “trophosome” tissue of the gutless tube worm Riftia was found to consist of procaryotic cells exhibiting ATP-generating and CO₂-reducing activity. Thus, three locations of chemosynthetic production are proposed: (1) within the subsurface vent system, (2) in microbial mats in the immediate surrounding of the vents, and (3) in various symbiotic associations with invertebrates. It appears that the predominant chemosynthetic production and most efficient transfer of organic carbon to the vent animals occurs via symbiosis.

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1 The complete text for this paper will be published in 1983 in Hydrothermal processes at sea floor spreading centers. P. A. Rona et al. [ed.] NATO Conference Series, Marine Sciences, Plenum Press, New York, NY.
Diapycnal Mixing in the Ocean Interior

CHRISTOPHER GARRETT

Department of Oceanography, Dalhousie University, Halifax, N.S., Canada B3H 4J1

To account for the existing patterns of temperature, salinity, and other properties of the ocean, to predict changes, and for proper regulation of waste disposal, we need to quantify the mean circulation and the role of mesoscale eddies, convection, and small-scale mixing processes.

Many oceanic processes, such as the mean circulation and perhaps mesoscale eddies, can be resolved in numerical models based directly on the laws of physics, but others will never be. Spatial scales of variability in the ocean range from 1 mm to 10 000 km, a factor of 10^10; it will always be necessary, in ocean circulation models, to parameterize the average effects of the smaller scale processes.

Diapycnal (or cross-isopycnal) mixing is an important class of processes requiring understanding and correct parameterization. There are three traditional approaches to the problem: inference, direct measurement, and process studies, (Fig. 1).

Inference

A value for the diapycnal diffusivity of a scalar may be obtained by fitting a model to an observed distribution. A simple example is Munk's (1966) derivation of a diffusivity of about 10^-4 m^2 s^-1 for the Pacific Ocean (excluding the top and bottom kilometre) from a model in which the mean temperature profile T(z) is achieved by a balance between upwelling (at an estimated rate) and mixing. More elaborate models, in Munk's and later work, make use of radioactively decaying tracers and of better measurements or estimates of the circulation. Attempts are also made to distinguish between the effects of diapycnal mixing and epipycnal (along isopycnal) advection and mixing, but ambiguities usually remain, in spite of remarkable progress which was described by Claes Rooth in this session and by other speakers later in the assembly.

Direct Measurement

Eddy Correlation

The most conclusive determination of oceanic eddy fluxes would come from measurements of the correlations between velocity and scalar fluctuations. However, the fluctuations in vertical velocity and temperature (or some other scalar) are largely due to internal waves and have a correlation coefficient that is <0.01. Measuring this with any statistical confidence would require an impossibly long record.

Dye Dispersion

Ewart and Bendiner (1981) reported the results of following the dispersal, for 3 d, of point releases of dye at 300 and 1000 m. Their work provides unequivocal evidence for diapycnal (and small-scale epipycnal) mixing, and further studies, as in the proposed Deep Ocean Tracer Release Experiment, may be uniquely valuable. However, the technical complexities, and expense, of measuring dye dispersal far below the sea surface make it seem unlikely that the technique can be used as widely as would be necessary to determine global variations in, and the correct parameterization of, diapycnal mixing.

Temperature Microstructure

The existence of small-scale (down to a few mm) wiggles in vertical profiles of temperature, in spite of the smoothing influence of thermal diffusion, suggests that some stirring is occurring, with resulting diapycnal transfer of heat. This idea is quantifiable (Osborn and Cox 1972) and Cox, Gregg, Gargett, and others have made numerous estimates of the vertical eddy diffusivity for heat at many different locations and times (see Gregg and Briscoe 1979 for a review). Values of 10^-6 to 10^-5 m^2 s^-1 are typical for the main or seasonal thermocline, but no clear dependence on larger scale variables, such as the mean stratification (i.e. depth) or mean shear is apparent as yet. The technique has certain drawbacks, and depends on assumptions that do not always hold, but its use to explore further the relationship of diapycnal mixing to other processes, particularly in the abyssal ocean, is most important.

Fig. 1. Schematic of approaches to diapycnal mixing.
Process Studies

Internal Wave Breaking

We expect that the rich spectrum of inertio-internal waves in the ocean will intermittently lead to overturning of isopycnals, or, more likely, to shear instability. From a kinematic point of view we expect that the vertical eddy diffusivity, $K_v$, is $\sim 1/100^2 \cdot T_e^{-1}$, where $Z$ is the vertical scale of a shear maximum and $T_e$ the time between mixing events in a vertical distance $Z$. Observed shear spectra (Garrett et al. 1981) suggest a value of about 1 m for $Z$; $T_e$ is unknown but is unlikely to be less than a typical internal wave period. If we take $T_e$ between $10^4$ and $10^5$ s, we have $K_v = 10^{-6}$ to $10^{-5}$ m$^2$ s$^{-1}$. From a dynamical point of view, the calculations of McComas and Müller (1981) suggest that nonlinear wave-wave interactions drain energy out of the near inertial, low-mode, energy-containing waves with a time scale, in the main thermocline, of about 100 d. If this energy cascades down to smaller scales where shear instability occurs, and if 20% of the energy lost goes into mean potential energy, then $K_v \approx 3 \times 10^{-6}$ m$^2$ s$^{-1}$ in the main thermocline.

We see that the kinematic and dynamic estimates of $K_v$ from internal wave observations and theory, and the typical value of $K_v$ obtained from temperature microstructure measurements, are all roughly consistent. (The long time scale of 100 d for the energy containing waves is also, at first sight, consistent with the apparent universality of internal wave energy levels.)

We may thus have a rough zeroth-order understanding of the diapycnmal diffusivity associated with internal wave breaking. However, we have very little idea of how $K_v$ depends on mean shear (or what the eddy viscosity might be in such a situation), or how it varies with depth. Eriksen (1982) has pointed out the important changes in the internal wave spectrum that occur within a few hundred meters of a sloping bottom, with the possibility of enhanced mixing. Later in this assembly, Ann Garrett will argue that, away from ocean boundaries, $K_v$ is inversely proportional to the Väisälä frequency $N$ (although the McComas and Müller (1981) theory weakly suggests that $K_v$ should be independent of $N$), so that a value of a few $\times 10^{-6}$ m$^2$ s$^{-1}$ at shallow depths increases to $10^{-4}$ m$^2$ s$^{-1}$ or more in the abyssal ocean, consistent with Munk's (1966) classical estimate. The suggestion is important and emphasizes the need for abyssal measure of temperature microstructure.

Double Diffusion

The existence of double-diffusive processes (particularly salt fingers) in the ocean is now clearly established. If staircase profiles of T and S are observed, then fluxes across the interfaces can be estimated using formulae derived from laboratory experiments. However, the following points should be made: (a) If a staircase with tread height $h$ is observed, then the fluxes, and hence $K_v$, are proportional to $h^{3/2}$. There is no theory for $h$ in terms of, say, the mean profiles of T and S. (b) The “interfaces” between layers occasionally seem to be made up of multiple thin layers. This would substantially reduce the fluxes. (c) Staircases are not always observed in cases where double diffusion could occur. Nonetheless, Schmitt and Evans (1978) and Schmitt (1981) have proposed a semi-empirical curve for $K_v$ as a function of the stability ratio $R_\theta = \alpha T/\beta S$, and, in a particularly intriguing study, Schmitt (1981) has used the dependence of $K_v$ on $R_\theta$ to account for the T-S relationship of the Central Waters.

The actual values for $K_v$ range from $10^{-5}$ m$^2$ s$^{-1}$ for $R_\theta \approx 1.9$ to more than $10^{-3}$ m$^2$ s$^{-1}$ for $R_\theta$ close to 1, and are positive for T and S, negative for the density of $\rho$. Double-diffusive processes cannot be responsible for diffusing dense bottom water back towards the surface.

Double-Diffusive Intrusions

In recent years it has become clear that double-diffusive effects occur not only in regions where the large-scale vertical gradient of either temperature or salinity is suitable, but also in frontal regions where T and S have large epipycnmal gradients. In such regions, intrusions some tens of metres thick develop and extend several kilometres laterally. They appear to change their T-S properties in a manner consistent with the existence of double-diffusive exchanges across upper and lower interfaces (Toole 1981), and have a thickness consistent with that predicted by Ruddick and Turner (1979) on the basis of laboratory experiments and simple theoretical ideas. However, their overall importance and correct parameterization has not yet been clearly established. Garrett (1982) has argued that the equivalent epipycnmal diffusivities are positive for salinity, negative for density (as for the usual double-diffusive processes), and generally negative also for temperature! He has suggested that thermohaline fronts develop as a consequence of stirring by the mean circulation and mesoscale eddies, and that the intrusive regions are merely the final stage of epipycnmal mixing, preventing the development of infinite epipycnmal gradients. The interesting point is that epipycnmal fluxes are associated with this epipycnmal mixing; Garrett (1982) has used an extremely crude model to derive a formula for the effective epipycnmal diffusivity and finds that it can be significant in regions with large epipycnmal gradients of T and S and weak vertical density stratification.

Baroclinic Eddies

The previous topic shows one possible interconnection between epipycnmal and baroclinic mixing, with the two mixing rates closely related. It is also possible that the flux across the average positions of isopycnals may be controlled by the horizontal eddy flux, in baroclinic eddies, across sloping isopycnals. This is certainly occurring near the surface due to features such as Gulf Stream rings; we do not know the extent to which it occurs in the ocean interior. Of course small-scale epipycnmal mixing processes are still required for the final modification of water masses, but the epipycnmal flux may be controlled by the baroclinic eddies. More work is required.
Boundary Mixing

It is well known that the properties of the central gyres may be determined at the sea surface, followed by advection along isopycnals into the stratified ocean interior. It has also been suggested (notably by Armi 1979 in recent years) that much of the diapycnal mixing in the abyssal ocean occurs in the turbulent bottom boundary layer, where stratified water can be mixed before being swept away into a nondiffusive interior. Simple estimates suggest that the process can be important, but that it depends on the cube of the mean speed above the bottom boundary layer and also on the efficiency with which mixed fluid is removed from the boundary. Neither is well known.

Summary

In this brief and incomplete review it is not possible to discuss the full state of our observational and theoretical understanding of diapycnal mixing in the ocean. However, it is clear that we do not yet have well-established values, or parameterizations, of diffusion rates for incorporation into numerical models of ocean circulation. This is particularly true for the abyssal ocean. Nonetheless, substantial progress has been made in recent years, through the traditional three approaches of inference, direct measurement and process studies. The approaches are complementary, and further work on all three is necessary. The work is technically exacting (as in the measurement of microstructure) and intellectually taxing (as in the calculation of internal wave interactions or in the problems of thinking clearly about horizontal versus epipycnal mixing) but the problems are exciting and the room for improvement in our understanding is very great. Perhaps by the time of the next Joint Oceanographic Assembly, a far more complete and unified picture will have begun to emerge.

References


THE OCEAN AND CLIMATE

Absolute Velocity Calculations from Single Hydrographic Sections

PETER D. KILLWORTH

Department of Applied Mathematics and Theoretical Physics, University of Cambridge, Silver Street, Cambridge CB3 9EW U.K.

The logical first step towards the definition of the large-scale ocean circulation (assuming it to be meaningful) involves a dense set of direct velocity measurements within the ocean. However, unlike their meteorologist counterparts, oceanographers have traditionally lacked such data. Those we possess, moreover, are badly aliased by tides, eddies, internal waves, and many other transient features. Since direct oceanic pressure measurements are also largely out of the question, it has become customary to try to discuss the mean ocean circulation in terms of — mainly — measurements of temperature and salinity.

There are many difficulties in the path of such a discussion, and these have been treated, either explicitly or implicitly, by many authors (Reid 1981 or Wunsch and Grant 1982) for reviews. The discussions can be categorised mainly on the level of reliance placed upon ocean dynamics in the circulation scheme concerned (by “ocean dynamics” one is including both the dynamics satisfied by the real ocean and the dynamics satisfied by the data, which may not necessarily be related).

There seem to be about four discernable levels of dynamics present in the literature on large-scale circulation. The level placing least reliance on dynamics is typified by Wüst’s (1935) core-layer method, which argues the necessity of spreading from an identifiable source of anomalous water properties (e.g. the low salinity subtantarctic intermediate water). This allows the inference of a direction of motion, and the approximate locations of velocity reversals between water masses deemed to be moving in opposite directions. There has been a recent return to this method, at least in a two-dimensional (meridional) sense, both by Wunsch and Minster (1982), who utilise linear programming methods to bound quantities like poleward heat fluxes, and by Shepherd (1980), who uses an ad hoc fitting of a mean flow on density surfaces to account for GEOSECS (GEOchemical SECTions Study) data. Worthington’s (1976) North Atlantic scheme discards dynamics and concentrates upon water masses.

The next higher level of reliance is the dynamic method (Defant 1941) which uses the “thermal wind” relationship to relate the vertical shear of flow normal to a hydrographic section to the horizontal density gradient within the section. A (guessed) level of no motion was then provided to yield a partly subjective picture of the normal velocity. This level was usually at great depth, or at least below the main thermocline. Only in regions like the southwest Atlantic and Pacific, where boundary layers of Antarctic origin were observed, was it necessary to raise the level of no motion to ensure equatorward flow at depth. The wide prevalence of sectional data has caused this method to be often used, with a concomitant reduction in emphasis and concern over the flow within, rather than normal to, the section (since the latter cannot be derived).

The appearance of, albeit incomplete and imperfect, global coverage by hydrographic data allowed a relaxation of the above subjectivity. It became possible to construct fairly reliable maps of geopotential anomaly between various pressure surfaces (although less accurate maps had been produced much earlier (Reid 1981). This presented a two-dimensional picture of the vertical shear of the horizontal flow field, by suitably following anomalies on, say, density surfaces, it was possible to put (nonflat) reference levels in such a way as to be consistent with the data. Leetmaa et al. (1977) used sections across the North Atlantic to examine the Sverdrup balance in this way.

The highest level of reliance on dynamics can be thought of as a consistency check on those dynamics. In the last 5 yr, three very different methods have appeared which use the same (or at least very similar) dynamical assumptions: the inverse method (Wunsch 1978), the β-spiral method (Schott and Stommel 1978), and the single section method (Killworth 1980). Since this paper will use the same dynamics also, it is worth examining the implications of the dynamical assumptions. We shall consider both what dynamics the ocean may satisfy, and what dynamics may be relevant to the data.

All three papers assume geostrophic balance, and hence the thermal wind relations, to hold. The regions of the ocean where this may fail are in the main well known (e.g. western boundary currents, equatorial systems, etc.). Provided the oceanic Rossby number is small, even quasigeostrophic flows are geostrophic to leading order, so that geostrophy is a reasonable approximation over much of the ocean. Because geostrophy and thermal wind are linear equations, furthermore, we can expect that “suitably averaged” data will also satisfy the same equations. Precisely what “suitable averaging” involves is unknown; the implicit assumption by most workers is that long-term space and time averaging is sufficient.
Mass and (some form of) density conservation are also assumed by the three papers: mass usually exactly, while the density equation may contain a source of noise. The mass equation is exact because, like geostrophy, the equations are linear and hence unaffected by long-term averaging. Density is less likely to be exactly conserved, for many reasons. Theoretically, there may be long-term time variation and eddy flux divergences present (although most estimates of oceanic mixing would make these terms small but not necessarily zero). Since advection is a nonlinear operation, furthermore, the averaged data will not necessarily yield “mean” densities which are advected by “mean” velocities without some (effective) diffusion term. Because of this, the inverse and β-spiral methods both attempt, in different senses, a best fit to the data. The single section method, in contrast, assumed density conservation to be exact also.

The inverse method makes no further assumptions. It uses many oceanographic stations around a closed area to give an underdetermined set of equations for reference velocities normal to the area; the underdetermined nature of the problem is handled by some rationale (e.g. minimum velocities at, say, 2000 m). Both the β-spiral and single section methods also use (implicitly and explicitly, respectively) the linear vortex-stretching equation. This, too, seems likely to hold in quiet regions unless there are strong lateral eddy stresses (Rhines and Holland 1979). The β-spiral method combines these assumptions with conservation of potential vorticity to yield an apparently overdetermined set of linear equations for the horizontal velocities at some reference depth. The best-fit solution can then be extended vertically by the thermal wind relationship to yield horizontal velocities. The single section method uses only density and its northward gradient, plus an estimate of Ekman pumping, to yield the velocity field and eastward density gradient, at the cost of an extra assumption: that there is a level of no motion present at some depth.

Clearly each method has advantages and disadvantages. The inverse method needs a lot of data, and has infinitely many solutions, but the mathematics involved yields error estimates on all quantities, which the other methods cannot do easily. It also places least emphasis on conservation of density. The β-spiral method relies more heavily on the derivation from dynamics (e.g. the potential vorticity conservation) and uses more data (density and its horizontal gradients) but yields a simple, easily comprehensible solution. Disadvantages include the need for accurate second vertical derivatives of density and the need for a horizontal velocity which spirals with depth. Finally, the single section method uses least data (in theory, but probably not in practice, two hydrographic stations would be sufficient) but suffers from the need for mixed data (i.e. Ekman pumping) and a level of no motion. However, like the inverse method, only first derivatives of data are required.

In this paper we examine an adaptation of the single section method designed to bring it closer to the other methods. The requirement of zero noise is relaxed, with a best-fit replacing it. This allows the velocity structure to be more general than before. However, the need for only a single section remains. The method can be thought of as an attempt to push the mathematical properties of the “thermocline equations” as far towards the real ocean as is prudent, and to see whether these properties hold up in the harsh light of data. At another level, one can think of testing the method as a check on the consistency of the assumptions of geostrophy, etc., as they relate to the real ocean.

The resulting method is described and applied to the North and South Atlantic. Sources of error are discussed later.

Equations

The equations to be used are slightly modified from those of Killworth (1980) by the inclusion of a non-zero mixing term in the buoyancy equation. Let \( p \) refer to the in situ density, and axes \( x \) east, \( y \) north, and \( z \) vertically upwards from the surface, with corresponding velocity components \((u,v,w)\). Let the vertical component of the Coriolis vector be \( f \), with \( \beta \) its northward gradient. (The axes may be turned through an angle \( \theta \) with no change in results, provided of course that \( \beta \cos \theta \) replaces \( \beta \).) Then the equations of geostrophy, hydrostatic balance, incompressibility, and density conservation then become:

\[
\begin{align*}
(1) & \quad -f(v\rho) = -px \\
(2) & \quad f(u\rho) = -py \\
(3) & \quad \rho z = -g \rho \\
(4) & \quad u_x + v_y + w_z = \frac{\beta w}{c^2} \\
(5) & \quad u\rho_x + v\rho_y + w \left( \rho_z + \frac{\beta \rho}{c^2} \right) = G \\
\end{align*}
\]

where \( g \) is the acceleration due to gravity and \( c \) the velocity of sound. Here \( G(x,y,z) \) represents the effect of terms like

\[-(\overline{w'\rho'})_x - (\overline{v'\rho'})_y - (\overline{w'\rho'})_z - \rho \]

on the density equation, where primes denote deviations from some suitably defined average, denoted by a bar, together with data-induced noise. We shall require that \( G \) be as small as possible, in a sense yet to be defined.

The thermal wind relations and vortex-stretching equations are then immediate:

\[
\begin{align*}
(6) & \quad V_x = -\frac{g}{f} \rho_x \\
(7) & \quad U_x = \frac{k}{f} \rho_y \\
(8) & \quad W_x = \frac{\beta V}{f} \\
\end{align*}
\]
where we see

\[ \rho \mu = U \]

for simplicity. Then (8) gives \( V \) in terms of \( W \), and (6), (8) give \( \rho_z \) in terms of \( W \) as

\[ V = \frac{f}{\beta} W_z \]

\[ \rho_z = -\frac{f^2}{\beta g} W_{zz} \]

and (5) may be rewritten, using (7), as

\[ UW_{zz} - U_z W_z - \frac{\beta g}{f^2} \left( \rho_z + \frac{g_0}{\theta} \right) W = F = -\frac{\beta g \rho}{f^2} G. \]

Equation (12) is a second-order ordinary differential equation for \( W \), with two boundary conditions, taken here as

\[ W = W_E, \quad z = -h \]

\[ W = -UH_z - VH_y, \quad z = -H \]

where \( W_E \) is \( \rho \) times the Ekman suction velocity, assumed to affect the interior of the ocean below some mixed layer, of depth \( h \). The bottom of the ocean is assumed uniformly sloping (gradients \( H_x \) and \( H_y \)) and frictionless; for a flat bottom, \( W \) would vanish at the bottom. We shall examine this bottom condition more fully later.

We shall assume throughout that density data are available on a N–S section, so that \( \rho, \rho_y, \rho_z, \) and \( c \) are known as functions of depth. However, \( \rho_z \) is to be deduced. From (7), we may write

\[ U = U_B + \bar{U}(z) \]

where \( U_B \) is the (unknown) value of \( U \) at the bottom, and

\[ \bar{U} = \frac{g}{f} \int_{-H}^{z} \rho_z dz \]

Then, if we write

\[ \gamma(z) = -\frac{\beta g}{f^2} \left( \rho_z + \frac{g_0}{\theta} \right) \]

the equation for \( W \) becomes

\[ UW_{zz} - U_z W_z + \gamma W = F \]

with boundary conditions (13), (14). If \( U_B \) and \( F \) were also known, (18) would have a unique solution.

The Problem for Zero Noise

Let us suppose, for this section only, that \( F \) vanishes identically. The properties of (18) were partially discussed by Killworth (1980). They depend crucially upon whether \( U \) vanishes anywhere in the range \(-H \leq z \leq -h\).

Suppose first that \( U \) does not vanish anywhere. Then both linearly independent solutions of (18) are well-behaved, since (18) has no singularities, and a linear combination of them may be found which satisfies the boundary conditions. Thus, provided \( U_B > -U_{\text{min}} \) or \( U_B < -U_{\text{max}} \) (so that \( U \) does not vanish), there are an infinite number of solutions as \( U_B \) varies. For large \( |U_B| \), for example, and a flat bottom, the first term of (18) dominates, and \( W \sim W_E(z + H) (H - h)^{-1} \). There would be no good reason to prefer one of these solutions to any other, in principle.

The evidence from \( \beta \)-spiral calculations such as those of Schott and Stommel (1978) suggests that in many parts of the ocean \( U \) does change sign with depth, however. It turns out that this can yield a possible closure to the problem, and a specific value for \( U_B \). Consider the solution of (18) near to a point \( z_0 \) where \( U \) vanishes. Then (18) is singular at \( z = z_0 \), as \( U \) is the coefficient of \( W_{zz} \). Assuming that \( U \) vanishes linearly at such a point, the two linearly independent solutions for \( W \) vary like

\[ W_1 \sim -(z - z_0)^2, \quad W_2 \sim (z - z_0)^2 \log|z - z_0| + U_z^2 \]

where

\[ J = \gamma(y - U_z) + \gamma U_z, \quad z = z_0 \]

is proportional to the \((y, z)\) Jacobian of density and potential vorticity.

The solution to (18) must in general involve both the \( W_1 \) and \( W_2 \) solutions. Since \( \rho_z \) is related to \( W_z \) by (11), inclusion of the \( W_2 \) solution would yield a logarithmically infinite solution for \( \rho_z \), which is physically unacceptable (the ocean is assumed slowly varying in space for geostrophy to hold). Therefore only under two sets of circumstances can an acceptable solution be found:

(a) if the coefficient of the \( W_2 \) solution should vanish. In other words, \( z_0 \), and hence \( U_B \), must be chosen so that the well-behaved solution \( W_1 \) satisfies the (in general nonhomogeneous) condition at \( z = -H \). Only if this solution then satisfies the condition at \( z = -h \) also has an acceptable solution been found. (For a flat bottom, the homogeneity of the problem allows the solution to be scaled so as to satisfy the condition at \( -h \).) This strategy, of making \( z_0 \), an eigenvalue to be found, was used for a flat bottom by Killworth (1980). It implies, from (19) and (10), that \( V \) and \( W \) both vanish where \( U \) does, so that a level of absolute no motion exists. Physically, this is obvious; since both density and potential vorticity are conserved on streamlines, and if surfaces of density and potential vorticity intersect in a curve, there can be no fluid flow at the intersection except along the curve. So if \( U \) vanishes, \( V \) and \( W \) will also, in general.

(b) if the coefficient \( J \) of the badly behaved part of the \( W_2 \) solution should vanish. By (20), this involves only those discrete values of \( z_0 \), and hence \( U_B \), at
which \( J \) vanishes. Both \( W_1 \) and \( W_2 \) are now well behaved, and can satisfy the boundary conditions. Physically, the surfaces of constant density and potential vorticity are tangential at points where \( J \) vanishes, so that there is no requirement for \( V \) and \( W \) to vanish when \( U \) does.

From the computational point of view, case (a) is preferable to (b), since evaluation of \( J \) involves second vertical derivatives of \( \rho \) and can thus be suspect. However, there are no physical reasons for preferring one option over the other. Indeed, at Killworth's (1980) point (20\(^\circ\)N, 30\(^\circ\)W) in the North Atlantic, there are several solutions of both types (a) and (b) possible. As Fig. 1 shows, with the exception of the very high Jacobian case, the solutions are almost indistinguishable. All solutions seem physically reasonable and very similar to the \( \beta \)-spiral calculations at that point (Schott and Stommel 1978); the predictions for \( V \) and \( \rho_\alpha \), from (10), (11), fit the solutions obtained from knowledge of \( \rho_\alpha \) and thermal wind well; and there seems no way to select either any one solution of a given type or one type of solution versus another.

Were this always the case, the method outlined above could be used at any point where \( U \) vanished at some depth, to derive the absolute velocity field and density gradient normal to the section, subject to the assumptions about the dynamics. However, when the method is applied to other points in the Atlantic, it can fail for either of two reasons:

\( (i) \) the possible solutions given by methods (a), (b) differ widely among themselves, and we lack a method to select any one solution; or

\( (ii) \) the profile of \( U \) is such that there must be more than one \( z \) for which \( U \) vanishes. In this case the well-behaved solution at one level of no E–W motion must in general contain part of the badly behaved solution at the other level of no E–W motion. Thus no well-behaved solution can exist.

The former problem can in principle be overcome by following well-behaved solutions as the latitude of the point under examination is varied. Presumably the physical solution would change smoothly with latitude, whereas the incorrect solutions would display some suitably abrupt behaviour and could be identified. But problem (ii) is more awkward. If no well-behaved solutions with levels of no motion can exist, what assumptions must be relaxed?

The purpose of this paper, which, incidentally, will also extend and generalize the method of Killworth (1980), permitting solutions of types (a) and (b) to exist, is to make the more realistic assumptions that there is noise in the density equation, but that this noise is minimal. In this way we return to the dynamical assumptions of the \( \beta \)-spiral and inverse methods. We shall also require, however, that the solution found for \( W \) be analytically well behaved (so that \( W_{zz} \) or \( \rho_\alpha \), be finite everywhere). This will turn out to yield a best-fit solution which is well defined in a certain sense.

**The Problem for Minimal Noise**

The constraints assumed in the zero noise section are quite strong. Suppose we specify \( U_{ji} \) such that a level of no (E–W) motion exists, and proceed to minimise the noise \( F \). Simple calculus of variations yields

\[
\delta \left[ \int_{-H}^{H} F dz \right] = \int_{-H}^{H} \delta W \left[ \frac{\partial F^2}{\partial W} - \left( \frac{\partial F^2}{\partial W_{zz}} \right) \right] dz
\]
\( \frac{\partial F^2}{\partial W_{zz}} = 0, \quad z = -h, -H \)

where \( F \) is given by (18). This yields

(23) \((UF)_{zz} + (U_x F_x)_z + \gamma F = 0 \)

(24) \( F = 0, \quad z = -h, -H \)

which is in general solved only by

(25) \( F = 0. \)

But the general solution for \( W \) of (25) has \( W_{zz} \) infinite when \( U \) vanishes. Hence we cannot simply seek to minimise noise without the further constraint, as indicated, that \( W \) be well behaved.

The method to be employed here is similar to the Galerkin technique. We write

(26) \( W = \sum_{i=1}^{N} a_i \phi_i (z) \)

where the \( \phi_i (z) \) are a suitable set of basic functions with which to expand \( W \). Trigonometrical functions suffer from Gibbs phenomena, so that we might choose, for example,

(27) \( \phi_i (z) = (z + H)^{i-1} \quad i = 1, 2, 3, \ldots \)

Then

(28) \( F = \sum_{i=1}^{N} a_i X_i \phi_i (z) \)

where

(29) \( X_i (z) = U \phi_{iz} - U_x \phi_{iz} + \gamma \phi_i. \)

We shall seek to minimise the integral of \( F^2 \) subject to (13), (14).\(^1\) Adopting Lagrange multipliers \( a_{N+1}, a_{N+2} \), to minimise

\[
\int_{-H}^{-h} F^2 dz + a_{N+1} \left( \sum_{i=1}^{N} a_i \phi_i (-h) - W_E \right) + a_{N+2} \left( \sum_{i=1}^{N} a_i \left[ \phi_i (-H) + \frac{f}{\beta} H_x \phi_{iz} (-H) \right] + U_B H_x \right),
\]

partial differentiation with respect to \( a_i \) gives

\[
\sum_{j=1}^{N+2} A_{ij} a_j = B_i \quad i = 1, 2, \ldots, N+2
\]

where

\[
\begin{aligned}
A_{ij} &= \begin{cases}
2 \int_{-H}^{-h} X_i X_j dz & i, j \leq N \\
\phi_i (-h) & i \leq N, j = N+1 \\
\phi_j (-h) & i = N+1, j \leq N \\
\phi_j (-H) + \frac{f}{\beta} H_x \phi_{iz} (-H) & i = N+2, j \leq N
\end{cases} \\
B_i &= \begin{cases}
0 & i \leq N \\
W_E & i = N+1 \\
-U_B H_x & i = N+2
\end{cases}
\end{aligned}
\]

is a symmetric matrix and

\[
\text{is a column vector. The coefficients } A_{ij} \text{ are determined solely by } (U(z)) \text{ and } (\gamma (z)) \text{ (and therefore by the choice of } U_B). \text{ If } H_x \text{ vanishes, the problem becomes homogeneous.}
\]

Equation (30) is thus an \((N+2) \times (N+2)\) set of simultaneous equations for the \( a_i \). When the \( a_i \) have been determined, the other quantities are found from

(33) \( W = \sum_{i=1}^{N} a_i \phi_i, \)

(34) \( V = \frac{f}{\beta} W_{zz} = \frac{f}{\beta} \sum_{i=1}^{N} a_i \phi_{iz}, \)

(35) \( \rho_s = -\frac{f^2}{\beta g} W_{zz} = -\frac{f^2}{\beta g} \sum_{i=1}^{N} a_i \phi_{izz}, \)

(36) \( F = \sum_{i=1}^{N} a_i X_i (z), \)

(37) \( \int_{-H}^{-h} F^2 dz = \frac{1}{2} \sum_{i=1}^{N} \sum_{j=1}^{N} A_{ij} a_i a_j, \)

(38) \( \bar{F} = \left[ \frac{1}{H-h} \int_{-H}^{-h} F^2 dz \right]^{1/2} \)

At the moment, \( U_B \) remains unspecified. It can now be chosen by seeking a minimum of (37) as \( U_B \) is varied. This is easiest done numerically, as the minimisation for \( U_B \) involves nonlinear terms in \( U_B \) (although \( X_i \) is linear in \( U_B \), so that the coefficients \( A_{ij} \) do not need to be recomputed each time). The result, after this final minimisation, will be the solution which best fits the data and minimises the noise in the density equation.

We may note some immediate consequences of the above. Suppose that the bottom is flat, or more precisely that \( UH_x \) vanishes. Then the resulting homogeneous problem scales with \( W_E \), and if \( W_E \) should vanish

\[\text{depth. Other weightings are possible and quite straightforward.}\]
anywhere, a trivial solution to the problem is $W = V = \rho_0 \tau = 0$. It is easy to see that except under unusual circumstances this would be the only valid solution, furthermore. Put another way, as $W_E \to 0$ we anticipate a solution in which $W$ vanishes at all depths. This is, for example, a property of the simplest class of solutions to the thermocline equations (Welander 1971) in which potential vorticity is a function of density alone; and the effect is clearly visible in Killworth (1980) (fig. 15). If there is a non-zero value of $H$, these comments would not necessarily apply. However, the tendency towards small values of $W$ and $V$ if $W_E$ vanishes must continue to hold, as we shall now see.

Rewriting (18), we have

$$\left( \frac{W_z}{U} \right)_z + \frac{\gamma}{U^2} W = \frac{F}{U^2} \quad \text{(small)}$$

so that a relevant vertical length scale $\delta$ for the problem is given from the definition of $\gamma(z)$ by

$$\delta \sim \frac{f}{N} \left( \frac{U}{\bar{U}} \right)^{1/2}$$

where $N$ is the buoyancy frequency\(^3\) given by

$$N^2 = -\frac{g}{\rho} (\rho_z + g \rho/c^2)$$

The nature of (39) is “exponential” if $U < 0$, “oscillatory” if $U > 0$. Taking values of $N$ in the North Atlantic of, say, $5 \times 10^{-3} \text{ s}^{-1}$ near 200 m and $1.5 \times 10^{-3} \text{ s}^{-1}$ near bottom, and $U \sim 1 \text{ cm} \text{ s}^{-1}$ gives

$$\delta \sim 450 \text{ m near surface}, \quad 1500 \text{ m near bottom}.$$ 

The rapid decay scale near-surface is clearly visible in the solutions of $\beta$-spiral problems (Schott and Stommel 1978; Behringer and Stommel 1980) as well as in those of Killworth (1980).

Thus the effects of east–west topographic gradients on the solution will be felt over only a distance of order 1.5 km vertically, provided $U$ is westwards at depth; a similar length scale will hold if $U$ is eastwards, but there may or may not be decay with height. It is likely, then, that the effects of bottom slope will be confined to the bottom 2 km and that the surface solution may under many circumstances be independent of the precise bottom condition used. We shall examine this possibility later.

The Case of Constant Coefficients

Before attempting to use oceanic data with this method, it is necessary to know if the method will work on some well-behaved analytical example. We consider the case when $U$, $\alpha$ and $\gamma$ are constants. After nondimensionalising, (18) may be written (Killworth 1980)

$$- (\mu + \lambda z)W_z + \lambda W_z + W = F \quad 1 \leq z \leq 0$$

where $\lambda$ is given and $\mu$ gives the unknown barotropic component, related to $\bar{U}_B$. Thus $(\mu + \lambda z)$ is proportional to $U$. Equation (42) has boundary conditions, for a flat bottom,

$$W = 0, \quad z = -1$$

$$W = 1, \quad z = 0.$$

For zero $F$, Killworth (1980) showed that the zero-noise solution to (42) involved the modified Bessel functions $I_2$ and $K_2$ of argument $2(\mu + \lambda z)^{1/2} \lambda^{-1}$. If $(\mu + \lambda z)$ changes sign in $-1 \leq z \leq 0$, the $K_2$ function is not permissible. If the $I_2$ function is to satisfy the boundary condition (43), it was shown that $\lambda$ lies in the range $0 \leq \lambda \leq 0.15$, and $0 \leq \mu \leq \lambda$, and

$$\mu = \lambda \left( 1 - \frac{\lambda}{\lambda^2} \right)$$

where $j_2$ is a zero of the second Bessel function. Only two such zeros yield solutions with interior levels of no motion, the zeroth and first. These give, for $\lambda = 0.1$ (the case to be considered here),

$$\mu = \lambda = 0.1, \quad U = 0 \text{ at } z = -1$$

$$\mu = 0.034, \quad U = 0 \text{ at } z = -0.34$$

and the first solution is clearly a rather special case.

The result of the noise minimisation in the minimal noise section is shown for various values of $N$ and a wide range of $\mu$ in Fig. 2. The reduction in total error if $N$ is increased by 1 is typically $1/2$–2 orders of magnitude. No clear minimum of the error is visible for $0 < \mu < 0.1$ (a level of no motion within the fluid) until $N$ reaches 7, when two obvious minima appear. These minima lie at $\mu = 0.034$ and 0.095, very close to the analytical zero-noise solutions in (46a and b). The minima become more sharply defined with greater $N$, as Fig. 3 demonstrates. The shape of the eigenfunctions, given by (33), is indistinguishable from the analytical solutions for $N \geq 7$. Hence the method of noise minimisation does yield the analytic zero-noise solutions in this simple case.

Application to the Ocean

It is straightforward to apply the method of the problem for minimal noise to oceanic data. At a given latitude and longitude, the values of $\rho$, $\rho_0$, and $c$ are required as functions of depth between the depths $h$ and $H$. The value of $h$ was taken uniformly at 200 m in what follows. We require the depth of the “surface” to be below at least the majority of the seasonal mixed layer, where geostrophy certainly fails, so this precludes calculations at too high a latitude (but see Stommel 1979, for a means of circumventing this restriction). However, the depth $h$ must not be so low that the value of $W_E$ bears little resemblance to the Ekman value $\text{curl} (\tau / f)$. 200 m represents a compromise in this matter.

Definition of the bottom depth $H$, and its horizontal gradients, presents many problems. The source of data for $H$ was the topographic tape prepared by Levitus and Oort (1977) which gives world topography averaged
over $1^\circ$ squares. The value of $H$ was taken as the value for that $1^\circ$ square, rounded to the nearest 50 m if necessary (most of the date already are rounded). The solutions are quite insensitive to the depth assumed, as it turns out. It is the values of $H_x$ and $H_y$ which are awkward. Precisely what length scales does the kinematic condition $w + u \cdot \nabla H = 0$ feel? Presumably the averaging of $\nabla H$ involves distances at least as large as an internal deformation radius, but beyond that fact we know very little. Too large an averaging interval (e.g. $15^\circ$ latitude or longitude) would erase prominent features like the Mid-Atlantic Ridge; too small an averaging interval ($2-3^\circ$, say) one feels may well give a result dominated by local features such as seamounts.

As a compromise, $\nabla H$ was computed by performing best-fit linear gradients to the Levitus–Oort data over a $6 \times 6^\circ$ square. The results were then examined visually to ensure that they fitted the author's (subjective) feelings as to the "correct" signs for the slopes. We shall discuss the effect of bottom slopes later.

The polynomials $\phi_i(z)$ used were the modified Chebyshev polynomials $T^*_n(z)$ which are defined on $0 \leq z \leq 1$ (Fox and Parker 1968). For many approximation problems, Chebyshev polynomials are known to give the most accurate and numerically well-behaved solutions. This seems empirically to be the case here also. In order to use the $T^*_n$, we nondimensionalise $z$ by

$$z' = \frac{z + H}{H - h}$$

and rescale $\gamma$ and $F$ accordingly by

$$\gamma' = \gamma (H-h)^2, \quad F' = F (H-h)^2.$$  

(Results will however be quoted in unscaled terms.)

The procedure, given a data source (see Data Sources), is as follows. The depth $0 \leq z \leq 1$ is split into 240 subintervals. At each value of $z$ involved, the part of $X_i(z)$ proportional to $U_B$ and the part dependent only on $\overline{U}$ are computed. This enables $A_{ij}$ to be calculated (using the trapezium rule for integration) as

$$A_{ij} = A_{ij}^k U_B^2 + A_{ij}^2 U_B + A_{ij}^3$$

where $A_{ij}^k$ are known and do not depend on $U_B$. Then, for a variety of values of $N$, typically 4(4)40, (30) is solved for equispaced values of $U_B$ (normally 100 values between $\pm 4$ cm s$^{-1}$), and the results for the mean-square noise $\overline{F}$ (38) plotted. This enables a visual check to see whether (a) the result is converging as $N$ increases, as we would require if the solution is to be meaningful; (b) approximately which values of $U_B$ yields the minimum noise, and (c) whether this minimum is sharp (as in the analytical example in the constant coefficients section, or, less satisfactorily, rather broad. Finally, a minimum-finder is used, with $N = 40$, to locate the minimum noise solution, and the results stored.

Data Sources

The choice of hydrographic data presents many problems. In the areas to be studied here (the North and South Atlantic), there are, respectively, many and few sources of hydrographic data. These are often of differing quality and reliability. Frequently, too, the reliable data (e.g. International Geophysical Year (IGY) sections) contain a great deal of eddy activity, which makes accurate estimates of long-term averages of density gradients somewhat tricky. (For example, Killworth 1980, notes that estimates of $\rho$, in the top 1000 m at Schott and Stommel's 1978 point B in the North Atlantic can be varied by a factor of 2 depending only on which IGY
sections are included in the data.) The best method is to take repeated sections through a point with the specific intention of acquiring long-term mean gradients (see the Eastward data, Behringer and Stommel 1980). Lacking such sections, however, one is forced to be selective and, hopefully, consistent.

The two sections selected for study are (a) in the North Atlantic, the line 30°W, 10–48°N, and (b) in the South Atlantic, the line 20°W, 10–48°S. These were chosen so as not to approach the equator too closely: the strong equatorial current systems and their instabilities make it unlikely that both components of flow remain in the mean geostrophic. The South Atlantic section follows Wüst’s (1935) Zentralschnitt closely, but continues on the same meridian while his section turns westwards, at 35°S when the Mid-Atlantic Ridge is encountered. The North Atlantic section again forms part of the Zentralschnitt and roughly overlies the Mid-Atlantic Ridge north of 40°N but is well east of the ridge further south. It passes into and through the Gulf Stream extension north of about 42°N. The sections are shown in Fig. 4.

Rather than use any specific combination of clean data (which would mean that the choice of representative data would vary from place to place), it was decided to use an existing data set which has already received use in the literature: the compilation and smoothing of pre-1973 MBT, XBT, and SD data onto standard depths and 1° squares by Levitus and Oort (1977). Whereas the smoothing and interpolation produces the odd effect of data at depths below the ocean bottom, at least these data have the advantage of uniformity in treatment and ready availability.

However, these data are less than satisfactory in many respects, at least at great depths where good data are sparse. Examination showed Atlantic temperature values at 4000 m at the equator 0.4–0.5°C colder than the IGY sections would suggest (whereas the salinity data agree well with the IGY data). The individual data points are somewhat less than smooth, and there are some apparently spurious deep density gradients (Wunsch and Harrison M.I.T., Cambridge, MS, personal communication). Fortunately, the results seem to depend mainly on the data above and within the main thermocline, with deep gradients playing a minor role.

The smoothing by Levitus and Oort (1977) was sufficient to yield reasonable estimates of the in situ density, but there was still too much noise present to permit evaluation of ρ_s by simple differencing of two neighbouring data values. Instead, for each point at which data was required, ρ, ρ_x, and ρ_y were evaluated from temperature and salinity values on a 10 × 10 set of 1° squares centred on the required point. These provided a least-squares linear fit to the data at each standard depth of the form

(50) \[ \rho = \bar{\rho} + x\hat{\rho}_x + y\hat{\rho}_y \]

where x, y are measured relative to the point in question. The three coefficients were then adopted for ρ, ρ_x, and ρ_y, respectively. The choice of a 10° square is a compromise: too small a square produces jagged behaviour of ρ_s due to noise in the data; too large a square means that linear trends can be omitted unless (risky) nonlinear fitting of the ρ surface is used. There is, simply, no optimal way either to acquire or to process the data.

The best fits to in situ ρ_s in the North and South Atlantic are shown in Fig. 5 and 6, and will be used to compare with the calculations. Note that rather larger values of ρ_s are found from this source of data at depth than maps of, say, geopotential anomalies would lead us to believe.

The Ekman pumping involves a knowledge of annual wind–stress τ, to evaluate \( w_E = \text{curl} (\tau/\rho_f) \). Although measurements are plentiful in the Northern Hemisphere, reliable wind data south of 30°S are few. The problem is compounded because the horizontal flux divergence between 0 and 200 m is unknown, so that the correct value of the Ekman pumping at 200 m is difficult to estimate. Because of this, it was felt that the precise value of the wind–stress curl was probably not required. As a rule of thumb, based on all the results of Killworth (1980), the vertical velocity at 200 m was taken to be half the surface estimate; the other 50% was assumed to be lost by the divergence of the energetic motions above 200 m.

The data set used was provided by Dr. P. Gurbutt of Ministry of Agriculture, Fisheries and Food, Lowestoff.
represented: in the North Atlantic at 30°W, \( w_E \) changes sign at about 13°N and again beyond 50°N; in the South Atlantic \( w_E < 0 \) until 48°S. This is in good agreement with Leetmaa and Bunker's (1978) estimates in the North Atlantic (their fig. 2), although the precise positioning of both sign changes of \( w_E \) may differ slightly.

**The North Atlantic Section (30°W, 10–48°N)**

Calculations were performed at intervals of 2° of latitude for the North Atlantic section. A convenient summary of the degree of fit is given in Fig. 8. The value of \( F \), or an individual value of \( F \), can be taken as a measure of the error in \( w \) (\( \Delta w \)) induced by noise. From (18)

\[
\frac{\beta N^2}{f^2} \Delta w \sim F
\]

so that a value of \( F \) of \( 10^{-15} \text{ s}^{-2} \), with suitable values of \( f \) and \( N \), gives \( \Delta w \) values of

- \( 8 \times 10^{-8} \text{ cm s}^{-1} \) at 10°N, 200-m depth;
- \( 1.4 \times 10^{-6} \text{ cm s}^{-1} \) at 10°N, 500-m depth;
- \( 1.5 \times 10^{-5} \text{ cm s}^{-1} \) at 48°S, 200-m depth;
- \( 2.3 \times 10^{-5} \text{ cm s}^{-1} \) at 48°S, 3500-m depth.

As Fig. 8 shows, \( F \) lies below \( 10^{-15} \text{ s}^{-2} \) except at the southern end of the section, and roughly decreases northwards. As a result, the average error \( \Delta w \) is of order \( 10^{-6} \text{ cm s}^{-1} \), which is very small. The increase in \( F \) at 10–12°N may reflect the eddy activity in the North Equatorial Current Systems. It is interesting that the effects of eddies in the Gulf Stream extension (north of 40°N, say) are not represented. There is no obvious correlation between the eddy noise as calculated here, and the map of eddy potential energy published by Dantzer (1977), nor with his estimates of isotherm displacements.\(^5\)

Alternatively, we can estimate the error in \( \rho_x \) from (18) and (11) as

\[
\Delta \rho_x \sim \frac{\beta F}{g \bar{U}} \sim 5 \times 10^{-14} \text{ gm cm}^{-4}
\]

if \( u \sim 1 \text{ cm s}^{-1} \); this is again small.

The noise is concentrated near the “surface” (i.e. 200 m), decaying rapidly in the first 50 m. Apart from 10°N, where \( F \) reached 1.5 \( 10^{-13} \text{ s}^{-2} \) at 200 m, all other values at 200 m were of order a few \( 10^{-14} \text{ s}^{-2} \) for lower latitudes, decreasing to less than \( 10^{-15} \text{ s}^{-2} \) beyond about 34°N. The deep values of \( F \) were everywhere tiny (10^{-17} to 10^{-16} \text{ s}^{-2} \) at low latitudes, an order of magnitude smaller at high latitudes). This is usually produced by small deep values for \( v, w, \) and \( \rho_x \), a point we shall return to.

These values of \( F \) can also be converted to an estimate of the size of cross-isopycnal (here vertical) mixing. Taking \( N \) as approximately \( 10^{-2} \exp(z/1000 \text{ m}) \text{ s}^{-1} \)

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\(^5\) Nor is the decrease of \( \bar{F} \) with latitude due to the decrease in \( w_E \). The value of \( F w_E^{-1} \) is significantly negatively correlated with latitude.
Fig. 7 Wind stress data used: (a) North Atlantic, (b) South Atlantic. Shown are (reading down) $\tau^x$, $\Delta \tau^y$, and the (halved) values of $W_E$. The three curves at the top of 7(b) are the Bunker data, the Han and Lee data, and (intermediate) the value used.

The degree of precision in the value of $U_B$ varied markedly but regularly from point to point. Figure 9 shows five “typical” sets of variations in noise with $U_B$ for varying numbers of polynomials. Adequate convergence seems to have occurred at all latitudes (i.e., the solution does not depend strongly on $N$). Almost always (except 10°N) a single minimum showed clearly; most latitudes showed sharply peaked minima for $U_B$. The range 26–36°N gave fairly “flat” minima; northward, however, the minima became increasingly more pronounced, as shown by the 48°N values. Hence the optimal values of $U_B$ were well defined, with some reservation about 26–36°N.

The values of $u_B$ stayed within about 0.6 cm s$^{-1}$ of zero at all but one point (Fig. 8), so that to a fair degree
Fig. 9. Noise variation as a function of $u_B$ at five North Atlantic latitudes. (a) 10°N, (b) 20°N, (c) 30°N, (d) 40°N, (e) 48°N.
These depths do not differ smoothly with latitude (nor is 28°N present), and this probably occurs because of both the method and the data. For example, apart from the different boundary conditions used, Killworth's (1980) point (a) is the same as the 20°N point here. While the solutions are qualitatively similar (both have westward surface and eastward deep flow; both have surface flows which are southward and downwelling), the shape of \( u \), given by the data values of \( p_x \), differs between the two, as do the magnitudes of \( v \) and \( w \).\(^6\)

The depths of no motion in the range 40–50°N are generally somewhat lower than the (first) reversal in sign of \( v \). This occurs around 1000 m, uniformly somewhat lower than Wüst's (1935) demarcation of water masses on the same section would suggest. However, they are in good qualitative agreement with the scheme of Leetmaa et al. (1977) which show southward flow above a level of no N–S motion which deepens northward from about 400 m at 16°N to 1000 m at 32°N. This also agrees with Defant's (1941) "depth of the lower tropospheric boundary," which he delineates from 400 m at 10°N to 900 m at 30°N.

Another view of the predicted gyral structure, and its depth variation, is shown in Fig. 12, which presents velocity vectors along the section in the top few hundred metres. The levels of no motion at 18–22°N are visible, and there is also evidence of \( \beta \)-spiralming at some latitudes, although at northern latitudes the velocity vectors are roughly parallel at each depth. Agreement with Wüst's (1935, fig. 47) circulation scheme at 200 m is excellent, save at his confused region 35–40°N.

Details of the calculated meridional flow are given in Fig. 13–15. These show the rapid decay of meridional energy in the first few hundred meters, with the diagrams dominated by the zero contour. The surface flow is southward except near the southern end of the section. The vertical velocity remains small (\( \lesssim 5 \times 10^{-5} \text{ cm s}^{-1} \)) except at latitudes 16 and 18°N (the latter is concentrated near-bottom). Figure 15 shows meridional velocity vectors, scaled to fit the size of the diagram (i.e. the arrows look horizontal because they reflect the properties of the solution). The almost complete lack of deep meridional flow is most apparent.

Finally, contours of the computed \( p_x \) are shown in Fig. 16, and may be compared with the Levitus–Oort (1977) values in Fig. 5. The comparison is at best qualitative, on several grounds. First, \( p_x \) is the second derivative of \( W \), so that its signal would be expected to be noisier than \( W \) itself. Second, regions of bad fitting (e.g. 12°N) display an oscillatory nature in the vertical. Third, as noted, there is reason to mistrust some of the deep density gradients in the data. Nonetheless, the surface configuration fits the data quite well, although its sign merely reflects the sign of the improved Ekman pumping. The most obvious difference is the lack of a negative \( p_x \) at depth at the northern end of the basin. If \( p_x \) decreased with depth from its surface value of 7.11 would

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\(^6\) Calculations with the IGY data used for 20°N, with the current method, gave results indistinguishable from those in Killworth (1980).
Fig. 11. Calculated North Atlantic values of $u$, $v$, and $10^4 w$ as functions of depth at five latitudes. Note the levels of no motion present.
Fig. 12. Calculated horizontal velocity vectors on North Atlantic section at depths of 200, 300, 400, 500, 600, and 700 m. The distance between 2-degree intervals corresponds to a 2 cm s⁻¹ velocity.

Fig. 13. Calculated contours of $v$ for North Atlantic section, contour interval 0.5 cm s⁻¹; negative values dashed. Note total preponderance of zero contour at depth.

Fig. 14. Calculated contours of $w$ for North Atlantic section, contour interval $5 \times 10^{-5}$ cm s⁻¹; negative values dashed.

Fig. 15. Calculated meridional velocity distribution for North Atlantic section. ($v$, $w$) arrows have been scaled so that their aspect ratio fits the box as drawn. Cutoff arrow length corresponds to 0.2 cm s⁻¹ horizontally.

Fig. 16. Calculated $\rho_c$, contour interval $4 \times 10^{-13}$ gm cm⁻⁴, zero contour doubled, negative values dashed.

suggest it should be negative at the northern end of the section (as computed). Is this the influence of ageostrophic terms in the vorticity balance due to the Gulf Stream extension?

How does this computed picture of flow at 30°W compare with observational schemes? Interpretation is difficult because of the wide variation within these schemes. Certainly maps of geopotential anomaly (Reid 1978, 1981) show predominantly east–west shear below 1000 m, with little tendency toward any baroclinic meridional flow. This is well confirmed by the current calculations. Above that depth (e.g. surface relative to 1000 db), there are indications of SW flow south of 22°N, SE flow to about 35°N, and E to NE flow north of there. Given the lack of information above 200 m in this calculation, the fit is quite reasonable, although the predicted gyre extends too far to the north. There is also good agreement with the Sverdrup calculations of Leetmaa et al. (1977), who found southward (meridional) flow at 200 m of 2, 1.3, and 1.5 cm s⁻¹ at 16, 24, and 32°N, with (meridional) flow vanishing below 400, 800, and 1000 m, respectively. The calculations here, on different data, give at 200 m, respectively 0.4 cm s⁻¹ northwards, 0.9 cm s⁻¹ south, and 0.6 cm s⁻¹ south, with north–south flow becoming weak about 330 m (but reaching 0.4 cm s⁻¹ at greater depths), 700 m and
1050 m, respectively. So the shape of the Leetmaa et al. (1977) scheme is well produced here.

Comparison with dynamic or inverse schemes is more difficult, since their solutions differ. Wunsch and Grant (1982), for example, show little flow over most of 30°W except a northeasterward tendency in the upper 500 m at the northern end of the section (whereas this calculation finds purely eastward flow there). Behringer and Stommel (1980), at 32°W, 28°N, find a level of no motion at 760 m, with southwestward flow at 200 m of 1.2 cm s\(^{-1}\) and mainly eastward flow of 0.3 cm s\(^{-1}\) at 1000 m. This calculation found (at 30°W but the same latitude) 1.3 cm s\(^{-1}\) southwestward at 200 m but westward flow of 0.3 cm s\(^{-1}\) at 1000 m, and no level of no motion, 28°N being the single point with that characteristic. Figure 8 shows how rapidly the computed solution is changing with latitude at 28°N.

A final comparison can be made with the heat flux calculations of Hall and Bryden (1982), which are probably the most complete attempt to date to evaluate "best-fit" velocities at a point, including barotropic flow. Figure 17 shows Hall and Bryden's (1982) fit at 25°N, together with the current results for 24°N, down to 1000 m. So in the subtropical gyre at least, this method seems to be giving reasonable results, although the barotropic components differ by about 2.5 mm s\(^{-1}\).

Overall, the quality of the solution is variable. At many points on the section, the solution seems acceptable and gives answers consistent with the data; at others, there is less reason to accept the solutions. How much of this variation depends on data sources, the assumed physics, or to bad behaviour of the method, is difficult to answer; we shall make some crude tests in the section on Errors.

The South Atlantic Section
(20°W, 10–48°S)

A similar set of calculations were performed for the south Atlantic. The degree of fit is shown in Fig. 18. The values of \(F\) were usually slightly higher than those in the North Atlantic, but the same decrease in noise polewards is visible. Again, the average error \(\Delta w\) is small, with highest values in the South Equatorial System, and no sign of any rise in activity as the Circumpolar Current is approached (as before, this is not merely due to the decrease in \(w_E\) with latitude).

The noise is again strongly "surface"-concentrated, with a decay scale of about 100 m. \(F\) reaches a peak of 8.7 \(\times 10^{-13}\) s\(^{-2}\) at 10°N, 200 m, is still just above \(10^{-13}\) s\(^{-2}\) at 16°N, but then decays poleward as for the North Atlantic. Deep values were consistently very small. Vertical diffusivities at 200 m are again of order 1 cm\(^2\) s\(^{-1}\).

The precision in the \(u_{bg}\) values was somewhat better than for the North Atlantic, as Fig. 19 shows, although there were quite rapid and inexplicable variations from one grid point to another: 38°S is almost as steep a minimum as 48°S, for example, whereas 40°S is rather flatter, as shown in Fig. 19. Again, the values of \(u_{bg}\) were almost always small, save for westward values of about 1 cm s\(^{-1}\) at 12–14°S. Hence the bottom serves as a rough level of no motion (Fig. 20a, b) except at low latitudes. Values of \(u\) at 200 m vary more widely than in the North Atlantic (reflecting the shear inherent in the data, of course), with westward flows north of 34°S of up to 4 cm s\(^{-1}\) giving way to eastward flows of up to 8 cm s\(^{-1}\) in the 40s.

Figure 21 shows vertical profiles of \(u, v,\) and \(w\) at the five latitudes. A great deal of vertical structure is present at latitudes 10–18°S, indicating probable poor fits. South of 18°S, however, a similar pattern to the North Atlantic recurs, with meridional motion confined mainly to the upper 1000 m and to within a scale height \(h\) of the bottom (See the problem for minimal noise). The magnitudes of the meridional velocities are, however, somewhat larger than in the North Atlantic.

There are again many points with levels of absolute no motion. With the same definition as before, latitudes with levels of no motion are (with obvious reservations about 10–18°S, due to the high vertical structure):
Fig. 19. Noise variation as a function of $u_B$ at five South Atlantic latitudes. (a) 10°S, (b) 20°S, (c) 30°S, (d) 40°S, (e) 48°S.
from about 37 to 42°S. Figure 20a is in good agreement with Defant's picture.

Figure 22 shows horizontal velocity vectors in the top few hundred metres (note the different scaling from Fig. 12). The gyral structure is visible, as are the (dubious) levels of no motion at the northern end of the section. Again, $\beta$-spiralling is only visible at the less poleward latitudes. The general pattern of Wüst's (1935, fig. 47) 200-m circulation scheme seems reproduced in Fig. 22, although the transition from westward to eastward flow is too far south. More detail of the calculated meridional flow is shown in Fig. 23–25. The strong surface concentration (except at northerly points) is visible on all three diagrams. The surface flow has a northward and downward component, with weak reversals at depth.

Finally, contours of the computed $\rho_v$ are shown in Fig. 26, and these demonstrate the inferior quality of the fit in the southern hemisphere. Comparison with the data in Fig. 6 shows little or no correlation except near the surface (although the southern latitudes have the wrong sign even there) and near-surface values of $\rho_v$ are grossly overestimated by the calculation. However, the calculation is in a gross sense reproducing, over much of the section, the feature of little E–W density gradient (seen clearly, for example, in Reid et al. 1977, fig. 3b).

There is a paucity of modern circulation schemes for the South Atlantic for comparison with the calculations at 20°W, largely because the evidence, strongly supported by this paper, is of little N–S motion below the top few hundred meters, so that most studies (e.g. Reid et al. 1977; Georgi 1981) have concentrated upon the more active western boundary layer. Certainly this paper would add weight to the belief that water mass spreading in the South Atlantic is not achieved by slow, "tongue"-like motions at all longitudes, as in Wüst's (1935) scheme, but in N–S movement in the boundary layer coupled with E–W flow into and out of that layer.

The smoothed inverse calculations of Fu (1981) also vary in their agreement with this calculation. Consider the meridional flow. The IGY data (his fig. 6) agree well at 16°S, to within its contouring interval, have the opposite sign at 24°S, and agree reasonably at 32°S. The Meteor sections (his fig. 10) seem slightly stronger than calculated at 15°S, the opposite sign at 21°S, but agree well at 28°S.

Since these flows are mainly weak, it is more straightforward to compare the E–W flows. Fu (1981) shows Antarctic Intermediate Water flowing westwards at 20°W (at depths of 600–850 m), in agreement with this calculation, between 10 and 35°S; the same holds for upper CPW, at depths of 1100–1350 m. Fu (1981) shows the upper North Atlantic Deep Water, at depths of 2000–2250 m, flowing west at 10–15°S, east further south, and southwards south of about 23°S. If anything, this is opposite to the current calculation. Lower still, however, in the lower NADW, the direction of flow again agrees well.

Schott and Stommel (1978) present $\beta$-spiral calculations in the South Atlantic, some of which may be compared with this calculation although the $\rho_v$ values
Fig. 21. Calculated South Atlantic values of $u$, $v$, and $10^{-4} w$ as functions of depth at five latitudes.
obviously differ strongly from those used here. The agreement is quite good, given the differing data. At 18°S, their point G (at 15°W) can be compared with this calculation. Schott and Stommel (1978; fig. 39) find westward flow of 2.5 cm s\(^{-1}\) at 200 m, with northward flow of 0.8 cm s\(^{-1}\). This can be compared with 3.5 cm s\(^{-1}\) and 1.5 cm s\(^{-1}\) respectively, found here. They find a level of very weak horizontal motion at around 670 m, compared with 725 m found here.

At 30°S, 15°W, Schott and Stommel’s point F (their fig. 37), they find 0.5 cm s\(^{-1}\) westwards and 0.35 cm s\(^{-1}\) northwards at 200 m; this calculation finds 1.5 cm s\(^{-1}\), 0.2 cm s\(^{-1}\), respectively. (This point adequately illustrates differences in data sources: the shear in \(u\) between 200 and 1000 m in Schott and Stommel (1978) is around 0.7 cm s\(^{-1}\); in this calculation it is nearer 0.3 cm s\(^{-1}\).)

The overall situation in the South Atlantic is thus less acceptable than in the North Atlantic for several reasons. However, the consistent finding in both hemispheres is the strong tendency towards very weak meridional flow below 1000 m, except when triggered by topography.

**Errors**

There are many sources of error in these calculations. Those involving the dynamics (i.e. the equations
used) have been discussed already. It does seem reasonable to explore the consequences of noise being present only in the density conservation equation, and, because that noise may be small, to see what minimal noise solutions look like.

This leaves the following errors: data error (both hydrographic and wind), and changes in the boundary conditions. Errors in wind data (i.e., the estimates of Ekman pumping) have a roughly linear effect on the solution. This is because the effects of the bottom boundary condition do not normally alter the entire solution, only that near the ocean floor. Hence altering \( w_F \) means that the upper part of the solution for \( v \) and \( w \), being approximately homogeneous, scales on \( w_F \). As a test of this, the case \( 10^5 \)S was recomputed with \( w_F \) taking the opposite sign. Except near the bottom, \( v \), \( w \), and \( \rho \) were very close to being the negatives of their original values. So little sensitivity to wind–stress data can be expected.

The hydrographic data used, as noted, are less than satisfactory. The data in the top 1000 m are believed reliable; it is the deep data which inevitably cause problems. One could make crude estimates of the reliability of the data by comparing them with estimates of, say, \( \rho \), from IGY or Meteor sections; except that these sections, being mainly E–W, are ill-posed for estimates of \( \rho \). Indeed, different sets of sections yield different estimates of \( \rho \).

As a test of the dependence of the solutions on the deep data, two tests were made (the second to be discussed in another context later). At \( 24^\circ \)N, the signs of \( \rho \) at 4000 and 5000 m were changed, and the solution recomputed. The minimum noise was reduced slightly, from \( 2.7 \times 10^{-16} \) to \( 2.4 \times 10^{-16} \) s\(^{-2}\). The bottom value of \( u \) remained weak \( (4 \times 10^{-3} \text{ cm s}^{-1}) \) so that the value at 200 m was reduced from \(-1.6 \) to \(-1.3 \text{ cm s}^{-1}\), due to the differing thermal wind at depth. The solution for \( v \) was changed by less than 2 mm s\(^{-1}\) near surface, and a fraction of that at depth. So, without performing what would be a difficult numerical error analysis, it seems that the solution may only depend weakly upon deep data and conversely, strongly upon surface data, which are more reliable.

The solution does depend on what depth the “surface” condition is applied, i.e., the value of \( h \). At \( 24^\circ \)N, an independent calculation was run with \( h = 100 \) m instead of 200 m (which would mean the mixed layer was below the depth \( h \) at times). The value of \( w_F \) remained unaltered. The mean noise \( \tilde{F} \) rose by a factor of 5, with \( F \) at 200 m doubling. The value of \( u \) was increased by 1 mm s\(^{-1}\); without suitable redefinition of \( w_F \), \( v \), and \( w \) cannot meaningfully be compared.

The most extensive set of test calculations were performed to test the relevance of probably the least justifiable of the boundary conditions: that of no normal flow at the bottom. Since bottom gradients are not well defined, the role of the benthic boundary layer is unknown, and deep values of \( \rho \) and \( \rho_0 \) may be suspect, a complete set of calculations were made with a uniform flat bottom at 4 km. (This is below the real bottom on the Mid-Atlantic Ridge, of course, but the data are defined there, as Fig. 5 and 6 show.) Noise values were all very similar to the cases with real topography (again confirming the observation that noise levels are concentrated in the active near-surface region). Values of \( v \) and \( w \) were very similar to the original calculation and decayed rapidly with depth. On almost the entire of both sections there was no meridional motion at depth (because, of course, there was now no boundary condition there to force a flow). The values of \( u \) at 200 m still reflected the gyre pattern, but a little less strongly in the North Atlantic and much less in the South Atlantic. This is due to the wider variation of \( u_B \) (which now has no direct influence on the bottom condition since \( H_x \) is zero): \( u_B \) now reaches \( \pm 2 \) cm s\(^{-1}\). Close to topographic features, especially the ridge, it would be difficult to associate these test solutions with a real, mass-conserving flow without a concomitant meridional flow. Even more levels of absolute no motion occur, and these are, not surprisingly, smoother from one latitude to another than in the original results.

The (subjective) conclusions one may draw from these tests are that in regions of weak topography then (a) the precise depth is immaterial; and (b) the solution is not strongly affected by varying the bottom condition. In regions of strong topographic gradients, however, the bottom condition does influence the solution (but with an effect decaying with height) and it is important to use the “correct” condition, however this may be defined.

Conclusions

This paper has pursued (to or beyond their ultimate?) the mathematical effects of requiring the geostrophic, hydrostatic, and mass balance constraints to hold on a N–S section, together with minimal noise, or error, in the density balance. We have shown how these requirements permit a usually well-defined minimal noise solution, which determines \( v \), \( w \), and \( \rho \), together with the hitherto unknown barotropic component of \( u \).

The results are at best a mixed success. On the one hand, there are some consistent findings. The decay of meridional motion with depth, which is predicted from the mathematical structure of the equations, is very interesting. It certainly agrees with geopotential anomaly maps, and suggests that deep waters may enter the subtropical gyres through E–W advection from western boundary layers, rather than the Wüstian picture of gradual meridional motion of water masses. However, the predicted E–W flow at depth, although weak, must eventually impinge on eastern boundaries, which will force meridional motion in the case of a basin (as anomaly maps show clearly, of course). However, the equations continue to show the same structure, and thus the same decay with depth, presumably, near eastern boundaries. How is this anticipated lack of meridional flow to be reconciled with the mass balance requirements? Theoretical models (e.g., Luyten et al. 1982) have similar problems.

Another consistent finding is the number of locations with a level of absolute no motion. In Killworth (1980) a level of no motion was necessity; in this study,
there is no such requirement, but levels of no motion occur anyway. They are also present in the $\beta$-spiral calculations (Schott and Stommel 1978; Behringer and Stommel 1980) but not, curiously, in inverse calculations (Wunsch and Grant 1982). The significance of such levels will need some assessment. Are they related to the depth of penetration of the wind-driven gyre, if such a concept is meaningful? On the other hand, the occasional jagged changes in depth of levels of no motion cause some fear that the results of the method depend too strongly on the bottom boundary condition in regions of topography.

There is also some lack of consistency, on the other hand — regions of bad fit or high vertical structure, for example, and regions of apparently good fit where one might not expect it (e.g. the Gulf Stream extension). Indeed, the results in the South Atlantic are typically less believable than those in the North. Fauly data, in the widest sense of the word, including assumptions as to what the data represent, are among the contributory factors here.

These calculations are not intended as a working method for the oceanographer wishing to calculate the flow within a N-S section. Inverse or $\beta$-spiral methods are more practical and both probably have a more “smoothing” effect on noisy data than does the method in this paper. Nonetheless, they are an attempt to explore the dynamics obeyed by a piece of ocean; that the results are in places equivocal must certainly say as much about the method as it does about the data and the dynamics.

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References


Shepherd, T. G. 1980. A two-dimensional model of the meridional transport of tracers in the deep ocean. Ocean Modelling No. 31. (Unpublished MS)


The Role of the Ocean in the Transient Climate Response to Increasing Atmospheric CO\textsubscript{2}\textsuperscript{1}

**Kirk Bryan**

NOAA and Princeton University, Box 308, Princeton, NJ 08540, USA

**Plan of the Numerical Experiment**

There are two important ways in which the ocean affects the climate response to increased production of CO\textsubscript{2} caused by the burning of wood and fossil fuels. The ocean is known to act as an important sink for CO\textsubscript{2}. It also acts as a sink for excess heat produced by the "greenhouse" effect of CO\textsubscript{2} on the global radiation balance. The first effect has received widespread attention. The second effect has only been the subject of a few studies (NAS 1979; Schneider and Thompson 1981; Bryan et al. 1982). However, the ability of the ocean to absorb heat is extremely important in that it can cause a significant delay in the actual onset of a climate change for a given level of atmospheric CO\textsubscript{2}.

A rapid increase in the "greenhouse" effect due to atmospheric CO\textsubscript{2} and other trace gases is an unprecedented event. Therefore, the usual option of studying climatic records is not available. The only recourse is to make use of mathematical models of the ocean and atmosphere and use indirect evidence based on geochemical tracers to judge the realism of the results. In the present study we have attempted to study the processes involved in a CO\textsubscript{2}-induced climatic warming through relatively detailed numerical models that have been developed in previous climate studies. The atmospheric component of the model has been the basis on many sensitivity studies of CO\textsubscript{2} changes (Manabe and Wetherald 1980; Manabe and Stash 1980). Essentially, the model is an advanced numerical weather prediction model, except that radiation and the planetary boundary layer are treated in more detail. The atmospheric model is coupled to an ocean model of equivalent horizontal resolution, but with enough vertical resolution to allow a good representation of the upper thermocline. Snow and pack ice are also included. The method of coupling the ocean and atmospheric models, and the relaxation process used to find climatic equilibrium are described in previous studies (Manabe and Bryan 1969; Manabe et al. 1979). A description of the ocean model is given in Bryan (1969) and in Bryan and Lewis (1979).

The geometry of the system is made as simple as possible. As shown in Fig. 1, the domain is a 120\degree of latitude sector, triply periodic in the zonal direction with mirror symmetry across the equator. In each sector there is an equal amount of land and sea. The model ocean bounded by meridians 60\degree apart corresponds roughly in size to the Atlantic Ocean. The procedure is illustrated in Fig. 2. Two equilibrium climate solutions are found for coupled models by the relaxation process mentioned above. One climate corresponds to a normal level of atmospheric CO\textsubscript{2}, and the other to four times the present level of CO\textsubscript{2}. Next, two numerical integrations of the coupled model in a synchronous mode are carried out starting at the normal climate equilibrium. In one case the atmospheric CO\textsubscript{2} is unchanged; in the other the atmospheric CO\textsubscript{2} is increased to four times the normal level. We will refer to this integration as the "switch on" experiment.

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\textsuperscript{1} Much of the material of this report is published in Science, 215, 56–58, Jan. 1982 (co-authors, F. G. Komro, S. Manabe, and M. J. Spielman) "Transient climate response to increasing atmospheric carbon dioxide."
Results of the Calculations

The response of globally average sea surface temperature to the “switch on” of CO₂ is shown in Fig. 3. The ordinate is the normalized response, \( R \), where

\[ R = \frac{T - T_0}{T_x - T_0} \]

The numerator on the right hand side of (1) is the difference between ambient and initial temperature, while the denominator is the difference between final equilibrium and the initial temperature. Figure 3 shows that the normalized response of sea surface temperature rises rapidly at first. Analysis shows that this rise is associated with heating of the mixed layer that has a time scale of only 2–4 yr. As heat exchange with deeper layers in the thermocline becomes important, the response slows down.

The dramatic differences in response between sea surface temperature and global air temperature, and between high and low latitudes are shown in Fig. 4. Previous CO₂ climate sensitivity studies (Manabe and Wetherald 1980), show that surface temperature is 4–5 times more sensitive to atmospheric CO₂ changes in polar regions than in the tropics. The same behavior was exhibited in the coupled model when the high and low CO₂ equilibrium climates were compared. The normalized response patterns shown in Fig. 4 must be interpreted with this in mind. A uniform value of \( R \) with latitude does not mean a uniform change in temperature at all latitudes but a temperature change that has the same pattern with respect to latitude as the difference between equilibrium climate for normal and high atmospheric CO₂. For zonally averaged air temperature shown in Fig. 4b, \( R \) does become nearly uniform with respect to latitude 15 yr beyond “switch on.” The behavior of zonally averaged sea surface temperature shown in Fig. 4a is much more complicated, but possible to interpret in terms of well-known features of the upper ocean circulation. The most rapid response takes place at “horse latitudes” near 30°N where wind drift produces surface convergence. A sharp gradient of response exists in polar latitudes along the ice front. The sea surface temperature is constrained to remain at the freezing point until the ice melts. The temperature can then rise rapidly. The greatest delay in response occurred near a latitude of 60°. In this subarctic zone, deep convection provides a rapid communication between the surface and deeper levels, which greatly delays the buildup of a surface temperature anomaly. An interesting area of high temperature variability exists just 10° off the equator associated with very low frequency changes in the zonal winds of the atmospheric model.

The vertical penetration of the zonally averaged anomaly in the ocean is shown (Fig. 5), 10 yr after “switch on.” The corresponding temperature changes in the atmosphere show a maximum response at the pole near the surface, with much more moderate change at lower latitudes. Note that in the very high atmosphere increased CO₂ actually causes a cooling! Within the ocean the strong stratification of the upper ocean at low latitudes confines the temperature anomaly to a shallow layer. At higher latitudes weaker stratification allows
FIG. 4. Latitude-time plots of the normalized response of the coupled model after the “switch on” of four times the normal atmospheric carbon dioxide. (a) The sea surface temperature, (b) The surface air temperature over both land and sea.

FIG. 5. The zonally averaged temperature anomaly generated 10 yr after “switch on” of atmospheric carbon dioxide.

much deeper penetration, while in the vicinity of the pole, the halocline again suppresses penetration. Vertical exchange of heat in the model takes place by three basic mechanisms: vertical diffusion, vertical advection, and convective overturning. When the “switch on” experiment is compared to the normal CO₂ case, analysis shows that convection is strongly suppressed, but the other components are not changed very much. Connection can only transfer heat upward and it is only important at high latitudes where heat loss normally occurs. In the “switch on” case, heat received at the ocean surface at lower latitudes is transported poleward by ocean currents. In the subarctic zone it tends to accumulate, since the usual convective upward pathway is partially closed. This provides a physical interpretation of the deep penetration of the thermal anomaly at 50–60° latitude shown in Fig. 5.

A parallel “switch on” experiment has been carried out with the coupled model in which the normal atmospheric CO₂ is doubled rather than quadrupled. The response is almost exactly proportional to the logarithm of the ratio of the atmospheric CO₂ to its normal value, in spite of the many complex processes included in the coupled model. This proportionality can be used to
apply the “switch on” results of the coupled model to a more realistic time history atmospheric CO₂ variation; this is needed for a useful prediction of the delaying effect of the ocean’s thermal inertia.

References


A linear numerical model of the tropical Pacific has been developed, forced by wind stress based on ship observations for the years 1961–70 (Busalacchi and O'Brien 1981). This is a description of the interannual variations in the position, strength, and nature of the Pacific equatorial current system as revealed in model pycnocline and velocities. This study will primarily be concerned with the area 155°W–145°W, 18°N–12°S for the years 1962–70. The interannual variations to be described are due to the first baroclinic mode.

For the purpose of this study, the Pacific equatorial current system consists of four currents and four complementary hydrographic features. The currents are the eastward flowing North Equatorial Countercurrent (NECC) and Equatorial Undercurrent and the westward flowing North Equatorial Current (NEC) and South Equatorial Current (SEC). The hydrographic features, from north to south, are the North Equatorial (NE) Ridge, Countercurrent (CC) Trough, Equatorial (EQ) Ridge, Equatorial (EQ) Trough, and South Equatorial (SE) Ridge. Unfortunately, as the model boundaries are at 12°S and 18°N near the ridges, the NE and SE Ridges are not defined in the model even though their slopes are.

The model and its limitations are discussed further in the next section. The governing equations and assumptions are described, with a brief discussion of the numerical techniques. Some of the general features of the model are also given. Later in the section, there is a discussion of the history and features of the wind data.

In the subsequent section, the seasonal signal and interannual variability of the individual currents is described and compared to observations. For continuity with past research, the seasonal signal is described for the zonally averaged section from 170°E to 140°W (Wyrtki 1974a; Busalacchi and O'Brien 1981). The variations with longitude are described by comparing different 10° zonally averaged sections and using current vector maps for 140°E–140°W. The description of the interannual variability is for the section 155°W–145°W. Some current and past observational studies concerned with this area are the Trade Wind Zone Oceanography (February 1964–June 1965), the NORPAX Hawaii–Taihi Test Shuttle Experiment from November 1977 to February 1978, and the Hawaii–Taihi Shuttle Experiment from January 1979 to June 1980.

Instead of the usual lack of data, the main problem for this study was too much data, over 15 million pieces of data covering a tremendous area and spanning 10 yr. The last year's data were excluded from much of the study due to noise from the initialization of the model. There may be some noise left in 1962, but it is probably small. Even then, the data base is huge, and many of the more common methods of data handling fail.

The data base record is too short for spectral analysis to show the interannual variations. There are too many events, with a time scale of a few months for current vector maps to give an adequate description of the variations without using an unmanageable number of maps. The problem becomes more manageable by limiting the discussion to one meridional section and using the current's zonal nature. Zonally averaging over 10° reduces the small perturbations due to local winds, but leaves intact the major or large-scale events. Averaging over a larger distance smooths or removes many of the interesting features. Hindsight suggests that averaging over a smaller distance, such as 5°, may have been better. Zonally averaging over 10° distorts the flow attributable to either planetary waves or eddies, enough that in some cases, they may appear to be a continuation of the current. This necessitates the use of other meridional sections and current vector maps as a check.

The philosophy behind this study is that whereas observations are limited in scope and duration due to the constraints of finance and logistics, numerical models can provide very dense output, subject to different limitations. The validity of the model, which depends on the assumptions made, numerical procedures used, and inputs, can be determined by comparisons with observations. Thus insight into observed phenomenon may be gained, and/or new, as of yet unobserved phenomenon suggested. A valid model can pose questions and provide a framework or parameters for further observational efforts, plus reinterpretation of past observations.

Review of Model and Observations

The Model

The data for this study were generated in a model by Busalacchi and O'Brien (1981). It is a linear, one-layer, reduced gravity, transport model on an equatorial beta-plane (see Fig. 1). The model dissipation mechanism is in the form of a constant horizontal eddy viscosity and wind stress as a body force. The interface between layers is permitted to move, and the barotropic mode is suppressed. The ocean is assumed to be a Boussinesq fluid.

The model equations are

\[
\frac{\partial V}{\partial t} = -\beta y k \times V - c^2 \nabla h + \frac{\tau}{\rho} + A V^2 V \\
\frac{\partial h}{\partial t} = -\nabla \cdot V
\]
where $V = U_i + V_j$

$U$ and $V$ are the zonal and meridional transports, and $h$ is the deflection of the interface. The initial depth of the upper layer is 300 m, and $\beta$ is \( \beta = 2.25 \times 10^{-11} \text{m}^{-1} \text{s}^{-1} \). A constant horizontal eddy viscosity, $A$, of 100 $\text{m}^2 \text{s}^{-1}$ is used. (See Appendix for list of symbols.)

A staggered grid with a 40-km grid spacing and a 2-hr time step, which satisfies the C–F–L stability condition, is used. All first-order derivatives are replaced by standard second-order center differences. The Coriolis terms are centered in time and averaged in space while the diffusive terms are treated by the Dufort–Frankel scheme. A leapfrog scheme is used for time derivatives. To prevent time splitting, a forward difference scheme is used every 5 d.

The model does not include bottom topography, any thermohaline circulation or other thermodynamics. The reduced gravity formulation retains only the gravest (first) baroclinic mode. The lack of vertical resolution does not allow the Equatorial Undercurrent to develop fully as a separate current. Ignoring friction, the model's physics at the equator are given by

$$\frac{\partial V}{\partial t} = -C^2 \nabla h + \frac{z}{\rho}$$

The wind stress of the trades could be expected to drive a westward surface jet, while the pressure gradient would drive an eastward current. If the transport of the surface jet is of a smaller magnitude than that of the Undercurrent, the interannual variability at the equator can be taken as indicative of that of the Undercurrent. This is possible as the Undercurrent's position is not known to deviate far from the equator (Knauss 1966; Taft and Jones 1973).

There is a general uplifting of the model's interface during the run. This is caused by a net curl of the wind stress and the open boundary conditions. As the location of the northern and southern boundaries prevents the NE and SE ridges from being well defined, the lack of physics outside of the model may prevent the ridges from reaching quasi-stability. The result would be smaller height differences between ridges and troughs. In fact, one of the major differences between the model and observations is that the height differences are one-third to one-half the observed, and therefore the currents are much less. However, it is well known that baroclinic mode models tend to preserve phase changes but the amplitude of horizontal currents and vertical displacements is reduced. Another major difference is that all major features are shifted roughly 2° southward (Busalacchi and O'Brien 1980).

With the previously mentioned exceptions, all of the major hydrographic features can be seen in the mean field of the upper layer thickness (Fig. 2). The CC Trough runs at an angle to the equator that coincides with the paths of Rossby waves as discussed by Busalacchi and O'Brien (1980). The trough has a sharper poleward slant east of 160°W. With a similar angle, the Equatorial Trough can be seen from 160°W to 5°S, 110°W. In the Northern Hemisphere's Fall, there is an eastward current on the northern slope of the Equatorial Trough, as reported in the literature (Tsuchiya 1974; Busalacchi and O'Brien 1980).

**Fig. 1.** Comparison of model geometry with Pacific Ocean. The model extends from 12°S to 18°N and 126°E to 77°W with no-slip east and west boundaries and open north and south boundaries (dashed lines) (fig. 1 of Busalacchi and O'Brien 1980).

**Fig. 2.** The mean thickness of the model's upper layer for the period 1961–70. Contour intervals and units are 10 m. The hatched area denotes land. Note the classical pileup of water in the western basin, and the Countercurrent Trough extending from 7°N, 130°E to 10°N, 130°W, and the Costa Rica Dome at 8°N, 98°W.
Wind Data

The model is driven by the mean monthly wind stress for the years 1961–70 from Wyrtki and Meyers (1975a, b) as subjectively analyzed by Goldenberg and O’Brien (1981).

Wyrtki and Meyers (1975a, b) created maps of the average monthly winds and wind stress over the Pacific Ocean for $2 \times 10^6$ latitude–longitude quadrangles from 5 million ship wind observations between 30°N–30°S for the years 1900–73, to study both the seasonal and interannual variability of the Pacific Ocean trade wind field. On this initial data processing, all observations greater than 40 m/s$^{-1}$ during typhoons were excluded.

To obtain a data set suitable for computer modeling, the Wyrtki and Meyers data were subjectively analyzed by Goldenberg and O’Brien (1981) for each month of 1961 through 1970 onto a 2° square grid, to match the approximate land boundaries used in modeling. The methodology consisted of drawing separate scalar maps of the two components of wind stress using standard synoptic techniques such as climatology, continuity, and topography (near boundaries). Climatology was used to fill the regions where little or no data existed. All monthly averages were weighted by the number of observations and checked for sign errors. Very large monthly average values that produced unrealistically strong gradients were adjusted or discarded and the maps were digitized onto a 2° square grid.

The wind spectrum is essentially white, with an area of high interannual variability centered just north of the equator in the central Pacific, and a band of high annual variability lying approximately along the mean position of the Intertropical Convergence Zone (ITCZ), extending east of the dateline (Goldenberg and O’Brien 1981). The interannual variability in the winds is responsible for the occurrence of El Niño events. The annual variability in the meridional stress ($\tau_y$) is due to the seasonal variation of position of the ITCZ, while that of the magnitude ($\tau$) and zonal stress ($\tau_x$) is due to variation (seasonal oscillation) in the Northeast Trades (Goldenberg and O’Brien 1981). In three longitudinal bands, Goldenberg and O’Brien (1981) found that for 11°N, 124–144°E, the wind stress is domined by the Southeast Asian Monsoon, and that the annual peak in energy in $\tau_x$ and $\tau_y$ combine to form both an annual peak and semiannual peak in $\tau$. In the eastern Pacific, 11°N, 122–142°W, along the mean position of the ITCZ, they found a prominent annual and small semiannual signal for $\tau_x$, $\tau_y$, and $\tau$. They also found some power in $\tau$, in some very low frequencies (<0.1 cycles/yr) and some energy in $\tau_y$ around 3.3 yr, though not at a significant level. In the central Pacific, at 3°S, 178°E–162°W, centered on Canton Island, the $\tau$ and $\tau_y$ spectrums are flat except for some energy around a period of about 2.2–2.5 yr, while the $\tau_x$ spectrum has a significant annual peak (Goldenberg and O’Brien 1981).

Observations

Even though there have been several major investigative efforts in the tropical Pacific, the observational coverage still leaves much to be desired. There are all ways trade-offs of area, duration, and density of coverage due to the constraints of logistics and finance. The longest running records are those of island sea levels for the central and western tropical Pacific (see Fig. 3). Wyrtki (1974a) used the sea-level records and geostrophy to monitor equatorial currents for 1950–70. Wyrtki (1974a, b) formed the zonally and time averaged dynamic height profile relative to 500 dbar for 140°W–170°E and 30°N–20°S using a collection of hydrographic stations. The monthly mean sea-level anomaly as determined for each station from the 20-yr mean, was linearly interpolated in the north–south direction between stations and superimposed on the average dynamic height profile. The sea-level difference between successive ridges and troughs is taken as an indication of the corresponding current strength. The drawbacks of the method are: zonally averaging causes some meridional smoothing due to zonal differences; spacing between stations, both in latitude and longitude, does not permit determinations of smaller scale features. Also the sea-level differences across currents do not account for change in slope due to variations in position of the topographic features.

In the western Pacific, cooperative study of the Kuroshio cruises have been conducted during the last 2 wk of January from 1967 to 1974 for the area 127–137°E, 34°N–1°S (Masuzawa and Nagasaki 1975). From March 1967 to May 1968, the ORSTOM Centre of New Caledonia conducted the Cyclone cruises, which included transects from 20°S to 4°S along 170°E (Magnier et al. 1973). In the central and eastern Pacific, there have been several studies. Specifically for the Equatorial Undercurrent, the Dolphin Expedition, in April through June 1958, investigated the region from 140 to 89°W, followed in September through November 1961, by the Swan Song Expedition, which did meridional transects from 5°N to 5°S along 140°W, 118°W, 96°W and 87°W, plus stations around Galapagos islands (Knauss 1960, 1966). From February 1964 to June 1965, the Trade Wind Zone Oceanography Investigation conducted 16 cruises in the area 26–10°N, 148–157°W (Seckel 1968). The Piquero

**Variability of the Equatorial Pacific Current System**

In this section, the seasonal signal and interannual variability of the individual currents, as they appear in the model, will be discussed and compared with observations. The seasonal signal is defined as the average year for the period 1961–70. The differences in phase of the seasonal signal will be discussed using the current as given in Table 1 for zonally averaged sections 170°E–140°W, 155–165°E, and 155–145°W. Except for the Equatorial Undercurrent, each current’s variations with longitude will be discussed separately for 140°E–140°W using current vector diagrams for the middle of March, June, September, and December. All velocities for the discussion of the seasonal cycle are determined using actual upper layer thickness.

**Table 1.** The east–west velocities of the model currents averaged from 1961 to 1970, over the respective latitudes for the given bands of longitudes. Eastward is positive.

<table>
<thead>
<tr>
<th>Current</th>
<th>170°E–140°W</th>
<th>155–145°W</th>
<th>155–165°E</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Lat.</td>
<td>Mean vel. (10⁻² m·s⁻¹)</td>
<td>Lat.</td>
</tr>
<tr>
<td>NEC</td>
<td>11°N</td>
<td>-6.1</td>
<td>12°N</td>
</tr>
<tr>
<td></td>
<td>15°N</td>
<td></td>
<td>15.5°N</td>
</tr>
<tr>
<td>NECC</td>
<td>4°N</td>
<td>9.0</td>
<td>4.0°N</td>
</tr>
<tr>
<td></td>
<td>7°N</td>
<td></td>
<td>7°N</td>
</tr>
<tr>
<td>EQC</td>
<td>2°S</td>
<td>3.3</td>
<td>2°S</td>
</tr>
<tr>
<td></td>
<td>2°N</td>
<td></td>
<td>2°N</td>
</tr>
<tr>
<td>SEC</td>
<td>3°S</td>
<td>-8.3</td>
<td>4.5°S</td>
</tr>
<tr>
<td></td>
<td>7°S</td>
<td></td>
<td>6°S</td>
</tr>
</tbody>
</table>

The interannual variability discussion will be based on the zonally averaged section centered on 150°W. Due to the long-term upwelling of the thermocline, the velocities for the UYT (contour of zonal velocity in latitude and time) diagrams are determined using a constant upper layer thickness of 200 m. The meridionally and zonally averaged currents, as defined in Table 1, use the actual upper layer thickness.

**Seasonal Cycle**

**Phase Relationship**

The seasonal cycle of the Pacific equatorial current system is well known. Wyrski (1974a) has shown that the system can be considered as consisting of two subsets, each containing a westward and eastward current. The Southern Hemisphere subset, consisting of the SEC and Undercurrent, is the strongest in the Northern Hemisphere’s spring and weakest in the fall. The Northern Hemisphere’s subset, consisting of the NECC and NEC, varies almost 180° out of phase with the Southern Hemisphere (Wyrski 1974a; Busalacchi and O’Brien 1980).

In the model for the section 170°E–140°W, there seems to be a lag of 3 mo between the strongest part of the SEC and Undercurrent (see Fig. 4). This lag may be due to the equatorial signal being a mixture of that of the SEC and Undercurrent. This would agree with the classical picture of the Undercurrent lying beneath the Equatorial Trough, with the SEC traveling along on both slopes; however, the section for 155–165°E shows that the SEC and Undercurrent are in phase (see Fig. 5, 6). In general, the sections at 155–145°W lead the sections at 155–165°E (see Fig. 5, 6, 7, 8). This difference is expected if the locally induced seasonal cycle is modified by westward propagating Rossby waves (Busalacchi and O’Brien 1980).

**North Equatorial Current**

In March (see Fig. 9), the NEC at 140°W is centered on 9°N. Between 160 and 165°W, the current shifts sharply to 12°N and broadens. The highest velocities are found in the east and west. In June (see Fig. 10), the core of the NEC is centered on 10–11°N. It reaches its highest velocities between 160 and 140°W where it is fed by the NECC. The NEC in turn feeds the NECC around 160–165°W. In the west, the NEC is broad and consistent. In September (see Fig. 11), the NEC’s core at 140°W...
Fig. 4. The east–west velocities of the currents, for the section 170°E–140°W, as defined in Table 1, for the average year. Positive velocities are eastward. Dashed lines are one root mean square of the error from the average year, where the

R.M.S.E. is defined as \( \left( \frac{1}{N} \sum_{i=1}^{N} (x_i - \bar{x})^2 \right)^{1/2} \);

\( N \) = number of observations, \( \bar{x} \) = mean.

Fig. 5. The zonal velocities for the average year for the SEC as defined in Table 1 for sections 155°E–165°E (dashed line), and 155°W–145°W, (solid line). Velocities are determined using actual upper layer thickness. Velocity at the figure's top is westward. Complete error bar is 2 standard error of the means. The standard error of the mean is defined as \( \text{R.M.S.E.} = \left( \frac{1}{N} \right)^{1/2} \); where symbols are as for Fig. 4.

Fig. 6. Same as Fig. 5, except for the Equatorial Undercurrent, and velocity at the figure's top is eastward.

Fig. 7. Same as Fig. 5, except for the NEC.

Fig. 8. Same as Fig. 6, except for the NECC.

lies around 14–15°N. The current shifts southward to 11–12°N by 175°E and broadens west of the dateline. The highest velocities are around 175°W. The weakness and narrowness of the current around 140°W may be due to the proximity of the boundary. In December, (see Fig. 12), the NEC is very weak and narrow. The core lies around 15°N at 140°W, and shifts southward as the current broadens to 11°N at 140°E. The NEC appears to be fed by the NECC from 140°W to 140°E. The highest velocities are found around the dateline.

**North Equatorial Countercurrent**

In March (see Fig. 9), the NECC is weak and narrow with the core centered on 5–6°N for 140°E–155°W. The current makes a rapid southward shift at 155°W to 3–4°N. The NECC is the strongest in the west where it is fed by the SEC and feeds the NEC. In June (see Fig. 10), the NECC is at its strongest and the core lies on 1–3°N from 140°E to 140°W. The NECC is fed by both the SEC and NEC from 140 to 150°E, and 175 to 170°W. The SEC also feeds it between 155 and 165°E, while the NEC feeds the NECC around 160°W and in turn is fed by the NECC between 145 and 140°W. In September (see Fig. 11), the NECC is narrower except west of 165°E, but it is still strong. At 140°E, the core of the NECC is located at 2–3°N, shifting to 4–5°N by 170–165°W. Around 155°W, the current splits into two parts. The major part shifts northward to 7–8°N. The southern part shifts to the equator, and may be either the Undercurrent or the previously mentioned Tsuchiya Current (Tsuchiya 1974;
Fig. 9. Current vectors for a section of the model for the middle of March. Velocities were determined using actual upper layer thickness. The figure is a 10-yr average using the timestep corresponding to the middle of March for each year. The hatched area is land. Vectors less than 0.03 m · s⁻¹ were not drawn.

Fig. 10. Same as Fig. 9, except for June.

Busalacchi and O'Brien (1980). In December (see Fig. 12), the NECC is weaker and generally shifted northward. The core of the NECC is located at 2–3°N from 140 to 165°E. Between 170°E and the dateline, the core shifts northward to 5–6°N. Either the Tsuchiya Current or Equatorial Undercurrent is evident east of 175°W. The countercurrent shifts north to 6–7°N by 170°W. East of 160°W, it curves slightly northward and weakens. From 150°E to 140°W, the NECC appears to feed the NEC.

**Equatorial Undercurrent**

The seasonal signal of the Undercurrent is not clear in Fig. 9–12. For the section 145–155°W, it consists of a general weakness in February and March, and a sharp weakness in August (see Fig. 6). The Undercurrent has a double peak in May to July, and is strong in the last quarter of the year with a peak in late November. Please keep in mind that velocities should be interpreted as anomalies; thus westward flow is a negative velocity anomaly.

**South Equatorial Current**

In March (see Fig. 9), the core of the SEC at 140°W is at 5°S. To the west, the SEC becomes narrower and shifts northward to reach the equator by 140°E. The
highest velocities are from 180 to 165°E. From 140 to 155°W, the SEC is being fed from the south and is losing water to the south and NEC west of the dateline. The SEC is also feeding the New Guinea Current. In June (see Fig. 10), the SEC is weaker, narrower and, west of 150°W, shifted southward. From 140 to 150°W, the current's core is still around 5°S, but shifts southward starting at 150°W to 8°S at 155°W. To the west, it shifts northward to 5–6°S by 175°E. The highest velocities are found from 175°E to 170°W. West of 155°E, the SEC is less than 3 cm/s. In September (see Fig. 11), the SEC is narrower and stronger from the dateline east to 140°W. The SEC at 140°W is still at 5°S, but shifts northward from 150 to 165°W to 1–2°S, where it has peak velocities between 165°W and 180. West of the dateline, the current weakens, and shifts southward between 170–165°E to 5–6°S, where it is fed from the south. The current increases slightly as it feeds the New Guinea Current. In December (see Fig. 12), the SEC is very weak, being greater than 3 cm/s only east of the dateline. At 140°W, the core lies around 6°S, and the current weakens, narrows, and shifts northward west of 155°W to 4–5°S. West of the dateline, the SEC is characterized by meanders, low velocities, and eddies.

Interannual Variability of the NEC

Figures 13e and 13f suggest that the North Equatorial Countercurrent crosses the NEC and passes out of the model. Model current vector diagrams show this to be a drawback of the UYT (contour of zonal velocity in
Fig. 13a. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m·s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.

Fig. 13b. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m·s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.
Fig. 13c. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m $\cdot$ s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.

Fig. 13d. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m $\cdot$ s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.
Fig. 13e. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m $\cdot$ s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.

Fig. 13f. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m $\cdot$ s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.
Fig. 13g. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m·s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.

Fig. 13h. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in $10^{-2}$ m·s$^{-1}$, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.
Fig. 13i. Contour of the zonal velocity as a function of latitude and time for the zonally averaged section, 145°W–155°W. Solid contour lines denote eastward. Contour intervals, in 10^{-2} m·s^{-1}, are: 2.5 for 5.0 east to 5.0 west; 5.0 for 5.0 to 10.0 east and west; 10.0 for 10.0 to 40.0 east and west; 20.0 for 40.0 to 100.0 east and west.

latitude and time) diagrams. Actually for the section 145–155°W, the Countercurrent disappears in November 1966. The westward flow south of the Countercurrent is due to either Rossby waves or eddies, as is the eastward flow north of 10° after November 1966. Later in January 1967, the NEC makes a rapid shift southward to 5°N, only to shift northward as the Countercurrent reappears. The beginning of 1962, late 1964 through early 1965, and 1968 are similar.

In January 1962, the core (velocity maximum) of the NEC is at 14–15°N (see Fig. 13a). After weak easterly flow, the core is at 10°N in March and follows the seasonal signal in position and strength.

In January 1963, the NEC’s core is located at 15–16°N (see Fig. 13b). By mid-January, a second velocity maximum is forming in the seasonal position of 10°N. Peak velocities occur in both in the latter half of February, although nearly twice as strong in the southern core. The northern core slowly broadens; the southern core makes a rapid southward shift in the first half of April, and strengthens to a stronger peak of 0.12 m·s^{-1} at 2.5–3°N in early May. Then the southern core shifts northward; in mid-June, the distinction between cores is lost, and the NEC is broad. In the second half of June, the core is at 11–12°N. The normal seasonal peak is delayed from mid-June to mid-August, and shifted to 13°N.

In late January 1964, the NEC peaks at 15–16°N (see Fig. 13c). The NEC’s velocity maximum is found in late April at 10°N, and shifts northward to follow the seasonal tract, with the exception of a peak in late August.

In February 1965, the NEC’s core is found at 11°S. For most of 1965, the NEC is strong, broad, and shifted slightly northward (see Fig. 13d). In December, the current shifts rapidly southward, and strengthens to a peak at 11–12°S in late December–early January. For the first 2 mo of 1966, the current continues its southward shift, while weakening (see Fig. 13e). In March, the NEC joins the westward flow that extends from 10°N to 9°S. The NEC separates from this flow in April, and drifts northward to an early seasonal peak at 9°N in late May. In the second half of July, a second velocity maximum develops at 15.5°N, and shifts northward to 17°N by late December. The first shifts northward to disappear at 11.5–12°N in early October. The events from October 1966 to April 1967 were discussed earlier in the section on the interannual variability of the NEC.

Located at 7°N in late January 1967, the current shifts northward as per the seasonal signal for the rest of the year, with the exception of two weak peaks in March and late September (see Fig. 13f). In February 1968, the NEC’s core is found at 10°N with a weak peak of 0.13 m·s^{-1} in March (see Fig. 13g). Thereafter, the seasonal signal is modified by an additional peak in October, and a general southern displacement of the core in the second half of the year. For much of 1968, the NEC is weak and broad.

In 1969, the current is stronger and broader but lacks a well-organized core (see Fig. 13h). The seasonal peak in June is displaced 2° south of the seasonal position, and preceded by a weaker peak in February at 12°N. In late December, the NEC has a weak peak at 15°N, and a second velocity maximum at 9°N, which becomes the core for 1970 (see Fig. 13i). During January and February, the northern maximum decreases as the southern one increases. The core strengthens to a seasonal peak at 9–10°N, and remains strong as it shifts northward. After a slightly weaker peak at 13.5–14°N in late
September, the NEC slowly weakens and shifts northward.

In general, the NEC demonstrates a high degree of interannual variability. The NEC has been observed to be characterized by fast westward flow alternating with slow eastward or westward flow (Seckel 1968, 1975). In Fig. 14, this alternating flow can be seen to form bands that are found at higher latitudes with time. A similar structure appears in the model (see Fig. 15). Seckel (1975) states that this feature is the result of baroclinic eddies.

**Interannual Variability of the NECC**

The North Equatorial Countercurrent is a narrow and often intense (up to 0.41 m s⁻¹) eastward current with high seasonal and interannual variability. It is usually found between the equator and 10°N, and is often associated with strong equatorial flows. The NECC is broadest in 1965, thinnest in 1967, and discontinuous in 1962, 1964, 1966, 1967, and 1970.

In June 1962, the NECC is found at 3.7°N separating from the flow at the equator (see Fig. 13a). The seasonal peak is delayed until late July at 6–6.5°N, which is farther north than usual, but it is the normal location of the current for July. For the remainder of 1962 and the first half of 1963, the current follows the seasonal signal, with the exception of a double seasonal peak. The first peak is at 2.5°N in May; the second at 3–3.5°N in July (see Fig. 13b). At the end of 1963, the current fails to shift southward, but continues to shift...
northward, and weakens (see Fig. 13c). After February 1964, this current may be an effect of eddies, and not truly part of the NECC.

The NECC is discontinuous in 1964. A second core separates from the equatorial flow in January, peaks at 4-5°N in late February, and disappears at 6.5-7°N in early April. It reforms at 8-8.5°N in the first half of July, and shifts northward to peak at 9.5-10°N in late September. After a rapid southward shift in November, the NECC peaks at 7.5-8°N in December.

In the first quarter of 1965, the NECC shifts southward from 7.5-8°N to a minor velocity peak at 3-3.5°N in mid-March (see Fig. 13b). The current has a seasonal peak of 0.35 m s⁻¹ at 6°N in June. The NECC is strong and broad for the second half of 1965, as expected since 1965 is an El Niño year. However, the current has displaced southward, and lacks the seasonal signal in position. It shifts slightly northward with a few meanders to 6-6.5°N.

In March and much of April 1966, the NECC is absent; it reappears at 4°N in late April (see Fig. 13a). There may be a seasonal peak in June, but the current is overshadowed by the equatorial flow from which it separates in July, and shifts northward as per the seasonal signal. At 7.5°N in mid-August, the Countercurrent has an unseasonal peak of 0.16 m s⁻¹, and virtually stops the northward drift. In November, the NECC disappears only to reappear in February 1967; the eastward flow from November to March 1967, north of 10°N, is due to an eddy or Rossby wave (see Fig. 13e, 13f).

The NECC reforms in the latter part of February 1967, but is soon overwhelmed by eastward flow at the equator. By late May, the NECC takes form and is found at 3.5-4°N, shifting northward and strengthens to a peak of 0.41 m s⁻¹ at 6.5-7°N in August. Rapidly weakening, the Countercurrent shifts northward to 9°N by late September. From November 1967 through February 1968, it shifts southward to 6.5°N with a peak of 0.16 m s⁻¹ in December or January (see Fig. 13c, 13g).

The NECC is very narrow in March and April, but is connected with a strong eastward equatorial flow from May to mid-July. The peak of the eastward flow is at the right time for the NECC's seasonal peak, although it is displaced too far south. In the last half of July, the NECC separates from the equatorial flow at 6-6.5°N, and shifts northward to 8°N by early September. Thereafter, the Countercurrent shifts southward to 6-6.5°N for a peak of 0.27 m s⁻¹ in the first part of October, and a stronger peak of 0.34 m s⁻¹ at 3°N in December.

In 1969, the NECC is strong, narrow, and heavily influenced by equatorial dynamics (see Fig. 13h). In January, the Countercurrent makes a rapid northward shift to regain its seasonal position. In the second half of February, the NECC has a strong peak of 0.31 m s⁻¹ in lieu of the seasonal weakness. From March to mid-November, the Countercurrent is strong but is clearly influenced by equatorial events. In November, the NECC separates from the equatorial flow and shifts northward to reach 6°N by late December.

In 1970, the NECC is weak and narrow when existent (see Fig. 13i). The Countercurrent is south of the seasonal position and dominated by events on the equator during the first quarter of the year. After disappearing in March, it reappears in April and strengthens to a peak of 0.32 m s⁻¹ in July. There is a weak branch of the NECC that shifts northward, with a peak of 0.17 m s⁻¹ in mid-October, and appears to split into two in December. The northern split is most likely to be a Rossby wave or eddy. Other than the branch, the NECC is lost in the equatorial flow.

The variability of the model's NECC is similar to the observed variability. Wyrtki (1974b) has the NECC strongest in late 1965, very strong in late 1963, end of 1968, and beginning of 1969, and very weak in early 1964 and 1970 (see Fig. 16). The discontinuities or absence of the NECC has been observed by Tsuchiya (1974), who notes that the NECC may either disintegrate or re-establish itself in less than 2 mo.

**Interannual Variability of the Equatorial Undercurrent**

The interpretation of the model data for the Undercurrent is based on the premise that only the variability, and not the mean flow, of the Undercurrent is represented in the model. Therefore, for this discussion, all velocities will be taken to be velocity anomalies (∆).

The variability of the Undercurrent is characterized by being able to change from a high negative velocity anomaly to a high positive anomaly, and back again, in a period of a few months. The Undercurrent is strong in mid-1962, late 1966 through mid-1967, late 1970, and strongest from late 1964 until mid-1965. It is weak in late 1963, late 1965 through early 1966, and the weakest from 1969 through early 1970.

In the first quarter of 1962, the Undercurrent strengthens to a peak in March, and a stronger peak (∆ = 0.51 m s⁻¹) in May (see Fig. 13a). It is weak in August and September, then strengthens to a seasonal peak (∆ = 0.38 m s⁻¹) in early December. For the first half of 1963, the Undercurrent follows the seasonal signal (see Fig. 13b). After an intense seasonal weakness (∆ = −0.45 m s⁻¹) in August and a minor peak in
September, the Undercurrent strengthens from mid-October to a strong peak ($\Delta = 0.45$ m·s$^{-1}$) in November.

In 1964, the seasonal signal is delayed 1 mo with the Undercurrent being weak in January through April (see Fig. 13c). It strengthens from October 1964 to a peak ($\Delta = 0.52$ m·s$^{-1}$) in January 1965 (see Fig. 13c, 13d). The undercurrent remains fairly strong through June 1965, but is weak ($\Delta = -0.46$ m·s$^{-1}$) in late August and early September. In October, it strengthens to reach a peak ($\Delta = 0.3$ m·s$^{-1}$) in late November.

The Undercurrent is weak for most of 1966 (see Fig. 13e). It has a very sharp peak ($\Delta = 0.48$ m·s$^{-1}$) in June, a weaker peak in early October, and remains strong until January 1967. The Undercurrent is weak in early February, but strengthens to a peak of 0.38 m·s$^{-1}$ in the latter half of April (see Fig. 13f). It has a very minor peak in July, the seasonal weakness in August, and a peak of 0.32 m·s$^{-1}$ in late October or early November.

The Undercurrent is weak from February to March 1968, and strong for the months of May and June (see Fig. 13g). It has two peaks in the latter half of the year, one in September and the other in December. Between those peaks, the Undercurrent is very weak ($\Delta = -0.43$ m·s$^{-1}$). In late March or early April 1969, it reaches a 10-yr low ($\Delta = -0.6$ m·s$^{-1}$), while in late October, it has an extremely strong peak ($\Delta = 0.64$ m·s$^{-1}$) (see Fig. 13h). Even more unusual is that the Undercurrent weakens and strengthens with a period of 2 mo.

In 1970, the Undercurrent is extremely weak ($\Delta = -0.52$ m·s$^{-1}$) in late March, has a minor peak in July, and an extremely strong peak ($\Delta = 0.73$ m·s$^{-1}$) in early October (see Fig. 13i). It remains strong for the rest of the year (see Fig. 17).

For the model's 10° zonally averaged section centered on 150°W, a comparison of some parameters, of the Undercurrent in the model and observations for the eastern mid-Pacific, shows some similarities. The model's local derivative with time of the zonal velocity is less than $3.7 \times 10^{-7}$ m·s$^{-2}$ for the period 1962 through 1970, while Knauss (1966) estimates it to be on the order of, or less than $10^{-7}$ m·s$^{-2}$. A comparison of EASTROPAC (April 1968), the DOLPHIN (April–May 1958), and the PIQUERO (July–August 1969) expeditions gives a range of 1.1–1.2 m·s$^{-1}$ for the variations of zonal velocity, while the model gives a range of 1.4 m·s$^{-1}$ (Taft and Jones 1973).

Interannual Variability of the SEC

For the section, 145–155°W, the SEC's interannual variability dominates the seasonal signal, with the core varying from near the equator to 7°S. In 1964 and 1969, the current is strong and broad with a northward displaced core, while in 1968, the SEC is narrow and meandering. The SEC is interrupted briefly in 1962, 1965, and 1967.

In the first half of 1962, the SEC has the seasonal peak in velocity but its position meanders from 4–5°S in January, to 6–7°S by late February, to 5°S by early May, and to 7–8°S in the first half of July with an unseasonal peak of 0.12 m·s$^{-1}$ (see Fig. 13a). In late August, the SEC is interrupted and reforms at 4–5°S. The SEC follows the seasonal signal except it is 1–2° south of the normal position where it remains for most of 1963 (see Fig. 13b). In mid-December 1963, the SEC shifts rapidly northward to 3–4°S.

In 1964, the SEC is stronger and broader than usual (see Fig. 13c). The current is not bounded to the south and does not appreciably decrease or become narrow in July. Until it regains its normal position in mid-October, it is 1–2° north of position. In December, it is found 1–2° south of position at 6–7°S. The SEC shifts northward to regain position by late February 1965 (see Fig. 13d). In the second half of June, the SEC disappears to reform around 2–3°S in the latter half of July. The current is disrupted a second time briefly in late October or early November at 4°S and reforms in the same position. It shifts northward in December to 3°S and a peak velocity of 0.27 m·s$^{-1}$. The SEC is broad at this point and remains so for the first half of 1966 (see Fig. 13e). The core is not clearly defined from February to June 1966.

In the latter half of August 1966, the core has a velocity of 0.175 m·s$^{-1}$ at 3°S. From August to mid-November, the SEC shifts farther south than usual to about 6–7°S. In the first quarter of 1967, the SEC shifts north and has a delayed seasonal peak in late March in the normal position (see Fig. 13f). The SEC grows stronger in July to a second weaker peak in August located farther south than usual. For September through November 1967, the SEC is absent. The SEC reforms at 2–3°S in December, and grows to a very strong peak in mid-March 1968, at 3–4°S (see Fig. 13g). In September and October, the current shifts rapidly south, only to shift northward in November and December to end up around 6°S. The SEC is broader than usual in October and November.

In 1969, the SEC is at its strongest, shifted northward of its normal position but going fairly straight (see Fig. 13h). However, the SEC is narrower than it was in 1964, as it is now bounded to the south. The normal seasonal peaks are delayed by 2 mo, but in July the SEC, although broader, shows the usual weakness. The SEC ends the year in the normal position of 5°S, but wider.

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**Fig. 17.** Zonal velocity for the model Equatorial Undercurrent as defined in Table 1 for the zonally averaged section 155°W–145°W. Velocity is determined by using actual upper layer thickness. Velocity at figure's top is eastward.
with westward velocities from the equator to the southern boundary. The SEC is weaker and shifted southward to 7°S for much of the first half of 1970 (see Fig. 13). For the latter part of the year, the SEC remains stronger than usual and is centered on 4–5°S without the usual shifts in position.

The observational data for the SEC for the 1960s is of an entirely different format than the model data. Wyrtki (1974a) has derived the height differences across the SEC using island sea-level records for the area, 172–150°W and extending south to approximately 17.5°S. Both data sets are in agreement that the SEC was very strong in 1964, early 1967, 1970, and very weak in late 1965 (see Fig. 18). However, the model has the SEC about average early in 1962 and absent in the latter half of 1967, while height differences have the SEC strong early in 1962, and average in the latter half of 1967. In late 1963 and 1969, the model has the SEC bounded by easterly flow to the south; height differences have the SEC weaker than suggested by the model.

Summary and Suggestions for Future Work

The model observations give similar descriptions of the variability of the equatorial currents (see Fig. 14, 15, 16, 18, 19). The model has the same general hydrographic features, such as the ridge and trough system, and the east–west tilt of the thermocline (see Fig. 2). These similarities give credence to what is observed in the model.

There is a phase difference in the seasonal signal of the equatorial currents with longitude. This causes the phase relationship between currents to vary from one part of the Pacific to another, and clouds the seasonal picture. The longitudinal dependence of the seasonal signal may be due to modulation of the basic seasonal signal by remote forcing.

A comparison of the two major El Niño years (1965, 1969) yields some major differences. In 1965, the SEC is weak, and the NEC and NECC are strong (Fig. 13d), while in 1969, the SEC and NECC are strong, and the NEC is broad but weak. This suggests that there may be some factor that decides which of the westward flowing currents is to be more strongly affected. However, more than two such events are needed for determination of cause and confirmation of difference.

The model suggests that the equatorial currents are dominated by interannual variations, and that the classical picture of orderly currents is incorrect. In particular, the NEC is characterized by fast westward flow alternating with slow eastward or westward flow as observed by Seckel (1968, 1975) and others. In the model, it appears that the larger features are caused by either eddies or Rossby waves. The effect must be linear as there are no nonlinearities in the model.

The NECC in the model is a narrow, and often intense current with large changes in position. Its current maximum varies from around 3 to 10°N. For the region around 150°W, the NECC has a propensity to disappear in the Northern Hemisphere's spring, when it is generally the weakest. As noted by Tsuchiya (1974), the NECC may form or disappear in a time scale of 2 mo or less.

The model's SEC is a fairly continuous current in the region 145–155°W. Its core varies from near the equator to 7°S. The SEC may be broad or very narrow, with extreme shifts in position as in 1968 (see Fig. 13g).

The Equatorial Undercurrent's mean flow is not represented in the model due to the lack of vertical resolution. Observations indicate that it is safe to assume that the Undercurrent does not vary much more than 1° off the equator for the region near 140°W. Therefore, the variability of the equatorial flow in the model may be taken as that of the Undercurrent without being concerned with meandering. No attempt to describe or ascertain Undercurrent meandering has been made. The local derivative with respect to time of the zonal velocity for the zonally averaged section, 145–155°W, is of the same magnitude (10⁻⁷ m·s⁻¹) as estimated by Knauss (1966). The range of the zonal velocity is 1.4 m·s⁻¹ compared to 1.2 m·s⁻¹ from observations (Taft and Jones 1973). The differences are slight considering the nature of the model and that many of the observations are of short duration and widely spaced in time.

These results suggest that it would be worthwhile to investigate the forcing of the interannual variations. Preliminary investigation using the currents, as defined in Table 1 for 145–155°W, found no correlation for Ekman pumping or curl of the wind stress as a major forcing.

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This needs to be checked out much more carefully; using such a large area may be inappropriate. The effects of an El Niño on equatorial currents can be investigated once the model is run for the 70s. In particular, several studies (Tsuchiya 1974) indicate that the NECC is strong (weak) when the ITCZ is in its northern (southern) position, and the ITCZ is displaced southward the year after El Niño (Donguy and Henin 1980). In the model, the NECC is weak and discontinuous the year after an El Niño or warming (i.e. in 1964, 1966, 1970). However, it is also discontinuous in 1962 and 1967.

The meandering of the Equatorial Undercurrent may be investigated by modulating a mean meridional profile of zonal velocity using the model results. Other projects could include such items as variations in exchange (mixing) rates between currents or oceanic areas.

Acknowledgments

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References


Appendix

A: horizontal eddy viscosity coefficient, 100 m² s⁻¹
C: baroclinic phase speed, (g'H)²/2, 2.45 m s⁻¹

acceleration due to gravity, 9.8 m s⁻²

g': reduced gravity, g(p₂ − p₁)/p₂, 0.02 m s⁻²

h: upper layer thickness

H: initial upper layer thickness, 300 m

i, j, k: x-, y-, z-directed unit vectors
t: time

u, v: zonal and meridional components of velocity

U, V: zonal and meridional components of transport

V: total transport vector

x, y, z: tangent plane Cartesian coordinates: x positive eastward, y positive northward, and z positive upward.

β: meridional derivative of Coriolis parameter, 2.25 × 10⁻¹¹ m⁻¹ s⁻¹

ψ: horizontal gradient operator

ψ²: horizontal Laplacian operator
densities of seawater

ρ, ρ₁, ρ₂: zonal and meridional components of wind stress, respectively
The Oceans and Atmosphere During Warm Geologic Periods

ERIC J. BARRON
National Center for Atmospheric Research,1 Boulder, CO 80307, USA

Warm equable geologic periods, such as the Cretaceous (65–140 million years ago), are of considerable interest in oceanography and climatology because they are such a large contrast from the present day. Estimates of the increase in globally averaged surface temperature for the mid-Cretaceous compared to the present day range from 6 to 14°C. Polar surface temperature estimates range from a mean annual ocean surface temperature of 0°C, which allows for seasonal subfreezing conditions, to 15°C for winter surface ocean temperatures (Barron 1982). The warmest estimate appears to be the more prevalent interpretation (Frakes 1979; Lloyd 1982). A number of hypotheses have been proposed to explain or describe warm paleoclimates, and these hypotheses have been extended to a wide range of other geologic studies.

A frequently cited hypothesis, originally described by Brooks (1928), states that during periods of reduced equator-to-pole surface temperature gradient, the atmospheric circulation would decrease in intensity. This hypothesis stems from the assumed importance of differential heating as the major drive for the atmospheric circulation. A seasonal analogy appears to support this hypothesis. In the summer hemisphere, the equator-to-pole temperature gradient is reduced and there is an observed reduction in the intensity of the westerly jet and the Hadley circulation (Palmén and Newton 1969).

The hypothesis that the intensity of the atmospheric winds would be greatly reduced during warm periods has been extended to the wind-driven surface ocean circulation. In addition, numerous authors (Berggren and Hollister 1974) have hypothesized that the intensity of the thermohaline circulation, the component driven by density differences, was much weaker during warm periods. This hypothesis appears to be a logical extension of the fact that high latitude cooling and salt exclusion during freezing drives the thermohaline circulation by producing extremely dense cool waters at high latitudes today.

The enigma is how warm poles can be maintained under conditions of a much reduced atmospheric and oceanic circulation. Since atmospheric transport by baroclinic eddies requires a relatively large supply of available potential energy, and hence a large meridional temperature gradient, it has been suggested that periods of reduced equator-to-pole surface temperature gradient were characterized by a reduced atmospheric heat transport (Kraus et al. 1979). This hypothesis also receives support from empirical studies comparing seasonal changes in meridional surface temperature gradients and meridional heat fluxes (Stone and Miller 1980).

Although it has been hypothesized that the wind-driven ocean circulation and the thermohaline circulation were weaker during warm periods, the assumption that atmospheric heat transport was less than at present has led to the conclusion that warm, polar temperatures must have resulted because the ocean transported more heat poleward (Kraus et al. 1979). It has been frequently suggested that solar continental configurations have a strong influence on polar temperatures by inhibiting or accentuating oceanic heat transport to the poles (Frakes and Kemp 1972; Kennett 1977). A large increase in the ability of the oceans to transport heat poleward has been proposed to explain the global warmth during the Mesozoic (Frakes 1979).

A number of recent modeling experiments are of importance in examining the problems and hypotheses described above. In many cases these experiments suggest that the hypotheses should be reevaluated. The interpretation of the warmth of polar and tropical temperatures during periods such as the Cretaceous is a factor of critical importance. There is also a clear need for additional model experiments designed specifically to examine the hypotheses that have been applied to the geologic record.

The Atmospheric Circulation and Meridional Heat Flux

A number of recent climate model simulations have a direct bearing on whether the atmospheric circulation was sluggish and characterized by a reduced meridional heat transport during warm geologic periods. Barron et al. (1981b) and Barron and Washington (1982a, b) considered the Cretaceous Period specifically. Held et al. (1981) examine changes in atmospheric heat transport due to an increased solar constant. Manabe and Wetherald (1980) examine the atmospheric circulation for a warmer earth due to an increase in carbon dioxide and an increase in solar constant.

The Meridional Heat Flux

Barron et al. (1981b) determine the total heat transport convergence required to achieve a specific equator-to-pole mean annual surface temperature distribution using an energy balance climate model. The energy transported poleward by the atmosphere in the model is divided into two components: sensible plus potential energy and latent energy. Both components are approximated as diffusion processes. Sensible plus potential energy transport is proportional to the meridional gradient of surface temperature, and latent heat transport is proportional to the meridional gradient of atmospheric water vapor concentration, which in turn is related to surface temperature. Outside the tropics, the diffusion coefficients are assumed to be proportional to the absolute value of the temperature gradient, and thus the diffusion is nonlinear.

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Starting with a Cretaceous experiment with specified Cretaceous geography and including specified cloud characteristics, Barron et al. (1981b) derive the incremental energy input at each latitude, $\Delta Q(\phi)$, required to match a desired temperature distribution. $\Delta Q(\phi)$ in this calculation can be divided into two additive quantities. First $\Delta Q(\phi)$ may be globally averaged to give $\Delta Q$ which is the globally averaged energy input required to reach the global average of the desired temperature distribution. $\Delta Q$ may be subtracted from $\Delta Q(\phi)$ to give $\Delta Q(\phi)$ which is the component of $\Delta Q(\phi)$ which arises solely from changes in meridional transport of energy. $\Delta Q$ cannot be due to a meridional transport of energy since the meridional transport term is required to be zero when integrated over a closed surface, such as the earth.

In the calculation of Barron et al. (1982) the “desired” temperature distribution (Fig. 1) consists of present-day equatorial surface temperatures and mean annual polar surface temperatures of 0°C. The specific Cretaceous experiment chosen requires less than a 1% increase in $\Delta Q$ to reach the globally averaged temperature derived from Fig. 1 (6° higher than at present). The planetary warming is due to Cretaceous geography and prescribed cloud–climate feedbacks. From this experiment the total transported energy convergence required to achieve the “desired” Cretaceous surface temperature distribution can be calculated. Both the latent and sensible atmospheric heat transport convergence are determined for this surface temperature distribution through an iterative process. The incremental energy input at each latitude $\Delta Q(\phi)$ represents an estimate of the total oceanic heat transport plus nondiffusive heat transport processes in the atmosphere.

The convergence of total transported energy required to achieve the distribution of Cretaceous temperatures in Fig. 1 is very similar to the present-day values (Fig. 2). This result suggests that if the heat transport can be maintained despite the decrease in equator-to-pole surface temperature gradient, at least a seasonally ice-free polar climate can be achieved with a change in geography and plausible cloud–climate feedbacks. In the model simulation both the sensible heat transport convergence (Fig. 3) and the latent heat trans-

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**Fig. 1.** A comparison of present-day zonally averaged mean annual temperatures and the “desired” Cretaceous temperature distribution.

**Fig. 2.** A comparison of present-day total heat transport convergence with the total heat transport convergence required to achieve the Cretaceous temperature distribution in Fig. 1.

**Fig. 3.** A comparison of present-day atmospheric sensible heat transport convergence with the sensible heat transport convergence calculated in the Cretaceous model simulation.
port convergence decreased compared to the present day. This result appears to support the hypothesis (Kraus et al. 1979) that the atmospheric heat transport would be less during periods of reduced equator-to-pole surface temperature gradient.

The results of model simulations for increased atmospheric CO₂ and increased solar constant by Manabe and Wetherald (1980) did not result in a decrease in total atmospheric heat transport. Manabe and Wetherald (1980) employ a General Circulation Model (GCM) of the atmosphere with a land and ocean sector. The ocean portion is an area with no heat capacity and no heat transport by ocean currents. The ocean is simply considered to be an infinite supply of moisture. The governing equations are the momentum equations, mass continuity equation, thermodynamical energy equation, and the water vapor continuity equation.

For a quadrupling of CO₂, tropical surface temperatures warm by 4°C and polar temperatures warm by as much as 15°C. The warming of the model atmosphere resulted in a large increase in the poleward transport of latent heat. Manabe and Wetherald (1980) suggest that during the Mesozoic, small temperature gradients may have been maintained by a large poleward latent heat transport. For both CO₂ increases and solar input increases, the poleward transport of dry static energy decreased, but the increase in latent energy flux resulted in a larger total atmospheric poleward energy transport than at present.

Held et al. (1981) describe the response of a two-level primitive equation model to solar constant variations. A case for a 6% increase in solar input resulted in approximately a 4°C increase in zonally averaged tropical surface temperature and a 16°C increase in zonally averaged surface temperature at 80° in latitude. For solar constant increases on either side of +2% of the present value, the differences in sensible heat transport and latent heat transport nearly cancel, leaving the total heat flux unaffected. Again these results suggest that latent heat flux may dominate during warmer climates, and the total heat transport by the atmosphere may be maintained despite a decrease in the equator-to-pole surface temperature gradient.

The polar warming due to the three different external forcings in Barron et al. (1981b), Manabe and Wetherald (1980), and Held et al. (1981) is very similar, 16, 15, and 16°C, respectively. The decrease in equator-to-pole surface temperature gradient is actually slightly greater in Manabe and Wetherald (1980) and Held et al. (1981) because of the increase in tropical surface temperatures (≈4°C) compared to the present day and to Barron et al. (1981a). Yet, these authors indicate that the total atmospheric heat transport may be maintained despite the decrease in surface temperature gradient because of the increased poleward latent heat flux during warmer climates. This result is in contrast to the Energy Balance Climate Model simulations of Barron et al. (1981b).

The differences in these model results may not necessarily be ascribed to the simple diffusive heat transport parameterization in the Energy Balance Climate Model. The difference in tropical sea surface temperature between Barron et al. (1981b), which was assumed to be present-day mean annual values, and the +4°C increase in Manabe and Wetherald (1980) and Held et al. (1981) may be more important. The tropical warming is apparently significant because saturation vapor pressure increases as a nonlinear function of increasing temperature. In Barron et al. (1981b) the latent heat flux is proportional to the meridional gradient of atmospheric water vapor concentration. Experiments described by Barron et al. (1981b) in which tropical surface temperatures increase compared to the present do result in an increase in latent heat flux. However, this increase in latent heat flux does not quite counterbalance the decrease in sensible heat transport due to the decrease in meridional surface temperature gradient.

The Intensity of the Atmospheric Circulation

It has been commonly assumed that the decrease in mean meridional surface temperature gradient during warm geologic periods would be associated with weaker winds, and hence a weaker wind-driven ocean circulation. The thermal wind relationship relates the horizontal temperature gradient to the vertical shear of the geostrophic wind. The intensity of the winds aloft will decrease if the vertically integrated meridional tempera-
ture gradient decreases. The increase in tropical surface temperature in the above simulations is also important in this respect because of the amplification of the effect of the change in the upper troposphere (Kraus 1973; Schneider and Dickinson 1974). The heat of condensation of water vapor increases the potential temperature of the air (the absolute temperature of the air if brought adiabatically to sea-level pressure). Because the saturation temperature increases as a nonlinear function of increasing temperature, the vertically integrated meridional temperature gradient may be maintained despite the decrease in the surface temperature gradient. In addition to lapse rate changes in the tropics due to moist convection, the polar warming serves to eliminate the polar temperature inversion without affecting tropospheric temperatures substantially. There is also the potential for polar lapse rate feedbacks associated with changes in surface albedo. Held et al. (1981) specifically note that the changes in mean atmospheric temperature gradients for an increase in solar constant are much smaller than changes in surface temperature gradients, implying pronounced changes in static stability.

Barron and Washington (1982a, b) derive a similar result using a version of the National Center for Atmospheric Research (NCAR) General Circulation Model (GCM) (Washington and Williamson 1977) with present March (Equinox) solar insolation. This version of the NCAR GCM has a coupled “mixed layer” model in which the ocean temperatures are calculated based on a surface energy balance. A control simulation with present-day geography and an experimental simulation using mid-Cretaceous geography were completed. The Cretaceous case results in a 5–10°C decrease in equator-to-pole surface temperature gradient compared to the control. Tropical surface ocean temperatures are 2°C greater than the control. It is of particular interest that the vertically integrated meridional temperature gradient is maintained despite the decrease in the equator-to-pole surface temperature gradient. The zonally averaged wind speed of the tropospheric jets in the Cretaceous case are very similar to the control. In addition, the Cretaceous model atmospheric circulation was characterized by reduced intensity of the surface winds at some latitudes but an increase at other latitudes. This reflects many factors including the distribution of continents.

The results of Manabe and Wetherald (1980), Held et al. (1981), and Barron and Washington (1982a, b) suggest that the total atmospheric heat transport may be maintained despite a decrease in equator-to-pole surface temperature gradient. Similarly, the assumption that the intensity of the atmospheric circulation will decrease for a decrease in equator-to-pole surface temperature gradient may not be justified. Most importantly, these characteristics of the atmospheric circulation depend on the distribution of temperatures and, in particular, the change in tropical sea surface temperatures. The increase in tropical sea surface temperature in the above model simulations could be model dependent and, at least for the Cretaceous, it is not possible to determine tropical sea surface temperatures more specifically than ±3–5°C from present-day values (Barron 1982).

The results of Barron et al. (1981b) indicate that if the total poleward heat transport can be maintained at close to present-day values, then with changes in geography and cloud cover, mean annual polar temperatures of 0°C can be achieved. However, if polar winter surface temperatures were near 15°C during the Cretaceous, then there exists a much greater problem.

The Thermohaline Circulation and Warm Polar Temperatures

The interpretation of polar temperatures during the warm, equable Cretaceous, and Early Tertiary is not separate from the hypotheses on the nature of the thermohaline circulation. Isotopic temperatures measured from benthic Foraminifera indicate that bottom waters were 15–17°C during this time period (Savin 1977). Since bottom water is formed at high latitudes today, a uniformitarian interpretation of the isotopic paleotemperature data is that they reflect the coldest high latitude ocean temperatures. This interpretation implies that winter polar surface ocean temperatures were near 15°C.

Peterson (1979) developed a two-dimensional steady convection model driven by imposed buoyancy sources to examine the thermohaline circulation. The buoyancy deficit drives a turbulent plume that entrains water from the interior. The entrainment results in an increased volume transport and decreasing density. The plume with the greatest buoyancy flux (not density) becomes the bottom water, while lesser plumes terminate at intermediate levels. In the case of two plumes of nearly the same buoyancy flux, a small perturbation can result in a dramatic change in the stratification.

Brass et al. (1982) extended the model results of Peterson (1979) to suggest that during warm periods, the strongest buoyancy flux may have originated in high evaporation basins where the buoyancy deficit was caused by high salinity rather than low temperature. Because of the association of bottom water with marginal seas and the fact that the evaporative flux is a function of the area, as well as the evaporation rate, Brass et al. (1982) suggested that the production of warm salty bottom water should be related to the area of epicontinental seas in the subtropic (~10–40° latitude). In support of this hypothesis they note the strong correspondence of bottom water temperatures, as determined by oxygen isotopes (Savin 1977), with the area of epicontinental seas over the last 100 million years (Barron et al. 1981a). For example, the differences in continental positions and sea-level variations resulted in 22 × 10⁶ km² greater area of ocean in the subtrópicos (present-day land area is 64.8 × 10⁶ km²) 100 million years ago compared to the present day. Of this increase, 16 × 10⁶ km² were relatively shallow marginal seas, caused by higher sea levels than at present.

The collection of isotopic paleotemperature data from individual basins at various paleodepths is essential to reconstruct the nature of bottom water formation. Unfortunately, Cretaceous paleotemperature data from open ocean paleoenvironments are extremely limited.

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because of a lack of suitably preserved specimens that can be used to measure oxygen isotopes. Estimates of Cretaceous deep water temperatures have been based on relatively few measurements (Douglas and Savin 1971, 1973, 1975; Saito and Van Donk 1974; Margolis et al. 1977; Boersma and Shackleton 1982). Recently, Barron et al. (1982) and Saltzman and Barron (1982) determined that the coarse crystalline shell of *Inoceramus*, an epibenthic clam abundant in Cretaceous deep-sea sediments, was suitable for oxygen isotopic analysis. For the Late Cretaceous well-preserved *Inoceramus* were recovered from a variety of paleodepths (<1000–4000 m) and from at least two cores each from the Indian, Pacific, and South Atlantic oceans. A striking feature of the isotopic data derived from *Inoceramus* is the difference in paleotemperatures at different depths. A site in the Angola Basin (Deep Sea Drilling Project Site 530A) had the warmest paleotemperatures (≈14°C) although considerably deeper than sites in adjacent basins (e.g. DSDP Site 355 in the Brazil Basin). The formation of bottom waters was clearly different in the two basins and the circulation was such that their properties were not homogenized. Saltzman and Barron (1982) suggested that bottom waters in the Angola Basin must have been more saline. During the Cretaceous the Angola Basin was bordered by extensive subtropical seas, in contrast to the Brazil Basin. Isotopic paleotemperatures from Pacific *Inoceramus* gave paleotemperatures of 5–7°C. These relatively cool temperatures may represent Pacific deep water formed by cooling at high latitudes or the result of mixing of cold polar deep water with warm saline low latitude deep water.

The data of Saltzman and Barron (1982) and the model results of Peterson (1979) and Brass et al. (1982) provide a more complex picture of Late Cretaceous thermohaline circulation than was previously assumed. The hypothesis that the intensity of the thermohaline circulation was much weaker during warm periods may be unjustified. Deep waters may have formed by both cooling in polar regions and by evaporation in the subtropics. Importantly, the measurement of very warm bottom water temperatures may not necessarily imply equally warm polar temperatures. However, even taking into account bottom water formed in the subtropics, some authors (Lloyd 1982) believe that winter polar ocean surface temperatures exceeded 14°C.

**Ocean Heat Flux**

The oceanic heat flux component during warmer climates is the most speculative aspect of the problem. There are no published model simulations that specifically examine the nature of the oceanic heat flux during warmer geologic periods. However, it is possible to determine the poleward oceanic heat flux that would be required to maintain polar ocean surface temperatures at the extremes of Cretaceous estimates: (1) 0°C mean annual surface ocean temperatures but allowing seasonal subfreezing conditions; and (2) warm winter surface ocean temperatures (10°C will be considered as a reasonable maximum).

An estimate of the poleward oceanic heat flux to achieve a 0°C mean annual polar surface ocean temperature may be based on the results of Barron et al. (1981b) described previously. The key element is that the total heat transport convergence by the oceans and atmosphere combined must be maintained at close to their present-day values. In the cases where the total atmospheric poleward heat flux is maintained or increased compared to the present day for a warmer earth (Manabe and Wetherald 1980; Held et al. 1981) the oceanic heat flux could even be slightly less than present-day values. For the case with present-day tropical surface temperatures described by Barron et al. (1981b) the total atmospheric heat transport decreased (Fig. 2, 3). The remainder of the heat transport required to maintain the present-day total heat transport is illustrated in Fig. 5 in comparison with an estimate of present-day oceanic heat transport. For purposes of comparison, an annual heat transport of 0.9 × 10^15 W crossing the 60°N latitude circle would be required to maintain polar temperatures at 0°C in the mean for the case illustrated in Fig. 5.

There are a number of plausible mechanisms for achieving a slightly greater poleward oceanic heat transport. Kraus et al. (1979) determine that a small amount of upwelling (2.3 × 10^6 cm · s⁻¹) of 10°C water due to a

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**OCEANIC HEAT TRANSPORT CONVERGENCE**

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**Present Day**

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**Polar Temperature 273 °K**

---

**Watts/m²**

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**N 80 60 40 20 0 20 40 60 80 S**

**LATITUDE**

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**FIG. 5.** A comparison of an estimate of present-day oceanic heat transport convergence with the incremental energy input at each latitude required to achieve the Cretaceous temperature distribution in Fig. 1 in addition to the model calculated sensible and latent heat transport convergence given in Fig. 3 and 4.
reversal of the thermohaline circulation would keep polar regions free of permanent ice. This estimate was based on the present-day Norwegian–Greenland Sea heat balance. At present the heat transport by the mid-latitude surface circulation opposes the thermohaline circulation. If there were a reversal of the thermohaline circulation, the poleward oceanic heat transport might increase. This idea is presently being tested by A. J. Semtner Jr. (National Center for Atmospheric Research, Boulder, CO, personal communication). It is also plausible that different geographic configurations may result in a greater high latitude penetration of oceanic boundary currents. As yet each of these ideas remains speculative.

An estimate of the poleward oceanic heat transport required to maintain warm winter polar surface temperatures (10°C) can be derived from GCM experiments described by Barron and Washington (1982a, b). Barron and Washington (1982a, b) repeated the Cretaceous experiment described in the previous section but with the minimum ocean surface temperature constrained to be 10°C. This assumption implies a large oceanic heat transport as hypothesized by several authors (Frakes 1979). Because the temperature of the mixed layer is calculated as a function of the surface heat balance, an equilibrium simulation should be characterized by a balance of the absorbed solar energy, the net longwave flux, the net sensible flux, and the latent flux at the surface. However, the specification of a constrained temperature minimum of 10°C results in a net energy flux from the polar surface to the atmosphere. This imbalance is an estimate of the implied oceanic heat transport required to maintain polar surface temperatures at 10°C.

Simulations for both present-day March and January insolation were performed by Barron and Washington (1982b) with Cretaceous geography. For March, the energy imbalance would require that $2.3 \times 10^{15}$ W of oceanic heat flux cross the latitude circle at 60°N. For January the imbalance would require that $6 \times 10^{15}$ W cross 60°N. This implied oceanic heat transport convergence is compared with the present-day values in Fig. 6. For perspective, if the $2.3 \times 10^{15}$ W were derived from the ocean area between 0 and 30°N this would be equivalent to 25 W/m². Simple energy balance considerations suggest that the 25 W/m² could potentially depress tropical temperatures by 12°C. If we assume that water at 15°C is transported to the poles, which then cools to 10°C, the volume flux which would be required to achieve a heat transport equal to $2.3 \times 10^{15}$ W crossing 60°N can be calculated. The volume flux, on the order of 112 Sverdrups, would require tremendous currents penetrating the Arctic Circle.

It appears that the hypothesis that increased oceanic heat transport can maintain polar temperatures of 15°C in winter, without solar insolation, cannot be justified. This is especially apparent if one realizes that the oceanic connections to the Arctic during the Cretaceous were not substantially larger than at present (Barron et al. 1981b).

**Summary**

A number of hypotheses have been proposed to explain or describe warm paleoclimates. The most frequently cited hypotheses suggest that the atmospheric circulation, the wind-driven ocean circulation, and the thermohaline circulation were considerably reduced in intensity compared to the present day. It has also been suggested that during periods of reduced equator-to-pole surface temperature gradients the total atmospheric heat transport was probably less than at present. Hence, warm poles must have been maintained by an increase in the poleward oceanic heat flux. These hypotheses have been extended to a wide range of other geologic studies. Recent climate model experiments indicate that many of the hypotheses which have been proposed to explain warm, equable paleoclimates should be reevaluated.

First, although the equator-to-pole surface temperature gradient may decrease, the vertically integrated temperature gradient may be maintained because of the amplification of an increase in tropical surface temperature in the upper troposphere and the potential for polar lapse rate feedbacks. Because of these factors, it is premature to conclude that the intensity of the atmospheric circulation and the wind-driven surface ocean circulation were less because the equator-to-pole surface temperature gradient was less in the past.

Second, the thermohaline circulation may not have been weaker because of warm polar temperatures. A strong buoyancy flux may have originated in high evaporation basins where the buoyancy deficit was caused by high salinity rather than low temperature.

Third, Manabe and Wetherald (1980) and Held et al. (1981) suggest that the total poleward heat transport

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**Fig. 6.** A comparison of an estimate of present-day polar oceanic heat transport convergence with the implied oceanic heat transport convergence to maintain polar surface ocean temperatures at 10°C for Cretaceous simulations with present-day January and March insolation.
by the atmosphere may increase during warmer climates, because of the increase in latent heat transport. It has been pointed out that this conclusion may depend on whether tropical temperatures were warmer than at present. If tropical ocean temperatures were similar to or cooler than at present, the total atmospheric heat transport might actually decrease.

Recent model experiments have also placed limits on the total poleward heat flux required to achieve both coolest and warmest estimates of Cretaceous polar temperatures. Barron et al. (1981b) determined that seasonally ice-free poles (a mean annual polar surface temperature of 0°C) can be achieved if the total heat transport convergence can be maintained at present-day values. This appears to be quite plausible. In contrast, a winter polar ocean surface temperature greater than 10°C would require an increase in oceanic heat transport which cannot be justified unless tremendous warm currents penetrated the Arctic Circle. The oceanic heat flux crossing 60°N would need to be more than 40 times the present values in order to maintain 10°C polar oceans in January. This does not necessarily imply that warm polar temperatures may not have occurred. A comparison of the January GCM experiment with polar oceans constrained to be 10°C with continental paleobotanical data indicates that the model planet may not be warm enough to explain some data in continental interiors (Barron and Washington 1982b).

These model experiments question many of the hypotheses that have been proposed to explain or describe warm paleoclimates. The warmth of polar and tropical temperatures during periods such as the Cretaceous appears to be a critical factor for both the intensity of the atmospheric and wind-driven ocean circulation and the meridional heat flux in the atmosphere. Solar surface temperatures near 0°C in the mean can be maintained if the total poleward heat flux is maintained near present-day values. If warm winter polar ocean temperatures must be maintained, then there is a much greater problem. The increase in oceanic heat transport that would be required to maintain polar winter ocean temperatures above 10°C for present-day winter insolation appears to be infeasible. Either polar temperatures were cooler than 10°C or large departures from present-day external or internal climatic conditions are required.

References


Margolis, S. V., P. M. KROOPNICK, AND D. E. GOODNEY. 1977. Cenozoic and Late Mesozoic paleoceanographic and


The chairman, Brian Rothschild, has asked me to discuss the problem of full commercial development of lightly used or unexploited fish stocks. He felt, and I agree, that such a paper would be appropriate at this session of the 1982 Joint Oceanographic Assembly, because many professional biologists working on fisheries development may get so involved with its biological aspects, such as stock assessment, that they may forget or ignore other important parts.

To introduce my topic, let me state what should be obvious. A viable commercial fishery is not possible without both a suitable stock of fish and a fully functioning market for the products produced. With no fish there is nothing to sell. With no market there is nowhere to sell it. An interesting issue that is somewhat peripheral to this topic, but one that is obviously of critical importance to overall fisheries utilization, is that while it takes both of these elements to get a fishery started, it takes a properly functioning management structure to keep it going. I will return to this point later.

Although there are two necessary elements in fisheries development, I will discuss market aspects only. I will, however, relate them, where appropriate, to the basic fisheries biological aspect. The reason for this approach is that my intended audience is already familiar with the biological aspects, indeed more so than I am. An interesting companion piece would be a similar work written by a fisheries biologist but directed at nonbiologists, giving main attention to the biological aspects of development but touching on the important interrelationships with the marketing aspects as appropriate. I am thinking of asking the chairman to write such a paper and present it at a future meeting of fisheries economists.

**Historical Examples**

Before going into a formal discussion of what I have called a properly functioning market, let me first describe the development of some well-known fisheries. A good deal of what follows can be made easier by reference to these examples.

Consider first the utilization of the northwest Atlantic fishery by the United States. While the northwest Atlantic has probably been a biologically productive area for thousands of years, it was not heavily exploited until relatively recent times. During the 1500s, fishing was well established, but the catch had to be salted because of the lack of other preservation techniques. Halibut, but especially cod, were the prime species because they were well suited to salting by their texture, size, and oil content. By the 1870s, the urban populations in eastern Massachusetts had increased significantly, and, in addition, the availability of improved refrigeration created a large market for fresh fish. At the same time, fresh meat became more readily available, which also lowered the demand for salted and dried fish. Since consumers generally prefer fresh to salt fish, this change in population and technology led to a change in the fishery. Because haddock was preferable to cod as a fresh fish, it began to be more heavily exploited. The relevant point here is that there was no apparent change in the relative abundance of the stocks. The change in exploitation rates was purely market induced.

Another important technological change occurred in 1905 when steam-powered otter trawlers were introduced. Previously, most fish had been caught by dories that were carried to the fishing grounds on sailing schooners. Once on the ground, the dories were put to sea with several men who hand-fished longlines. The steam-powered otter trawlers were much more efficient than dories, and provided more safety for the fishermen. In addition to pulling the trawl, the steam could also power deck-mounted winches to retrieve the catch. Steam power also decreased travel time which lowered costs and allowed for fresher fish.

Before steam power, Gloucester and Provincetown were the principal ports for the New England fishery. Their main advantages were proximity to the fishing grounds and harbors which were easily accessible by sailing schooners. However, Boston rapidly became an important fishing port, because steam power reduced travel time to the banks, and made it easier to navigate its hazardous channels. Boston itself provided a larger market for fish and, in addition, there were railroad connections which opened up the entire New England market. As the Boston fleet expanded, the city developed a support system to process, market, and store fish. A fish pier was developed which housed an exchange to sell fish and considerable freezing and cold storage facilities. All of these combined to improve the market for the fish, which encouraged heavier exploitation of the stocks.
The market was not perfect, however. Customers far from Boston were somewhat skeptical of fish because they were never sure of their quality. Fish were shipped whole and the retailer sliced them for individual sales. The development of commercial filleting changed this, however. When the fish were filleted in Boston, the weight of the product was reduced by 60%, which greatly reduced transportation costs, and in addition allowed for better icing and hence higher quality.

Quick freezing was also introduced at this same time. When fish are frozen slowly, ice crystals form in their flesh, destroy the cell structure, and make the thawed fish considerably inferior to fresh fish. With quick freezing, however, this does not occur; the thawed fish has a texture and taste very similar to fresh fish. This again expanded the market for fish and hence led to higher exploitation of the banks. As time went on, other innovations did the same thing. Refrigerated motor trucking in the 1930s also helped expand the market.

Refrigerated trucking, filleting, and quick freezing also led to the heavy use of red fish, which had not previously been exploited. When it was discovered that red fish fillets tasted much like freshwater perch, this previously unused resource became profitable. People had a taste for the perchlike flavor, and the filleting, quick freezing, and refrigerated trucking provided a way to get it to midwestern markets. The development of the red fish market was also helped by the space made available in Gloucester, when much of the previous activity moved to Boston.

The movement of the cotton industry to the south also helped develop the northwest Atlantic fishery. Many people who previously worked in the cotton industry now choose fishing, and this labor supply allowed for a profitable operation of filleting plants in many cities, especially New Bedford.

As another example, consider the Pacific halibut fishery. This fish stock has been available for many years and although it had been exploited since the opening up of the Pacific frontier, it was not until the last decade of the 19th century when the railroads provided transportation to the populous eastern markets, that the Pacific halibut fishery became important. The transportation costs were still relatively high, but the concurrent decline of the Atlantic halibut decreased supply and hence raised the price, making it possible to cover them. Also, as the fishery grew, bulk shipments became possible which lowered per unit transportation costs. As the railroad expanded into Canada, other stocks became exploited, not because they were necessarily any different than before, but just that they were closer to a railroad terminal.

Components of Fishery Markets

From the above, it should be obvious that such things as consumer taste and technologies of harvest, processing, and distribution are very important in the commercial utilization of biologically abundant stocks. Let us now turn to a more formal analysis of these issues. It will be useful to discuss them in terms of obstacles to, rather than necessary components of, development because this is the way they are most often encountered in practical applications.

A biologically viable fish stock is only one part of a well-functioning fishery industry. There must be an economically viable market as well. It must be possible to harvest, process, and distribute the fish products such that the participants earn a reasonable return and, at the same time, consumers obtain a product at a price comparable to similar goods. Some of the obstructions that can prevent the development or improvement of a viable fishing industry even in the presence of a productive fish stock include:

a) Consumers, because of cultural tradition, habit, low incomes, or other reasons, may be reluctant to consume that particular kind of fish.

b) The market may be restricted due to ineffective distribution channels.

c) There may be ineffective processing or harvesting equipment.

d) There may be a lack of ports and harbor facilities.

e) There may be restricted access to capital markets which prevents individuals or firms from borrowing money to finance activities that would remedy other impediments.

f) There may be a lack of interest in fisheries development (at least relative to other forms of activity) by the government agency charged with encouraging economic development.

Consumer acceptance is obviously a prerequisite for utilization of a particular type of fish. The protein content is important in consumer acceptability, but texture, appearance, and smell can also be critical. In some instances, however, these attributes are not as important as familiarity, habit, and cultural or religious restrictions. On the other hand, if consumer income and hence the demand for fish is low, consumer acceptability is really a matter of price. Obviously if people do not choose to consume a certain type of fish, for whatever reason, the fact that a viable stock is available for exploitation is of very little economic consequence.

Another impediment to development is imperfect distribution channels. Fish is a highly perishable product, and the size of the market depends, among other things, upon how well a suitable product can be distributed.

There are many reasons why market channels fail to develop. In some instances there is a lack of roads or trucks; in others it may be difficult to establish a final retail outlet; in still others there may be a lack of post-harvest preservation technologies. On other occasions, all that is lacking is an entrepreneur willing to take on the role of middle man between the fisherman and the final consumer. There is often a "chicken and egg" type problem. Individuals are hesitant to go to the expense of setting up marketing channels unless they can be assured of a reasonably large and steady supply, but fishermen are hesitant to build a fleet that could provide such a supply until they are assured of a steady market. Since it is very difficult for one group to commit to a course of action independently, oftentimes no action is taken. In
still other instances, the very size of the country prevents the development of a viable industry. Some fish products, such as fish meal, have such a low unit value that large-scale production is necessary to earn a reasonable return. If the country's demands for these products, including existing export potential, are small relative to the scale of operation necessary for profitability, an industry will not develop.

Another reason why viable fisheries fail to develop is that existing harvesting and processing technology may be so inefficient that costs are high relative to the prices of substitute products. The problem may be complicated by the lack of access to capital markets which prevents the introduction of new technology, or by a restricted market size which would make the introduction of the new technology unprofitable because it requires a large-scale operation to obtain low unit costs. A further complication is the lack of trained workers familiar with and willing to use the new technologies.

Related to this is the lack of port and harbor facilities. Such facilities are necessary for a viable fishing industry, and yet they are not something that can easily be built by a private individual or firm. When the types of structures needed are modest, fishermen cooperatives may be formed to construct them. Restricted access to capital markets or the lack of managerial talent may prevent it from occurring, however. In other cases, the facilities that would be the most appropriate would be far too expensive to be constructed by cooperatives. The problem is much more complex when the facilities needed for fisheries development are part of large and diversified harbor and port requirements.

In some instances a lack of access to the market for loans can prevent the development of a fishery. For example, banks may be hesitant to loan money to fishermen in small villages in dispersed areas because of a lack of familiarity with their working and repayment habits or the lack of opportunities to collect loan payments easily. This may be so even if the loans would allow for the development of a productive small fishery that besides being a useful contribution to the economy, could easily provide enough income to repay the loans.

Finally, it is sometimes the case that while some of the obstacles mentioned above could be removed by appropriate governmental policies or actions, the government chooses not to do so. Sometimes this is done because it is felt that they are not appropriate things for the government to do. In many developing countries, however, all potentially profitable development paths cannot be taken at the same time. The government has to decide which projects to undertake given their limited budget and the relative payoffs in terms of value of output, employment, net foreign exchange earnings, uncertainty of the final outcome, etc. Depending upon the actual situation, the overall development goals, and the views of the administrators, fisheries development is sometimes ignored.

In summary, anything that affects the price at which fishermen can sell their output or the cost of harvesting, processing, and distributing their catch will have an effect on the viability of the fisheries industry. If there is no viable fishing industry, that there is a highly productive fish stock will be of little consequence.

Fisheries Development

If one or more of the above impediments exist, the fish stock will not be exploited, or will be exploited at a level below its full biological potential. It is important to realize, however, that if the impediments are artificially induced or if they can be removed at costs that are low relative to the benefits to be provided by an efficiently operating fishery, then the fishery will be operating below its economic potential as well. If one were to review all the fishery development projects in the world over the past 20 yr, they could be classified according to which of the above impediments they were trying to overcome.

It would go well beyond the scope of this paper to discuss the problems of fisheries development in any great detail. In the current context, however, one point is very important. Although Mount Everest, according to some mountain climbing aficionados, is to be climbed simply because it is there, fisheries development impediments are not necessarily to be broken just because they are there. Opening up a fishery can be very costly, both in the short and the long run, and development should not be encouraged unless the benefits of the fishery are worth the costs. While it may seem a waste to see a biologically abundant species go unused, it can sometimes be a greater waste to open up commercial exploitation. These comments are not to be interpreted as an argument against fisheries development, but only against fisheries development at all costs.

The Paradox of Fisheries Development

One of the basic tenets of fisheries economics is that with open access, a fishery will operate inefficiently because vessels will enter the fleet until total revenue equals total cost and not until the value of the last unit produced is equal to its marginal cost. When this is so, government intervention to control the level of fishing can provide the potential for a net increase in the value of goods and services produced, if management costs are less than potential gains. Similarly, a fundamental presumption of economic development is that although there may be certain barriers to the development of particular industries, if these obstacles are removed the industry will be able to operate successfully, independent of further government intervention. The synthesis of these two canons is that economic development policies for fisheries should include the provision for a change in the impetus of government intervention from that of an accelerator during the process of removing the restrictions to that of a brake once the fishery is established. In the real world, however, it has usually been the case that exclusive attention is focused on breaking the barriers and it is forgotten, that once moving, an open-access fishery will be by its very nature, proceed too far.

Development projects in fisheries are fundamentally different than most other economic development projects due to the fugitive nature of fish stocks. In most countries in the world today, all that it takes to fish is
ownership or control of the appropriate gear. Since there are no explicit property rights to the fish, they are fair game for all. This gives rise to what I call the paradox of development in open-access fisheries. Simply put, the paradox is that otherwise potentially profitable development activities in open-access fisheries will result in lower gains (and sometimes even net losses) than would be possible if proper concern were given to management. The paradox of development in open-access fishing can be explained as follows. Successful development projects raise the net returns to fishing, and therefore more people find it profitable to fish. This results in an expansion of the fleet and a reduction in fish stocks. The net effect of development, therefore, is often more effort catching less fish from a smaller stock at higher costs. While the above is a somewhat extreme and simplified explanation of the paradox, it makes the point. Development projects without management components will be less beneficial than would otherwise be the case.

If there is a moral to this story, it can be gleaned from the last two sections. Breaking the impediments to fisheries development will not necessarily always be a good idea, but if it is, in order to capture all the potential gains, the fishery must be appropriately managed.
Pacific Tuna: Biology, Economics, and Politics of a Large Fishery Resource

JOHN E. BARDACH

Research Associate, East-West Center, Adjunct Professor, University of Hawaii, Honolulu, HI 96817, USA

This paper deals with one of the world's most valuable living resources, the tuna stocks of the Pacific Ocean. It attempts to juxtapose their ecology to the economic and political determinants of their utilization. An attempt at such an approach is important for a number of reasons. Increasing fishing pressures coupled with improvements in the technology of fishing gear, fish finding, and fish processing have greatly increased abilities and incentives to catch fish. At the same time proper management measures and effective development programs should be based on adequate information about fish and the environment in which they live. That this information is often deficient, as it is in the case at hand, hardly needs to be stressed.

The ocean environment is variable and so are the stocks that depend on it; the more they are exploited, the more sensitive they become to ocean variability and the more difficult it will be to apply timely and effective management measures (SCOR/WG-67, 1982). When the abundance of several stocks is governed by this ocean variability and when they are exploited by the same fishery, it may be difficult indeed to apply timely and effective management measures for one or the other of these stocks. In addition, when they differ in status regarding levels of exploitation and opportunity for expansion of fishing, as is the case with the tunas of the Pacific (see below — the present status of stocks), a juxtaposition of biological, economic, and political particulars related to their exploitation may be particularly valuable. It must be added, however, that there are many unknowns in tuna biology and that fishery policies and economics especially in the southwestern Pacific are very much in a state of flux, prominently related to the now general acceptance of 200-mi Exclusive Economic Zones (EEZs).

Together with a need for a sound scientific base of conservation or development regulations, it must be mentioned that economic and perhaps political considerations can override or delay implementation of management measures accepted, as necessary, even by policymakers. This is clearly shown by the course of events in the Peruvian anchovy fishery after its collapse in 1972, and by the persistence of trawling in the Gulf of Thailand after biologists pointed out unmistakable danger signals to the stocks of bottom fish.

Overriding as the needs may be of various nations for food, and for deriving economic benefits from fishing or by selling access to it, it is my tenet that even a cursory and data-limited look at fish and fisheries can be enlightening to both scientists and economic decision-makers alike. Therefore, there follows a section on the biology of the principal Pacific tuna species and the environmental variables that determine their distribution, one on the fisheries with emphasis on what nations fish in what other nations' EEZs, and one that relates these two topics. In it, I will speculate on the resolution of present and possible impending conflicts relating to the tuna fisheries of the Pacific Ocean, especially the southwestern portion.

Determinants of Tuna Distribution

The tunas proper or Thunnini, a tribe of the family Scombridae, have four genera and 13 species, predominantly of tropical and subtropical distribution (Klawe 1977). As in the comprehensive review of Pacific tunas by Sund et al. (1981), discussion will be restricted here to the six principal commercial species listed below:

<table>
<thead>
<tr>
<th>Scientific names</th>
<th>Common names</th>
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<tbody>
<tr>
<td>Thunnus albacares (Bonnaterre)</td>
<td>Yellowfin</td>
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<tr>
<td>Thunnus alalunga (Bonnaterre)</td>
<td>Albacore</td>
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<tr>
<td>Katsuwonus pelamis (Lineeus)</td>
<td>Skipjack</td>
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<tr>
<td>Thunnus obesus (Lowe)</td>
<td>Bigeye</td>
</tr>
<tr>
<td>Thunnus thynnus orientalis (Temminck and Schlegel)</td>
<td>Northern Bluefin (Pacific)</td>
</tr>
<tr>
<td>Thunnus maCOYI (Castelnau)</td>
<td>Southern Bluefin</td>
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</tbody>
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The tunas of commerce are voracious, large to very large predators representing one of the peaks of fish evolution. Their evolutionary path leading from near-shore, smaller species to widely roaming ones of larger size, permits them to spread into the top layers of the world's warm, open seas. They are notable for their torpedo-like body shape and their abilities of elevating their body temperatures differentially over that of the environment and of regulating it to a certain extent by means of variously arranged red muscle within the body musculature and adaptations of the circulatory system (Collette 1978; Sharp and Pirages 1978; Sharp and Vlymen 1978; Sund et al. 1981).

They have been called "thermoconserving" rather than homiothermous by Dixon and Brill (1979). Their elevated body temperatures over that of the surroundings enable them, in the main, to swim faster than most other fishes with rapid recovery after rigorous exercise. While pursuing their prey with frequent bursts of very high speed, they are also said to have enhanced vision due to the elevated temperature of eyes and brain, and to have uniquely evolved to colonize vast reaches of the tropical and subtropical seas. At the same time, the property of thermoconservation with an attendant high metabolic demand poses limits of temperature levels and oxygen concentration on their distribution. These limits vary with species and size; for instance, the smaller Skipjack are in danger of losing heat faster than the very large northern Bluefin tuna which can therefore operate, as it were, in colder, northern waters. Absence of a swim bladder in adult Skipjack, while endowing
them with the capacity of very fast, especially vertical swimming bursts, forces them to expend more effort on routine swimming than would be demanded of the other more buoyant species that have swim bladders. Skipjack, therefore, have a higher oxygen demand than the other species of tuna. It is this property that restricts their depth distribution (Sharp 1978).

The high metabolic rate of tunas is borne out by the ratios of their respiratory surface to body weight; this ratio is in the range of those of mammals rather than of fishes as Tota (1978) shows for Bluefin tuna. It is certain that their blood characteristics resemble those of mammals as Morrison et al. (1978) show for Albacore; their high rate of metabolism is obviously related to their thermoregulatory capabilities. In consequence, their daily food needs, especially those of smaller, juvenile fishes, can be as high as 25–30% of their body weight (Sund et al. 1981); in contrast, daily food intakes of other juvenile fishes are around 15% and those of adults between 3 and 8% of body weights (Lagler et al. 1978). Thus, it is imperative for tunas that their day to day locations coincide, at least within limits, with high concentrations of food organisms. This obligatory coincidence is especially important for sexually maturing, prespawning fish.

In general then, the limiting environmental variables for tunas are temperature and oxygen concentration; they bracket the tuna to a shallower water layer in the eastern than in the central and western tropical Pacific, a limitation that is also reflected by the success of purse seineing for Yellowfin near the coasts of Mexico and Central America. Concentrations of food organisms engendered by oceanic fronts and by the spreading of upwelling water masses (Murphy and Shomura 1972) further limit their prevalence as exemplified by the succession of algae, zooplankton, and carnivorous micronekton, the latter serving as tuna food a few degrees south of the equator in the southwestern Pacific (Donguy et al. 1978).

Salinity by itself does not seem to be a barrier to tunas; Atlantic Bluefin migrate into the Mediterranean (Rivas 1978) with its regions of considerably lower and higher salinities than those that prevail in the mixed layers of the tropical and subtropical Pacific. Salinity may, however, be an indicator of tuna prevalence, through the salinity characteristics of currents in which tuna drift (Seckel 1972); also, salinity levels indicate mixing stages of upwelled with surface waters where, in turn, are found suitable food organisms for the tuna (Donguy et al. 1978).

It is a characteristic of tuna and seems a consequence of their evolution-engendered physiology that they engage in very long migrations, even across the entire breadth of the Pacific as shown for Albacore by Foreman (1980), for Skipjack by Forsbergh (1980), and for Bluefin, which also make spectacular north–south treks extending over 60 or more degrees of latitude, by Bayliff (1980).

Knowledge of migrations comes from the recaptures of tagged fish, and since these are still relatively few in number for any of the species, they pose more questions than they answer. In some instances, migration records suggest that subpopulations within the stocks do mix; in other cases, however, they point to some division of subpopulations (Wilson 1981) with the clear need for long-term records. Thus, there may be varying effects of fishing in one location on the fish yields in others.

Related to migrations are also the questions of orientation and pathfinding. Correlation of times elapsed between tagging of Skipjack tunas on the west coast of America and their recaptures near Hawaii with calculated displacement times of floating objects in the current between the two locations suggests that they drift in the current (Seckel 1972; Williams 1972). Displacement of tuna over long distances with a current thus may apply to certain migration routes of adults as well as larvae and juveniles.

Daily movements in search of food occur within the current. However, not all tuna migrations can be explained in this fashion since tuna movements also occur between current systems (Nakamura 1969). Various theories have been advanced about the long-range orientation of tuna with the most promising recent one being that the fish have a geomagnetic sense. This hypothesis is borne out by physiological and behavior research of Walker (1982), establishing that Skipjack tuna have high concentrations of magnetite crystals in the ethmoid bone complex with no occurrence of this substance in any other organ or structure. What is more, live fish could be successfully trained to a reversal in the magnetic field surrounding them. Orientation to variations in the earth's magnetic field is the most "practical" way of maintaining a genetically determined path on long distance migrations (when they are not determined by drifting in a current), and tuna appear to share this previously widespread capacity with passenger pigeons, salmon, and even bees (Dizon 1982).

The Distribution of Principal Tuna Species in the Pacific Ocean

The descriptions presented here, albeit selectively, are based on Bayliff (1980a, b), on Sund et al. (1981), and on NOAA's Technical Memorandum 15 which is a Status Report on World Tuna and Billfish Stocks (1981). Present knowledge of the ranges of the various species is, in the main, based on fishing records, especially those of the Japanese who are the most active and widely roaming pursuers of tuna in the Pacific, on sampling data of fishery research vessels of various nations, prominently including stomach analyses to establish larval distributions, and on pertinent data from oceanographic cruises.

As already mentioned, the main environmental limitations of tuna ranges, aside from food, are temperature and levels of oxygen. The various species have, however, evolved peculiar characteristics of size and/or physiology that permit them to fully but somewhat differentially exploit various regions and niches of the mixed layer in the tropical and subtropical seas. For instance, their large sizes endow Bluefin with good powers of thermoconservation and thus permit them to
invade colder waters than can be tolerated by other species; the smaller Skipjack with the higher metabolic rate than those of any other species of tuna must guard against overheating. It is, thus, barred from certain warmer waters. Albacore seems to prefer somewhat cooler temperatures than some other tunas and ranges a bit further to the north and to the south. On the other hand, Bigeye, as its name implies, has not only evolved attributes of vision that seem to permit good prey perception in the twilight of greater depths, but is also tolerant of somewhat lower oxygen levels than other tunas (Sharp 1978), and perhaps a bit more tolerant of lower temperature; in any case it is found at greater depths than other tunas.

Spawning areas are more restricted than the general range of Albacore, Yellowfin, Bigeye, and Skipjack and extend over wider reaches in the western than in the eastern Pacific, most probably because of temperature and oxygen conditions that the fish can tolerate; optimal temperature conditions for the larvae appear to be higher than those for the adults, at least for Yellowfin. Compared with the other species treated here, the southern and northern Bluefin tunas have more restricted and distinct spawning grounds located in the warmer waters of their total ranges.

Yellowfin

The latitudinal distribution of Thunnus alalunga in the Pacific extends from near 40°N to about the same degrees in the south, with the northward spread of the species being a bit more pronounced in the western than in the eastern Pacific. Catches on longlines are greater per unit of effort or per modular surface area between the tropics of Cancer and Capricorn with peaks near the equator, especially in the west. Juveniles extend poleward to about 23°N or 23°S and the comfortable temperature for Yellowfin tuna seems to lie between 20 and 30°C with occasional, apparently feeding, excursions into colder waters.

They first mature at the beginning of their 3rd year, and adults exceed 160 cm fork length; in fact, the record size of Yellowfin so far is 209 cm, a fish that may well be over 10 yr old, although the fishery relies, in the main, on 2- to 4- to 5- yr-old fish. In the western and central Pacific, truly pelagic fish mature at about 100 cm with peak modes that are somewhat larger; along the west coast of Central America and close to islands, spawners of about 60 cm have been captured. Spawning occurs in the northwestern and southwestern reaches of their distribution during the summer months in either region. In the central and western Pacific, closer to the equator, Yellowfin spawn all year. As the isotherms converge towards the equator in the eastern Pacific, spawning there appears to be restricted to between the equator and about 15°N. The limit below which spawning does not seem to occur is around 25°C, as ascertained by the distribution of larvae captured in experimental tows.

Comparisons of longline catches among several species suggest that Yellowfin tend to be in shallower water than the Bigeye although both of them roughly share an overall distribution. Available tagging returns indicate that Yellowfin migrations are not as extensive as those of other tunas. They mainly engage in north-south migrations, closer to land, but they may drift with some currents such as Kuroshio and East Australian Current. Biochemical investigations of their blood characteristics suggest that spawning cohorts exist, implying genetic differences among groups of fishes in an area; however, it doesn’t confirm the existence of geographically distinct substocks. Nevertheless, seasonal catch data and sampling for larvae suggest the existence of three, more or less, independent stocks of Yellowfin in the eastern, central, and western reaches of the Pacific Ocean, respectively (Fig. 1).

Yellowfin are indiscriminate, sometimes surface feeders with the size of their prey being limited by the gaps between their gill rakers and the distensibility of their esophagus. More than other tunas, they are wont to associate with dolphins, especially at the region where the thermocline is shallow.

The biological reason for the association of tuna and porpoises remains unknown. Speculations are that it helps both animals to feed or that it facilitates orientations by the tuna in an otherwise fairly trackless environment. The first speculation is supported by the fact that tuna have a keen sense of smell and can smell prey from afar, while porpoises have no olfactory sense at all. They, in turn, have organs that enable them to use powerful ultrasonic echo-location by means of which they track their prey. Thus, in a biological sense, “the lame would lead the blind” to clear potential mutual advantage. Whether a shoal of prey is smelled by the tuna or “heard” by the porpoise, both species would home in on it since both animals have excellent vision which permits them to maintain their association (Atema 1980).

Schools of Yellowfin and Skipjack tend to congregate beneath floating objects of some size. As these may give shelters to prey fishes which are pursued by tuna with a very keen sense of smell, it should be possible to fashion chemical attractants that could attract and retain tuna near buoys around which purse seines would be deployed (Ikehara and Bardach 1981). Pertinent experiments presently conducted in Hawaii seem promising (K. Holland, National Marine Fisheries Service, Honolulu, HI, personal communication).

Albacore

The distribution of Thunnus alalunga including their respective spawning grounds shows a clear division into north and south Pacific populations, with no mixing being assumed to occur between the northern and southern hemispheres. During northern and southern summers, they are not found beyond the 15.6°C isotherm in either ocean. In the northeastern Pacific, at least, 19°C seems to be their preferred upper temperature. These thermal ranges are characteristic of the so-called Transition Zone between the California and the north Pacific current systems; such zones between other currents are also rich in forage. They seem to guide migration paths across the Pacific being followed, somewhat differentially, by several year-classes (2-5, if not 6-yr-olds) of Albacore which get to be 10 yr old (rarely,
Fig. 1. Distribution of Yellowfin larvae in the Pacific, indicating that there are three spawning areas (from Bayliff 1980a).

Fig. 2. Albacore distribution in the Pacific. Hatched lines indicate areas of surface catches (from Bayliff 1980a; Surd et al. 1982).
now) and mature at the age of 5. The biggest Albacore recorded measured around 125 cm fork length, but the modal lengths of the two most frequently caught size-age groups are 65 and 80 cm. In a part of their range (Fig. 2), they seem to swim deeper than the reach of standard longlines (about 30 m).

At least, two subpopulations or substocks have been postulated to exist on the basis of tagging returns of northern Albacore; nothing is known yet on segregation into subpopulations of south Pacific Albacore. The minimum spawning size so far ascertained seems to be around 90 cm and spawning grounds of the north and south Pacific populations reach to about 150 and 105° Western Longitude, respectively, lying well to the equator of the hemispheric 16°C winter isotherms. Areas of larval occurrence also roughly overlap with the main longline fishing grounds, but more so in the south than in the north (Fig. 2).

**Skipjack**

*Katsuwonus pelamis* shares, with some minor deviations, the distribution ranges of Yellowfin and Bigeye tuna. Restrictions to which they are subjected, such as temperature and oxygen concentration, and their anatomy and physiology (absence of a swim bladder in the adult, high rate of metabolism, size relative to danger of “overheating”) have already been mentioned. For instance, they may explain the slightly further north- and southward reach of the overall range of Skipjack than of Yellowfin as well as its depth restraints with regard to long excursions or extensive travel in deeper water, compared to the Bigeye. They also explain size which is smaller than that of the other main commercial species in the tribe. While Skipjack of just over 100 cm have been caught, which would be 12 yr or older, they are an anomaly inasmuch as this size exceeds limits deemed possible on the basis of physiological calculations (Kitchell et al. 1978). The sizes of captured fish range in the main between 30 and 70 cm fork length in different regions. Skipjack mature in their 2nd year of life.

It is important for future management considerations to mention that the Pacific Skipjack has at last five but perhaps several more fairly distinct subpopulations (Fig. 3). The genetic segregation also seems to apply to temperature preferences, at least as shown by optimum temperature ranges for commercial fishing; for example, the substock near the east of Papua New Guinea has its optimum between 28 and 30°C, while that of the

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**Fig. 3.** Skipjack tuna distribution, subpopulations, and migrations in the Pacific (from Bayliff 1980a,b; Bardach and Matsuda 1980).
substock near New Zealand, where waters are colder, lies between 17 and 23°C (Sharp 1978). Substocks have been established on the basis of studies of their serum esterases and also by close, seasonal analyses of longline catches (Matsumoto 1975); the latter investigations postulate as many as 14 different groups of Skipjack which, however, need not represent differential genetic segregation.

It should be added that tagging returns have shown extensive Skipjack migrations to exist, some in a north-south direction close to land masses and some across a part or most of the Pacific (Fig. 3). Matsumoto (1975) writes: “the apparent movement of groups of Skipjack in the Pacific appeared to coincide with the circulation of the major ocean currents. The movement was counterclockwise in the Southern hemisphere and clockwise in the Northern hemisphere, except in the Eastern Pacific where the movement appeared to be counterclockwise, corresponding with the flow in the North equatorial water mass.” Rothschild (1965) ties the extensive migrations to eastward feeding treks and to subsequent returns to spawning grounds in the central equatorial Pacific. As has been mentioned, at least much of these travels is likely to be drift in currents. Northward feeding and southward spawning migrations also seem to exist in the westernmost Skipjack subpopulations in both hemispheres.

The spawning range is considerably smaller than that of the growing and maturing fish; it is again wider in the west extending from about 35°N at 140°E to about 25°S at 170°E. It converges, however, to a band of about 20° roughly centered on the equator towards the south and central American coasts. There are also two isolated areas in the South Pacific in which larvae have been found, one to the east and one to the west of New Zealand at about 30° latitude.

The extent of overlap of the various subpopulations in the spawning regions is not known nor the kind and degree of genetical and perhaps behavioral isolation where they mingle as several of them appear to do in the equatorial Pacific at about 180°. Some geographic segregation between the substocks may also prevail; tagging returns in the western Pacific suggest, albeit very tentatively because they only cover the span of a year or less, that the Papua New Guinea/Solomon Island stock does not mix appreciably with that which is caught in the

Fig. 4. Distribution of (dotted lines) and longline fishing grounds of Bigeye tuna in the Pacific. Darker areas denote higher hook rates and hatched lines indicate areas of surface fisheries (from Bayliff 1980a; Sund et al. 1982).
region of the New Hebrides and American Samoa (P. T. Wilson, Fisheries Department, Port Moresby, Papua New Guinea, personal communication).

**Bigeye**

*Thunnus obesus* is found across the entire Pacific Ocean with isolated specimens having been caught as far north as 47° near the north American coast and as far south as New Zealand, at about 40°S. The main fishing areas for them are somewhat restricted in latitude (Fig. 4) as, indeed, are spawning areas. Bigeye larvae have been reported from about 30°N near Japan to about 20°S between Australia and New Caledonia but the prevalence of larvae in experimental net tows becomes attenuated to a band between the equator and 20°N as one approaches the Americas. Nothing is known about segregation into subpopulations, and their migrations are still poorly understood, even though one tagged fish was shown to have covered just over 1000 nautical mi. They first mature at the end of their 3rd year and can reach more than 180 cm in fork length; as a species, they are considerably larger than Skipjack and also larger than Albacore. First maturity has been recorded for them at the end of their 3rd year. Longline catches show that fish sizes increase towards the eastern reaches of the Pacific.

As already mentioned, the various species of tuna have partitioned the overall environment of the mixed layers in tropical and subtropical seas with some physiological and anatomical specializations, permitting differential exploitation therein. Bigeye occur, in general, deeper than the other tunas in the western and central tropical Pacific where the thermocline is deeper than in its eastern reaches. They are often found in or just below the thermocline and thus tend to escape normal, and in some areas even deep-set longlines. They seem to tolerate a somewhat lower oxygen concentration than the other tunas and their eye size, at least, betokens twilight vision. In the central Pacific, they have been shown to feed mostly on fish and squid, but, like the other tuna species with which they share a geographic range, they are, and indeed must be, highly opportunistic feeders. They feed over their entire depth range both during day and night as shown by nighttime longline catches; the other tunas are mostly day feeders.

**The Bluefin Tunas**

Atlantic Bluefin (*Thunnus thynnus thynnus*), Pacific Bluefin (*Thunnus thynnus orientalis*), and southern Bluefin (*Thunnus macoyi*) are the largest of the tunas and the most "advanced" in evolution. They share special features in their vascular systems which enable them to elevate their body temperature considerably above that of the environment (Collette 1978). They can thus invade temperate and even colder waters as is well shown by records of Atlantic Bluefin tuna, the first mentioned species, which feeds in waters off Norway that are between 12 and 16°C (Rivas 1978). The text of plate 7 in Sharp and Diazon (1978) illustrating the arrangement of their red muscle and its blood supply reads as follows: "They lack the central vascular system and as a group represent the most modern of tunas. Their habitats range from the most extreme continuous cold in adult *T. macoyi* to the most varied in *T. thynnus* (5° to 30°C.). A well developed gas bladder and lateral

![Fig. 5. Migrations of Northern (stippled lines) and Fishing Grounds for southern Bluefin tuna (solid lines) in the Pacific. Note Sp marking respective Tropical Spawning Grounds (from Bayliff 1980a; National Marine Fisheries Service 1981).](#)
cutaneous vasculature are common to this group. External temperatures of the two example species have been measured to be in excess of 20°C above ambient.

Another feature that is shared by the Bluefin group of tuna species and that indicates their warm water origin is its spawning grounds. These are very restricted compared to their very extensive total ranges: Pacific Bluefin spawn in an area southeast of Japan between Japan and the Philippines Islands, while the southern Bluefin spawn in the Indian Ocean between the Indonesian Archipelago and Australia (Fig. 5). Juvenile northern Bluefin migrate from the spawning area to the seas between Tasmania and New Zealand, others swim first with the Kuroshio and then all the way across the Pacific to California, respectively, later to return to their western Pacific breeding grounds. There is a possibility, albeit unconfirmed, that the southern feeding group of the northern Bluefin represents a different subpopulation from the northern one.

Southern Bluefin move south from their spawning grounds and enter the westwind drift regions of the southern seas with catches spreading from the mid-Pacific in a circumpolar fashion, between below 30–50° southern latitude all the way around the globe to South African waters (Fig. 5). They may attain an age of 20 yr, considerably exceeding in size that of the larger specimens in the longline catches which have a mode of 150–170 cm fork length and are believed to be 7–9 y old (Murphy and Sutherland 1980). Fish predominate in the diet of the southern Bluefin, although cephalopods and crustaceans are also represented.

The northern Pacific Bluefin appears to be more short-lived than its southern cousin and to grow fairly fast; the oldest captured fish in the western Pacific was estimated to be at least 9 yr old, and from fish reared in captivity, the age of first maturity was determined to be 3 yr. They also seem to have a preponderance of fish in their diet, although local feeding on red crabs, squid, and other invertebrates is reported.

Fisheries and the Status of Stocks

In 1979, sixty-eight percent (68%) of the world catch of principal commercial tuna species came from the Pacific Ocean with conservatively estimated annual exvessel value of U.S.$1.6 to 1.7 billion (FAO 1980; NMFS 1981, 1982). On the retail market and/or processed, the total value is, of course, a multiple of that just noted. The gear with which the various species of tuna are fished is mostly nonselective, with deep longlines for Bigeye being somewhat of an exception here. In the South Pacific, the longline and, in places, purse seine fishery for southern Bluefin is also becoming a one-species fishery. Purse seining, taking more than one species, however, is the most prevalent method of capture over most of the Pacific especially in its northeastern portion where a shallow thermocline concentrates the fish near the surface. Currents that produce food through upwelling on the downstream sides of islands and reefs have the same effect (Fig. 6). Multiple pole and line fishing and longlines are also indiscriminate in whatever catch (Bayliff 1980; Bardach and Matsuda 1980). Under such fishery conditions some stocks may indeed be more threatened than others. This particularly applies to Yellowfin in the northeastern Pacific.

In Table 1, fisheries of the eastern Pacific are considered separately from those in the western and central Pacific, in part because there is relative geographic segregation of stocks and in part because the fishery in the former area (Commission Yellowfin Regulation Area or CYRA of the Inter-American Tropical Tuna Commission, IATTC, see 'The Eastern Pacific section') is managed more intensively than the fisheries of other regions. Most of these fisheries are not yet, and apparently need not yet be, under a management regime that restricts the fishery as to season, location, and gear. (The IATTC region, by the way, is the only one of the prime tuna areas of the Pacific where the Japanese do not take at least half of the catches.) The range of the southern Bluefin with its one species fisheries is another region where restrictive management measures have been applied (Table 1). This noted, further remarks on the various fisheries will now follow.

Yellowfin

The fishery for Thunnus albacares in the eastern Pacific is largely done with purse seines (Fig. 7); its former pole and line components are steadily decreasing. Skipjack, Albacore, Bluefin, and Bigeye are incidentally taken even though Yellowfin is the species of preference with Skipjack in second place. Seasons and quotas for Yellowfin are recommended by the Inter-American Tropical Tuna Commission (IATTC) to the various member governments of the Convention under which the IATTC operates (see next section). In 1979,
<table>
<thead>
<tr>
<th>Species/stock/fishery</th>
<th>Catch at or near MSY with about optimum effort being expended under current fishery management</th>
<th>Increase in catch likely with an increase in effort</th>
<th>Increase in catch likely with change in management provisions</th>
</tr>
</thead>
<tbody>
<tr>
<td>Yellowfin, E Pacific</td>
<td>No, recent catches and effort above MSY and optimum with current age structure</td>
<td>No</td>
<td>Yes, increasing the age of first capture would yield substantial improvements in total catch over time</td>
</tr>
<tr>
<td>Yellowfin, W Pacific</td>
<td>Yes, especially for longline</td>
<td>Some increase in surface fishery catches possible; total yield may increase</td>
<td>—</td>
</tr>
<tr>
<td>Albacore, S Pacific</td>
<td>Currently about 25% below MSY</td>
<td>Yes, may be feasible with expansion of surface fishery</td>
<td>Possible to reduce longline effort substantially with little reduction in catch</td>
</tr>
<tr>
<td>Skipjack, E Pacific</td>
<td>—</td>
<td>Increases in catch likely with increased effort, especially if smaller fish are caught without affecting recruitment</td>
<td>—</td>
</tr>
<tr>
<td>Skipjack, W Pacific</td>
<td>—</td>
<td>Stock appears underexploited</td>
<td>—</td>
</tr>
<tr>
<td>Bigeye(^b)</td>
<td>Does not, at present appear to be overexploited; however, caution is advocated in E Pacific</td>
<td>Further longline modifications may increase total tonnages landed</td>
<td>—</td>
</tr>
<tr>
<td>Bluefin, N Pacific</td>
<td>—</td>
<td>—</td>
<td>Western Pacific–Eastern Pacific catches have declined substantially but at present cause and therefore cure are unknown. Analyses will be performed</td>
</tr>
<tr>
<td>Bluefin,(^b) S Pacific</td>
<td>Claimed to be the most heavily exploited of the tuna and over its MSY at that; in spite of self-imposed restrictions by Japanese longline fisheries (1971) and by Australian government (1976) on purse seining relative abundance of adults hasn't yet increased</td>
<td>No</td>
<td>Uncertain</td>
</tr>
</tbody>
</table>

\(^a\) Abstracted from NOAA Technical Memorandum NMFS, Status Reports on World Tuna and Billfish Stocks, 1981.

\(^b\) Abstracted from IATTC Report No. 2 (Bayliff 1980).

The fishery yielded about 195 000 t of Yellowfin which dropped to 146 000 t in 1980 (IATTC 1981). Vessels of 17 nations participated in the fishery, with the United States and Mexico taking the largest share followed by Ecuador, Japan, South Korea, Panama, Spain, and Venezuela; vessels under Bermudian registry, and Costa Rica plus a few others took minor tonnages (Fig. 8).

Purse seining is also practiced in the northwest and southwest Pacific with about 50% of the catches going to Japanese fishermen (Fig. 7). In the west central and south equatorial parts of the Pacific, there is mainly a Japanese longline fishery for Yellowfin that also takes Skipjack, with neither species being in danger of overexploitation in this region. The Yellowfin catch in the entire Pacific in 1979 was 375 000 t of which the Japanese took 26% (FAO 1980).

**Albacore**

The fisheries of *Th. alalunga* are undertaken with longlines, mainly by Japan which took 67% of the Pacificwide catch of about 110 000 t in 1979 (FAO 1980); Korea and Taiwan also participated in the fishery. There is also a live bait, pole and line fishery in the spring for fish migrating in the Kuroshio current near Japan and
Fig. 7. Areas of purse seining for Yellowfin in the western and eastern Pacific (from Bayliff 1980a).
another near New Zealand. Trolling for Albacore is done by North American and Japanese fishermen in the eastern Pacific. It is uncertain if MSY (Maximum Sustainable Yield) for Albacore is approached with most caution regarding an increase in fishing effort being advocated for the southern ocean longline fishery.

Skipjack

*Katsuwonus pelamis* is believed to be the most abundant of the tunas with an oceanwide MSY being estimated at 2, if not more, million metric tons. The total Skipjack biomass in the Pacific including the southeast Asian seas, not its MSY, is estimated to be as high as 10 million metric tons (Kearney 1981). The catch of Skipjack in the Pacific was about 566 000 t yielding 80% of the world catch of this species (FAO 1980); 56% of the Pacific catch was taken by Japan, mainly in the southwest Pacific and in the EEZs of newly independent island nations. The fishery for Skipjack is pursued with bait and multiple pole and line boats in Hawaii and near other islands; however, purse seining is becoming prevalent, overall, especially as baitfish are becoming scarce, and as purse seining, where possible, is the more efficient method of securing the tuna. In the Philippines, a special method of purse seining is employed, relying on encircling, especially for the younger fish which tend to associate with floating objects (Bardach and Matsuda 1980). If smell attractants can be developed for Yellowfin (see above), they may probably be applied to purse seining for Skipjack as well.

The Philippines took 40 000 t of Skipjack or 14% of the catch in FAO statistical area 71 (essentially the south-west Pacific) in 1979 while Indonesia took 30 000 t, or about 9%; Papua New Guinea took about 24 000 t or 7.7% of this area’s catch (FAO 1980).

**Bigeye**

Little can be added about this species to the entries in Table 1 except to say that the longline fisheries of Japan took 77% of the Pacific Bigeye catch of 134 000 t in 1979 (FAO 1980) and that Bigeye of fresh sashimi grade can fetch $3750 per tonne exceeding even the price of equivalent grade of Yellowfin in the Japanese market (NMFS 1982). It is believed that Bigeye fisheries should not be expanded appreciably.

**Bluefin Tuna**

It should be added to the entries of Table 1, concerning northern and southern Bluefin tuna, that northern Bluefin catches are shared in the main by Japan and the United States of America (30%) (FAO 1980), Japan derives most of her Bluefins from the western Pacific while the United States’ share is taken by purse seines in the eastern Pacific. The catches were about 20 000 t in 1979 (FAO 1980), and both nations have shown concern over the status of the stock(s) with restrictions on vessels being implemented for the Japanese fishery.

Two-thirds of the southern Bluefin tuna catches (33 000 t in 1979) (FAO 1980) go to Japan with the rest accruing to the Australian fishery. Both nations have implements some controls and restrictions over times and numbers of vessels; for instance, Japan has instituted closed areas and seasons mainly to protect the spawning area and juveniles while Australia has limited the number of purse seining vessels because they take too many small fish.

**Regulation and Management Problems of the Pacific Tuna Fisheries**

Taking the tuna of the Pacific Ocean as a whole, there appear two broad categories of concern: one about conservation and allocation of shares in the harvest and the other of equity of apportioning the revenues from the fishery. The former mainly apply to Yellowfin in the eastern Pacific and to the region of southern Bluefin fisheries in the southern oceans while the latter is of relevance in the equatorial and subequatorial portion of the western Pacific Ocean. There one finds numerous new independent nations all of which have declared Exclusive Economic Zones (EEZs) together covering the most important tuna grounds predominantly ployed by distant water fishing nations (Fig. 9). It is noteworthy, however, that there appears to exist less, if any, need for restrictive conservation measures in these ocean reaches (Table 1).
Biological reasoning predicates conservation measures for a highly migratory species to be so coordinated that they encompass its entire range or that of distinct subpopulations of a species if these do not mix to any appreciable extent. Unfortunately there are still substantial gaps in knowledge about the substocks of the Bigeye, the Skipjack, Bluefin, and to some extent also of the Albacore and even, though far less so, of the Yellowfin.

The Eastern Pacific

That the pursuit of management goals for highly migratory species requires a substantial degree of international cooperation need hardly be stressed. Given the old tradition and practices that fish are a common property resource and therefore belong to their taker, such cooperation has not been and in some instances is still not easy to achieve. It has been best, though still not well, achieved in the eastern Pacific under the aegis of the Inter-American Tropical Tuna Commission (IATTC). That Commission operates under the authority and direction of a Convention originally entered into by the Republic of Costa Rica and the United States. The Convention, which came into force in 1950, is open to adherence by other governments whose nationals fish for tropical tunas in the eastern Pacific Ocean. Under this provision, Panama adhered to it in 1953, Ecuador in 1961, the United Mexican States in 1964, Canada in 1968, Japan in 1970, and France and Nicaragua in 1973; Ecuador withdrew from the Commission in 1968, Mexico in 1978, and Costa Rica in 1979 (IATTC 1980).

The principal duties of the Commission under the Convention are (a) to study the biology, ecology, and population dynamics of the tunas and related species of the eastern Pacific Ocean to determine the effects that fishing and natural factors have on their abundance; and (b) to recommend appropriate conservation measures so that the stocks of fish can be maintained at levels that will afford maximum sustainable catches if and when Commission research shows such measures to be necessary.

In 1976, the Commission’s duties were broadened to include problems arising from the tuna–porpoise relationship in the eastern Pacific Ocean (IATTC 1981). The most important feature of the Commission and the Convention under which it operates is that it enables regulations of the eastern Pacific tuna fishery to be based on scientific studies and that a need for such studies and their underpinnings is well recognized and supported by the member governments of the convention. In 1980, aside from the gathering of statistics on catches, these studies were directed mainly towards certain aspects of tuna biology. Landings and status of vessels, catches per single day of fishing (CSDF), and catches per ton of carrying capacity (CTCC) were ascertained in the statistics category. As for biology, attention was given to population structure and migrations largely through tagging; subpopulation studies based on inquiries into the morphology and the biochemical physiology of the fish;
food studies; age studies of several species; assessment of distribution of Yellowfin by time and area; studies of tuna–dolphin relations with the intent of enhancing the protection of the cetaceans; oceanography, and tuna ecology, to mention only the most important ones.

In summary, based on models constructed from several of the previously mentioned research efforts, it appears that the fishery for Yellowfin has exceeded the allowable maximum sustained yield (AMSY) (Fig. 10). The upper limit of catches in the area which comes under Commission purview should not exceed 155 000 t per year to sustain eventually an AMSY for Yellowfin; it was set at 160 000 for 1981 (IATTC 1981).

The take of Skipjack in the Commission area need not be restricted at present but “it is of prime importance to define the stocks or subpopulations of Skipjack in the Pacific Ocean which has not yet been satisfactorily done.” (IATTC 1981). Only the continuation of such studies will permit estimates of allowable fishing intensities for Skipjack in the several Pacific fishery regions where this species occurs.

Investigations into Bigeye and northern Bluefin biology are not sufficiently advanced to recommend management measures for them in the Commission area; coordination of Bluefin research with that of Japanese biologists in western Pacific waters is important.

To the relative paucity just lavished on the IATTC, there must be added the recognition that it and the Convention under which it operates could not stem the overfishing of Yellowfin. Fishing success during the years after World War II led to overinvestment in boats and gear. Add to this trend, the fashioning of more effective and costly gear such as large purse seine and seiners, and the fact that tuna were relatively more important in the economies of nations other than the United States. Further consider a stance towards the governance of the ocean commons under the influence of the age old tenet of fish being true common property resources and the partial failure to curb too heavy a fishing effort can well be explained. Since the costly vessels can only be retired after they are fully or nearly amortized, it will take years to repair the damage.

Nations in the western Pacific, where no restraints on the main fishery (Skipjack) seem necessary, might well heed what has happened in the eastern Pacific over the last 30 yr or so, and prepare now for the most effective communal management stance possible. That this will be far more difficult for them than the valiant, albeit late attempts, that are now being made by the eastern Pacific tuna fishing nations, will become obvious in the following passages.

The Western and Central Pacific

The western Pacific exemplifies, perhaps more than any other ocean region, the conundrum posed to national decisionmakers and to the fishery scientists advising them, what the Law of the Sea (UNCLOS III) convention says about the governance of fishery resources in the Exclusive Economic Zones (EEZs). Even before the Convention was adopted in New York in April 1982, the freedom of the seas for fishing that had characterized man’s use of living ocean resources was essentially abolished, and 99% of the world marine fish catch is now and will henceforth be taken in zones of national jurisdiction. True sharing of surplus stocks is predicated in the proposed LOS treaty as is consultation among nations with regard to them: yet it also says that “the Coastal State has the exclusive authority to determine the allowable catch of all types of fish in its exclusive economic zone” (Article 61/3, The Convention on the Law of the Sea, 1982).

The western Pacific, especially the area where tuna abound, corresponds roughly to that of the South Pacific Commission. Before proceeding with a discussion of that region and its tuna management problems, it ought to be mentioned that substantial tuna fishing also occurs in the waters near the Philippines and Indonesia (in the former mainly with floating leaf shelters and purse seines), and that returns of extensive tuna taggings from sites within the Commission area could not be secured from the waters of these two nations; migrations and the degree of mixing of stocks between the western Pacific and these southeast Asian waters cannot now be ascertained. The central tropical Pacific, an area of longstanding, almost exclusively by Japanese vessels, has international waters with the exception of Hawaii: it poses different management problems again.

But back to the western Pacific; the aggregate EEZs of nations within the South Pacific Commission extend over 29 390 000 km² while the land of 20 island nations, with an aggregate population of about 5 million, covers only 551 and some thousand square kilometres, presenting a sea to land ratio of 53:1 (Kearney 1981).

These ocean reaches are the very cauldron of Skipjack tuna production with an outstanding stock now pegged at 3.5 million metric tons as estimated from monthly catches and tagging returns. This suggests that a severalfold increase in harvest over the present one of around 300 000 t may be possible without depleting the resource (Kearney 1982). This statement, however, says nothing about the influence of substantially increased Skipjack catches on the harvest of this species in other regions, although speculations still based on insufficient facts tend to point to some distinctness of substocks and

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**Fig. 10. Relationship between catch and fishing intensity for Yellowfin tuna caught by longline boats in the Pacific, 1955–71** (modified from Suzuki et al. 1978). Stripped areas enclose spread of data points for 1955–60 and 1961–71, respectively, indicating that further intensive fishing may begin to deplete the resource (from Bardach and Matsuda 1980).
only moderate effects of fishing in one area on the spawning stocks in other, in some cases remote, regions.

Highly migratory species other than Skipjack have received some attention also by the South Pacific Commission with the result that moderate increases in effort seem permissible for Yellowfin longlining without damaging the stocks in the area.

The Commission, with headquarters in Noumea and New Caledonia, has a wide variety of research and development concerns, from health to the role of women in island societies, agriculture and trade, with fisheries and fisheries research being of prime importance among them. The Commission also pays attention to inshore, especially reef resources, but what its Tuna and Billfish Assessment Program tells the island nations is heavy news for them indeed, as it indicates that tuna may be very instrumental in satisfying their needs, wants, and aspirations to accompany quick entry into a world society of nations. With the exception of Papua New Guinea and Nauru which do have mineral resources, tuna appears as the prime means by which a nation can procure for itself the foreign currency it needs for development. As resource owners in the EECZs, they presently rely largely on license fees, negotiated nation by nation with the fishing companies of governments of the technically advanced nations, mainly of Japan, to obtain some benefits from the tens of thousands of tons of tunas these foreign vessels pull from their respective waters every year.

Joint ventures for tuna fishing and processing are also coming into being and while these would raise revenues and give employment and technology transfer benefits to the resource owners, if properly set up, there is the argument that true national ownership of fishing vessel ports, processing facilities, and shipping capabilities to market the fish abroad would eventually assure the island nations the full benefit from “owning” the tuna. Such a development will take time, quite a long time at that, and it will only be possible if all or several of the island nations make the management of their tuna resources a common cause.

Considering finances first, a 1000-t tuna purse seiner required U.S.$7 million to build and $2 million per year to operate in 1980, and longline vessels have replacement values of $1 million and up (Kearney 1981). There is also a lack of infrastructure and trained personnel in the island nations, underscoring the difficulties facing them to attain fuller fisheries independence.

It is the intent of the proposed Law of the Sea Treaty that developing nations should exercise control over the fish resources of their EECZs. The first step thereto might well be to evolve a common negotiating stance for leasing arrangements, and to issue guidelines for forming joint ventures equitable to both partners (Marten et al. 1981).

While the Pacific islands have a vast ocean in common, and in most cases a colonial heritage that tends to be a unifying factor in policymaking, there are others that make it difficult to reach and make operational a common stance towards outsiders with regard to laws and regulations, or “harmonization” as this is called in legal-political parlance.

Not only do distance and poor communications separate them; there are also more substantial ethnic and cultural differences among them than is commonly assumed, and strong economic inequalities also exist. Added to all this may be a laudable behavior trait that they have in common, namely the reaching of agreement in weighty matters by thoroughly talking towards a consensus, often referred to as “the Pacific Way.” It is evident then that there are indeed substantial obstacles to reaching a common negotiating stance with outsiders that are as different from them, in most ways, as are the Japanese, the Koreans, and the Taiwanese, and also the Americans, the nations which do virtually all the fishing in the southwest Pacific.

Fortunately, there is no need for conservation measures at this time, but there are also virtually no enforcement surveillance and control mechanisms. However, the biology of tuna predicates and the Law of the Sea recognizes that “The coastal state and other States whose nationals fish in the region for tuna and other highly migratory species must cooperate directly or through appropriate international organizations to ensure conservation and optimum utilization, both within and beyond EECZs. Where no appropriate international organizations exist, those nations engaged in the fisheries shall cooperate to establish such organizations and participate in their work” (Article 64/1, The Convention on the Law of the Sea, 1982). However, the features of such “appropriate” organizations are unspecified. Yet, at the same time the rights of the coastal State within its EECZ are fully protected, in spite of this specific article, so that full jurisdiction over tuna therein by coastal States is preserved (Article 64/2, The Convention on the Law of the Sea, 1982).

The South Pacific Commission would very likely qualify as “appropriate” the organization specified in the above Convention article for factfinding and recommending conservatory measures. The island nations, however, did not perceive it as the appropriate body to lead them in ventures of common interest. Many of the island nations became dissatisfied with the constraints imposed by the Commission. Delegates felt that it was impossible to discuss political matters freely and that the developed countries dominated the discussion. As a result, the island-countries formed the South Pacific Forum (Forum), which met officially for the first time in August 1971. The current membership consists of Australia, New Zealand, Papua New Guinea, the Solomon Islands, Fiji, the Cook Islands, Kiribati (formerly the Gilbert and neighboring Islands), Western Samoa, Nauru, Niue, Tuvalu, Tonga, and Vanuatu (formerly the New Hebrides). The operating arm, or “Secretariat,” of

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1 In 1947, six of the developed nations with territories in the South Pacific organized the South Pacific Commission (SPC). These countries were: Australia, France, Netherlands, New Zealand, United Kingdom, and the United States. The Netherlands withdrew in 1982 after its territories achieved independence. As island dependencies gained independence, they joined the Commission in their own right.
the Forum is the South Pacific Bureau for Economic Cooperation (SPEC). SPEC's activities to date have focused on developing free trade among its members. In 1979, the South Pacific Forum adopted a convention establishing the South Pacific Forum Fisheries Agency (SPFFA) to coordinate regional fishing concerns.

In addition, seven small Pacific Island nations formed a combine, the so-called Nauru Agreement, signed in February 1982. The reasons for its formation were lack of progress to date of the SPFFA in approaching this goal, as well as realization, from tagging returns and fishing success in various reaches of the western Pacific, that during part of their life cycles, some stocks of Skipjack remain more or less discrete, and albeit only relative, proximity to one another. Participants to this Agreement are the Palau Islands (now the Republic of Belau), the Federated States of Micronesia, the Marshall Islands, Kiribati (in part), Nauru, the Solomon Islands, and Papua New Guinea (Fig. 11). Speculations have been voiced that some rivalries among new island nations may also have been in this compact of the westernmost among these; in any case, the Nauru Agreement is not popular with the remaining Forum members. They fear the Nauru Agreement may have a negative impact on consensus in the larger area and indeed on the Forum itself.

If Pacific island nations in whose waters the tunas occur were eventually to gain full benefits from them, they should work towards developing their own management plan and the required abilities to produce, process, and market the various products which can be derived from the fishery. However, it must also be said that almost all of the parties lack the expertise and finances to put together such a development plan, and that outside technical and financial assistance will have to be extended to them in order for it to be undertaken.

The seven nations feel that not only do they have a better chance than the larger group of South Pacific Forum nations to reach agreement on principles but also that, as a smaller group, they could work better towards operating their fisheries themselves, including the procurement of the necessary finances. That expectation is reinforced by the knowledge that they have both possession and the law on their sides, since the adoption of the LOS convention (P. T. Wilson, Fisheries Department, Port Moresby, Papua New Guinea, personal communication). What is more, time for agreement may well be of the essence as trends point to an increasing share of purse seineing in tuna catching, also in the western Pacific, as likely further development of fish aggregating and attracting devices makes this fishing method even more effective (see earlier, under Biology of Yellowfin and Skipjack).

In 1976, nine Skipjack and tuna purse seiners fished the waters bounded by Kiribati, Nauru, the Solomon Islands, Papua New Guinea, Palau, the Federated States of Micronesia, and the Marshall Islands. These vessels had a combined carrying capacity of less than 4000 t and over the year landed 14 000 t of Skipjack and Yellowfin valued at about U.S.$10 million. By 1981, the fleet had expanded to between 50 and 60 vessels from 11 countries with a combined carrying capacity of about 40 000 t and a current capital value in the order of U.S.$200 million. It is likely that during the course of 1981 these vessels will take in excess of 100 000 t (Franklin 1980).

The increase in seiners and the concomitant phase-out of Japan-based long-liners largely reflect rising fuel costs. This being so it would be of advantage to base seiners as close to the stocks as possible, and fuel cost-influenced developments may lead to new fishing relations in the southwestern Pacific. Korean and Taiwanese seiners may soon fish in joint venture with, or as suppliers of fish to distant water nations, Japan in the main. Islands-based fleets may do so later. Purse seiners have very distinct economics of scale and they catch large tonnages. Sooner rather than later attention to conservation instead of development measures will be needed for the tuna fisheries of the western Pacific. Since the establishment of a conservation oriented management organization for the entire western Pacific with its necessary surveillance tools is likely to be far away, it seems sensible to begin with a smaller and more manageable area, namely that of the Nauru Agreement in spite of the caveats mentioned relating to substock identity and to SP Forum policy. The signatories to it may perhaps set a pattern to be followed by other subgroupings if not the remaining nations in the South Pacific. One must remember here that the rate at which the exploitation of tuna may increase will depend on the world tuna market, its fluctuations, and its absorptive capacity. Especially if that market increases, failure to reach a common stance or lengthy procrastination thereof would reduce opportunities for economic development of the island nations; such inaction could indeed also endanger one of the last, easily accessible, and large underexploited fishery resources.

Concluding Remarks

Limits of fish distribution are recorded by geographic coordinates; fish, of course, do not "respect" these but rather those set by environmental variables. For tuna, they are mainly temperature and the prevalence of large-scale concentrations of food. Isotherms and fronts between current systems shift from year to year, and tuna concentrations will shift with them. Two sample cases in point here are: (1) variability in the location of 35 parts per thousand salinity in the subequatorial southwest Pacific is determined by variations in upwelling; these then result in the shifting of zones of productivity which, in turn, concentrate the tuna (Donguy et al. 1978); and (2) the depth of the thermocline and with it tuna where abouts in the equatorial eastern Pacific are determined by the state of the El Niño-associated variations of the Southern Oscillation (IATTC 1980).

If one were able to delimit tuna distribution based on these oceanographic variables rather than on catch records with regard to latitude and longitude, one would certainly arrive at a better prediction of fish productivity while also explaining year-to-year fluctuations in fishing success in any one location. (G. Seckel, National Marine
Fig. 11. The approximate Seaspaces of Nauru Agreement Nations are bounded by the solid lines.
Fisheries Service, San Diego, CA, personal communication). Unfortunately there are two obstacles to such an intuitively sensible approach: (1) Political boundaries to which controls of the prevalence of vessels are tied and remain fixed and do not shift; and (2) there is still insufficient oceanographic and meteorological information to predict with some certainty the movements of these oceanic parameters.

Still, emphasis should be placed on exploring the extent to which attention to oceanographic variables that determine shifts in the distribution of tuna could effectively be injected into the devising of rules of ocean governance. This would facilitate the development of fisheries where still possible, and the conservation of fish stocks where necessary.

Evolutionary trends towards the establishment of substocks or subpopulations are driven by deriving reproductive and/or feeding advantages from the partitioning of a larger range. Yet in the fluid and shifting environment of the surface layer of the tropical and subtropical oceans, complete separation of these subunits of a species is unlikely, and mixing occurs to varying degrees. It may well be postulated that drawing down one substock by intensive fishing, creating some regional food surplus, would lead to increased migration of members of a neighboring stock that is partially isolated in genetic terms, into the region of the former.

Thus incipient overfishing of the first stock could well be masked for a while and lead, together with shifts in oceanographic parameters mentioned above, to fluctuations in fishing success that are both misleading and difficult to explain. In connection with the high investment which tuna fisheries demand today, such in-migrations may have delayed, and may still delay, the putting into practice of conservation measures for the eastern Pacific stock of Yellowfin.

Increased attention has been advocated to the identification of substocks mainly through physiological and tagging researches. Such emphasis is clearly necessary before one can effectively coordinate oceanwide research on tuna and before one can justify, based on that research, certain management practices that reflect the interaction among the subpopulations of various species of tuna. At present tuna research efforts in the eastern and western Pacific are not planned in a joint fashion nor are Japan-based investigations closely tied to those of the IATTC or the SPC; still, research results are quite freely exchanged. Yet oceanwide phenomena, that affect fish through associated environmental variables, are being investigated by meteorologists and oceanographers; the Ocean Atmosphere Climate Interaction Studies (OACIS) now being planned by the Global Atmospheric Research Program (GARP) prominently comes to mind here. G. Seckel (National Marine Fisheries Service, San Diego, CA, personal communication) has proposed to include certain fisheries investigations in this large internationally planned and executed study so as to fashion a better bridge between fishery-related events in the eastern, the central, and also the western Pacific which is a crucial region for the genesis of the oscillation (Wyrtki 1979).

It is questionable if such cooperation among disciplines can be accomplished. Still, its aim would be to approach a synoptic view of oceanwide events and phenomena that are clearly related to one another and perhaps to indicate if there are management decisions to be taken in the future that transcend the present geographic reaches of various research entities.

Attaining harmonization — the deliberate alignment of the laws of different nations for the purpose of fulfilling their national interests — especially in affecting tuna fisheries in various parts of the Pacific and prominently in dealing with licensing and joint venture arrangements in the western Pacific may be more difficult still. Yet, the nations joined in the IATTC, the SPC, the South Pacific Forum Fisheries Agency and those of the Nauru Agreement are interested in one way or another in facilitating such harmonization. An important question in this regard is whether the framework of political geography encompasses a distinct zoogeographic region. If it does, that is, if substocks in it range only minimally into other geographic and political regions, it makes sense to pursue the harmonization of fisheries laws and regulations of a larger region in a more or less piecemeal fashion. This would also apply to nations that make common cause to accomplish year-round licensing for different fishing seasons North and South of the equator, such as Fiji and Kiribati; one may well doubt, in the latter case, that only one subpopulation is involved in this arrangement.

The question of subpopulation identity now most prominently arises for Skipjack in the area of the Nauru Agreement with regard to the neighboring southeast Asian seas, the seas of island nations to the east, and of Australia and New Zealand to the south. If the fish mix freely and they may well do so over a period of several as opposed to 1 or even 2 yr, one must embark on the more difficult path of bringing together more national entities over a wider area sooner rather than later. Whatever the answers to this particular question and to those raised earlier relating to changes in fishing techniques and fishing bases, it is clear that there needs to be better communication and understanding than now prevails among politicians, economists, and scientists who must together deal with these problems even though they approach them from different vantage points. This paper attempts to make a small beginning towards reaching this goal.

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References


1982. Summary of the state of the Skipjack resources. South Pacific Commission, Noumea, New Caledonia. 4 p. (mimeo)


Multispecies Effects on Recruitment and Community Structure

JOHN R. BEDDINGTON

International Institute for Environment and Development (IIED), 10 Percy Street, London W1P 0DR, England

Abstract

There is a wealth of evidence to show that marine communities alter their species composition as fisheries progress, and the search for explanations of this phenomenon is a central task of fisheries science. In temperate waters the mechanisms operating in this process are difficult to identify as recruitment, which occurs in seasonal bursts, dominates the dynamics of the system. Furthermore, in temperate fish stocks, this recruitment is only loosely tied to the abundance of the adult stock and substantial variations occur in an unpredictable manner. Simple models to predict recruitment must therefore depend upon detailed information on the prerecruit stages. Some possibilities for such models are discussed.

In contrast, the Southern Ocean, which is dominated by krill (Euphausia superba) and its complex of predators, is somewhat easier to investigate. Here the recruitment of the mammalian predators is closely linked to the abundance of the adult stock, and changes may be monitored and investigated. Specific models linking krill and its predators are presented.

For completely different reasons, the complex multispecies communities of the tropics may also be investigated with more ease. Here, although there are a wealth of species, the processes of growth, death, and reproduction occur continuously, and the changes in the community structure are more easily explained. Some models for this process in the Gulf of Thailand are considered.
Fisheries Resources of the Northwest Atlantic — Some Responses to Extreme Fishing Perturbations

BRADFORD E. BROWN

National Marine Fisheries Service, Northeast Fisheries Center, Woods Hole Laboratory, Woods Hole, MA 02543, USA

AND RALPH G. HALLIDAY

Department of Fisheries and Oceans, Fisheries Research Branch, Bedford Institute of Oceanography, Dartmouth, N.S., Canada B2Y 4A2

Fishery resources in the offshore areas from the Gulf of St. Lawrence to Cape Hatteras (Fig. 1, International Commission for the Northwest Atlantic Fisheries (ICNAF)/Northeast Atlantic Fishery Organization (NAFO) Subareas 4, 5, and 6) have experienced significant perturbations due to fishing during the past 25 yr. Prior to 1960, this area was fished almost exclusively by United States and Canadian fishing vessels. These vessels concentrated primarily on groundfish, Atlantic herring,1 and sea scallops, (Placopecten magellanicus), species of high market value to Canada and the United States. Although herring constituted a major fishery, it was predominantly directed towards juveniles that were harvested in inshore areas and packaged as sardines. Distant water fleets from European nations and Japan expanded their fisheries in the 1960s.

The sum total of these fisheries affected the entire offshore finfish plus squid resources including the groundfish, e.g. Atlantic cod, haddock, flounder, etc.; pelagics, e.g. Atlantic mackerel and herring; semipelagics, e.g. silver hake; and Illex and Loligo squid.

Beginning in the early 1970s, ICNAF began to restrict fishing mortality in these waters. Prior to the mid-1970s, a full array of restrictions that did reduce total fishing effort were in place. Beginning in 1977 the effects of enactment of the 200-mi economic zones by Canada and the United States resulted in further reductions, especially drastic in the distant water fleets' fishing effort in the waters south of Nova Scotia. However, after extended jurisdiction, there were increases in fishing effort in various coastal state fisheries.

This paper will review the historical patterns of nominal catches (i.e. those catches reported to the appropriate governmental statistics agencies and in this case recorded in NAFO files in Dartmouth, N.S., Canada) and the abundance trends of the fisheries resources, and will discuss some hypothesis as to the responses of these stocks in the face of extreme perturbations resulting from heavy fishing pressures.

Brief Description of the Fishery Resources

The species composition of the fishery resources of the area from the Gulf of St. Lawrence to Cape Hatteras consists of the following general categories: 1. Demersal fish of shallow water areas (less than 100 m). These species are found in only limited numbers south and west of Georges Bank and then predominantly in the winter (e.g. cod, haddock, yellowtail flounder, and winter flounder). 2. Demersal fish of deeper areas (greater than 100 m) are found most frequently in the Gulf of Maine, along the edges of Georges Bank, and the edges and deeper basins of the Scotian Shelf. These species are found in only limited numbers south and west of Georges Bank and then predominantly in the winter (e.g. redfish, witch flounder, cusk, and argentine). Redfish are also abundant in the Gulf of St. Lawrence. 3. Pelagic and semipelagic species are distributed over both shallow and deeper water areas. The specific area depends on the season (e.g. mackerel, herring, pollock, silver hake, spiny dogfish and Illex squid). The latter four species are scarce in the Gulf of St. Lawrence. 4. There are species that are more southerly in occurrence, winter in the deeper water offshore areas in the Mid-Atlantic, and move inshore in the summertime and north and east onto Georges Bank and the southwestern part of the Gulf of Maine, but only rarely further north (e.g. summer flounder, scup, butterfish, and Loligo squid).

Fisheries management in Canada and the United States has generally accepted stock definitions as follows: mackerel have both a northern and southern stock component which overwinter together in the Mid-Atlantic area; herring have separate spawning stock complexes in the southern Gulf of St. Lawrence, Nova Scotia, Georges Bank, and the Gulf of Maine, but with some intermixing of the latter three outside the spawning season. The southern species components which move onto Georges Bank are generally a single stock within these areas. The shallow and deeper water groundfish are separate stocks in the Scotian Shelf, Georges Bank, Gulf of Maine, and in some cases the southern New England region. Cod and haddock have been considered separate for the first three areas listed above, and yellowtail flounder for all four. The semipelagic silver hake stock has also been considered to be possibly separate in all four areas.

The predominant fish predators in this region are silver hake, cod, dogfish, and bluefish. Although not considered in this discussion, there are also large pelagics such as swordfish, bluefin tuna, and the large pelagic sharks that occupy the highest trophic levels with the ability to feed on quite large fish.

1 Common names of fish are those given in Robins 1980; the term Atlantic before cod, mackerel, and herring is dropped after the first usage.
Historical Patterns of Nominal Catches in Subarea 4

In the late 1950s, nominal catches were about 500,000 t annually, then increased rapidly from 1961 to reach a peak of 1,225,000 t in 1970. This was followed by a steady decline in total annual catch, except for a secondary peak in 1973, to a low of 670,000 t in 1977. Most recently, total catches have gradually increased and, in 1980, were just under 800,000 t (Fig. 2).

Much of the increase in catch in the 1960s was supported by expansion of pelagic fish catches, almost exclusively herring, from about 100,000 t in the late 1950s to almost half a million t in 1969. This was followed by a steady decline to recent levels of about 175,000 t. Groundfish catches also increased in the early 1960s and fluctuated around 500,000 t between 1963 and 1969. This was followed by a second increase to a peak catch of almost 800,000 t in 1973 and then by a precipitous decline back to the 1954–55 level of about 300,000 t. Most recently, there has been a steady increase in groundfish catches, the 1980 level being almost 450,000 t. Shellfish catches have increased substantially in the late 1970s to 100,000–150,000 t from a level of less than 50,000 t prior to 1975. Much of this increase resulted from the development of a squid (Illex) fishery on the Scotian Shelf but also, in part, reflects expansion of queen crab (Chionoecetes opilio) and shrimp (Pandalus) fisheries, mainly in the Gulf of St. Lawrence.

Fig. 2. Nominal catch in Subarea 4, 1954–80.
The fluctuations in groundfish nominal catches were in large part due to developments in the small-mesh trawl fishery for silver hake on the Scotian Shelf. The first peak in silver hake nominal catch in 1963 was followed by fishery collapse, but there has been a sustained fishery since 1969 with peak catches of about 300 000 t reported for 1973 (Fig. 3). Fluctuations in redfish catches have also been significant. After a period of stable catches in the mid-1950s to mid-1960s at about 50 000 t annually, redfish catches increased steadily to 170 000 t in 1973 but rapidly declined to, and remained at, a level of 30 000 t subsequent to 1976. Trends in cod catches are in marked contrast to most other species, showing remarkable stability during the late 1950s and 1960s at slightly over 200 000 t. Subsequent to the peak catch of only 269 000 t in 1970, catches declined steadily to 130 000 t in 1977. By 1980, catches had returned to 230 000 t, the level of the 1960s.

**Trends in Resource Abundance in Subarea 4**

Estimates of resource abundance are available from both commercial fisheries data and from research vessel surveys, but neither provide a comprehensive picture of resource trends. Estimates from fisheries data are limited to those species supporting the largest, most important fisheries and, in particular, cod, haddock, and herring. Standardized research vessel bottom trawl surveys which gave comprehensive coverage were initiated for the Scotian Shelf (Div. 4VWX) in 1970 and for the southern Gulf of St. Lawrence (Div. 4T) in 1971. Their usefulness in determining resource trends varies with species (Halliday and Koeller 1981). Survey results indicate that the biomass of all finfish and squid in Div. 4T has more than doubled between the early 1970s and the early 1980s (Table 1). In contrast, there is no trend in the estimate of total biomass on the Scotian Shelf (Table 2). There are, however, identifiable trends in the components of the biomass.

**Pelagic Species**

Although substantial stocks of mackerel and capelin occur in Subarea 4, the fishery for pelagic species has been directed very largely towards herring. There are two major herring stock complexes in the area centered in the southern Gulf of St. Lawrence (Div. 4T) and in the Bay of Fundy (Div. 4WX). Based on commercial fisheries data, the biomass of Div. 4T herring has been estimated to have declined from over 1 million t in 1969 to something less than 100 000 t by 1980 (CAFSAC 1982a). Biomass of Div. 4WX herring is estimated to have peaked in 1967–68 at about 650 000 t (age 1+) and again in 1973–74 at about 500 000 t, but declined to a low of about 300 000 t in 1977–78. There was another substantial increase in 1979–80, however (Sinclair and Iles 1981).

Catchability of pelagic species to the research vessel bottom trawl surveys which have been conducted is low.

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**Table 1. Minimum biomass estimates from Canadian research vessel surveys — Div. 4T (t × 10^{-3}).**

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### Table 2. Minimum biomass estimates from Canadian research vessel surveys — Div. 4VWX (t × 10−3).

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and the results are subject to substantial annual variability (Tables 1, 2). Survey biomass estimates for pelagic species in Div. 4T (mainly herring) show a substantial decline from the early to late 1970s of almost an order of magnitude; this is consistent with the decline in herring estimated from commercial data. In the Scotian Shelf survey, moderate quantities of herring were caught prior to 1974. From 1974 on, very few herring were caught and, although catches of other pelagics, particularly mackerel, increased, total pelagic catches also tended to be lower. Thus, survey estimates of herring abundance on the Scotian Shelf do not correspond with those calculated for the Div. 4WX stock from commercial data. The latter, however, must be considered by far the more reliable.

Estimates of mackerel stock size are not available for Subarea 4 alone, stock assessments being conducted for all northwest Atlantic stocks combined. It is likely, however, that mackerel abundance on Subarea 4 follows fairly closely trends in abundance estimated for the total area. Overall mackerel biomass in the northwest Atlantic was low in the 1960s, increasing to a peak of about 2.5 million t (age 2+) in 1970–71 and declining thereafter to about 600 000 t by 1979 (Maguire 1981a).

Capelin are restricted to the northern part of the area, the main concentrations occurring in Div. 4RS and, to a lesser extent, in Div. 4T. No reliable estimates of trends in abundance are available.

**Groundfish Species**

The broad trends in cod and haddock stock abundance in Subarea 4 (estimated by sequential population analysis based on commercial fisheries data) have been similar. The Scotian Shelf (Div. 4VWX) and southern Gulf of St. Lawrence cod stocks were large in the early 1960s, declining substantially in abundance until the mid-1970s and, most recently increasing rapidly to levels as high or higher than those of the early 1960s (Fig. 4). The other major cod stock in the area, the Div. 3Pn-4RS stock in the northern Gulf of St. Lawrence, showed similar trends (Gavaris and Bishop 1981). The trough in haddock abundance occurred in 1972 and was, therefore, slightly sooner than those in the cod stocks (Fig. 4). Research vessel survey abundance estimates for these stocks agree well with those from commercial fisheries data (Scott et al. 1982).

In the southern Gulf of St. Lawrence, surveys estimate that American plaice abundance more than doubled in the early 1970s and maintained this higher level (Table 1). There is also an indication that white hake abundance increased in the late 1970s. There are no readily discernible trends in biomass of the remaining groundfish species.

On the Scotian Shelf, surveys suggest that there was some increase in flatfish biomass in the early 1970s, but this does not compare with the increase in biomass of American plaice in Div. 4T in the same period (Table 2).

![Fig. 4. Temporal trends in the beginning of year biomass of cod and haddock on the Scotian Shelf and cod within the Southern Gulf of St. Lawrence (Scott et al. 1982).](image-url)
There is also an indication that pollock abundance was greater in the latter half of the period (mean estimate 1976–81 is 2.6 times that for 1970–75). On the other hand, there appears to have been a substantial decrease in redfish abundance in most recent years. There are no readily discernible trends in biomass of the remaining groundfish species.

**Invertebrate Species**

Research vessel survey results indicate that the development of the offshore squid (*Illex*) fishery on the Scotian Shelf in the mid-1970s corresponded with the beginning of a period of high squid abundance (Table 2). Prior to 1975, squid catches in Div. 4T surveys were rare, but squid have since occurred there each year in significant quantities (Table 1). Expanding shrimp catches in the Gulf of St. Lawrence have been accompanied by increasing commercial vessel catch rates, suggesting that abundance has also been increasing (CALSAC 1982b). Although there are no reliable commercial or research catch rate data series available to indicate abundance trends in the main snow crab fishery in the southern Gulf of St. Lawrence, there is reason to attribute recent increases in catch, at least in part, to an increase in stock abundance (Bailey 1981).

**Discussion of Subarea 4**

The expansion of the pelagic, mainly herring, fisheries in Subarea 4 in the late 1960s corresponded to the passage of large year-classes of both herring and mackerel through the fishery. Catches could not be sustained and fell, as resource abundance fell, to low levels by 1979–80. The expansion of shellfish fisheries in the latter part of the 1970s seems to have been based on an increase in the productivity of the resource itself.

Trends in the groundfish resource are more complex. In the silver hake fishery, which has been of major importance on the Scotian Shelf, catches have fluctuated closely with abundance (Waldron and Harris 1982). The decline in cod and haddock resources can be ascribed to apparent recruitment failure in the case of haddock and depressed recruitment levels in the case of cod (e.g. Maguire 1981b; O'Boyle 1981; Sinclair and Maguire 1981; White et al. 1981). The decline occurred at moderate to high exploitation rates. Recovery since the mid-1970s has resulted from high recruitment levels during a period of moderate to low exploitation rates following imposition of fishery controls in the early to mid-1970s.

The recovery of cod and haddock stocks to historical abundance levels is a major step in the recovery of the groundfish community species composition to historical configurations. There are some indications, however, that pelagic species abundance may be at, or close to, historically low levels, and invertebrate species may be in a period of above average abundance.

**Historical Patterns of Nominal Catch in Subareas 5 and 6**

In the late 1950s, nominal total catches were about 400,000 t annually. These increased in the 1960s and reached a peak of 1.2 million t in 1972. This was followed by a decrease in the total annual catch to 360,000 t in 1978 after which catches have begun to increase. In 1980, catches reached 420,000 t (Fig. 5).

In the early 1960s, groundfish catches increased rapidly reaching a peak of almost 650,000 t in 1965 (Fig. 6). They then declined quite rapidly reaching 120,000 t in 1970, but recovered somewhat to approximately 275,000 t in 1973. Catch increased again to approximately 290,000 t in 1980. Principal groundfish include cod, haddock, silver hake, red hake, and pollock. The initial high levels of groundfish catch were supported by haddock, silver hake, and red hake, and slightly later on with more moderate increases in cod.

The increase in the early 1970s was due to silver hake. Recoveries in the later period have been cod, haddock, and pollock catches with the silver hake and red hake fisheries being considerably smaller at the present time with the restrictions on the distant water fleet and limited utilization of these species by the United States and Canada. Redfish, which had high catches in the 1950s, declined throughout this period to present low levels.

Landings of flounders increased in the 1960s to a peak of over 80,000 t in 1969 (Fig. 7). They decreased somewhat until 1977 (45,000 t). Catches of these species have increased in the latter part of the 1970s.

Other groundfish such as angler, cusk, and ocean pout, fluctuated widely throughout this period (Fig. 7). These fluctuations may reflect to a certain degree reporting problems, particularly prior to 1970, as these species are often caught in smaller numbers (i.e. as bycatch) in the principal groundfish fisheries. This group decreased in the late 1960s and early 1970s, and then stabilized at 20,000 t, considerably below that existing in the 1950s. Other fish and pelagics such as scup, spiny dogfish, skates, and butterfish were initially at relatively low levels of catch (approximately 30,000 t) in the 1950s and early 1960s, followed by an increase in 1969 to 123,000 t (Fig. 7). After this a significant decline took place, catches stabilizing at about 50,000 t since 1977.

Landings of principal pelagics, i.e. herring and mackerel, had pre-1960 catches averaging about 70,000 t.
trawl surveys give comprehensive coverage over the Georges Bank, Gulf of Maine, and Mid-Atlantic areas. Since autumn 1963 and spring 1967, they have been conducted by U.S. research vessels. Prior to 1968, they went only as far south as Hudson Canyon; since then they have been conducted south to the Cape Hatteras area. These regular autumn and spring research vessel surveys have been supplemented occasionally by U.S. vessels surveying in summer and winter. Regular inshore surveys have also been conducted by the state of Massachusetts. In addition, research vessels from the Soviet Union, Poland, the Federal Republic of Germany, the German Democratic Republic, and France have cooperatively surveyed the area at various times (Azarovitz 1981). Catch per tow indices are given in Table 3.

Survey results indicate that the offshore finfish and squid biomass in this entire area decreased by 65% in the 1964–75 period (Fig. 8) (Clark and Brown 1977). The trend then reversed itself as resources began to recover (Clark and Brown 1979) and in the 1980–81 period, biomass had increased about 50% of the way towards that existing in the 1960s (Resource Assessment Division 1982).

Discussion of Subareas 5 and 6

In examining trends in fishery resources over time, it is important to consider three interrelated factors that drive fishery systems: the physical environment, fishing, and biological interactions. The predominant factor in many fisheries systems is the driving force of the relative strengths of recruiting year-classes (Hennemuth et al. 1980). It is generally considered that it is in the survival of early life history stages, i.e. eggs and larvae, that the physical factors are most critical. Obviously the number of initial spawning products is determined by spawning stock size. The relative abundances of predators and food, independent of the interactions of physical environment, are also important. The effects of the physical environment, except for long-term climatic trends, tend to average out over time as resources have evolved to exist in the environment in which they are found. This likewise is true in terms of the interactions with the

Fig. 6. Nominal catch of principal groundfish and principal pelagics in ICNAF/NAFO Subareas 5 and 6, 1954–80.

Fig. 7. Landings of flounders, other finfish, and squids in ICNAF/NAFO Subareas 5 and 6, 1954–80.

(Fig. 6). Pelagics then increased greatly to a total of 675 000 t in 1971. The initial increase was predominantly herring, followed by mackerel. Landings maintained themselves at high levels through 1973, and then began to drop considerably; they reached the lowest level in 1978, 53 000 t, increasing moderately to 86 000 t in 1980.

Prior to 1970, squid fisheries were virtually nonexistent in this area (Fig. 7). Then they increased very rapidly, reaching almost 60 000 t in 1973, and maintained themselves at a high level to the mid-1970s. Catches then decreased as a result of extended jurisdiction restrictions; the total catch of 1980 was 35 000 t. This fishery initially was directed to Loligo squid, and later to both Loligo and Illex squid.

Trends in Resource Abundance Subareas 5 and 6

Estimates of resource abundance are available from commercial and recreational fisheries data, and from research vessel surveys. Taken together, they provide a comprehensive picture of resource trends. Estimates from fisheries data are limited to those species supporting the largest and most important fisheries such as mackerel, herring, haddock, silver hake, cod, and yellowtail flounder. Standardized research vessel bottom
Table 3. Stratified mean catch per tow (kg) for selected species of finfish and squid, NEFC autumn bottom trawl survey data, 1967-1980, Mid-Atlantic, southern New England, Georges Bank, and Gulf of Maine (Strata 61-76, 1-30, and 36-40).

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<sup>a</sup> Less than 0.05.
<sup>b</sup> Does not include data for tunas, sharks, swordfish, American eel, or white perch.
predator/prey biological components in the ecosystem. Thus, to a considerable extent, the variations in recruitment can be dealt with as a statistical distribution problem (Hennemuth et al. 1980).

The entrance of humans, however, as an extremely heavy predator, introduces a factor outside of those with which the species has evolved to coexist. To some extent fishing can be considered another form of predation to which these resources have adapted. This is certainly logical when fishing effort is moderate and when the market system causes the fishing effort to respond to the densities of the prey. However, this area of the northwest Atlantic was subjected to extremely heavy fishing pressure that not only did not fluctuate with abundance of prey but to some extent increased as prey decreased. The overall effects of this extensive predation have been noted by Brown et al. (1976), and Clark and Brown (1977, 1979) (Fig. 8).

The effects of fishing can be twofold. First, there is the direct reduction of an individual resource, thus reducing its presence in the ecosystem for a considerable period of time. The length of the time the resource is depressed in abundance obviously depends to a certain degree on the presence and shape of any stock recruitment relationship and the chance interactions with environmental conditions that would produce strong year-classes despite small spawning stocks. This kind of effect would, however, be expected to be entirely reversible over a fairly short time period. Second, beyond the effect on individual stocks directly there is the potential disruption of normal competitive and predator/prey interrelationships which could alter the structure of the ecosystems and result in a change in species composition and dominance. Changes of this sort might be expected to be of a longer-term, and possibly semipermanent nature.

Species Effects

During the past 25 yr, the fisheries in Subareas 5 and 6 responded to the occurrence of strong year-classes. The 1960 and 1961 year-classes of herring established the abundance upon which the offshore fishery developed (Anthony and Waring 1980a, b). The very large 1963 year-class of haddock drew into the fishery additional U.S. efforts and new USSR and Canadian effort (Clark et al. 1982). Given the mixed species nature of bottom trawl fisheries, this resulted in increased mortality on cod and other groundfish species. The distant water fleets next turned to silver hake which was of high abundance as a result of strong year-classes entering the population in the early 1960s (the exact years differ between stocks). The silver hake fishery used small mesh nets fished on or close to bottom. These were capable of catching year-old groundfish (the traditional U.S. and Canadian trawl fisheries used mesh that was most effective for 2.5 yr and older groundfish and flounder) and as a result contributed significantly to mortalities of young fish for a wide range of species. After silver hake, the next major resource subjected to greatly increased fishing pressure was mackerel, as the large 1966–67 year-classes entered the population attracting distant water fleets.

The direct effect of fishing on these resources is relatively easily measured. The large year-classes that attracted these increases in the fisheries would have sustained populations at considerable larger size for much longer periods of time had removals been kept at modest levels. In fact, 6–8 yr later, in many of these fisheries, the much lower harvest was still sustained primarily on the remnants of these large year-classes. There is no question that if fisheries managers desired a more stable fishery resource base, a moderate fishing level would have achieved that goal for the above resources.

The next question worth addressing is whether the extremely heavy fishing pressure, with its depression of spawning stock size, had any effect on the length of time between strong year-classes and thus whether the very heavy removals resulted not only in a direct trade-off between harvest now or over a longer period, but actually affected basic productivity. Evidence from the short 25-yr time span of observations is inconclusive. Furthermore, attempts to derive analytically stock recruitment relationships for these stocks during this period have not resulted in the statistically based relationships desired. Nevertheless one can examine the situation with regard to a number of these stocks and determine whether effects of lower spawning stock size can be implied.

Herring have different patterns of population trends (Fig. 9, 10) for the Gulf of Maine and Georges Bank stocks (Anthony and Waring 1980b). Both stocks increased in the 1960s with the entry of the strong 1961 and 1962 year-classes. After peaking in 1968, the stocks dropped precipitously to levels lower than those existing in the early 1960s. During the period of abundant spawning stocks, entering year-classes were not of exceptional size. The 1970 year-class, however, was quite large and was spawned by stocks of modest size, but by 1972, spawning stocks were below those existing in the early 1960s. On Georges Bank, the 1970 year-class raised the spawning stock up to the levels existing in the early 1960s for only 1 yr, 1974, and then spawning stock decreased rapidly to become virtually nonexistent by 1978. No other year-class of any strength has come into the Georges Bank stock since it reached these extremely low levels. In the Gulf of Maine, however, spawning stock size rebuilt in 1974 and although subsequently reduced by fishing did not reach extremely low levels. Strong year-classes were produced in 1976, 1977, and 1979. The present situation is that the herring stock in the Gulf of Maine, where the spawning stock was rebuilt, is in a robust condition while that on Georges Bank, where the spawning stock was fished to very low levels, has completely collapsed.

Haddock produced poor year-classes in 1964–65 while spawning stock was still of reasonable size compared to previous levels (Clark et al. 1982). The extent that the presence of the extremely large (much larger than observed in the previous 30 yr) 1963 year-class dampened the following year-classes is not known. There has been no repetition of such a large year-class with the total population being abundant. After 1966,
the stocks reached extremely low levels (Fig. 11) and a strong year-class did not occur until 1975. The 1976 and 1977 year-classes, also from low spawning stock size, were poor, while the 1978 year-class was strong having been the product of the increased spawning stock resulting from recruitment to it of the 1975 year-class. By 1975, it had been possible to build up a spawning stock from the very lowest levels as a result of ICNAF restrictions on fishing mortality. Such restrictions also protected this year-class from heavy bycatch as young fish. The evidence is certainly circumstantial and supported by limited data, but there is an indication that the frequency with which strong haddock year-classes appear is decreased when the stock size is extremely low.

The silver hake situation is less dramatic. Three separate management units, considered to correspond to stocks, were used during the period of international management: southern New England–Mid-Atlantic, Georges Bank, and Gulf of Maine (Anderson et al. 1980). All three stocks were at high levels in the early 1960s with the Gulf of Maine being at its highest early in that period (Fig. 12), Georges Bank by 1964 (Fig. 13), and southern New England–Mid-Atlantic by 1965 (Fig. 14). Fishing patterns differed in the Gulf of Maine from those in the offshore area. The Gulf of Maine had a moderate level of fishing from the U.S. inshore fleet for a long period of time. Landings were maintained at a fairly constant level through 1965 and then underwent a gradual decrease to a very minimal fishery at the present time. Stock biomass decreased into the early 1970s and then increased slightly, and has maintained itself at a much lower level than existed earlier. There have been no strong year-classes either during the period of relatively high stock biomass in the middle 1960s or during the period of low spawning stock that has existed since then. On Georges Bank and southern New England–Mid-Atlantic areas, fisheries increased greatly and peaked at total catches of almost 150,000 t in the Southern New England–Mid-Atlantic areas and 250,000 t on Georges Bank. As a result of these catches stock biomass decreased rapidly. The fishery diverted to other areas and even at the lower spawning stock biomass,
incoming above average year-classes did enter the fishery in the early 1970s, causing a slight build up of the stocks which was paralleled with an increase in landings. Since then, stocks have again decreased to an extremely low level on Georges Bank but to a lesser extent in the southern New England–Mid-Atlantic stock. The southern New England–Mid-Atlantic stock is producing modest size year-classes at a spawning stock that is approximately one-quarter of the average of the peak period. Georges Bank stock, however, has produced very small year-classes from spawning stocks now only 1/10th those of the early period. Again the evidence is circumstantial with some indication that there has been a decreased probability of good year-classes when the spawning stock has reached extremely low levels, as on Georges Bank.

Mackerel is the other large stock that underwent wide fluctuations in this period (Fig. 15). Mackerel stocks increased greatly in size with the entry of the 1966 and 1967 year-classes (Anderson and Pociorkowski 1980). This caused the stock size to increase fivefold, and increased catches immediately followed. ICNAF restrictive regulations were instituted that halted the inevitable stock decline as it reached the approximate level that it was in the early 1960s. Even more severe restrictions were imposed after extended jurisdiction. The stock has begun to rebuild with entry of a moderate size 1978 year-class and has maintained itself at about the level that triggered the earlier population increase. It should be recognized, however, that the mackerel population has only occasionally been observed to have large increases in stock size, i.e. in the periods 1835–40, 1880–84 and in the early 1920s (Sette and Needler 1934) and it may well be a relatively long period of time before exceptional year-classes occur again.

**Ecosystem Effects**

Theoretical efforts have been devoted to hypothesizing that in marine systems, predator/prey and competitor interactions have significant effects in population regulation and that heavy removals by fishing may upset this balance and result in energy being transferred from one segment of the ecosystem to another (Ursin 1982;
Sissenwine et al. (1982) examined predator/prey and competitive relationships on a quantitative basis using fisheries and survey data for Subareas 5 and 6. They found no statistical evidence for such interactions. Foster (1982) applied Pope's (1979) multispecies cohort analysis to stocks on Georges Bank utilizing silver hake and cod as predators with mackerel and herring being the predominant prey species (in the case of silver hake, cannibalism makes silver hake an important prey as well as predator). Only minimal predatory effects were indicated and these were mainly increased estimates of year-class size as 1-yr-olds. There were some shifts in relative strengths of year-classes as a result but no evidence of a dramatic effect on changes in year-class strength at age of entry to fisheries due to predation. Although Foster's (1982) is a preliminary study with limited input data and further refinements may illustrate more impact of predator/prey relationships, such statistically correlated information is not available at the present time.

One can, however, look at population changes and speculate whether or not competitive or predator/prey interactions took place. Sherman et al. 1981 (Fig. 16) examined the rapid increase in sand lance larvae combined with the decreases in mackerel and herring and hypothesized that replacement might be taking place. While the overall data are suggestive, it is difficult under this hypothesis to account for large quantities of sand lace in locations such as the Mid-Atlantic where mackerel and herring do not concentrate. It is possible that environmental conditions which allowed a large sand lance year-class to survive were helped along in some areas by the relatively low abundance of mackerel and herring.

Illex and Loligo squid increased greatly in population abundance (Fig. 17, 18) (Lange and Sissenwine 1980, 1982) at the same time as the mackerel and herring, and after many of the groundfish stocks, declined. This shift made the overall production in the ecosystem more stable than that of the individual species components.

Predatory fish also underwent major shifts in abundance during this 25-yr period. Silver hake decreased greatly in abundance from being the dominant fish predator in the early part to a much lesser role in the latter years. Cod was more stable in abundance but did decrease to some extent during the middle period recovering only towards the end. Dogfish (primarily spiny dogfish) underwent a major increase in the mid-1960s to mid-1970s period (Fig. 19) as did bluefish (Fig. 20) (Resource Assessment Division 1982). Dogfish and bluefish, like larger cod but unlike silver hake, can feed on larger prey (at least larger juveniles) easily. However, bluefish and cod were found to be preying very heavily on sand lance during the latter part of this period in samples observed by staff of the Northeast Fisheries Center. As with the prey species the predatory fish seemed to maintain a more stable biomass as a whole than did the individual species.

In examining the extent of shifts in the resource structure it is useful to examine separately the basic

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**Fig. 16.** Decline in herring and mackerel biomass in the area Cape Hatteras to Gulf of Maine (Sherman 1981) and successive increase in ammodyte larvae.

**Fig. 17.** Abundance index and nominal catch of long-finned squid in ICNAF/NAFO Subareas 5 and 6.

**Fig. 18.** Minimum biomass estimates from survey cruise data and nominal catch of short-finned squid in ICNAF/NAFO Subareas 5 and 6.
groundfish, flounder, the semipelagic, and pelagic components. All groups were subject to very heavy fishing and were depressed in population size. Despite this, the present groundfish and flounder components look very much like they did at the beginning of the period. Cod and haddock are still the dominant groundfish species on Georges Bank and in the Gulf of Maine. Yellowtail flounder and winter flounder are still the predominant flounder component on Georges Bank and in southern New England, summer flounder in the Mid-Atlantic, and American plaice and witch flounder in the Gulf of Maine. Examination of the species communities would indicate the same form as prior to the heavy fishing although all of these species were fished quite heavily. Haddock had the greatest fluctuation in population size followed by yellowtail flounder and then cod to a lesser extent. Nevertheless this did not result in a shift in species dominance or in the explosion of other previously less abundant species. A minor shift is indicated in that cod is relatively more abundant than haddock now as opposed to the early period.

In the semipelagic component, pollock showed a moderate decline and a gradual increase in numbers to somewhat higher levels than had existed earlier. Silver hake on the other hand maintains itself now at consider-ably lower levels than in the early 1960s and has not recovered to the same type of dominance.

Pelagics show the greatest effect, with mackerel and herring being far below peak abundance levels. Mackerel are perhaps within normal historic bounds but herring are now virtually nonexistent on Georges Bank.

**General Discussion**

There are broad similarities in the faunal composition and in the history of fisheries development throughout the southern half of the NAFO Convention Area (Subareas 4, 5, and 6) considered here. This review suggests that there have also been generally similar responses in resource productivity to changes in fishing pressures.

The area is characterized by a high diversity of commercially important species which have supported intensive, diverse, and rapidly changing fisheries. There are insufficient data and analyses available at this stage to make conclusive statements on responses of this biological system to perturbations due to fishing. The dynamics of the major species populations can be reconstructed in any detail only for the last 25 yr or so. We are, however, witnessing the first results of a fairly unique circumstance in fisheries management where the intensity and relative distribution of fishing pressure were drastically altered by management intervention, made possible by ICNAF regulations and coastal state extensions of fisheries jurisdiction. For this reason alone, this review may be of some interest, and the early indications of ecosystem responses could prove of value in fisheries management planning.

The heavy fishing pressure that built up during the 1960s and early 1970s resulted in substantial reductions in stock size of the species subject to directed fishing. Much greater stability in stock sizes and yields could have been obtained by control of exploitation rates at moderate levels. Examination of recruitment patterns in four species that have undergone very wide fluctuations in spawning stock size, herring, mackerel, haddock, and silverside, does not provide general evidence that the probability of good recruitment was reduced at low stock sizes. Reduction of Georges Bank herring spawning stock size to a very low level was, however, followed by a very low recruitment. At the very low spawning stock sizes for Georges Bank haddock, only one strong year-class occurred in over a decade, whereas when spawning stock sizes were larger strong year-classes occurred regularly.

Release from extreme fishing pressures has resulted in a rapid recovery of the major groundfish stocks, particularly cod and haddock, to abundance levels comparable to the early 1960s, and the groundfish community has a species composition distribution and abundance not greatly different from that before the rapid expansion of fishing. The expansion of offshore invertebrate fisheries in the 1970s was apparently supported by an increase in abundance of these resources. This increase was coincident with the decrease in important components of the groundfish and pelagic fish communities. In one case (queen crab) there is statistical and
empirical evidence that the increase resulted from reduced (cod) predation (Bailey 1981). To the extent that this proves to be generally true, a reversal in the success of offshore invertebrate fisheries with recovery of fishfin components of the ecosystem can be anticipated.

The pelagic component of the ecosystem was also greatly reduced in abundance by heavy fishing in the 1960s and 1970s but has not recovered subsequent to extension of jurisdiction. The long-term history of mackerel fisheries in the northwestern Atlantic suggests that periods of very high abundance, such as that which occurred in the early 1970s, may be temporally widely spaced events. Thus, rapid increase in abundance in the time frame since the reduction of fishing pressure can perhaps hardly be expected. In the case of herring, distant-water fisheries concentrated on the Georges Bank stock to a large extent, and stock collapse had occurred by the time of extension of jurisdiction. Subsequent complete cessation of fishing has not, as yet, led to any indications of recovery. At the time of extension of jurisdiction, other herring stocks were exploited solely and fully by the coastal states and hence did not experience any significant change in management approach (i.e., reduction in exploitation rate) as a result of this event. Continuation of a long-term trend of poor recruitment to Gulf of St. Lawrence stocks combined with moderate to high exploitation rates has resulted in a substantial decline in this once very large stock complex. Although Gulf of Maine–Bay of Fundy stocks are maintaining fairly high productivity, the overall abundance of herring in the northwest Atlantic is at its lowest in the period that can be documented. It has been suggested (Sherman et al. 1981), on the basis of an observed increase in abundance in the southern part of the region considered here (Sherman et al. 1981, Northeast Fisheries Center, Woods Hole, MA, unpublished data), that sand lance may be replacing herring and mackerel. The argument is not completely convincing, however, as the documented increase in sand lance abundance has occurred in the Georges Bank–southern New England and Mid-Atlantic Bight regions whereas distribution of the feeding phases of mackerel are extensive in more northern areas. Furthermore, herring are at very low levels in the Gulf of St. Lawrence, but an increase in sand lance abundance has not been observed. The trends in pelagic fish abundance can be satisfactorily explained by natural trends in recruitment compounded by the direct effects of fishing and, in the case of Georges Bank herring, by the effects of low stock size on recruitment, without invoking the concept of ecological replacement.

Our speculations leave us with the conclusion that there is no clear evidence that fishing pressures have changed the basic productivity patterns of the system. First indications are, at least for the groundfish community, that release from two decades of extreme fishing pressures results in a tendency for fairly rapid return to its previous state.

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References


New Ideas on the Fluctuations of the Clupeoid Stocks off California

REUBEN LASKER

National Oceanic and Atmospheric Administration, National Marine Fisheries Service, Southwest Fisheries Center, P.O. Box 271, La Jolla, CA 92038, USA

AND ALEC MACCALL

California Department of Fish and Game, c/o Southwest Fisheries Center, P.O. Box 271, La Jolla, CA 92038, USA

Fisheries scientists have been intrigued with the boom and bust phenomenon that characterizes many fisheries but is particularly exemplified by the clupeoid fisheries, i.e. sardine, anchovy, menhaden, herring. Some of the clupeoids achieve huge populations with biomasses in the millions of tons. Exploitation by coastal nations is for human and animal food, the latter by a reduction of fish to meal which is usually included in diets of poultry and cattle to provide essential amino acids for protein metabolism and growth. Many nations have suffered the economic and social distress of industrial failure due to the collapse of a mainstay clupeoid fishery. Today the clupeoids make up about one-third of the world's fish catch. However, between 1968 and 1971 the catch of the Peruvian anchoveta averaged 10 million t per year and alone contributed 15% of the world fish catch. Figure 1 illustrates the precipitous decline of the Peruvian anchoveta (Engraulis ringens) catch and some of its related consequences, for example the rise in the cost of fish meal on the world's markets and the substitution of soybean (another protein source) for fish meal.

California coastal fisheries in the late 1930s and through the 1940s rode the economic boom of a thriving sardine (Sardinops sagax) fishery. Catches of this species for all California ports totalled as much as 658 000 t in the 1936-37 season but the usual annual catch through the mid-1940s was 400 000–500 000 t (Marr 1960). By 1952, the sardine catch had fallen disastrously and, except for a small resurgence in 1958, the sardine has "disappeared" from California waters (Fig. 2). That this collapse was unremarkable and more or less typical of

![Graph showing the Peruvian anchoveta catch from 1957 to 1977 and associated dollar prices for fish meal and soy.](image1)

Fig. 1. The Peruvian anchoveta catch from 1957 to 1977 is shown with concomitant dollar prices for fish meal and soy on the world market (after Barber et al. 1980).

1 Authorship is arranged alphabetically.

![Graph showing North American Pacific coast sardine landings.](image2)

Fig. 2. North American Pacific coast sardine landings. The dashed line in the San Francisco panel indicates landings made at high seas plants (after Marr 1960).
what we have come to expect from clupeoid fisheries today, can be seen by comparing similar catch records from other clupeoid fisheries. As one example, 1936 was also a peak year for the catch of the Japanese sardine, *Sardinops melanosticta*. Its disappearance was almost complete by 1945 with a small resurgence in 1951-53 (Fig. 3). Similarly, Hempel (1978) has shown that the North Sea herring (*Clupea harengus*) has seen, if not a disappearance of the fishery, a huge decline in catch from a high of 1.2 million t in 1965 to about 200 000 t in 1975 (Fig. 4). Other North Atlantic clupeoid fisheries have shown similar rapid declines (see Schumacher 1980), as has the pilchard (*Sardinops ocellata*) off Namibia, Africa (Troadec et al. 1980).

While collapses are common, so too are recoveries. Most notable in recent years have been the increased fisheries on the Japanese sardine, *S. melanosticta*, (Kondo 1980), and the Peruvian–Chilean sardine, *Sardinops sagax* (O. Rojas, Instituto de Fomento Pesquero, Santiago, Chile, personal communication). At its peak in 1936, the catch of the Japanese sardine was slightly over 1.6 million t. After its decline, very few sardines were caught from 1960 through 1972. Figure 3 illustrates the dramatic comeback made by this species, with the 1981-82 catch at about 2.5 million t. Kondo (1980) has said that this resurgence is attributable to the outstanding success of the 1972 year-class. Lea documented a similar outstanding year-class (1904) of herring (*Clupea harengus*) which sustained the North Sea herring fishery for many years (cited by Hardy 1959).

The loss of the Peruvian (and Chilean) anchoveta fishery is being offset, albeit not completely, by an increase in the catch of the Peruvian–Chilean sardine (Fig. 5). A comparison of Fig. 1 and 5 shows that the anchoveta declined in both Peruvian and Chilean waters simultaneously, coincident with a widespread El Niño condition, an unusual warming of the anchoveta habitat. Similarly the sardine catch has increased explosively in both countries. This is not to suggest that species from other fish families do not have similar dramatic ups and downs. Haddock (*Melanogrammus aeglefinus*) for example, has shown a remarkable increase in its North Sea population during a simultaneous collapse in the herring population (Hempel 1978).

While fishing is an obvious source of mortality, scientists who have studied the fluctuation of fish populations have not yet come to any definitive conclusion as to what degree fishing is the source of any clupeoid

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[Fig. 3] Catch of the Japanese sardine (after Kondo 1980; and K. Kondo, Tokai Regional Fisheries Research Laboratory, Tokyo, Japan, personal communication).

[Fig. 4] Catch of the North Sea herring (after Schumacher 1980).

[Fig. 5] The 1970–80 Chile catch of the Peruvian–Chilean anchovy and sardine (O. Rojas, Instituto de Fomento Pesquero, Santiago, Chile, personal communication).
collapse. In an interesting paper, Clark and Marr (1955) argued for opposing viewpoints regarding the loss of the Pacific sardine. Clark believed that each Pacific sardine year-class was related to stock size and was density dependent. Marr, on the other hand, argued that poor year-classes were due to adverse environmental factors (chiefly temperature affecting spawning time) and caused the sardine's disappearance (density independence); however, he added that at the smallest stock sizes year-class size is a function of spawning stock size. Logically, some form of density independence must be operative to account for fish population outbursts, but neither Clark nor Marr commented on what might induce a successful year-class at low stock levels.

In an elegant paleoecological investigation, Soutar and Isaacs (1969, 1974) sought to reconstruct a history of the fluctuations in some of the common fish populations off California by identification of fish scales in undisturbed sedimentary cores from local anaerobic basins, such as the Santa Barbara Basin near Los Angeles. Figures 6 and 7 show nearly 2000 yr of apparent biomass fluctuations for Pacific sardine and northern anchovy (*Engraulis mordax*). Figure 8 shows that extreme biomass fluctuations, both precipitous declines and rapid increases, are common where no fisheries have ever been present.

A few years after the Pacific sardine collapse, the northern anchovy *Engraulis mordax* began to increase in California and Baja California waters. By 1960, the sardine was virtually gone, but egg and larval surveys indicated that the anchovy had become the abundant clupeoid in California waters. By 1965, the anchovy spawning biomass exceeded 4.5 million t. Except for a brief period in the early 1950s, there was no substantial fishery for this species until 1966. A substantial decline in the population occurred between 1975 and 1978. Although a small fishery (ca. 200,000 t) was established during those years, only a tiny fraction of the decline from 3.5 million t to 1 million t could be accounted for by fishing. Studies over the past decade on the northern anchovy off California and Baja California have allowed the development of some generalizations which may be applicable to the fluctuation of clupeoid populations in general.

**Ecological Requirements for Larval Survival**

Hjort's (1913) original suggestion pinpointing the early first-feeding larva as the most vulnerable stage in the life history of fishes is still the basis for many interesting hypotheses. He believed that the presence or lack of food at larval first-feeding decided the size of the year-class in annually spawning herring. This idea has been resurrected from time to time in the literature, and has received experimental support from a number of investigations where the laboratory food requirements of fish larvae have been compared with available food under natural conditions. For example, laboratory work on anchovy larvae in California (O'Connell and Raymond 1970) indicated that first-feeding anchovy larvae require higher food densities (as determined in the laboratory) than has been usually reported from its habitat (Beers and Stewart 1967); (see review by Hunter 1977).
In a series of papers, Lasker (1975, 1978, 1981a) showed that food is indeed present at high enough concentrations, usually in coastal waters, at local densities sufficient to insure feeding in a high proportion of first-feeding anchovy larvae. Furthermore, he indicated that survival may be a function of ocean stability (Lasker 1981b) where maintenance of high numbers of food organisms is dependent on the lack of water turbulence. When turbulence is strong, due to storms, upwelling, etc., concentrations of food organisms may be dispersed reducing the number of food particles to below feeding-threshold densities, and making it impossible for newly feeding anchovy larvae to survive. The kind of food available is another factor, since some foods would not support growth in anchovy larvae.

**Egg Mortality and Larval Production**

Mortality of northern anchovy eggs has only recently received much attention in relation to subsequent larval survival. Hunter and Kimbrell (1980) determined that anchovy adults, because of their filter-feeding capability, can and do strain out and ingest congregated as well as isolated anchovy eggs. The patchiness of eggs (prey) and the schooling behavior of the adults (predators) make the coincidence of predator and prey particularly important in determining the number of eggs that will survive. Probably other schooling organisms, e.g. sardines and euphausid shrimps, (Theilacker and Lasker 1974) can reduce the number of surviving offspring.

In 1980 and 1981, very similar overall egg production rates occurred, but egg mortality rates were very different in the two successive years (Table 1). This resulted in a very much higher number of early larvae in 1981 than in 1980. At this writing (July 1982), the 1981 year-class seems to be a very poor one. Thus, environmental factors having little to do with the number of larvae produced seem the more likely explanation for the resultant year-class size in this case.

Recent studies by Methot (1981) incorporating birth date determination of anchovy recruits in which he used a precise aging technique, otolith daily annulus counts (Brothers et al. 1976; Methot and Kramer 1979), permit the determination of relative survival through the spawning and larval production period. Northern anchovy have a protracted spawning from December through May. Peak spawning, however, is confined to the February–March–April period. By age-dating recruits resulting from the 1978 and 1979 spawning period, a relative picture of larval survival was obtained and comparisons could be made with the abiotic factors prevailing during the larval period. Methot (1981) showed that in 1978, despite the heaviest larval production in late February, the majority of recruits were born in March and April (Fig. 9). Lasker (1981a) attributed this to the series of severe storms that swept through the anchovy spawning grounds in December 1977 through February 1978 and ceased only by mid-March 1978. Turbulence from these storms appeared to reduce larval food concentrations below threshold for first-feeding and should have resulted in reduced larval survival. However, data from 1979 showed a reversed pattern with higher survival in winter relative to spring (Fig. 9). Methot (1981) favors the idea of Parrish et al. (1981) that spring upwelling may result in transport of larvae offshore where they are lost to the main population.

**Correlations between menhaden year-class strength and Ekman transport** characterize the interesting work of Nelson et al. (1976). Because of the estuarine dependence of menhaden larvae, these must be carried into estuaries from offshore spawning grounds in order to survive. Nelson et al. (1976) showed that indexes of westward transport toward U.S. east coast estuaries from offshore spawning areas were positively correlated with indexes of yearly survival.

Larval drift has also been implicated in excessive larval mortality of other species, for example by Parrish and MacCall (1978) who studied Pacific mackerel (*Scomber japonicus*) year-class survival in relation to upwelling indexes. They found strong negative correlations between upwelling and good year-classes. The Kondo (1980), Nelson et al. (1976), Methot (1981), Parrish et al. (1981), and Lasker (1981a) conclusions, while arrived at by different approaches, all depend on Hjort’s (1913) idea that it is lack of food that affects larval survival and assumes the effect of predation on eggs and larvae to be minimal or constant.

### Table 1. Variability of standing crop of larvae due to differential mortality.

<table>
<thead>
<tr>
<th>Year</th>
<th>Specific egg production (eggs g⁻¹ d⁻¹)</th>
<th>Survival to hatch</th>
<th>Exponential mortality rate for larvae (d⁻¹)</th>
<th>Calculated specific standing crop of larvae (larvae g⁻¹ d⁻¹)</th>
<th>Larval abundance from surveys (10¹² larvae)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1980</td>
<td>0.3065</td>
<td>0.359</td>
<td>0.168</td>
<td>65.5</td>
<td>18.11</td>
</tr>
<tr>
<td>1981</td>
<td>0.3260</td>
<td>0.705</td>
<td>0.164</td>
<td>140.1</td>
<td>28.60</td>
</tr>
</tbody>
</table>

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Differential Fecundity

Compounding this complexity is the differential fecundity of the northern anchovy from year to year. For as yet unknown reasons, the northern anchovy increased its egg production per gram of female per day by 30% in 1981 over 1980. Thus the interplay of a variety of factors must determine eventual recruitment: (1) female fecundity and egg production; (2) egg mortality which depends on the number and coincidence of egg predators including the spawners themselves; (3) larval food availability and physical ocean factors including turbulence and drift; and (4) larval mortality (including starvation and predation). To what degree each of these contributes to larval survival is as yet unknown.

Cannibalism and Stock Expansion

Fishery management usually relies on conventional “black box” models of overall population dynamics such as production models or stock-recruitment relationships (SRR). These models are assumed to represent an integration of local mechanisms over the extent of the stock in space or time. Rarely are specific mechanisms identified and integration actually performed. However, when this is attempted, the results may differ from conventional models.

Behaviorally, anchovy spawning appears to be a risk-spreading strategy of covering all possibilities. Individual anchovies may spawn at 6- to 7-d intervals, or about 20 times in a season (Hunter and Leong 1981), so that an individual’s eggs encounter a wide temporal range of environmental conditions. Within the geographic limits of spawning, eggs occur ubiquitously. As a result of this widespread temporal and geographic coverage, conditions favorable for larval survival, although relatively unpredictable, tend to be utilized wherever and whenever they occur.

Murphy (1977) observed that clupeoid fishes characteristically expand and contract their range with changes in overall stock abundance. This behavior is demonstrated clearly by the distribution and abundance of anchovy larvae off southern California (Fig. 10). MacCall (1980a) has shown that this geographic behavior is a logical consequence of density-dependent habitat selection, given the following assumptions: (1) the spawning habitat is most favorable near the center of the range, and deteriorates toward the periphery; (2) the local spawning habitat becomes less favorable as local density of spawners increases, for example, because of cannibalism (e.g. Hunter and Kimbrell 1980); and (3) fish individually attempt to spawn in the most favorable locations. The resulting distribution of spawner abundance should approach the “ideal free distribution” of Fretwell and Lucas (1970) wherein all fish experience approximately the same quality of spawning habitat. At low stock abundances the fish are concentrated in the most favorable localities. At high abundances the density increases in those previously favorable areas, but due to a density-dependent decrease in the quality of those habitats, the stock also expands into surrounding previously unoccupied areas of marginal quality. Thus, in terms of the risk spreading spawning strategy, the expected reproductive success in formerly poor peripheral areas now is equally attractive to the expected reproductive success in central preferred areas that have a high risk of cannibalism. While fish are probably unable to sense these risks directly, information such as water temperature and food abundance (which responds to grazing intensity) may be sufficient to govern adaptive geographic movements.

The overall effect is expansion of the range as stock abundance increases. Local density (and consequently, density-dependent effects) increases more slowly than does total abundance. If fishermen concentrate their activity in areas of highest density, catch per unit effort ($C/f$) will be insensitive to changes in total abundance even if $C/f$ accurately reflects true local density. MacCall (1976) suggested that Pacific sardine $C/f$ varied as a power function of stock abundance ($N$), $C/f = aN^b$, where $-1 < b < 0$, as is consistent with the above geographic behavior.

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2 While the widespread extent of anchovy spawning has long been recognized, the degree of spatial and temporal saturation is remarkable. In recent California Current ichthyoplankton surveys using a small net (0.05 m² opening), three nightly age groups of anchovy eggs are discernible from embryonic development prior to hatching. Of those samples containing at least three eggs of any age, about 50% contained eggs from all three previous nights of spawning.
Fig. 10. Geographic distribution of northern anchovy larvae at low and high population sizes.
This geographic expansion model produces some surprising departures from the traditional fishery models. Gulland (1977) reports a production model originally developed by W. W. Fox Jr. wherein $C/f$ varies according to the above power function. The resulting production model predicts stock collapse at levels of effort only slightly above that producing maximum sustainable yield (MSY). Also, depleted stocks would have difficulty recovering from this collapse unless effort is reduced drastically. Csirke (1980) describes a stock-recruitment model of Ricker form, where egg production is proportional to abundance, but subsequent survival is a function of mean adult density (measured by $C/f$) rather than total abundance. Csirke’s model is therefore consistent with the above geographic expansion model. While local dynamics show decreasing recruitment at high density, geographic expansion offsets this local decrease, so that the overall stock-recruitment relationship (SRR) loses much of the domed shape characteristic of the traditional Ricker model. The resulting SRR appears almost asymptotic (Fig. 11).

The shape of the general clupeoid SRR has been open to debate. Harris (1975) distinguished between stock-dependent (intercohort) processes, which tend to produce a domed SRR, and density-dependent (intra-cohort) processes, such as competition among larvae, which tend to produce a less curved, asymptotic SRR. Cannibalism has often been suggested as the most likely regulatory mechanism for filter feeding clupeoid fishes (Murphy 1967; Csirke 1980), but as noted above, this mechanism produces a domed SRR. Cushing’s (1971) empirical observations of clupeoid SRRs indicated that they are not domed, but only slightly curved. Stock and recruitment data on California sardines and anchovies are in agreement with Cushing (MacCall 1979, 1980b). According to Harris’ (1975) criteria, Cushing’s contention that clupeoids lack a strongly domed SRR would require that cannibalism not be a major regulatory mechanism.

Hunter and Kimbrell (1980) have compared abundances of eggs in northern anchovy stomachs with production of eggs from their gonads, and concluded that cannibalism may account for 32% of the egg mortality. An increased mortality rate of eggs at larger anchovy population sizes also can be inferred from the relationship between egg abundance and production of hatched eggs or larvae (Fig. 12). Egg abundance is calculated as a regional census estimate according to the method of Smith (1972). This abundance was divided by the mean time to hatching as indicated by the mean water temperature. Larva production is derived from the time-zero intercept of abundance upon larval age (Hewitt 1982). At large population sizes, production of larvae per egg decreases as can be seen by the deviation of the relationship from proportionality in Fig. 12. Due to the inactive nature of eggs, this differential mortality is more likely stock dependent than density dependent (sensu Harris 1975, see above).

The geographic expansion model allows reconciliation of a stock-dependent regulatory mechanism and a slightly curved SRR. If accompanied by adaptive changes in geographic distribution and utilization of habitat, a stock-dependent process such as cannibalism need not produce a strongly domed SRR. The less curved SRR which results implies weaker regulation of abundance (i.e., weaker density dependence). As a corollary, studies of clupeoid fishery productivity which wrongly assume the strongly density-dependent Ricker curve will tend to overestimate MSY, underestimate the biomass necessary to produce MSY, overestimate the fishing effort necessary to produce MSY, and generally overestimate the resilience to overfishing.

![Fig. 11. Modification of the Ricker stock-recruitment relationship due to geographic expansion at increased abundance (dashed line).](image)

![Fig. 12. Relationship between anchovy egg abundance (corrected for temperature-specific duration) and production of hatched larvae. The probability that the two variates are proportional is approximately 10%.](image)
Reconsideration of Paleosedimentary Evidence and Sardine–Anchovy Competition

Fish scales preserved in anaerobic sediments of the Santa Barbara Basin have provided several clues to the nature of the sardine–anchovy relationship (Soutar and Isaacs 1969, 1974). Unfortunately, interpretation of these clues has been equivocal. The evidence has been of two types: scale deposition rate (SDR) is assumed to be indicative of fish abundance, while the size (width) of the scales is assumed to be indicative of fish size.

Soutar and Isaacs (1974) show scale deposition rates for thirty-one 5-yr periods (pentads) from 1810 to 1965 A.D. (Fig. 8). The most prominent feature of these time series as shown by Smith (1978) is much lower variability of the anchovy population relative to the sardine population. This has been taken to be evidence that the sardine has been the more variable of the two, but this conclusion must be tempered by our knowledge of the geographic behavior of clupeoid stocks. The Santa Barbara Basin is near the central area of preferred anchovy habitat, so we must expect local anchovy abundance to be “buffered,” that is, to vary much less than relative abundance of the total central stock (cf. Fig. 10). Soutar and Isaacs' (1974) calibration of anchovy SDR (Fig. 13) confirms this tendency toward a “buffered” response to changes in central stock abundance. Thus anchovy abundance is likely to have been more variable than is suggested by the time series of its SDR in the Santa Barbara Basin. We know less about the preferred habitats of sardines in the absence of fishing, and whether the Santa Barbara Basin is central or marginal. The SDR would correspondingly reflect a buffering or exaggeration of the total stock abundance. Soutar and Isaacs' (1974) calibration to sardine biomass (Fig. 13) is imprecise, but suggests a slight tendency toward exaggeration of changes in total abundance.

Soutar and Isaacs (1974) conclude that the low sardine SDR from 1865 to 1880 is similar to those following 1940 (Fig. 8), making the latter decline indistinguishable from previous natural fluctuations. While this interpretation superficially appears justifiable, we must note that sardine abundance has not recovered in the past 40 yr, contrary to the pattern of the 1870s. Soutar and Isaacs' (1974) calibration of sardine SDR loses definition at about 700,000 t biomass, where zero counts become common (Fig. 13). Thus the sardine biomass could have ranged from 0.5 to 1 million t during the “disappearance” in the 1870s. In contrast, recent sardine abundance has been less than 10,000 t (MacCall 1979). In view of this supplementary information, the scale deposition data do not support the previous conclusion that the recent decline in sardine abundance is indistinguishable from natural prefishery fluctuations. Also, sardines took 15 yr or less to recover from the relatively low abundance of the 1870s. It is reasonable to expect a similar capacity for recovery nowadays off California, if sufficient reserve biomass were to be maintained by an appropriate program of fishing restraint.

In an attempt to define the nature of sardine–anchovy interactions, Soutar and Isaacs (1974) calculated a Spearman rank correlation coefficient of +0.34 between sardine and anchovy SDR. The negative correlation that would be expected from the commonly assumed competition between these species is not evident at the 5-yr or longer time scale. Rather, there appears to be a slight tendency toward parallel variation, perhaps due to similar responses to large-scale environmental conditions. This allows a speculation that the increase in anchovy biomass from 1950 to 1975 could well have been accompanied by an increase in sardine biomass, had there not been an intense fishery on the latter. Therefore we invoke a density-dependent explanation for potential long-term recovery, although random, density-independent effects dominate over the short term.

![Graph A](image1.png)

**Fig. 13.** Relationship of fish scale deposition rate to stock abundance off California. A. sardine, B. anchovy (adapted from Soutar and Isaacs 1974).
The evidence from scale sizes is more suggestive of possible sardine–anchovy interactions. Previous attempts to infer individual fish lengths from scale widths have been confounded by the range of scale sizes on individual fishes. However, we can more safely infer relative changes in the distribution of fish lengths from relative changes in the distribution of scale widths, under the usual assumption that scales grow in proportion to fish length. The 31 pentads from 1810 to 1965 contained 326 anchovy scales for which widths could be determined (data provided by A. Soutar). If these scales are separated into two groups corresponding to high and low anchovy SDR (respective weighted mean SDRs are 16.9 and 9.7 scales/1000 cm²/yr), the two scale width distributions are nearly identical. However, if the scales are separated into groups corresponding to high and low sardine SDR (respective weighted mean Sardine SDRs are 7.0 and 1.2 scales/1000 cm²/yr, weighted mean anchovy SDRs are both 13.3 scales/1000 cm²/yr), the width distributions are significantly different (Fig. 14, Kolmogorov–Smirnov D Statistic: P < 0.01). The mean anchovy scale width during periods of high sardine SDR is 4.94 mm, while during periods of low sardine SDR, the mean width is 5.71 mm. Assuming a cubic relationship between mean scale width and mean fish weight, the average anchovy is approximately 54% heavier during periods of low sardine SDR.

While this size–abundance relationship is consistent with the competition hypothesis, it is not the only explanation. It is possible that environmental conditions favorable to sardine abundance are associated with low growth rate, higher mortality rate, or a relatively more offshore distribution of small anchovies. Anchovies have exhibited some of these phenomena since the late 1970s in southern California, where they have tended to be smaller, younger, and now mature at a relatively small size (Mais 1981; Hunter and Macewicz 1980). Coincidentally, sardines are showing signs of increased abundance (R. Klingbeil, California Department of Fish and Game, Long Beach, CA, personal communication), but are still much too scarce to have a significant impact on anchovies. These phenomena, whose causal relationships are unclear, are consistent with the paleosedimentary patterns. Unfortunately, the paleosedimentary record provides little evidence on causal mechanisms driving the patterns of apparent fish abundance.

Appreciation

Clupeoid fisheries have been studied intensely for nearly half a century. While some of the ideas presented in this paper may be new, others have existed in various forms for many years. Nonetheless, we feel that clupeoid fishery biology is approaching a new synthesis which will allow better understanding of stock fluctuations. Yet even this synthesis is not completely new: almost all the components appear in Sette's (1943) conceptual outline of the relationships among fish life history stages, fisheries, and the biotic and abiotic features of their environment (Fig. 15). Sette's paper has been a foundation of much of the subsequent research on clupeoid fisheries of California. The success of that research must be attributed, in part, to the completeness of Sette's original vision.

![Fig. 14. Cumulative frequency distributions of anchovy scale widths from anaerobic sediments of the Santa Barbara Basin. A. Periods of high sardine scale deposition rate. B. Periods of low sardine scale deposition rate.](image1)

![Fig. 15. Sette's (1943) research plan for studying recruitment of the Pacific sardine.](image2)
Acknowledgments

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References


PALEOCEANOGRAPHY

Oceanic Crises during the Last 150 Million Years: Prelude

JÖRN THIEDE

Department of Geology, University of Oslo, Blindern, Oslo 3, Norway

Abstract

Paleoceanography is a young field of marine geosciences which has largely developed over the past decade. The first textbook to describe the subject was released only 2 yr ago (Schopf 1980). It is concerned with the history of the oceans, their physiography, their water masses, and the food webs of the marine organisms. There is no doubt that the success in unraveling this history is closely linked to the development of new techniques to sample crustal rocks and sediments of the ocean floors, and to the determination of their age and qualities. We are able to present some of these results to the community of oceanographers because we have learned to measure these qualities in terms of quantitative well-defined variables and to express these qualities in terms also understood within the other marine disciplines.

A prerequisite of being able to make the advances in this new field of marine geosciences is a detailed knowledge of the age of the ocean crust and the resulting understanding of the evolution of the individual ocean basins (Sclater et al. 1971). The oldest oceanic crust under the modern oceans has been found to be 150–180 million years old. The age structure of the oceanic crust provides us with a tool to carry out detailed quantitative reconstructions of the horizontal plate tectonic movements since mid-Mesozoic times. As ocean crust ages and moves away from the midocean ridges, it also cools and sinks in a very regular and predictable fashion, allowing us to reconstruct not only the horizontal, but also the vertical movements of the ocean floor in a quantitative and fairly detailed manner. Today we are therefore able to trace the physiographic evolution of the ocean basins through the past 150 million years or so, and to describe the temporal changes of the physical shape of the depressions that house the oceans. Many times these plate tectonic movements had a direct impact on distributions and properties of the oceanic water masses. This type of physiographic reconstructions will never be possible for the pre-Mesozoic geologic history of our planet; and the geologic history of the oceans during the past 150 million years, therefore, offers some unique chances for good and quantitative reconstructions.

The next example of studies (Fig. 1) offers an impression of our growing abilities to reconstruct properties of the oceanic water masses in the geologic past, and I have purposely chosen the CLIMAP reconstruction describing synoptically the environment of the surface of our globe during the last glacial maximum 18 000 years ago (CLIMAP 1976). The isotherms of the oceanic surface water masses, here expressed in °C, offer a vivid impression of the intensive boundary currents of that time and of the shifts of the climatic belts, especially in the polar and subpolar regions. This map is an example of the important advances in our capabilities to date quantitatively and to correlate marine sediments on a global scale, which is a prerequisite for reconstructions of this type. This map illustrates that we have learned to express our results in terms of quantitatively defined variables that can be understood and used by colleagues from other marine disciplines; it is also an example of our problem of trying to communicate with other oceanographers. This is a problem of scales (Suess and Thiede 1983), mostly time scales. The nature of our geological sampling techniques and of our measurements generally results in synoptic reconstructions or in time series where the individual data points span over a few hundred or a few thousand years, and only in very exceptional, but very exciting cases are we able to obtain a temporal resolution of individual years or even seasons.

The next example (Fig. 2) gives an idea, not of a synoptic reconstruction of the oceans at a certain time interval, but rather it presents a series of measurements, describing the changes of ocean properties through time. It illustrates the fluctuations of oxygen isotope ratios in specific shell materials collected from the ocean floor and spanning in time over the past 60–70 million years, and of the occurrence of reworked deep-sea sediment material which has been displaced by strong bottom currents. These curves reflect the response of the oceans to the general climatic deterioration that has affected our globe since late Mesozoic times and that finally has led to the extreme climatic regimes of the ice ages during the latest part of the geologic past. Whereas we had rather modest temperature gradients between surface and bottom, or if you so will, between tropical

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1 Present address: Geologisch-Paläontologisches Institut und Museum, Christian-Albrechts-Universität, Olshausenstrasse 40/60, D-2300 Kiel, Federal Republic of Germany.
Fig. 1. Surface of the ice-age world, approximately 18,000 y ago (simplified after CLIMAP 1976). Coast lines correspond to roughly 100 m lowered sea level. The symbols over the land areas mark climatically very different zones: A. Snow and ice, B. Sand deserts, C. Dense vegetation, D are ice-free water surfaces with isotherms in centigrades.

Fig. 2. Oxygen isotope ratios in benthic foraminifers through the past 70 million years, and distributions of reworked and displaced pelagic fossils in the central North Pacific Ocean (from Thiede et al. 1981).

and polar regions during Mesozoic times, this deterioration resulted in maximal gradients in Quaternary times. This evolution was not a smooth process, but happened in a stepwise function with times of slow change alternating with phases of relatively rapid change.

In the following sequence of topics it has been attempted to address mainly times of rapid changes, times when the oceans and their depositional environments went through crises, and times when the oceans were very different from the oceans that we know today. It is many times questionable whether our fantasy, which is guided by the modern analogue, is good enough to imagine how old oceans might have worked. The four topics of this session address first the paleogeography of the ocean basins, and then the crises in the Mesozoic, Tertiary and Quaternary world ocean. It is clear that these topics can only address certain aspects of the wide field of paleoceanography (Berger 1981), but the intervals of rapid change or crises of the oceans are probably those that have found most interest hitherto.

References


The Depth of the Atlantic Ocean Through Time

L. MEINKE AND J. G. SCLATER

Department of Earth and Planetary Sciences, Massachusetts Institute of Technology, Cambridge, MA 02139, USA

Abstract

In the past 10 to 15 yr, the detailed mapping of the topographic features of the oceans has formed the basis for a revolution in the way scientists think about dynamic processes on the surface of the earth. We use these new ideas to account for the structural features of the Atlantic Ocean, now and in the past.

To explain the topographic features of the Atlantic and the placement of types of sediment in the deep sea, plate tectonics is used. By using magnetic patterns, the age of the seafloor can be determined and a complete history of relative plate motion developed. We can also relate heat flow to age which can be used to determine the thermal history of a region. This is important to the search for hydrocarbon resources.

From our knowledge of the temperature field we can calculate the depth of the seafloor as a function of age (Fig. 1). In round numbers, if we assume that a ridge is 2500 m deep, then the 3000-m isobath occurs on 2 million yr-old crust, the 4000-m isobath on 20 million yr-old crust, and the 5000-m isobath on 50 million yr-old crust. Using all this information we can show how the Atlantic developed at four times.

At 165 Ma, the continents fit together well (Fig. 2), but by 125 Ma (Fig. 3), North America and Africa have separated forming the North Atlantic with depths up to 4000 m. The ocean is largely closed to water flow to the north and probably to the south. At 80 Ma (Fig. 4), the North Atlantic has depths over 5000 m with water circulation to the world’s oceans. The South Atlantic, which is less developed, has depths of about 4000 m and is divided into two basins. The northern basin is enclosed and isolated, the southern is open to the oceans to the south. By 36 Ma (Fig. 5), the Atlantic is much as it is today with circulation from the south to the north. In the

Fig. 1. (a) Relation between mean depth and age for the North Atlantic and North Pacific. The shaded area represents an estimate of the scatter in the original points used to determine the mean data. The solid curve is the theoretical elevation from the plate model. The dashed curve is the elevation calculated by assuming that the lithosphere thickens with time. (b) Plot of depth versus the square root of age, emphasizing the 1½ dependence of depth on age for crust of less than 80 million yr of age. Note that the boundary layer model breaks down at 80 Ma and that a much better match in older crust is given by the plate model (from Sclater et al. 1980 after Parsons and Sclater 1977).

Fig. 2. The reconstructed position of the Atlantic continents at 165 Ma (from Sclater et al. 1977).
last 20 million yr, Norwegian seawater finally penetrated the Atlantic, establishing the present circulation system.

Although we cannot relate the changes in morphology to the distribution of sediments nor can we reconstruct deep and shallow water currents because local effects are too influential, we can explain some puzzles; for example the occurrence of calcareous sediments below the calcium carbonate compensation depth in sediments recovered from very deep areas of the ocean floor.

We can also use the morphology in practical applications. If we assume that continental shelves and midocean ridges have roughly the same subsidence, which recent drilling results support, we can construct a complete temperature, time, and depth history for a location on the shelf. This can help in predicting where oil and natural gas may be found on the shelf.

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References


Trends and Events in Mesozoic Oceans

HANS R. THIERSTEIN

Scripps Institution of Oceanography, University of California, San Diego, La Jolla, CA 92093, USA

Abstract

Research in Mesozoic paleoceanography is focused on the behavior of global environments under conditions that are not observable today and have not occurred in the more recent history of the earth. Gradual changes in global climates related to the tectonic breakup of the early Mesozoic supercontinent Pangaea and the concurrent transition from a hemispherical super-Pacific (180 Ma ago) to the modern dispersed oceans are being modeled quantitatively and need testing by appropriate geological evidence (Thompson and Barron 1981). Major evolutionary radiations of the modern plankton groups, which occurred over the past 250 Ma, must have changed the global cycling of many elements (Tappan and Loeblich 1973). The most outstanding topics of Mesozoic paleoceanographic research, however, are (1) the mid-Cretaceous events, which led to the deposition of over 60% of the source rocks of giant hydrocarbon reservoirs in less than 5% of Phanerozoic time (Arthur and Schlanger 1979; Jenkyns 1980); and (2) the enigmatic global mass extinctions at the end of the Mesozoic Era (Russell 1979; Thierstein 1982).

Mid-Cretaceous Events

The global deposition of dark, organic, carbon-rich, mid-Cretaceous sediments in marine environments has spurred a lively debate about the causes for such a geological anomaly. The arguments were originally based on lithology and elevated organic carbon contents of these rocks and led to three hypotheses for their origin (Schlanger and Jenkyns 1976): (1) slow renewal of deep waters because of low latitudinal temperature gradients and high sea level; (2) tectonic topographic restriction leading to silled, stagnating basins with estuarine circulation; and (3) increased fertility resulting in expansion of oxygen minimum zones. A wealth of additional information has since been collected. Stratigraphic, paleobathymetric, and geographic facies distribution patterns have been refined (e.g. Arthur 1979; Thierstein 1979; Graciesky et al. 1982). Compositional analyses of the organic matter have revealed that the proportion of terrestrial and marine supply for the observed kerogens was highly variable in space and through time (e.g. Summerhayes 1981; Tissot and Pelet 1981). Observed metal enrichments in mid-Cretaceous black shales have been interpreted as evidence for euxinic deep-water conditions (Brumsack 1980). The relative importance of the various proposed mechanisms which have led to organic carbon-enriched Cretaceous shales remains, however, under discussion (e.g. Weisssert 1981). Difficulties remain because variability in concentrations of components may be caused by higher input of a tracer (e.g. increased primary productivity), by higher preservation of a tracer (e.g. high sedimentation rate and deep-water anoxia), or by decreased input of diluents. The difficulties encountered with interpretations of concentration ratios can be demonstrated by the observed carbonate and organic carbon variability in the North Atlantic basins. Cretaceous deep-sea sediments show a wide range of carbonate contents caused in part by variable nannofossil supply and in part by variable nannofossil preservation (Thierstein 1979). The sharp stratigraphic decrease in carbonate contents in the Aptian, observed in sections from the western North Atlantic, leads to an apparent increase of organic carbon contents measured as a proportion of the bulk sediment (Fig. 1) near the Cenomanian–Turonian boundary (about 93 Ma ago). If the organic carbon contents are recalculated as proportions of the noncarbonate fraction to avoid the influence of carbonate dilution, the western North Atlantic basin shows significant organic carbon enrichments throughout the early Cretaceous (Fig. 1). Similar plots for eastern North Atlantic sites show that stratigraphic changes of the carbonate dilution are much smaller, and that the depositional history may have been quite different (Fig. 2). The timing of organic carbon-enriched intervals appears to be highly variable among open ocean sites (Thierstein 1979) as well as among

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**Fig. 1.** Effects of carbonate dilution on the stratigraphic distribution of organic carbon contents in DSDP sites of the western North Atlantic. Triangles indicate two or more overlapping data points. Data from Thierstein (1979).
outcrops on land (Jenkyns 1980). This suggests worldwide oxygen-poor deep waters through most of the Cretaceous, leading to highly enhanced organic carbon deposition by local processes acting at various times in various places. Further advances in Cretaceous paleoenvironmental analyses are to be expected from refined chronologic control allowing direct comparison of mid-Cretaceous with Holocene accumulation rates of major and minor sediment components (e.g. Bralower 1982).

**Terminal Cretaceous Extinctions**

Any observed anomaly occurring in the geological record has to be evaluated with respect to steady state processes and background or long-term average rates. Currently two such presumed anomalies converge at the same stratigraphic level: rapid change of fossil contents and anomalous geochemistry in Cretaceous–Tertiary boundary sediments. Many pertinent aspects of this event, however, have remained under heated discussion (Christensen and Birkelund 1979; K-TEC II, 1982; AAAS 1982; Silver et al. 1982), such as: What are the rates of evolutionary change? What are the relative magnitudes of evolutionary discontinuities (or are mass extinctions real)? If they are real, were they selective? What are conceivable causes and mechanisms leading to mass extinctions? Can anyone of the conceivable mechanisms be shown to be more likely than others?

Based on a broad and updated fossil data set, Raup and Sepkoski (1982) demonstrated that in the terminal Cretaceous Maastrichtian stage the extinction rate for families of marine invertebrates and vertebrates was about six times higher than the long-term Phanerozoic extinction rate. Their terminal Cretaceous extinction rate increase was the youngest and second highest among the five significant extinction events they identified through the entire Phanerozoic Eon. An analysis of the record of five major fossilized marine plankton groups indicates that 85% of the species present in latest Cretaceous marine sediments did not survive into the earliest Tertiary (Thierstein 1982). These extinctions are clearly outstanding when compared to the extinction rates in previous Cretaceous stages and substages. Total planktonic foraminifera species diversity, number of first appearances, and extinctions per stage or substage are shown in Fig. 3. The average Upper Cretaceous extinction rate (Lower Cenomanian–Lower Maastrichtian) amounts to 5.8 ± 4.5 (one standard deviation) planktonic foraminifera species per substage and increases to 33 species in the Upper Maastrichtian Substage. The diversity patterns of calcareous nanofossils are displayed in Fig. 4. Their extinction rates increased from an Upper Cretaceous average of 9.4 ± 8.4 species per substage to 53 species in the Upper Maastrichtian Substage. These plankton extinctions all occurred within Polarity Zone Gubbio G, which has an estimated duration of 0.47 Ma (La Brecque et al. 1977). High resolution stratigraphic studies in sections from around the world demonstrate that the Cretaceous–Tertiary boundary is always characterized by a gradual replacement of fossils over a sediment thickness of 1 or 2 m; only where effects of bioturbation are taken into account does the replacement of fossils become geologically instantaneous (Thierstein 1981). Postextinction sediments are usually characterized by lowered influx of carbonate fossils and dominance of assemblages by a few opportunistic taxa. There is no evidence of accelerated speciation rates in the earliest Tertiary (Fig. 3, 4). Mass

![STAGE NUMBER of PLANKTONIC FORAMINIFERA SPECIES](image)

**Fig. 2.** Stratigraphic distribution of organic carbon expressed as a proportion of the bulk and the non-carbonate fraction in eastern North Atlantic DSDP sites. Triangles indicate two or more overlapping data points. Data from Thierstein (1979).

**Fig. 3.** Cretaceous and earliest Tertiary diversity change of planktonic foraminifera. Data for Barremian-Lower Cenomanian from Guerin (1981), for Upper Cenomanian–Upper Maastrichtian from Pessagno (1967), for Danian from Smit (1981).
extinctions among the marine plankton at the end of the Maastrichtian are therefore real and global.

The record of other fossil groups is far less well established, but the available data indicate that significant reductions in generic diversities abound and that environmentally these were highly select (Fig. 5). Shallow marine environments appear to have been most severely affected. The fact that deep-sea benthic foraminifera show no increase in extinction rates (e.g. Dailey 1982) suggests that unfavorable conditions in surface waters cannot have lasted for very long. The palaeontological evidence appears consistent with several catastrophic scenarios, such as sudden global changes in oceanic surface salinities or temperatures, and with short-term darkness, caused by excessive stratospheric dust loads (Thierstein 1982). A global blackout has been suggested as a possible consequence of a large extraterrestrial body impact (Alvarez et al. 1980). The enrichments of siderophile elements in many Cretaceous-Tertiary boundary clays strongly support an extraterrestrial body impact scenario (e.g. Alvarez et al. 1980; Ganapathy 1980; Smit and Hertogen 1980; Orth et al. 1981; Hsu et al. 1982). The physical consequences of a bolide impact appear likely to result in a short-term global blackout (e.g. O’Keefe and Ahrens 1982; Toon et al. 1982). The uniqueness of noble element enrichments in boundary clays and their close association with mass extinctions, however, may be in doubt (e.g. Wezel et al. 1981; Ganapathy 1982; Riedel and Sanfilippo 1982). Future improvements in our understanding of inorganic and organic noble element geochemistry will undoubtedly reveal whether a bolide impact is a necessity, rather than one of several possible explanations for the observed terminal Cretaceous mass extinctions.

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References


Climatic Crises in the Cenozoic

N. J. SHACKLETON

Godwin Laboratory for Quaternary Research, University of Cambridge, Free School Lane, Cambridge CB2 3RS, England

Abstract

It is generally appreciated that the Cenozoic has been a period of climatic deterioration over the globe. However, it is only through the study of deep-sea sediments that we have learned the nature and details of this so-called deterioration. Many types of evidence are available from deep-sea sediments; I shall discuss only the oxygen isotope method.

Figure 1 shows a recent data set from DSDP Leg 74 (Shackleton et al. 1982); this is in many respects similar to data sets published by other workers (Douglas and Savin 1971, 1973; Boersma and Shackleton 1977). It also shows at the bottom, oxygen isotope data from various species of benthonic Foraminifera, all adjusted in accordance with their estimated departures from isotopic equilibrium (Duplessy et al. 1970; Shackleton and Opdyke 1973) and connected by a line to aid the eye. At the top of Fig. 1 analyses of planktonic Foraminifera are shown; of the many species analyzed, those that apparently calcified closest to the sea surface have been used for this figure. The scale to the left represents isotopic composition with reference to the P.D.B reference standard; to the right, a scale in °C is positioned so as to be valid for times when there was no appreciable amount of ice on Antarctica. Shackleton and Kennett (1975) argued that such a scale was valid for the whole of the Cenozoic prior to late in the Middle Miocene. Events depicted in Fig. 1 will be discussed later.

In the latest Maastrichtian, significant high-frequency variation is indicated; no deep-water temperature change was detected immediately across the boundary between the Maastrichtian and the Danian with a sampling gap of a few thousand years, but it is obvious from the figure that the impression of a temperature change might have been gained if fewer or different samples had been analyzed. A general point that should be made is that throughout the record depicted in Fig. 1, there is significant high-frequency variability. So far as surface temperature is concerned, it is not possible to make any meaningful statement about

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**Fig. 1.** Oxygen isotope record in surface-dwelling planktonic (above) and calibrated benthonic (below) foraminifera from sites drilled during DSDP Leg 74 on the Walvis Ridge in the South Atlantic. Analyses (Shackleton et al. 1982) are plotted according to the timescale of Berggren and Kent (1982).
the Cretaceous–Tertiary boundary since there are few planktonic species in the early Danian and they may well not have calcified at the surface (Boersma et al. 1979).

The Early Eocene may be described as a crisis of warmth; deep-water temperatures were remarkably high at that time. However, in the South Atlantic, it seems that surface water was as warm in the Late Paleocene as in the Early Eocene. A map of surface temperature at this time (Shackleton and Boersma 1981) shows that the latitudinal temperature gradient was less than half its present value, making the understanding of global heat transport at this time a fascinating problem for future work.

The timing of the first cooling after this anomalous period is well controlled to early in the Middle Eocene, rather a surprising finding in view of the high diversity of planktonic faunas of the Middle Eocene. Previously, the timing of this event had been poorly constrained. The most dramatic cooling event was evidently at or very soon after the Eocene–Oligocene boundary. Although the details of the boundary itself are not revealed in Fig. 1, it is important to note the rather positive $\delta^{18}O$ values in the basal Oligocene. These suggest deep-water temperatures of the order of $2^\circ$C in the absence of any ice sheet. An alternative hypothesis has been put forward (Matthews and Poore 1980) that the Antarctic ice sheet did form at this time, in which case computed temperatures would be somewhat higher. It seems quite likely on the basis of the data in Fig. 1 that an ice sheet did form in the Early Oligocene but that it disappeared within a few million years; it is very unlikely that any ice was present at the base of the Middle Miocene, and the changes during the later Middle Miocene are almost certainly due to the “permanent” (up to the present) formation of the ice sheet. Figure 1 shows dramatically the extraordinary growth in temperature gradient over that interval. Data from low latitudes shows that the dramatic change is in the gradient between mid- and high latitudes; the temperature difference between mid- and low latitudes has not changed significantly over this interval. However, there is little doubt that both mid- and low latitude surface waters have actually warmed over the past 15 million yr. Even taking no account of the ocean isotopic change associated with Antarctic glaciation, a small rise is indicated in Fig. 1, and taking this factor into account, one should increase this apparent warming to about $4^\circ$.

In conclusion, three major problems are exemplified by Fig. 1: (1) How did the ocean and atmosphere function in the early Eocene with deep water temperature about $12^\circ$? (2) Why was there such a sudden change at the end of the Eocene? and (3) Why have low- and mid latitudes warmed so dramatically in the Neogene?

References


Pliocene–Quaternary: Glacial–Interglacial Crises

J. C. Duplessy, P. L. Blanc, and M. R. Fontugne

Centre des faibles radioactivités, Laboratoire mixte CNRS-CEA, 91190 Gif-sur-Yvette, France

Abstract

The time of the inception of continental ice caps in the northern hemisphere has been a bone of contention for many years. Direct evidence for an early date (3.1 Ma) has been obtained twice through radiometric dating and magnetostratigraphy of volcanics associated with subaerial glacial deposits (McDougall and Wensink 1966; Curry 1966). In the marine environment, oxygen isotopic analyses of Foraminifera and magnetostratigraphy point to the same early date for boreal glacialization (Shackleton and Opydke 1977). However, palynological studies in northern Europe show a rather different pattern, as the time of the first cooling in the North Sea basin appears to be closer to 2.5 Ma (Zagwijn 1974; Van Montfrans 1971).

Blanc et al. 1982 bring additional isotopic data on the initial boreal glaciation, as recorded in the North Atlantic sediments from D.S.D.P. site 116, this site being one of the northernmost from which pre-Quaternary ice rafted debris has been reported (Berggren 1972a). D.S.D.P. Site 116 (57° 29‘ N, 15° 55‘ W, 1151 m) was drilled on the Hatton Rockall plateau. For planktonic foraminiferal calcite oxygen isotope analyses were performed on *Globigerina bulloides* monospecific samples. Digital values are reported in Blanc and Duplessy (1982).

We also conducted organic carbon isotopic analyses on the finer (<150 μm) fraction of the sediment samples. Sackett et al. (1965) have shown that there is a temperature dependence of the organic carbon isotopic ratios of marine plankton. Indian Ocean and North Atlantic plankton analyses by Fontugne and Duplessy (1981) yielded a temperature coefficient of 0.35‰ per °C, consistent with those obtained for phytoplankton culture (Degens et al. 1968) and theoretical calculations (Libby 1972). Furthermore, Fontugne (1978) has shown that the organic matter in recent sediments preserves the isotopic ratio of the present plankton. Therefore these observations provide a new method for estimating sea surface paleotemperature change from the carbon isotopic composition of the sediment organic matter. The analytical procedures have been described by Fontugne and Duplessy (1978). The results from the quantitative census of planktonic Foraminifera and the ice rafted detritus index at site 116 (Poore and Berggren 1975) are compared with the sediment carbon isotope ratio and the planktonic foraminiferal calcite oxygen isotopic ratio in Fig. 1.

Before the 3.1 Ma event, the isotopic ratio of planktonic foraminiferal calcite is low, showing that no important boreal ice accumulation had taken place yet. The isotopic ratio of organic carbon is rather high, ranging from −22.17 to −23.21 per mil vs. P.D.B., equivalent to temperatures of 14 – 11°C, in good accordance with the present-day temperature on the site (winter SST = 10°C; summer SST = 15°C). The mid-Pliocene climate on Hatton Rockall was thus equivalent to the present one, with a transitional planktonic fauna. On the European side of the Atlantic, the climate was warm and humid: this is the time of the Brunssumian flora in the Netherlands and of swamp forests on the Mediterranean, French, and northeastern Spanish shores (Cravatte and Suc 1981).

The drop in δ18O values between 81 and 87 m marks the inception of glaciation in the northern hemisphere.

![Fig. 1: Comparison between four climatic indicators at site 116: Ice-rafted detritus index (Poore and Berggren 1975),
\[
\text{Nb detrital grains} \times 100
\]
\[
\text{Nb detrital grains} + \text{Nb plank. foram.}
\]
3.1 Ma ago and is correlative with the disappearance of *G. puncticulata* and a strong increase of *Globigerina bulloides* which reflects the contact between subarctic and transitional waters above the Hatton Rockall plateau (Pujol 1980). At the same time, the first low-level glaciation is found in Iceland, and extensive mountain glaciation occurred as far as the Californian Sierra Nevada. However, the European climates were still warm: the Reuverian flora of the Netherlands is still rich in thermophilous plants but is indicative of a less humid climate than the Brunssumian. On the western Mediterranean shores, the swamp forests tend to disappear. No serious cooling had taken place yet on the Hatton Rockall plateau, although the planktonic foraminiferal assemblages indicate that the subarctic convergence was close to the site. The persistence of the North Atlantic Drift was probably responsible for the mild north European climate of the time.

In the interval from 3.1 to 2.3 Ma, several glaciations occurred, limited to the western side of the Atlantic, as several tills are found in western and northeastern Iceland. These glaciations had no major influence on the European climates apart from the total disappearance of the swamp forest from the western Mediterranean shores through the inception of a summer dry season (Cravatte and Suc 1981).

The first subarctic water pulse on the Hatton Rockall plateau is marked by the arrival of ice rafted detritus, 2.7 Ma ago. However it is only close to 2.3 Ma that the polar front reached the Hatton Rockall plateau for the first time. This is marked both by a decrease of the organic carbon isotopic ratio (equivalent to a temperature drop to ca 5°C) and by an arctic foraminiferal assemblage dominated by *N. pachyderma* (left coiling). This event must be coeval with the beginning of the Pretiglian Dutch stage, when the timber line was south of the Netherlands for the first time (Zagwijn 1974). Such a retreat of the boreal forest in northern Europe also implies the appearance of an ice cover over Scandinavia. On the western Mediterranean shores, only a further reduction in humidity is recorded with the appearance of steppic floral elements (Cravatte and Suc 1981).

Thus for the first time, at about 2.3 Ma, the oceanic polar front occupied a zonal position across the whole width of the North Atlantic Ocean, south of the Hatton Rockall basin, while previously it occupied a rather oblique one from Newfoundland to southeastern Iceland, in a manner similar to that described for the time of deposition of Ash layer 1, about 9800 yr ago (Ruddiman and Glover 1972, 1975; Ruddiman and McIntyre 1981). In conclusion, the paleoclimatic study of D.S.D.P. site 116 shows that the North Atlantic Ocean, which in the middle Pliocene was as warm as at present, maintained from 3.1 to 2.3 Ma between its western and eastern side a climatic disparity stronger than that observed today. This oceanic pattern was responsible for the fact that the initial high latitude ice cover was established on the western side of the Atlantic (Iceland, Greenland, North America). It is only when the polar front became zonal that an ice cover was established on the eastern side, thus explaining the lag between initial European and North American glaciations.

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**References**


I must say I was flattered by Eric Simpson's request to make this summary but at the same time apprehensive. I am not actually sure what he asked me to do but I am not going to worry too much about it. I would like to say that although we geo-scientists were perhaps not as appreciative as the ocean water scientists, certainly we did appreciate the opportunity to be present when the albatross award was given. We are certainly as appreciative as others of the importance of the work that George Deacon has done. Despite Sir George's comment, I decided that the most unifying slide I could produce would be a spectral analysis because although we don't all use spectral analysis, I think all branches of oceanography use it and this slide (Fig. 1) makes a good backdrop to some of the things I want to comment on. This was produced as a cartoon by Murray Mitchell. It is not a real spectral analysis and it therefore doesn't matter that it was in fact produced to describe climate and I am going to pretend and that it is a spectral analysis depicting variance in the ocean through a range of geological periods. The frequency scale represents periods from $10^{10}$ years to $10^{-4}$ years and is not, in fact, the full period range in which I am interested in; my clarinet goes up to periods of something like $1.7 \times 10^{-11}$ yr although I'm not going to use my clarinet we are sometimes concerned with higher frequency events.

I must say first that it would have been a privilege to hear Dr. Monin, had he been here, and it would have been very nice to have heard Karl Hinz also. So the first real scientific excitement was Dr. Monin's replacement by Bill Hay. I think we all enjoyed his talk and learned a lot from it and it set the scene for some of the important speeches at this meeting, and in particular, the stimulating interaction between modellers and people who make

![Diagram](image_url)

**Fig 1.** Distribution of variance in the ocean-climate system as cartooned by J. Murray Mitchell (*Quaternary Research*, Vol. 6, p. 481–493). Probable origin of the more prominent peaks is indicated.
observations in oceanography. The first scientific session I want to mention briefly is the one on convergent plate margins. Only 50% of the speakers turned up but for me personally, as it were, a tourist in plate-margin geology, I must say Seiya Uyeda's review talk there made up for the loss of some of the others. It is very difficult to give a review that really summarizes a big field and yet seems exciting and gives the impression of continual new evolution of ideas and he achieved that.

Let me digress for a moment to this slide (Fig. 1). I think we all believe in the Gulf Stream and I think it will be reasonable as far as people in my side of the community here are concerned to say that we think we can see the Gulf Stream as well from the sediments underneath it, containing the fossils of the faunas and floras that characterize it, as one can see it from a satellite looking down upon it. In fact over periods from perhaps $10^5$ to $10^6$ years, we have a pretty good idea of the variability in the Gulf Stream. One of the other sessions that I was present in as a tourist, as it were, was concerned with the Gulf Stream. I heard a very distinguished oceanographer talking about it and he had spent a very large amount of money floating little things in the water and watching them for a year. It is very expensive to track floating objects in the open ocean for a year and the summary word that dropped from his lips at the end of his talk was “chaos.” There is so much energy up at the high frequency end of the spectrum in ocean circulation that it is actually very difficult for physical oceanographers to see what is going on at lower frequencies.

So going back to Seiya Uyeda’s talk, one of the really staggering observations he made and one which I think one could certainly not have predicted at the last JOA, is that whereas physical oceanographers have great difficulty making observations that relate to periods of a year or more, Seiya Uyeda was able to talk about motion of the plates on the surface of the earth in terms of energies down near $10^7$ to $10^8$ yr periods and he was able to confirm his observations from the first motion of earthquakes with periods which will be somewhere up at the top of the frequency scale of Fig. 1. He is able to extrapolate over 14 orders of magnitude and come to the identical conclusion about geophysical plate motions. I am sure there are clever mathematicians from one or other of those Cambridges who can give us a few non-dimensional numbers and say it is obviously the case, but I think to most of us as human beings that is just staggering. I should also say that after Hilde's talk I have a vivid memory of subducting plates breaking up into little teeth like a cheese grater and carrying sediments down into the subducting zone I mention that because, although sometimes I wondered about the physics involved in some of these reconstructions, I felt that there were a lot of very good physicists there working on it and I am sure that if it was not right last week, it will be right next week.

At this point, let me say that I was given freedom to be very subjective and, of course, one has to be very subjective in making such a summary “off the cuff.” In any case, I did not hear everything. I was told last night that one of the good sessions that I did not hear concerned the high energy environment where the ocean impinges on the continent and I think it is clear that there are many reasons why this area is worth further study. I have noticed that the science of geomorphology has evolved in a very satisfactory direction in the past few years and I understand that session showed that scientific geomorphology has had a big impact on understanding high energy coastlines. I mention that at this point because I suspect that there will be considerable interaction between the people who work on coastal evolution today and the people who try to track it historically in seismic profiles deep in the passive margins.

I, again in a somewhat touristic way, learned a great deal from the sessions and discussions on the ocean hydrothermal systems. I am very interested in global elemental budgets and it is clear that hydrothermal systems have a big impact on global budgets in the ocean. Hydrothermal mineralization is important from the economic point of view as it affects the economic value of minerals that are to be found below the sea-floor indirectly for what we can learn about mineralization in rocks as they now exist on the continents. The time scale on which hydrothermal activity has been observed is a little uncertain at the moment and Claude Lalou had a very nice poster session demonstrating the time scale on which the hydrothermal mounds vary. Considering that it is only so recently that we started learning about hydrothermal systems, I think another poster session which was very exciting was one by Mary Delaney who has devised a geochemical technique which attempts to look at the variability of hydrothermal systems over the whole ocean history. I think this is one of the many areas in which smart geochemists are teaching us a lot about ocean history.

A group of sessions were concerned with fluxes into the ocean and through the ocean and through the sea-floor. I don’t know whether I was actually intended to discuss these but I feel I have to because those of us who work in paleoceanography have discovered that one of the most stimulating and important aspects of paleoceanography is oceanic budgets as they fluctuate in sediment through time. Up until recently we have had very little data from the modern environment with which to compare flux rate measure in the sediments. It is interesting that Jerry Prospero was able to make observations in the period range 10$^{-1}$ to 1 yr from sampling stations around the Pacific in order to measure flux rates of terrestrial material being blown onto the ocean surface and to satisfactorily extrapolate down to $10^3$ to $10^4$ yr periods over which one integrates looking at the sediments.

The very good session organized by Sus Honjo was concerned with the various rapidly growing number of experiments concerned with sediment traps deployed in the water column to look at the flux of terrestrial and biogenic matter. By clever technology, they are both able to see the variability and time scales controlled by the seasonal cycle (indeed sometimes in other shorter time scales; one trap accidentally caught a generous bloom of one particular diatom species that happened to be falling through the water column and passed one of the
number of traps deployed). But in general they were able to look at the variability in the fluxes in the range 10^-1 to 10^ yr periods and extrapolate through to the sort of time periods that we are concerned with looking at flux rates in the top part of deep-sea sediments.

I was also very fascinated to see one feature in the sediment trapping data which in fact was precisely mirrored in some new data concerned with flux rates over a set of deep-sea drilling sites deployed down a vertical transect in the south Atlantic which is the geological analogue of a set of sediment traps. I think we are going to be concerned with many more such experiments comparing flux observations in real time with analogous fluxes observed in geological sediments at different points in the water column.

The session on the interface at the bottom of the ocean between the ocean and sediment was also stimulating and I feel it is perhaps appropriate at this point to mention that one of the other English people in this session, Nick McCave, gave one of his three talks in that session. (I noted that every one was different.)

I mentioned earlier Bill Hay’s useful talk in the opening session and I do not want to embarrass him by coming back to it directly but mention that, as I come towards areas of my own interest, one of the very important growth areas in paleoceanography is the interaction with various kinds of modellers, be they very expensive atmospheric modellers or less expensive modellers of particular parts of the ocean system or the sediment system. An example of interaction between that community and those of us who make the observations in the sediment was Eric Barron’s beautiful talk on the climate in the Cretaceous period when we know that temperature distribution of the oceans was dramatically different from that today. He addressed the question of to what extent one can work towards understanding such a situation and to what extent models that one can create from the information we now have help us to better understand the system and help us to pinpoint the areas where we should go to gather more data. I am thinking also of such posters displays as John Southam’s concerned with the modelling of the oxygen minimum layer and of the dissolved oxygen content within the water column. It is a very short step from that, and one which I encouraged him to take, to modelling other geochemical and chemical variations within the water column. In that context I should say that the evolution of chemical oceanography is very important to us geologists because in general the sediments do not register salinity. Very often they do not register temperature either, but they do register some of the chemical parameters which are intimately related with these physical parameters in the ocean today. And so when modellers such as John Southam are able to model chemical and isotopic variations in the water column then we can directly compare their model predictions with observations in the sediment.

I am now going to mention to you the sessions towards the end of this meeting which I have been more personally concerned with. One of them was the session on paleoceanography which I must say was not a review session on what we know of paleoceanography; rather, it comprised a very elegant portrayal by a geophysicist of what we have learned about the evolution of ocean basins followed by three very personal accounts of what three individuals in the field feel is exciting in paleoceanography. I think many of you have heard of the CLIMAP Project which reconstructed the temperature distribution of the oceans during the last ice age. I think it is probably fair to say that that project was of most interest to climatologists rather than to geologists who are interested in changing climate as they observe the data on the continents, and I think the biggest impact that the CLIMAP Project had in communication with a big audience was the International Quaternary Association’s meeting five years ago where a room this size was packed with Quaternary geologists anxious to hear about global climate as it was reconstructed in the ocean. I think since that time our community has evolved towards communicating better with oceanographers; I think we are doing something which warrants the name paleoceanography. I should perhaps mention in this context since I am sure that Ed Boyle will be too modest to mention it that geochemists are making a very big impact in paleoceanography and that his own poster session was a very exciting example of what bright Cambridge people can do in careful geochemistry to tell us something about paleoceanography in deep water circulation in a language that communicates with chemical and physical oceanographers actually observing the oceans. So I think paleoceanography is a science which is growing and is growing in the direction of increased interaction with other kinds of oceanographers, and I think we paleoceanographers are the area of earth science represented this meeting who benefit most from the interactions provided by a JOA.

Finally to the chronology session and the cartoon in Fig. 1. I think of the person in my shoes or sandals in six years’ time and wonder if he will use the same slide as a backdrop to a review of what he heard in the sessions. If so, I believe that he will be able to use a slide based on real data rather than Murray Mitchell’s imagination. I suspect he will be able to choose between several very different slides based on real data, and in fact I think that the time element is becoming very well integrated into paleoceanography.

The final session for us was the session on chronology and organized by SCOR Working Group 63 — Chronology and Marine Sediments. Valery Krasheninnikov made a very significant point in his contribution here that only 15 years ago when people talked of global stratigraphy and global chronology, they took global to mean that they could recognize the same zones in the U.S.S.R. and England and Australia and South America and perhaps in Antarctica if they could only get the rocks, but completely ignored the fact that two-thirds of the globe is under water. Stratigraphy and chronology are not only important to paleoceanographers but are extremely important to the world at large. Of course all our energy and mineral resources depend ultimately on
traditional geological stratigraphy and chronology, and the development of stratigraphy and chronology is extremely important. As Valery Krasheninnikov pointed out, we now can start talking about truly global stratigraphy and chronology. He showed just how much we have learned in the last 15 years as a result of deep-sea drilling and the availability of sediments over the ocean floor, and he showed how much more we will be able to learn with a few more, or in fact, many more sequences in the ocean basins.

The chronology session was cleanly illustrated by Ian McDougall from a strictly numerical time point of view and by Dennis Kent who I think built on the feature I pointed to first, that one really can extrapolate over 14 orders of magnitude in plate motion. Therefore, one can use plate motion to smooth geological time scales. Then one can obtain a time scale which is appropriate to measuring fluxes and the variations in geological processes on an appropriate time scale to perceive what is going on. Let me come back to the sea level problem. We do not know whether sea level variability is 1) within the band width where Murray Mitchell put no bumps with period $10^5$ to $10^7$ yr but which is where it would appear to be on the basis of Peter Vale’s reconstructions, or 2) whether the time scale of sea level change is down here in the $10^8$ yr range which is where Murray Mitchell would put tectonic energy in the ocean system, or 3) whether in fact as I indicated in one of my talks sea level variation would be expected to be found in the range of $10^4$ yr to $10^5$ yr periods. One can not address that sort important question without good chronology.

I think perhaps the final talk for the geologists was a very exciting one; certainly it was to me. Al Fischer from Princeton spoke about energy in precisely the band width $10^4$ to $10^5$ yr. I should say that this band width in which ocean variability is forced by well-understood variations in the earth’s orbital system. Al Fischer demonstrated and convinced those of us who weren’t already convinced, I think, that this peak of spectral energy at periods $10^4$ to $10^5$ yr is present in ocean history at least through the past 600 million years. This is extremely exciting as a model. One has a forcing function acting on the oceans throughout their varying geological history whose effects we can monitor, and also we have this, if I may use the phrase “the geophysical tuning fork”, present to tune our geological chronology perhaps right through the past 600 million years. I think it is really a pity that more of you weren’t present to hear this presentation and see the beautiful pictures of oceanic rocks in Italy bathed in the Italian sun because it really is incredibly exciting to think that the whole of ocean history has been forced by an astronomical rhythm that we have a good physical understanding of. I think that all aspects of geological oceanography are going to sense this rhythm over the next few years and as I say, I think something like this cartoon will be based on real data over the whole of the geological column by the next JOA. Thank you very much.
Summary — Physical Oceanography

GEORGE CRESSWELL
CSIRO Division of Oceanography, P.O. Box 21, Cronulla, N.S.W. 2230, Australia

Last night at the lobster dinner four of my countrymen told me that whatever I said today, many people would not be satisfied. That was something for me to reflect upon as they happily planned their surfing and sightseeing expedition for this morning. Well then this is a report to you as a multi-disciplinary group on some of the highlights of the physical oceanography facet of this JOA. But before I get on to that I am reminded of Nova Scotia’s famous writer from 150 years or so ago, Thomas C. Haliburton. He created Sam Slick, the Yankee clockmaker and at one stage Sam was asked: “Pray, what are your impressions of the present state and future prospects of Halifax?” In that case Sam answered: “If you will tell me when the folks there will wake up then I can answer you; but they are all asleep.” Well physical oceanography, unlike Sam’s opinion of the Halifax of 150 years ago, is certainly not asleep, I can vouchsafe for that. There is a vibrant feeling of optimism as new discoveries are being made, as new problems are tackled and often solved, and as truly ingenious instrumentation is added to the suite of tools that are available to us.

What impressed me most was that computer and laboratory models seem to be approaching reality. We have seen models of the global oceans, even a model of the oceans of the Cretaceous period; and an eddy resolving model of the North Atlantic of Bill Holland’s that was presented to us as an animated movie that truly glued us to our seats as we watched eddies forming, moving, and interacting with the parent Gulf Stream and with the Continental Shelf near Nova Scotia. Another movie presentation, this time from Paul Linden, showed us a coastal current with eddies strongly reminiscent, to me anyway, of the situation off Western Australia which we have looked at with satellite infrared sensing and with satellite drifters. In Paul’s case, creating drifters was much easier than in our case. He was able to simply sprinkle some pieces of paper onto his laboratory model.

The global numerical models, among other things, enabled us to see what the increase in CO₂ might do to the earth’s climate and the delaying effect that the ocean will have on that climatic change. The chances of success of those types of models have been improved by transient tracer studies which provide an indication of CO₂ pathways into the ocean interior. Models that seem to be very close to reality are those of the Florida group under the direction of Jim O’Brien. Ship-derived winds for the Equatorial Pacific were used to drive their model and we were encouraged to compare the results with reality. We could see that there is a depression of the thermocline in the Western Pacific due to enhanced trade winds and that when these die away a Kelvin wave propagates over to Peru taking a couple of months to get there, creating a downwelling and arresting the productivity that is normal in that area. This is El Niño and this interpretation was originally due to Klaus Wyrtki Rossby waves that propagate westward from South America and at some distance from the equator were detectable back at the western side of the Pacific. One of the key things seemed to be that this overall phenomenon is likely to have an effect on the weather of the United States and so by carefully studying the wind stress on a weekly basis, it is possible to come up with a prediction of some of these climate perturbations. Jim asked the question: “Well, what is the best way to do this?” It was his opinion that we should use a scatterometer on a satellite. He did raise the question of what is this prediction worth, and the answer apparently is something between half and one billion dollars.

This is perhaps a suitable time to move on to satellite-associated techniques. Robert Cheney showed us the satellite altimeter data from Seasat where it was used to measure sea-surface topography from which currents can be derived. Regions having a high variability were identified from these data and we were surprised to find that the eddy kinetic energies were very similar to those presented by Phil Richardson from his drifter data and I guess Bob and Phil are getting together to make sure that this is meaningful. We have now got satellite infrared measurements of the sea-surface temperature to a precision of 0.1 degree with a spatial resolution of one kilometre and we can see several of these now in case you were not at the meeting. Richard Legeckis produced these pictures. This one is of the Gulf Stream. The next picture is of the Gulf of Tehuantepec in Mexico with upwelling due to a jet of offshore winds. And finally, this is a little bit closer to home for some of us, is the East Australian Current wrapping itself around an eddy. We saw, in fact, satellite infrared imagery used as supporting data in many papers and here I can quote from Sus Tabata’s poster: “Without the satellite data it would have been difficult to establish with any certainty that such events occurred, principally due to the lack of sufficient ship data points.” So the satellite IR has become a useful tool for us. The major problem, however, that is facing most of us oceanographers scattered around the world, is getting our hands on the imagery and so some sort of system is needed to enhance these images and to distribute them. The other satellite techniques that looked exciting were measurements of ocean color, winds using cloud drifts, and insolation among other things.

I mentioned earlier some of the ingenious instruments that are now available to us: acoustic profilers that give us underway currents while we are on board ship. For those of us that have not worked with the GEK, it is an instrument for measuring currents while underway where it is necessary to change direction by 90 degrees.
every hour or so. If you have been faced by an angry cook at mealtime because you have changed the direction of the ship, then something like this, this acoustic profiler, in addition to giving good scientific information, has a social benefit at least while we are at sea. We have seen some very elegant microstructure instruments and velocity profilers used by the Canadians and developed here, in one instance, at BIO. Our understanding of fronts and shelf processes is advancing rapidly through mathematical, observational, and laboratory studies. Yesterday we saw the unravelling of the menagerie of processes that contribute to what we observe when we put a current meter on the continental shelf and we have seen how and when fronts are degraded and enhanced. This slide incidentally is one of Stewart Turner’s and it shows double-diffusive processes where two water masses, one over here and one over there, are mixing. This seems to be established as a process that is likely to be important in the ocean and he gave examples of observations.

Well, at this stage I have more or less come to the end of my talk. I think that perhaps Warren will grant me a once-only privilege as a rapporteur to show 60 seconds of my own data. What you are seeing is a computer display movie of drifting buoys in the East Australian current system and the interaction of eddies with one another and with the continental shelf. I think it gives an idea of the challenge that faces us in understanding some of the processes in the ocean. One undercurrent at the meeting is the need to get more bright young people into the field, perhaps to get them out of other branches of physics or out of other career streams like medicine. As Sam Slick would say, we want some bright young fellows as sharp as needles.
Summary — Chemistry of the Ocean

EDWARD A. BOYLE

Department of Earth and Planetary Physics, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139, U.S.A.

Perhaps in most people’s minds and certainly as represented by the talks at this meeting, the major problem in chemical oceanography is the carbon dioxide problem. This was represented by several sessions here — one in particular on carbon dioxide and seawater, the transient tracers session, and then also in the geological sessions where paleocarbon dioxide results were discussed. Ten years ago, as probably represented at the last Assembly, the carbon dioxide system, was the subject of strenuous argument where we could not really agree on thermo-dynamic constants for carbon dioxide, or how to measure these properly.

As you saw from Pete Brewer’s talk, all of these problems are now resolved and we have an accurate database, and accurate means for interpreting carbon dioxide parameters in the ocean. We can anticipate that over the next 10 yr, we will actually be able to see changes in the carbon dioxide content of the ocean resulting from the atmospheric increase. Taro Takahashi summarized our current state of knowledge of CO₂ in the oceans quite well.

The transient tracers of the ocean project is largely justified as an attempt to predict the evolution of the carbon dioxide transient in the atmosphere. I suspect for many of those involved with the project the real justification is that it will tell us much about ocean circulation. The transient tracers give us the opportunity to run giant dye-tracer experiments on the whole ocean over the next many years, at least as long as there is tritium left. Freon will let us continue this experiment for many more years into the future. Wolfgang Roether’s talk on the tritium and freons, based on the 1972 data, showed that these tracers contain independent information. As chemical technology is advanced, these measurements can be made rapidly and more precisely. We will be seeing many more exciting results coming from the evolving distribution of transient tracers.

Although calcium carbonate has been a major topic of marine geochemistry in the past, the information that is beginning to emerge from the interpretation of carbon isotope data in deep-sea sediments is very exciting. We saw (in Jean-Claude Duplessy’s talk on glacial carbon isotope stratigraphy and in Nick Shackleton’s talk on longer time scales) that we will be able to look at the evolution of the carbon cycle through time. The next Joint Oceanographic Assembly will reap a harvest of very exciting information on the evolution of the carbon dioxide system and this is going to help us predict the evolution of carbon dioxide in the future.

Of course, chemical oceanography is more than the carbon dioxide problem. As represented at this meeting, chemical oceanography was seen mainly at its boundaries with other disciplines. For example, the transient tracers problem is mainly physical rather than chemical. We also saw the geological boundaries, in particular, the sedimentation flux session, in which we can now directly measure fluxes of chemicals to the seafloor. Through the use of radionuclide and organic geochemical information we can specify how the flux that we see at the seafloor has originated from the overlying water column. The biological boundary, perhaps not discussed so much at this meeting, is obviously an important interface as well. The discovery of the hydrothermal vents at the mid-ocean ridge crest has opened a whole new field of marine biology, and chemists continue to play a role in the question of primary productivity in the ocean. As we saw from John Martin’s talk, there still is debate over how much of a geochemist you have to be before you can measure primary productivity in the ocean. Although these aspects of the boundaries of chemical oceanography and the interactions with other disciplines were very well represented by the meetings here, chemical oceanography, for its own sake, was not much in evidence except at the poster sessions and as parenthetical comments by geochemists talking during other sessions. For example, John Martin very briefly mentioned trace element chemistry in his talk. He certainly has made a major contribution to this field and was not really given the time to talk about it. Ten years ago very little was known about trace element distributions; currently in the pages of Nature, two or three new elements each year are added to the list. The reason I mention the trace elements so strongly, apart from my own biases, is that we’re starting to see how we can perhaps develop a predictive science of chemical oceanography. Michael Bacon’s talk on the use of radionuclides was a good example of this evolving ability. In particular, I refer to a diagram that Mike showed, where, through the use of thorium and protactinium and lead isotopes, and also data on the distribution of copper and manganese in the ocean, chemical process we call scavenging can be drawn together into a very simple and elegant model despite the diverse chemistry of these elements. We may anticipate in the future, as we learn more about the basic surface chemistry of these marine particulates, that we can perhaps even start to predict the chemistry of elements in the ocean before we measure them rather than the other way around as it is done now.

The work on the chemistry of the sediment traps, shows that the other process dominating the chemistry of the ocean, the removal of chemicals from the surface ocean by settling biogenic debris, will also be amenable in the future to a more quantitative approach.

The section which unfortunately missed out on one of the more exciting developments in marine chemistry over the last ten years: the discovery of the hydrothermal vent systems and the recognition that these are
perhaps the dominant force governing the chemistry of the ocean. Ten years ago we would have been using the reverse weathering model of Sillen Garrels, and Mackenzie to explain the chemistry of the ocean. Today there is a rear-guard attempt to defend this but, most people are aiming at proving or disproving the importance of hydrothermal circulation at ridge crests. I believe that the next assembly will be seeing quite a bit more evidence on the extent to which this new discovery actually does influence the chemistry of the ocean.

One other aspect in which chemical oceanography probably will be developing into a more mathematical field perhaps, is in the modelling of chemical distributions of the ocean, combined with physical oceanographic models of deep ocean circulation.

Many years ago Ku and Verhonis used the simple oceanographic model of Stommel and Arons to model the oxygen distribution in the ocean. By the time of the GEOSECS (GEOchemical SECTIONS Study) expedition, this path had been broadened into a super highway in which 20 or so chemical properties were being modelled in the ocean but, in fact, at that point the physical oceanographers had lost confidence in their simple models of ocean circulation and had gone on to more complex models.

From Jorge Sarmiento's talk, we saw a beginning of modeling ocean chemistry using more realistic ocean physics. Over the next several years, we will see progressively more sophisticated coupled physical-chemical models of the ocean and ocean chemistry.

Although there are several exciting new tracers which were mentioned only in passing here, they will probably be more important in the future; for example, argon-39. There are only a dozen or so data points today.

In terms of general interest in marine chemistry, I mentioned carbon dioxide as perhaps the one aspect of marine chemistry that everyone recognizes. We also saw some evidence of an interest in marine pollution, the use of the ocean as waste space, and the transfer of materials from the atmosphere to the ocean.

There was a session on the benthic boundary layer in which we heard talks on the chemical properties of deep-sea sediments and their diagenesis. Over the last 10 yr, chemical oceanography of sediments has evolved from mainly being a study of solid phases (chemistry of sediments and mineralogy) to one in which pure water studies of the distribution and reactivity of chemicals in marine sediments and quantitative modelling of the chemical diagenesis of sediments has become much more important. This was partially represented at this meeting, but I think at the next assembly we probably should see quite a bit more of that.

In summary, I would like to say that this meeting has touched on some of the very exciting aspects of chemical oceanography in its boundaries with other disciplines. It would be very good to see, in future meetings, a little more of chemical oceanography for its own sake. Certainly the aspects of marine chemistry represented at this meeting gave other disciplines a good idea of the uses to which chemical oceanography can be put in their service.
Summary — Biological Oceanography

A. R. LONGHURST

Department of Fisheries and Oceans, Bedford Institute of Oceanography,
P.O. Box 1006, Dartmouth, N.S., Canada B2Y 4A2

Asked if his porridge was good, the Philosopher said: "Perfection is finality. Finality is death. Nothing is perfect. There are lumps in it."

(James Stephens, The Crock of Gold)

In the short time at my disposal, I shall try to give you my personal version of the general trends in our subject of biological oceanography, as illustrated during the last two weeks at this Assembly. Unlike some of my predecessors in this ritual, I shall take a rather optimistic view of our progress — though there are, as the philosopher said, lumps in it — and I shall try to concentrate more on the positive aspects than bemoan the fact that progress is not uniform; in any event, it would be unreasonable to expect that this should be so.

We have made important advances in recent years, and many but not all of these have been reflected in the papers presented to this Assembly. Properly to appreciate the rapidity of our progress, you must put out of your mind any expectation of a kind of ecological plate tectonics breakthrough (which isn't going to happen) and also any thought that biological oceanography is second-rate because it doesn't need large, visible, multiship operations except in unusual circumstances: it has become fashionable to suggest that these two factors have in some way contributed to the relatively unspectacular progress of ecology through its incredibly difficult field in comparison with the spectacular recent progress of the earth scientists and some aspects of ocean physics.

Perhaps at the outset I should remind such doubters that we have indeed come of age; we now have our own second-level acronym — FIBEX, the First International BIOMASS Experiment in the Antarctic Ocean — and are well on our way to planning the second — SIBEX, wouldn't you know?

Because it was suggested, in earlier versions of today's ritual, that biological oceanographers are not very good at integrating their work with that of physicists, I am going to begin by suggesting that the situation today is marked by a renewal of collaboration between ecologists, physicists, and geologists that is very healthy. This stems principally from recent progress in describing the dynamics of mesoscale processes in the ocean; the eddy fields of the northwest Atlantic, off Australia, and in the California region have all seen intense ecological research to investigate the evolution of ecosystems trapped within them, and perhaps more importantly to investigate the consequences for continental shelf ecology of a mesoscale eddy overrunning the shelf. The existence of even smaller coherent eddies able to transport packages of expatriate water and biota right across ocean basins — as in the case of "Meddy" which traversed the MODE pattern on its way westwards across the Atlantic — will surely excite ecological and zoogeographic work in the coming years. Perhaps even more fruitful of collaboration with the physicists has been the development of a general theory for continental shelf frontal systems, at the coastal and oceanward margins, and between shallower and deeper parts of the shelves. In the last decade we have seen a great deal of previously confused description come into focus, and shelf processes at last seem to make sense.

Associated with this real breakthrough in understanding of shelf processes, which would have been impossible without satellite imagery and numerical simulation, has come a renewal of work on the origin and fate of organic sediments produced on continental shelves; this subject was discussed on several days over the past weeks, and it is my belief that geologists will increasingly turn to biologists to understand the origin of massive slumps of organic sediment to the ocean floor, and the ecologists will increasingly need to know what is being slumped in order to confirm their production models. Finally, an understanding that upwelling processes may be caused not only by regional wind fields, but also by distant forcing has led biologists to consider the consequences of Kelvin and Rossby waves, especially in the interpretation of long biological time series. In all these ways, it is no longer true — if indeed it ever was — to regard biological oceanography as a discipline starved of input from the ocean physicists.

In some circles recently, questions have been asked concerning the amount of attention paid by biological oceanographers to the recent advances in holistic and theoretical ecology, with the implication that we of the sea are not really aware of what is going on in the wider discipline of ecology; however, attendance at a recent advanced study institute in Bordeaux would have convinced such doubters that the liaison is really very close and that there is a highly developed invisible college of biological oceanographers preoccupied with the theoretical development of their subject. Much progress has been made in recent years, and although the reductionist approach to ecology will continue to be used and useful, I personally believe that the new Scientific Committee on Oceanic Research (SCOR) Working Group on "Ecological Theory in Relation to Biological Oceanography" that was established here in Halifax during this Assembly represents not a beginning but part of a healthy growing point of biological oceanography. This topic was, I think, underrepresented at this Assembly, but is none the less real for that.

In discussing collaboration between biological oceanographers and other disciplines I also suggest that another field, scarcely heard from at the last Assembly, is opening up and of which we shall hear very much more in the coming years. I refer to paleooceanography, the
history of oceans and ocean basins. It will surely require input from contemporary ecology to make sense of how the oceans, which existed during periods of warm polar regions, could have functioned biologically. It is hard to comprehend an ocean having less than a 10°C temperature range over all latitudes and all depths, or how the deep oceanic biota could have evolved subsequent to anoxic episodes covering whole ocean basins or after events such as the Cretaceous/Tertiary boundary. We shall probably have to turn to the Black Sea as a present-day example of an isothermal ocean during an anoxic episode. It is fortunate we have such an example left to us; what we used to think of as an oddity, we should perhaps now look at as a model of a stage in the evolution of all oceans as we now know them. I expect biological oceanographers will wish to look afresh at the Red Sea and Mediterranean as exemplars of developing ocean basins.

The justification for all of our expensive research is because without it society cannot get answers to some of the large problems of the day: environmental impacts of offshore development, the crisis in fisheries management, and worries about increasing atmospheric CO₂ or changing global climates. How, in this regard, are we measuring up? Change there has certainly been in the last decade, but perhaps not all for the better.

One very striking change for the worse, which was not at all reflected in this Assembly, has been the mushrooming growth of scientific reports produced directly for the environmental review process but which, unlike normal written communications in science to produce "public knowledge," have not been subjected to dispassionate peer review before being used to weigh public decisions. It may be, therefore, that our present system fails to ensure that decisions are taken in the light of the best scientific advice available as a result of much public expenditure.

Turning to what was discussed at this Assembly in applied biology, I take the same view as the speaker who six years ago reviewed biological papers at Edinburgh. Pollution research, as reported to the Assembly, seems to lack direction, and we heard little of what is new and is most likely to be useful — perhaps because the principal advances are currently in physiological fields whose practitioners are not attracted to this Assembly. With a single notable exception, we have heard no descriptions of decadal-scale changes in levels of marine pollution, or in our perceptions of it. Have the doomsdays of the sixties really given place to indifference, and if so is this new lack of concern really justified by a belief that marine pollution effects compared, for example, with the effects on ecosystems produced by fishing activities, are less important than we thought at one time? Although, as we have seen at this Assembly, it is not invariably so, there have been in the past few years sufficient examples of the ecological consequences of fishing that I venture to predict that at the next Assembly this may have become a matter for serious concern.

On the other hand, this Assembly did properly reflect our concern for the current crisis in fisheries management, the reality of which is not in doubt. In recent years, the changing Law of the Sea has placed new responsibilities on coastal states for their offshore fisheries, the global fish catch has been faltering, and several important stocks have collapsed often to be replaced by less desirable species. Finally the deteriorating economic situation has made capital-intensive fishing a very chancy business. At a time when the simplistic mathematical approach to management of earlier decades is being replaced by methods more demanding of ecological understanding, much of what has been discussed at these sessions has had high relevance to the development of the new common-sense approaches to fisheries management that will be so much needed in the coming decades.

In connection both with fisheries problems and the effects of pollution, I have to note the almost complete absence at this Assembly of papers based on analysis of long time series of biological data describing the mechanisms of natural spatial and temporal variability. I suppose that this reflects the generally low level of interest in this activity, which is greatly regretted to be. Apart from data from the commercial fisheries, our programs to monitor biological change in the sea can only be described as trivial compared with what needs to be done. As has been said so often in the past without discernible effect, not only does this mean that we are unable to describe long-term natural biological variability in the ocean but we are almost totally unable to measure the consequences of our fishing or polluting activities against the background of natural changes. All the more important, therefore, to nourish short-cut physiological approaches, and all the more regrettable that they had no place in this Assembly.

Turning now to the basic work that occupies the majority of us, I am of the opinion that striking and important progress has been made since the last Assembly, and that we really have advanced our understanding of how animals and plants exist in the oceans. Many, but not all, of these advances have been reflected in the papers given during the past two weeks. I shall mention only a few of the tangible new findings, concentrating on some of those that were simply not foreseen a decade ago.

Without doubt the most unexpected has been the discovery of a community of animals associated with the hydrothermal vents situated along midocean ridges. Fuelled by sulphur-oxidizing chemotrophic bacteria developed either around the vents, or within the living tissues of the spectacular new organisms themselves as symbionts, these communities of large animals living totally independently of plant production in the upper lighted layers of the ocean will feature prominently in all future ecological texts. Their general impact on abyssal communities is still to be assessed, but we can suppose that it will not prove to be as great as the global geochemical consequences of the vents around which they cluster. Perhaps the most surprising feature of the new findings is the suggestion that the same way of life is everywhere around us in the large, haemoglobin-containing clams of black estuarine muds.
Less spectacular but certainly of more significance in the general economy of the oceans are the very small picoplankton whose existence and importance has come to be appreciated only within the last few years. Chlorophyll-containing, photosynthetic procaryotic cells less than 1 μm in diameter — we now know that they form a significant and sometimes dominant component of plant production in all parts of the ocean, though to a lesser extent in high than in low latitudes. Their extremely small size implies their unusually efficient use of available light and rapid uptake of nutrients, so that there is a great deal still to be learned of the dynamics of their growth. What is known so far is sufficient only to indicate the broad outline of their way of life.

These findings concerning the autotrophic picoplankton will cause us to reexamine much of our previous work on global oceanic plant production, and will certainly give us food for thought when we examine the ecology of the paleoceans where this very primitive life-form was perhaps even more important than it remains in our oceans today.

More locally significant, we now know that at the bottom of shallow seas a very important element of the zooplankton emerges to enter the pelagic ecosystem only at night, hiding during the day in the interstitial water of sandy deposits. The role of this demersal plankton is only just beginning to be worked out, and will certainly modify our thoughts about the ecology of coral reefs and sea-grass meadows. Also during the last few years we have continued our examination of filter feeders other than the copepods which previously occupied us almost exclusively. As a result, we are developing an understanding of how the very smallest food particles can be utilized by large, even very large, organisms.

In the future, we shall probably hear more than we have at this Assembly about the role played by the biota in the oceanic sink for atmospheric CO₂, and how marine plants respond to changing CO₂ availability. The formal papers presented to the Assembly have not reflected recent research into the fate of much of the plant material produced over continental shelves, and the balance between shelf and open ocean pathways in the transport of atmospheric CO₂ into the deep ocean; this was a minor disappointment of the Assembly for me.

Turning now to the other side of the coin, we have also made much progress in measuring our uncertainties. Some things that we thought we understood fairly well now seem again in doubt. For example, our estimates of net oceanic plant production are diverging rather than converging, and a spread of several orders of magnitude is currently under debate. Ten years ago such uncertainty would have seemed impossible. Similarly, we are currently having the greatest difficulty in demonstrating the effects of herbivore grazing on spatial distributions of phytoplankton populations. Not only do the dynamics of seasonal blooms not now apparently demand grazing by herbivores as in the textbook case, but also the details of phytoplankton distribution, such as the formation of the subsurface chlorophyll maximum, appear not to require the differential grazing pressure that undoubtedly exists. This Assembly has shown that we are well aware of these uncertainties. I wonder if those who pay for our advice are as aware as they should be of the level of uncertainty that remains in our understanding of biological processes in the ocean.

Finally, I want to say a few words about where we have been working, and with what tools. One of the trends to be illustrated by this Assembly is our general renewal of interest in high latitude ecosystems. I suppose it is a coincidence that the Antarctic Treaty will be due for renewal in a few years, and that in Canada we are much concerned about the effects of massive tanker traffic through the Arctic during the next decade, but the result has been a renewal of Arctic and Antarctic biology; this time we are deploying the most recently available tools of oceanography, very different from those used by the DISCOVERY expeditions that laid so much of the groundwork of Antarctic science. It will be interesting to see what we make of the old problems with our new skills; this Assembly has pointed the way things are likely to go in the next decade.

I think that there is also a new realism abroad concerning the tools at our disposal. Unlike a previous commentator, I don't find that biologists are now shy of mathematical and computer techniques — in fact, rather the reverse, for they realize it is only by such means that they can hope to make sense out of their multivariate subject. Realism, too, has come to the use of such special tools as the large plastic bags, or mesocosms, that were so fashionable at the time of the last Assembly and so ignored this time. From being the answer to all our prayers, they have become what they should have been all along, just another shot in our locker — very useful for some things, not very good for others. We now see them as no substitute for work at sea, but as an experimental adjunct to it.

Another tool very susceptible to enthusiasm appears to be settling down, too. After several decades of development, remote sensing of the sea surface from earth-orbiting satellites is now capable of producing data of value to biological oceanography; despite what has sometimes been claimed, the presentations during this Assembly appear to confirm that most of the profit to biologists will be indirect. What is observable in radiation signals from the sea surface can bear only a statistical relationship with biological processes down through the water column, but the physics of the sea surface that can be observed in satellite imagery directly reflects regional processes that involve the whole water column. Elevation and slope of the sea surface are capable of measurement, and are direct consequences of regional circulation patterns in a way that cannot be hoped to have biological analogues. Nevertheless, development is proceeding on refining the precision and sensitivity of instruments capable of sensing surface chlorophyll concentrations over wide areas; what use they can be put to may become clearer by the next time we meet.

More generally, the electronic revolution has finally come of age in the last few years and biological oceanographers have not been backward in utilizing its
possibilities. Now that we can put microprocessors to work for us in subsurface instrumentation, the capability of our new tools for sensing, observing, capturing, and manipulating throughout the whole depth of the water column is limited only by our imagination and our budget.

My final comment concerns the way in which we deploy our resources at sea; we are frequently criticized, mostly by our colleagues in the funding agencies, for the fact (as I have already mentioned) that we tend not to work as large coordinated teams in the same way as physicists, especially those concerned with air/sea interaction processes, commonly do. I personally attribute this not to disdain towards planning on our part but to the different nature of the problems facing physicists and biologists; when the problems have a spatial component on the meso- or macro-scale, biologists have no difficulty in coordinating large programs like FLEX, CUEA, FIBEX/Biomass, CalCOFI, and the Warm/Cold Core Rings experiments; where we perhaps fail is to intercalibrate our techniques at sea together, or collectively to test hypotheses that are troubling us, in the same way that has been done by the optical and chemical oceanographers. Perhaps we can hope that this will be one of the trends to occupy us until next time we meet.

As you may have detected from my remarks, I find myself more interested in the good porridge than in the lumps floating in it; I am convinced that too great preoccupation with those bits of our subject that are not advancing as fast as the rest is counterproductive, and I have a real hope that when we all meet again in 1988, my quotation of the philosopher’s comments will no longer be appropriate.
LIST OF PARTICIPANTS

Aagaard, Knut  
School of Oceanography  
University of Washington  
Seattle, Washington 98195  
USA  
Tel.: (206) 543-7300

Abdelrazik, Sobhy M.  
504-1094 Wellington Street  
Halifax, Nova Scotia B3H 2Z9  
CANADA  
Tel.: (902) 422-7732

Ackroyd, David Roger  
Plymouth Polytechnic  
Drakes Circus  
Plymouth, Devon  
UNITED KINGDOM

Addison, Richard  
Marine Ecology Laboratory  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3279

Airey, Doris  
CSIRO Division of Oceanography  
P.O. Box 21  
Cronulla, N.S.W. 2230  
AUSTRALIA  
Tel.: 523-6222

Alicaraz, Miguel  
Instituto de Investigaciones Pesqueras  
Paseo Nacional S/N  
Barcelona 3  
SPAIN  
Tel.: (003) 310-6416

Aless, Anwar C.C.  
Faculty of Science  
King Abdul Aziz University  
P.O. Box 9028  
Jeddah  
SAUDI ARABIA

Alexander, Harold C.  
Mechanical Engineering Department  
Technical University of Nova Scotia  
P.O. Box 1000  
Halifax, Nova Scotia B3J 2X4  
CANADA  
Tel.: (902) 429-8300

Alexander, Vera  
Institute of Marine Science  
University of Alaska  
Fairbanks, Alaska 99701  
USA  
Tel.: (907) 474-7531

Al-Kaisi, Kamal A.  
Arabian Gulf University Project  
P.O. Box 26809  
BAHRAIN, (ARABIAN GULF)

Allan, Tom  
Institute of Oceanographic Sciences, Wormley  
Godalming, Surrey GU8 5UB  
ENGLAND

Allanson, Brian R.  
Department of Zoology  
Rhodes University  
P.O. Box 94  
Grahamstown,  
SOUTH AFRICA  
Tel.: 461-2428

Allen, John S.  
School of Oceanography  
Oregon State University  
Corvallis, Oregon 97331  
USA  
Tel.: (503) 754-2928

Almagor, G.  
Marine Geology Division  
Geological Survey of Israel  
30 Malkhe Yisrael Street  
Jerusalem 95501  
ISRAEL

Amos, Carl  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
Tel.: (902) 426-7736

Anderson, Carl  
25 Lawnsdale Drive  
Dartmouth, Nova Scotia B3A 2N1  
CANADA  
Tel.: (902) 469-9756

Anderson, Frank  
NRIO/CSIR  
P.O. Box 320  
Stellenbosch 7600  
SOUTH AFRICA

Anderson, Robert  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3181

Andrews, Daniel  
Université du Québec à Rimouski  
Rimouski, Québec  
CANADA  
Tel.: (418) 724-1773

Angel, Martin V.  
Institute of Oceanographic Sciences  
Wormley, Godalming  
Surrey GU8 5UB  
UNITED KINGDOM  
Tel.: (042) 879-4141
Antezana-Jerez, T.
Department of Oceanography
Universidad de Chile de Valparaiso
Casilla 13D, Vina Del Mar
CHILE
Aoki, Saburo
3-14-29 Akabanedai
Kita-Ku, Tokyo 115
JAPAN
Apel, John R.
Applied Physics Laboratory
Johns Hopkins University
Johns Hopkins Road
Laurel, Maryland 20707
USA
Tel.: (301) 953-7100
Arai, Mary Needler
Department of Biology
University of Calgary and
Pacific Biological Station
Nanaimo, British Columbia V9R 5K6
CANADA
Tel.: (604) 758-5202
Aranuvachapun, Sasithorn
Department of Oceanography, Code 68AU
Naval Postgraduate School
Monterey, California 93940
USA
Tel.: (408) 646-3226
Ayala-Castanarres, A.
Instituto de Ciencias Del Mar
University of Mexico
Apartado Postal 70-157
Mexico 04320
MEXICO
Tel.: 548-2766
Bacon, Michael P.
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543
USA
Tel.: (617) 540-1090
Baig, Stephen Robert
US Department of Commerce
1300 Harrison Street
Hollywood, California 33019
USA
Tel.: (305) 350-4310
Department of Oceanography
University of Washington
Seattle, Washington 98195
USA
Tel.: (206) 543-7160
Baker, Karen S.
Scripps Institution of Oceanography
University of California
San Diego A-030
La Jolla, California 92093
USA
Tel.: (714) 452-2350
Balch, Norval
Institute of Oceanography
Aquatron Laboratory
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3874
Bardach, John E.
Resources Systems Institute
East-West Center
1777 East West Road
Honolulu, Hawaii 96848
USA
Tel.: (808) 944-7510
Barlaam, Alessandro
Consiglio Nazionale delle Ricerche
Piazzale Aldo Moro 7
00185 Rome
ITALY
Barross, John A.
School of Oceanography
Oregon State University
Corvallis, Oregon 97331
USA
Tel.: (503) 754-3597
Barron, Eric J.
National Center for Atmospheric Research
P.O. Box 3000
Boulder, Colorado 80307
USA
Tel.: (303) 494-5151
Barstow, S.F.
Continental Shelf Institute
P.O. Box 1883
Hakon Magnussonsgt. 1B
Trondheim 55548
NORWAY
Tel.: 15660
Barth, Hartmut
Commission of the European Communities
200, rue de la Loi
B-1049 Brussels
BELGIUM
Bary, Brian Mck.
Department of Oceanography
University College
Galway
IRELAND
Tel.: 091-7894
Basso, Diane
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-9501
Battaglia, Bruno
Institute di Biologia del Mar CNR,
Riva 7 Martini
1364/A Venice
ITALY
Bayoumi, Ahmad Al-Refai
Institute of Oceanography & Fisheries
A.S.R.T.
101 Kasr El Ainy Street
Cairo
EGYPT
Be, Allan W. H.
Lamont-Doherty Geological Observatory
Palisades, New York 10964
USA
Tel.: (914) 359-2900

Bedell, Elizabeth
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
Tel.: (902) 424-7007

Behairy, A. K.
Faculty of Marine Science
King Abdul Aziz University
Jeddah
SAUDI ARABIA

Behensky, James F. Jr.
Joint Oceanographic Institutions Inc.
2600 Virginia Avenue N.W., #512
Washington, D.C. 200372
USA
Tel.: (202) 333-8276

Behrman, Daniel
Foundation for Ocean Research
11696D Sorrento Valley Road
San Diego, California 92121
USA
Tel.: (714) 453-6550

Belzile, Nelson
310, des Ursulines
Rimouski, Québec G5L 3A1
CANADA

Bennett, Andrew
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
Tel.: (902) 426-2039

Berger, Anthony R.
Department of Energy, Mines and Resources
601 Booth Street, Room 177
Ottawa, Ontario K1A 0E8
CANADA
Tel.: (613) 995-4927

Berner, Robert Arbuske
Department of Geology and Geophysics
Yale University
New Haven, Connecticut 06511
USA
Tel.: (203) 436-4526

Biggs, Douglas C.
Department of Oceanography
Texas A&M University
College Station
Texas 77843
USA
Tel.: (713) 845-3423

Bigo, Ralph
Canadian Forces Weather Services
Site 30, P.O. Box 8, RR #2
Windsor Junction, Nova Scotia B0N 2V0
CANADA

Blaber, S.J.M.
Zoology Department
University of Natal
P.O. Box 375
Pietermaritzburg, Natal 3200
SOUTH AFRICA

Blackburn, T.H.
Institute for Ecology and Genetics
Aarhus University
Munkegade
DK 8000 Aarhus C
DENMARK

Blasco, François
C.N.R.S.
39, allée Jules-Guesde
31400 Toulouse
FRANCE

Bobbitt, Judith
Memorial University
P.O. Box 8021
St. John's, Newfoundland A1B 3M7
CANADA
Tel.: (709) 335-2683

Boisson, M.
Centre scientifique de Monaco
16, boulevard de Suisse
Monte Carlo
MONACO

Boller, Jack W.
National Academy of Sciences
2101 Constitution Avenue
Washington, D.C. 20418
USA
Tel.: (202) 334-3119

Bonn, Ferdinand
Université de Sherbrooke
Sherbrooke, Québec J1K 2R1
CANADA
Tel.: (819) 565-4523

Bouchard, Ginette
Université du Québec à Rimouski
310, des Ursulines
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1784

Boudreau, Bernard
Department of Geology
Yale University
P.O. Box 6666
New Haven, Connecticut 06511
USA
Tel.: (203) 436-0721

Bowden, Kenneth F.
Oceanography Department
University of Liverpoolool
P.O. Box 147
Liverpool L69 3BX
UNITED KINGDOM
Tel.: (051) 709-6022

Bowen, Anthony J.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-7082
South Australia 5042
AUSTRALIA
Tel.: (088) 275-3911

Callison, Richard David
"Craignlea"
95 Dundee Road, West Ferry
Dundee
SCOTLAND

Calvert, S.E.
Department of Oceanography
University of British Columbia
Vancouver, British Columbia V6T 1W5
CANADA
Tel.: (604) 228-2482

Cameron, Heather A.
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 996-5783

Campbell, John S.
MDMD-MET ESO
Robert Bosch Street 5
6100 Darmstadt
FEDERAL REPUBLIC OF GERMANY
Tel.: (615) 188-6545

Campbell, Neil J.
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2039

Cary, Andrew G.
School of Oceanography
Oregon State University
Corvallis, Oregon 97331
USA
Tel.: (503) 754-2525

Carney, Robert S.
121-12th SE #503
Washington, D.C. 20003
USA
Tel.: (202) 547-5369

Carritt, Dayton E.
4 Eaton Crescent
Amherst, Massachusetts 01002
USA
Tel.: (413) 253-5725

Carstens, Torkild
Norwegian Hydrodynamics Laboratories
P.O. Box 4115 Valentinlyst
700 Trondheim
NORWAY
Tel.: 759-2300

Cartwright, David E.
Institute of Oceanographic Sciences
Bidston Observatory
Birkenhead L43 7RA
UNITED KINGDOM
Tel.: (051) 653-8633

Cedeno, Gilberto
Instituto Oceanographico
Apartado 94
Universidad de Oriente
Cumana
VENEZUELA

Cederlof, Ulf
Institute of Oceanography
P.O. Box 4038
S-400, 40 Goteborg
SWEDEN

Chandy, John V.
Engineers India Ltd.
H 5, 10 Malviya Nagar
New Delhi 110017
INDIA
Tel.: (033) 112-5134

Chapman, Anthony
Department of Biology
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-2160

Charnock, Henry
Department of Oceanography
University of Southampton
Southampton S095NH
UNITED KINGDOM

Checkley, David
Marine Sciences Institute
University of Texas at Austin
Port Aransas, Texas 78373
USA
Tel.: (512) 749-6711

Chen, L.C.
National Research Council
1411 Oxford Street
Halifax, Nova Scotia B3H 3Z1
CANADA
Tel.: (902) 426-3241

Chen, Senqiang
South China Sea Institute of Oceanology
58 Xinggang Road
Guangzhou
CHINA

Cheney, Jerry
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA

Cheng, Ruixiang
Third Institute of Oceanography
Xiamen, Fujian
CHINA

Chesselet, Roger
CNRS
15, Quai Anatole
Paris 75700
FRANCE

Chin, Yun Shan
Institute of Oceanology
7 Nan Hai Road
Quindao
CHINA
Chouchan, M.S.
13, rue Poirier de Narcy
Paris 75014
FRANCE
Tel.: 539-4930

Chriess, Terry
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-8849

Church, John Alexander
Division of Oceanography CSIRO
P.O. Box 21
Cronulla N.S.W. 2230
AUSTRALIA

Church, Thomas M.
College of Marine Studies
University of Delaware
Newark, Delaware 19711
USA
Tel.: (510) 664-1455

Cintron-Molero, Gilberto
Department of Natural Resources
P.O. Box 5584
Puerto de Tierrito 00902
PUERTO RICO

Clark, Candye
10C–UNESCO
7. Place de Fontenoy
Paris 75700
FRANCE

Clark, Robert Louis
Department of Oceanography
U.S. Naval Academy
Annapolis, Maryland 21406
USA
Tel.: (301) 267-3561

Clarke, R. Allyn
Beford Institute of Oceanography
Atlantic Oceanographic Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3144

Clotworthy, John H.
Joint Oceanographic Institutions Inc.,
2600 Virginia Ave. N.W.
Washington, D.C. 20037
USA
Tel.: (202) 333-8276

Collin, Arthur
Department of Energy, Mines and Resources
580 Booth Street
Ottawa, Ontario K1A 0E4
CANADA
Tel.: (613) 996-1331

Collison, W.
21 Gilroy Street
Smiths Falls, Ontario
CANADA

Conover, Robert
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2817

Cooote, Arthur R.
Chemical Oceanography Division
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2528

Corless, Bruce H.
Woods Hole Oceanographic Institute
Woods Hole, Massachusetts 02543
USA
Tel.: (617) 548-4480

Cornford, Alan B.
Institute of Ocean Sciences
P.O. Box 6000
9860 West Saanich Road
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-8335

Corredor, Jorge
Department of Marine Sciences
University of Puerto Rico
Mayaguez 00708
PUERTO RICO
Tel.: (809) 889-2482

Cossa, Daniel
Sciences et levés océaniques
Sciences de la mer
B.P. 15500
901 Cap Diamant, Québec
Québec G1K 7X7
CANADA
Tel.: (418) 694-7781

Costlow, John D.
Duke University Marine Laboratory
Beaufort, North Carolina 28516
USA
Tel.: (919) 728-2111

Couture, Richard
2, rue Wolfe
Lévis, Québec G6V 3X2
CANADA
Tel.: (418) 883-1422

Crawford, William R.
Institute of Ocean Sciences
P.O. Box 6000
Sidney, British Columbia V8L 4D2
CANADA
Tel.: (604) 656-8369

Crease, James
Institute of Oceanographic Sciences
Wormley, Godalming
Surrey
UNITED KINGDOM

Creeden, John J.
17 Murray Circle
Deacon, Margaret
Institute of Oceanographic Sciences
Wormley, Godalming
Surrey GU8 5UB
UNITED KINGDOM
Tel.: (042) 879-4141

De Cae, Roland
Huskey Oil Operations Ltd.
P.O. Box 6525
Calgary, Alberta T2P 3G7
CANADA

Degens, Egon
Geologische Institut
Bundesstrasse 55
2000 Hamburg 13
FEDERAL REPUBLIC OF GERMANY

Delaney, Margaret Lois
Massachusetts Institute of Technology
E34-205 MIt
Cambridge, Massachusetts 02139
USA
Tel.: (617) 253-5733

Delecluse-Roy, Pascale
Musée National d'histoire naturelle
Laboratoire d'oceanographie physique
43–45, rue Cuvier
75231 Paris
FRANCE

Demers, Serge
Centre Champlain
B.P. 15500
901 Cap Diamant, Québec
Québec G1K 7Y7
CANADA
Tel.: (418) 694-7781

Deming, Jody W.
Marine Biology Research Division A-002
University of California at San Diego
La Jolla, California 92037
USA
Tel.: (714) 452-2935

Dennman, Kenneth L.
Institute of Ocean Sciences
P.O. Box 6000
9860 West Saanich Road
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-8346

De Oliveira, E.
Departamento Botanica
Universidad Sao Paulo
C. Postal 11461
Sao Paulo
BRAZIL

De Verdiere, Colin
Centre océanographique de Bretagne
B.P. 337
Brest 29273
FRANCE
Tel.: (009) 845-8055
Devonald, F. Kim
National Academy of Sciences. Ocean Policy Committee
2101 Constitution Avenue
Washington, D.C. 20418
USA
Tel.: (202) 334-2817

Dinn, Don
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3700

Di Santillio Scotto, Salvatore
Consiglio Nazionale delle Ricerche
Piazza Aldo Moro 7
00185 Rome
ITALY

Djurfeldt, Leif
Institute of Oceanography
P.O. Box 4038
S-400 40
Goteborg
SWEDEN
Tel.: (003) 112-8013

Dobec, James
Marine Environmental Data Services Branch
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2011

Dobson, Frederick W.
Bedford Institute of Oceanography
Ocean Circulation Division
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3584

Dolezalek, Hans
1812 Drury Lane
Alexandria, Virginia 22307
USA
Tel.: (703) 765-3771

Dou, Zhenxing
Institute of Marine Environment Protection
Dalian
CHINA

Dowidar, Naim
Department of Oceanography
King Abdul Aziz University
P.O. Box 1540, Jeddah,
SAUDI ARABIA

Drewry, Joanna
52 Scrivens Street
Ottawa, Ontario K2B 6H1
CANADA
Tel.: (613) 995-2075

Drinkwater, Kenneth
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3582

Dubois, Jean-Marie M.
Université de Sherbrooke
Sherbrooke, Québec J1K 2R1
CANADA
Tel.: (819) 565-4523

Duce, Robert Arthur
Graduate School of Oceanography
University of Rhode Island
Kingston, Rhode Island 02881
USA
Tel.: (401) 792-6256

Dudley, Walter C.
Department of Oceanography
University of Hawaii
2525 Correa Road,
Honolulu, Hawaii
USA

Dunbar, Maxwell J.
Institute of Oceanography
McGill University
Eaton Building
3620 University Street
Montreal, Quebec H3A 2B2
CANADA
Tel.: (514) 392-5714

Duplessy, J.C.
Centre des faibles radioactivité
91190 Gif-sur-Yvette
FRANCE
Tel.: 907-7828

Durpaire, Jean Pierre
Agence spatiale européenne
18, avenue Ed. Belin
31055 Toulouse
Cedex
FRANCE
Tel.: (336) 153-1112

Durvasula, Subbarro
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3837

Dyrssen, David
Chalmers University
S-41296 Goteborg
SWEDEN
Tel.: (003) 181-0100

Edel, Howard R.
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2045

Edwards, Alasdair
The University
Zoology Department
Newcastle Upon Tyne NE1 7RU
UNITED KINGDOM

Edwards, Karen
Institute of Marine Affairs
P.O. Box 3160
Eriksson, Victor
The International Council for Scientific Exploration
P.O. Box 1
Stockholm 118 26
Sweden
Tel.: (18) 338-08-54

Eisen, Robert
Institut für Meeresforschung
Kaiserswerther Allee 6
2000 Hamburg 70
Germany
Tel.: (040) 360-55

Elmgren, Rolf
University of Umeå
Department of Biology
S-901 87 Umeå
Sweden
Tel.: (090) 962-62

Elsveld, J.J.
Netherlands Institute for Sea Research
P.O. Box 59
Texel
NETHERLANDS

Elter, Bernhard
Biologische Anstalt Helgoland
Litoralstation
Hafenstr. 3D-2282
FEDERAL REPUBLIC OF GERMANY

Elizarov, Anatoli
All Union Research Institute of Marine Fisheries and
Oceanography
17 Krasnoselskaya
R-140 Moscow
USSR

Elliott, James A.
Bedford Institute of Oceanography
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2502

Ellis, Celia A.
Institute of Oceanographic Sciences
Brook Road
Wormley
Godalming, Surrey GU8 5UB
UNITED KINGDOM
Tel.: (042) 879-4141

El-Sabbh, Mohammed I.
Département d’océanographie
Université du Québec
300, des Ursulines
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1755

El-Sayed, Sayed Z.
Department of Oceanography
Texas A&M University
College Station
Texas 77843
USA
Tel.: (713) 845-2134

Elsherif, Hesham
Oceanic Institute
University of Hawaii
Honolulu, Hawaii 96822
USA
Tel.: (808) 832-5200

Elnes, Abdirahman
Islamic University
College of Science
Department of Oceanography
P.O. Box 21363
Madina
SAUDI ARABIA

El-Sayed, Sayed Z.
Department of Oceanography
Texas A&M University
College Station
Texas 77843
USA
Tel.: (713) 845-2134

Eppler, Richard W.
1969 Loring Street
San Diego, California 92109
USA
Tel.: (714) 452-2338
Ferraz, Luiz Antonio de Carvalho
Diretoria de Hidrografia E Navegação, Ilha Fiscal
20.091 Rio de Janeiro
RJ BRAZIL
Tel.: (021) 253-5583

Field, John G.
Zoology Department
University of Capetown
Rondebosch 7700
SOUTH AFRICA

Fieux, Michèle
Musée national d'histoire naturelle,
Laboratoire d'océanographie physique
43–53, rue Cuvier
75005 Paris
FRANCE

Fine, Rana H.
National Science Foundation
1800 G Street N.W.
Washington, D.C. 20006
USA
Tel.: (703) 357-7906

Fischer, Alfred
544 Alexander Road
Princeton, New Jersey
USA

Fisher, Nicholas S.
IAEA Laboratory of Marine Radioactivity
Musée océanographique
PRINCIPALITY OF MONACO
Tel.: (993) 330-1514

Fiuza, Armando F.G.
Physics Department and Geophysical Centre
University of Lisbon
Rua da Escola Politecnica–58
1200 Lisbon
PORTUGAL
Tel.: 608-0289

Fleet, Andrew James
British Petroleum Research
Chertsey Road
Sunbury-on-Thames
Middlesex
UNITED KINGDOM
Tel.: (081) 234-8251

Flemming, B.W.
Department of Geology
University of Capetown
Rondebosch 7700
SOUTH AFRICA

Flemming, N.C.
Institute of Oceanographic Sciences
Wormley
Godalming, Surrey
UNITED KINGDOM

Flos, Jordi
C. Rocafort 246
Barcelona
SPAIN

Fofonoff, Nicholas P.
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543

USA
Tel.: (617) 548-1400

Fogg, G.E.
Marine Science Laboratories
Menai Bridge
Anglesey, Gwynedd LL59 5EH
UNITED KINGDOM
Tel.: (024) 871-2641

Foldvik, Arne
Geophysical Institute
University of Bergen
5000 Bergen
NORWAY

Ford, William L.
9 Boulderwood Road
Halifax, Nova Scotia B3P 2J3
CANADA
Tel.: (902) 477-4265

Foster, Theodore Dean
Applied Sciences
University of California
Santa Cruz, California 95064
USA
Tel.: (408) 429-4780

Fournier, Robert O.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia
CANADA

Fowler, Scott
Laboratory of Marine Radioactivity
Musée océanographique
Monte Carlo 9800
MONACO
Tel.: (933) 015-1400

Fraga, F.
Instituto de Investigaciones Pesqueras
Muelle de Bomzas
Vigo
SPAIN

Franco, A.S.
R. Das Granjas, 11
Granja Viana
Cotia (Sp) 06700
BRAZIL

Freeland, Howard
Institute of Ocean Sciences
P.O. Box 6000
Sidney, British Columbia V8L 4B2
CANADA

Freeman, Nelson G.S.
Bayfield Laboratory
P.O. Box 5050
Burlington, Ontario L7R 4A6
CANADA
Tel.: (416) 637-4380

Frouin, Robert J.
Laboratoire d'optique atmosphérique
Université des Sciences
59650, Villeneuve d'Asco
Lille
FRANCE
Tel.: (002) 091-9222
Fu, Tianbao
Chemical Oceanography Division
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3978

Fuerterer, Dieter Karl
Universitaet Kiel
Olshausen Str 40–60
2300 Kiel
FEDERAL REPUBLIC OF GERMANY

Funnell, Brian M.
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
UNITED KINGDOM

Gagnon, Jean
Marine Environmental Data Service
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2007

Gagnon, Mario
INRS-Océanologie
310, avenue des Ursulines
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1747

Gallagher, James J.C.
US Navy Underwater Systems Center
New London, Connecticut 06320
USA
Tel.: (203) 447-4673

Galocha, Rene Garcia
United Nations Development Project (Cuba)
1 United Nations Plaza,
New York, New York
USA

Garcia, Maria Laura
Institut Nacional de Pesca
Casilla 5918
Guayaquil
ECUADOR
Tel.: 040-1773

Gardner, Grant A.
Department of Biology
Memorial University
St. John's, Newfoundland A1B 3X9
CANADA
Tel.: (709) 737-7524

Garrett, Ann E.
Institute of Ocean Sciences
P.O. Box 6000
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-8254

Garrett, C.J.R.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA

Garrett, John
Institute of Ocean Sciences
P.O. Box 6000
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-8274

Gautier, Catherine
California Space Institute
Scripps Institution of Oceanography
La Jolla, California 92039
USA
Tel.: (714) 452-4936

Gendron, André
Université du Québec à Rimouski
310, avenue des Ursulines
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1747

Gerbier, Alain
1390 Sherbrooke Street West
Montréal, Québec H3G 1J9
CANADA

Gerges, Markram A.
UNDP-Tripoli, Libya
P.O. Box 20, G.C.P.O.
New York, New York 10017
USA
Tel.: (212) 214-5260

Gershey, Robert
Atlantic Research Laboratory
1411 Oxford Street
Halifax, Nova Scotia B3H 3Z1
CANADA
Tel.: (902) 426-8280

Gilat, Eliezer
Ministry of Research
Agriculture Research Organization
P.O. Box 699
Haifa
ISRAEL

Gilmartin, Malvern
Center for Marine Studies
14 Coburn Hall
University of Maine
Orono, Maine
USA
Tel.: (207) 581-2587

Given, Robert R.
P.O. Box 398
Avalon, California 90704
USA
Tel.: (213) 743-4113

Glennie, Charles
Marine Environmental Data Services Branch
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2014

Godshall, Fredric A.
National Oceanic and Atmospheric Administration
1036 Pinecrest Drive
Annapolis, Maryland 21403
USA
Tel.: (202) 634-7379
Godson, Warren
Atmospheric Environment Service
4905 Dufferin Street
Downsview, Ontario M3H 5T4
CANADA
Tel.: (416) 667-4919

Goldberg, Edward D.
Scripps Institution of Oceanography
La Jolla, California 92093
USA
Tel.: (714) 452-2407

Gonella, Joseph
Musée national d’histoire naturelle
Laboratoire d’océanographie physique
43-53, rue Cuvier
75005 Paris
FRANCE

Goodfellow, Ron
Goodfellow Associates
61 Southwark Street
London SE1 1SA
UNITED KINGDOM
Tel.: (001) 928-8999

Goodman, Keith Stewart
BP International Ltd.
Britannic House
Moor Lane
London
UNITED KINGDOM

Goodman, Robert
Elsevier Scientific Publishing Co. Inc.
52 Vanderbilt Avenue
New York, New York 10017
USA
Tel.: (212) 867-9040

Gordon, Donald
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3278

Gower, J.F.R.
Institute of Ocean Sciences
P.O. Box 6000
Sidney, British Columbia
CANADA
Tel.: (604) 656-8258

Grainger, Richard
Department of Fisheries
Fisheries Research Laboratory
Abbotstown
Co Dublin
IRELAND

Greatbatch, Richard John
Department of Applied Mathematics
Silver Street
Cambridge CB3 9EW
UNITED KINGDOM

Greenberg, David A.
Atlantic Oceanographic Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2431

Gross, Grant
National Science Foundation
1800 G. Street N.W.
Room 609
Washington, D.C. 20550
USA

Groustra, T.F.
Ministerium Education & Science
2 Stadhouders Plantsden
The Hague 2500EP
NETHERLANDS

Grundlingh, M.L.
CSIR
P.O. Box 17001
Conagella 4013
SOUTH AFRICA

Gu, Hongkan
Institute of Oceanology
Academia Sinica
7 Nan-Hai Road
Quindao
CHINA

Guinasso, Norman L.A. Jr.
Department of Oceanography
Texas A&M University
College Station
Texas 77843
USA
Tel.: (713) 845-7031

Guza, Robert T.
Scripps Institution of Oceanography
La Jolla, California 92093
USA
Tel.: (714) 452-4334

Hakim, Abdul
Department of Marine Biology
University of Chittagong
Chittagong
BANGLADESH

Hall, John Kendrick
Marine Geology and Geomathematics
30 Malchei Israel Street
Jerusalem 95501
ISRAEL

Hamilton, Douglas
Department of Geology
University of Bristol
Bristol BS8 1TR
UNITED KINGDOM
Tel.: (002) 722-4161

Hamilton, Gordon R.
Office of Naval Research
Arlington, Virginia 22217
USA
Tel.: (202) 696-4398

Han, Gregory
Science Applications Inc.
Heacock, John G.
Office of Naval Research Code 425
Arlington, Virginia 22217
USA

Head, Erica
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2817

Healey, Patrick M.
National Water Research Institute
867 Lakeshore Road
P.O. Box 5050
Burlington, Ontario
CANADA
Tel.: (416) 637-4215

Heath, G. Ross
School of Oceanography
Oregon State University
Corvallis, Oregon 97331
USA
Tel.: (503) 754-4763

Helleur, Robert
8E-244 Sir John A. MacDonald
Kingston, Ontario K7M 5W9
CANADA
Tel.: (613) 547-3019

Hemmen, G.E.
The Royal Society
6 Carlton House Terrace
London SW1Y 5AG
UNITED KINGDOM

Hempel, Gotthilf
Institute for Polar Research
Columbus-Center
2850 Bremerhaven
FEDERAL REPUBLIC OF GERMANY

Hendry, Malcolm
Geology Department
University of West Indies
Mona, Kingston 7
JAMAICA

Hendry, Ross M.
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3142

Herman, Alex
Department of Fisheries and Oceans
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2446

Herranz, Pedro
Instituto Espanol de Oceanografia
Departamento de Geologia Marina
Alcala 27
Madrid 14
ESPAÑA

Box 338
Key Biscayne, Florida 33149
USA
Tel.: (305) 361-3700

Hanna, Rifaat G. Messika
Institute of Oceanography Egypt
11 Abassy Street
Cairo
EGYPT

Hannon, James M.
Sippican Ocean Systems Inc.
7 Barnabas Road
Marion, Massachusetts 02738
USA
Tel.: (617) 748-1160

Hare, Kenneth
Trinity College
Toronto, Ontario
CANADA

Hargave, Barry
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3188

Harrison, William
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3837

Harvey, John
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
UNITED KINGDOM

Hassan, E. Mohamed
Department of Marine Sciences
Qatar University
Box 2713
Doha
QATAR

Havard, David A.
Plessey Marine Research Unit
Wilkinthrop House
Templecombe, Somerset
UNITED KINGDOM
Tel.: (009) 657-0551

Hay, William W.
Joint Oceanographic Institutions Inc.
2600 Virginia Avenue N.W.
Suite 512
Washington, D.C. 20037
USA
Tel.: (202) 333-8276

Hayes, Dennis E.
Lamont-Doherty Geological Observatory
Palisades, New York 10964
USA
Tel.: (914) 359-2900
Herrera, Luis
Inteves S.A.
P.O. Box 76343
Caracas
VENEZUELA

Hessland, Ivar R.
Geologiska Institutionen
Box 6801
S-113 86 Stockholm
SWEDEN

Heydorn, Allan E.F.
NSIO
CSIR
P.O. Box 320
Stellenbosch 7600
SOUTH AFRICA

Hibiya, Toshiyuki
Earthquake Research Institute
University of Tokyo
13-4 Kugayama
5-Chome Suginami-Ku
Tokyo 168 B3H 122
JAPAN
Tel.: (003) 333-9087

Hilde, Tom
Geodynamics Research Program
Texas A&M University
College Station
Texas 77843
USA
Tel.: (713) 845-8487

Hill, H. W.
Ministry of Agriculture
Lowestoft
Suffolk NR33 OHT
UNITED KINGDOM

Hill, Phillip R.
Bedford Institute of Oceanography
Atlantic Geoscience Centre
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3932

Hills-Colinvaux, Llewellya
Ohio State University
Department of Zoology
484 W. 12th Avenue
Columbus, Ohio 43210
USA
Tel.: (614) 422-9632

Hirotta, Itaru
Tokai University
1000 Orito
Shimizu-Shi
Shizuoka-Ken
JAPAN
Tel.: (054) 334-0411

Hisard, Philippe
Antenne ORSTOM/COB
B.P. 337, 29273 Brest
FRANCE

Holland, William R.
National Center for Atmospheric Research

P.O. Box 3000
Boulder, Colorado 80303
USA
Tel.: (303) 494-2067

Holloway, Peter
Department of Civil Engineering
University of Western Australia
Nedlands, W.A. 6009
AUSTRALIA

Holman, Rob
School of Oceanography
Oregon State University
Corvallis, Oregon 97331
USA
Tel.: (503) 754-2296

Honojo, Susumu
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02540
USA
Tel.: (617) 548-1162

Hooker, Richard
Marine Science Research Laboratory
Memorial University
St. John's, Newfoundland
CANADA

Horne, Edward
Bedford Institute of Oceanography
Marine Ecology Laboratory
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 861-2147

Horton, Charles
Naval Oceanographic Office
Code 9110, NSTL Station
Bay St. Louis, Mississippi 39522
USA
Tel.: (601) 688-4135

Hoshino, Michihei
Tokyo University
1-19-4 Kohinata
Bunkyo-Ku
Tokyo
112 JAPAN

Hoyle, Phyllis
Institute of Marine Affairs
Box 3160 Carenage Post Office
Carenage, Trinidad
WEST INDIES
Tel.: (625) 102-1491

Hu, Zhenhou
Foreign Affairs Bureau
Chinese Academy of Sciences
Peking
CHINA

Huang, Joseph Chi Kan
NOAA Office of Research and Development
Special Programs Office
6010 Executive Boulevard
Rockville, Maryland 20852
USA
Tel.: (301) 443-8415
Hughes, Sherry Elaine  
Department of Oceanography  
Dalhousie University  
Halifax, Nova Scotia B3H 2W3  
CANADA  
Tel.: (902) 429-7764  

Hulsemann, Kuni  
Biologische Anstalt Helgoland  
Notkestr 31  
2000 Hamburg 52  
FEDERAL REPUBLIC OF GERMANY  

Hung, Tsu-Chang  
Institute of Oceanography  
National Taiwan University  
Taipei 107  
CHINA  

Hunter, Keith A.  
Chemistry Department  
University of Otago  
P.O. Box 56  
Dunedin  
NEW ZEALAND  

Huntley, David A.  
Department of Oceanography  
Dalhousie University  
Halifax, Nova Scotia  
CANADA  
Tel.: (902) 424-3683  

Hurley, Desmond Eugene  
New Zealand Oceanographic Institute  
P.O. Box 12-346  
Wellington North  
NEW ZEALAND  
Tel.: 086-1189  

Hutcheson, Michael S.  
Atlantic Oceanics Co. Ltd.  
46 Fielding Avenue  
Dartmouth, Nova Scotia B3B 1E4  
CANADA  
Tel.: (902) 463-1360  

Hutchings, Laurence  
Sea Fisheries Institute  
Private Bag X2  
Roggebaai, Cape Town 8012  
SOUTH AFRICA  

Hutchison, William Watt  
Earth Science Section  
Department of Energy, Mines and Resources  
580 Booth Street  
Ottawa, Ontario K1A 0E4  
CANADA  
Tel.: (613) 992-5910  

Ingram, Richard Grant  
Institute of Oceanography  
McGill University  
3620 University Avenue  
Montreal, Quebec H3A 2B2  
CANADA  
Tel.: (514) 392-5718  

Irbe, George J.  
Atmospheric Environment Service  
4905 Dufferin Street  
Downsview, Ontario M3H 5T4  

CANADA  
Tel.: (416) 667-4677  

Irwin, Brian  
Marine Ecology Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-7416  

Ishino, Makoto  
Tokyo University of Fisheries  
4-5-7 Konan Minato-Ku  
Tokyo  
JAPAN  
Tel.: (03) 471-1251  

Jacques, Guy  
Laboratoire Arago  
66650  
Banyuls-sur-Mer  
FRANCE  
Tel.: (166) 888-0040  

Jain, Suresh C.  
Moniteq Ltd.  
630 Rivermede Road  
Concord, Ontario L4K 1B6  
CANADA  
Tel.: (416) 669-5334  

James, Ian D.  
Institute of Oceanographic Sciences  
Bidston Observatory  
Birkenhead, Merseyside L43 7RA  
UNITED KINGDOM  

Jamieson, David  
Atlantic Research Laboratory  
National Research Council  
1411 Oxford Street  
Halifax, Nova Scotia B3H 3Z1  
CANADA  
Tel.: (902) 426-8279  

Jannasch, Holger  
Woods Hole Oceanographic Institution  
Woods Hole, Massachusetts 02543  
USA  
Tel.: (617) 548-1400  

Jayarman, Killugudi  
Press Trust of India  
4 Parliament Street  
New Delhi  
INDIA  

Jimenez, Roberto  
Instituto Nacional de Pesca  
Casilla  
5918 Guayaquil  
ECUADOR  
Tel.: 040-1772  

Johnson, Bruce David  
Oceanography Department  
Dalhousie University  
Halifax, Nova Scotia B3H 4J1  
CANADA  
Tel.: (902) 424-3671
Joiris, Claude  
Lab Voor Ekologie  
Vrije Universiteit, Brussel  
Pleinlaan E B 1050  
Brussels  
BELGIUM

Jones, Henry A.C.  
Marine Environmental Data Services Branch  
240 Sparks Street, 7th Floor West  
Ottawa, Ontario K1A 0E6  
CANADA  
Tel.: (613) 995-2007

Joyce, Terrence M.  
Woods Hole Oceanographic Institution  
Woods Hole, Massachusetts 02543  
USA  
Tel.: (617) 548-1400

Juan, Veichow C.  
Institute of Oceanography  
National Taiwan University  
Taipei  
CHINA

Jumars, Peter A.  
Naval Research Office 422 CB  
800 North Quincy Street  
Arlington, Virginia 22217  
USA  
Tel.: (202) 696-4533

Kamamouri, H.  
California Institute of Technology  
Pasadena, California  
USA

Katsaros, Kristina B.  
Department of Atmospheric Sciences  
University of Washington  
Seattle, Washington 98195  
USA  
Tel.: (206) 543-1203

Kawana, Kichiichiro  
Government Industrial Research Institute  
15000  
Hiromachi  
Kure City, Hiroshima 737-01  
JAPAN

Ke, P.J.  
Fisheries and Ocean Services  
P.O. Box 550  
Halifax, Nova Scotia B3J 2S7  
CANADA  
Tel.: (902) 426-6285

Keen, Charlotte E.  
Atlantic Geoscience Centre  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3413

Keen, Michael  
Atlantic Geoscience Centre  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2

Kerr, Adam  
Canadian Hydrographic Service (Atlantic)  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3497

Killworth, Peter Douglas  
Department of Applied Mathematics and Theoretical Physics  
Silver Street  
Cambridge CB3 9EW  
UNITED KINGDOM  
Tel.: (002) 235-1645

Kils, Uwe  
Hohenrude 50  
D23 Kiel  
FEDERAL REPUBLIC OF GERMANY  
Tel.: (004) 313-4546

King, Kenneth  
RSMAS, Biology & Living Resources  
University of Miami  
4600 Rickenbacker Causeway  
Miami, Florida 33149  
USA  
Tel.: (305) 350-7301

Klaassen, W.  
Institute of Meteorology  
Princetonplein 5  
3504 GC Utrecht  
HOLLAND

Koblenz-Mishke, Olga J.  
USSR Academy of Sciences  
Institute of Oceanology  
23 Krasikova Street  
Moscow  
USSR

Komen, G.T.  
Royal Netherlands Meteorological Institute  
KNMI, Postbus 201  
3730 AC De Bilt  
NETHERLANDS

Krancke, Kate  
Atlantic Oceanographic Laboratory
Lawrence, Donald John
Atlantic Oceanographic Laboratory
Bedford Institute of Oceanography
P. O. Box 1006
Dartmouth, Nova Scotia
CANADA
Tel.: (902) 426-2431

Lawrie, John R. N.
Bedford Institute of Oceanography
Ocean Circulation Division
P. O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-4872

Lea, Brian
Dobrocky Seatech Ltd.
P. O. Box 2278
Station C
St. John's, Newfoundland
CANADA
Tel.: (709) 364-2981

Lebel, J. U.
Département d'océanographie
Université du Québec à Rimouski
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1757

Legeckis, Richard
6152 White Oak Avenue
Temple Hills, Maryland 20748
USA
Tel.: (301) 449-5286

Legendre, Louis
GIROQ, Département de biologie
Université Laval
Québec, Québec GIK 7P4
CANADA
Tel.: (418) 656-5788

Leith, William MacDonald
PROCYON
Pagasuley Road
Constantia
SOUTH AFRICA

Lennon, G. W.
School of Earth Sciences
Flinders University of South Australia
Bedford Park
SOUTH AUSTRALIA

Lett, Patrick F.
Lett Marine Consultants
24 Mt. Pleasant Avenue
Dartmouth, Nova Scotia B2Y 3Y8
CANADA
(902) 422-1687

Lewis, E. Lyn
Frozen Sea Research Group
Institute of Ocean Sciences
P. O. Box 6000
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-7270

Lewis, John B.
Redpath Museum
McGill University
859 Sherbrooke Street West
Montreal, Quebec H3A 2K6
CANADA
Tel.: (514) 392-5989

Lewis, Keith
Galveston Marine Geophysics Laboratory
R. Buffler Galveston Marine
700 The Strand
Galveston, Texas 77550
USA
Tel.: (713) 765-2173

Li, Faxi
Department of Oceanography
Amoy University
Xiamen, Fujian
CHINA

Li, Shang Hao
Institute of Hydrobiology
Academia Sinica
Wuhan
CHINA

Li, William
Marine Ecology Laboratory
Bedford Institute of Oceanography
P. O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-7410

Lie, Heung-Jae
Korea Ocean Research
P. O. Box 17
Yeong-Dong
Seoul
KOREA

Linden, Paul F.
Department of Applied Mathematics and Theoretical Physics
Silver Street
Cambridge CB3 9EW
UNITED KINGDOM
Tel.: (002) 235-1645

Lindstrom, Eric
School of Oceanography
University of Washington
Seattle, Washington 98195
USA
Tel.: (206) 543-9838

Linsky, Ronald
United Nations Development Program
P. O. Box 812
19 Keate Street
Port of Spain
TRINIDAD AND TOBAGO
Tel.: (809) 625-1021

Littler, Mark
Department of Ecological and Evolutionary Biology
University of California
Irvine, California 92717
Mann, Kenneth
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3696

Mao, Hanli
Institute of Oceanology
Academia Sinica
7 Nanhai Road
Qingdao
CHINA

Marsh, John
Plymouth Polytechnical Institute
Drake's Circus
Plymouth, Devon PL4 8AA
UNITED KINGDOM

Martin, John H.
Moss Landing Marine Laboratories
Moss Landing
California 95039
USA
Tel.: (408) 633-3304

Marumo, Ryuzo
National Committee for SCOR Japan
Yarai-Chi
59 Shinjuku-Ku
Tokyo
JAPAN 162
Tel.: (003) 269-5041

Matsunaga, Katsuhiko
Department of Chemistry
Faculty of Fisheries
Hokkaido University
Hakodate
041 JAPAN
Tel.: (013) 841-0131

Maul, George
Atlantic Oceanographic and Meteorological Laboratories
(NOAA)
4301 Rickenbacker Causeway
Miami, Florida 33149
USA
Tel.: (305) 361-4343

Maurea, G.
Petro Canada Exploration Inc.
Calgary, Alberta
CANADA

McCaffrey, M.S.
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543
USA

McCave, I.N.
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
UNITED KINGDOM

McClain, E.P.
4403 Weldon Drive
Temple Hills, Maryland 20748
USA
Tel.: (301) 894-6723

McClatchie, Sam
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA

McCulloch, T.D.
Canada Centre for Inland Waters
867 Lakeshore Road
Burlington, Ontario L7R 4A6
CANADA
Tel.: (416) 637-4339

McDougall, Ian
Australian National University
P.O. Box 4, Canberra
A.C.T. 2600
AUSTRALIA

McDougall, Trevor J.
Research School of Earth Science
Australian National University
P.O. Box 4 Canberra
A.C.T. 2600
AUSTRALIA
Tel.: (060) 249-3412

McKendrick, John D.
Naval Ocean Research
NSTL Station
Mississippi 39529
USA
Tel.: (601) 688-4864

McLachlan, Anton
Zoology Department
University of Port Elizabeth
P.O. Box 1600
Port Elizabeth 6000
SOUTH AFRICA

McLachlan, J.
National Research Council
1411 Oxford Street
Halifax, Nova Scotia B3H 3Z1
CANADA
Tel.: (902) 426-8274

McMillan, John G.
National Science Foundation
1800 G Street N.W.
Room 613
Washington, D.C. 20550
USA
Tel.: (202) 357-7837

Mellouk, Mervich
Bureau National Océanique
Centre océanologique de Bretagne
B.P. 337
29273 Brest
FRANCE
Tel.: (009) 845-8055

Melling, Humfrey
Institute of Ocean Sciences
Box 6000
Sidney, British Columbia V8L 4B2
CANADA
Tel.: (604) 656-8252

Mendes Da Silva, Eduardo
Abteilung für Biogeographic
Universität de Saarlandes
FEDERAL REPUBLIC OF GERMANY

Mendez, Astrid
Department of Marine Science
University of Puerto Rico
R. U. M. Mayaguez
PUERTO RICO

Mendoza, Luis Octavio Zesati
Universidad Autonoma de Baja California
Apartado Postal No. 13
Ensenada
Baja California
MEXICO

Merlivat, Liliane
Centre d'études nucléaires de Saclay DRA
B.P. 2, 91191 Gif-sur-Yvette
FRANCE
Tel.: (06) 908-2473

Miller, Robert J.
Department of Fisheries and Oceans
P.O. Box 550
Halifax, Nova Scotia B3J 2S7
CANADA
Tel.: (902) 426-8108

Milley, Christopher
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA

Millisward, Geoffrey Eric A.A.
Plymouth Polytechnical Institute
Drake's Circus
Plymouth, Devon PL4 8AA
UNITED KINGDOM

Miroshnikov, Igor
8, 11th Liniya
Leningrad V-34
USSR

Misra, R.K.
Fisheries and Oceans Canada
P.O. Box 550
Halifax, Nova Scotia B3J 2S7
CANADA
Tel.: (902) 426-6208

Monahan, Edward C.
769 Hartwell Street
Teaneck, New Jersey 07666
USA
Tel.: (408) 646-2309

Monteiro, Jose Hipolito
Servicos Geológicos de Portugal
R Academia Das Ciencias 19-2
Lisboa
PORTUGAL 12000

Moore, Robert
Marine Science Institute
University of Texas
P.O. Box 7999
Austin, Texas 78712
USA
Tel.: (512) 471-4816

Moore, Robert M.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3871

Moraitou-Apostolopoulou, Maria
Zoological Laboratory
University of Athens
Athens 621
GREECE

Morcos, Selim
Division of Marine Sciences
UNESCO, 7, Place Fontenoy
75700 Paris
FRANCE
Tel.: (331) 577-1610

Morel, Andre
Laboratoire physique et chimie marine
B.P. 8
Villefranche sur Mer 06230
FRANCE
Tel.: (339) 355-5656

Morell, Julio M.
P.O. Box 1153
Lajas 00667
PUERTO RICO
Tel.: (809) 899-2482

Morelli, Carlo
Istituto di Miniere e Geofisica Aplicate
Viale R. Gessi 4
34123 Trieste
ITALY

Morison, Miriam
Department of Fisheries and Oceans
Marine Environmental Data Services Branch
7th Floor West
240 Sparks Street
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2012

Morrison, M. Ann
Department of Geological Sciences
University of Birmingham
P.O. Box 363
Birmingham
UNITED KINGDOM
Tel.: (021) 472-1301

Motoda, Sigeru
Faculty of Marine Sciences
Tokai University
Orido, Shimizu 424
JAPAN
Tel.: (054) 334-0411

Muir, Langley R.
Department of Fisheries and Oceans
7th Floor West
240 Sparks Street
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2045
Munbodh, Munesh  
Ministry of Agriculture  
Registrar-General Building, 3rd Level  
Port Louis  
MAURITIUS

Mwaiseje, Boniface  
Zooology Department  
University of Dar Es Salaam  
P.O. Box 35064  
Dar Es Salaam  
TANZANIA

Nagashima, Hideki  
Institute of Physical and Chemical Research  
Hirosawa 2-1, Wako-Shi  
Saitama 351  
JAPAN

Tel.: (048) 462-1111

Nagata, Yutaka  
Geophysical Institute  
University of Tokyo  
Bunkyo-Ku  
Tokyo 113  
JAPAN

Tel.: (003) 812-2111

Nagaya, Yutaka  
Laboratory for Radiological Science  
Izokaki 3609  
Nakaminato-Shi  
Ibaragi-Ken  
JAPAN

Tel.: (029) 265-7141

Nakano, Masito  
4-8-14 Kunitachi-Shi  
Tokyo 186  
JAPAN

Tel.: (042) 572-4280

Nalewajko, Czeslaw  
Life Science Division  
Scarborough College  
Military Trail  
West Hill, Ontario M1C 1V3  
CANADA

Tel.: (416) 284-3218

Nawab, Zohair  
Saudi Sudanese Red Sea Commission  
P.O. Box 5886  
Jeddah  
SAUDI ARABIA

Needler, George T.  
Atlantic Oceanographic Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA

Tel.: (902) 426-7456

Nelepo, Boris  
Marine Hydrophysical Institute  
Lennn Str 28  
Sevastopol 353000  
USSR

Nemoto, Takahisa  
Ocean Research Institute  
University of Tokyo  
1-15-1 Minamidai  
Nakano-Ku, Tokyo  
JAPAN

Newton, Alice  
Marine Science Laboratories  
Menai Bridge, Anglesey  
Gwynedd, North Wales  
UNITED KINGDOM

Tel.: (024) 871-2641

Nishimura, Kunie  
Katayma Chemical Works Co. Ltd.  
2-10-15 Higashiwaji  
Higashiyodogawa, Osaka  
JAPAN

Tel.: (006) 322-0176

Njoku, Eni G.  
168-314 Jet Propulsion Laboratories  
4800 Oak Grove Drive  
Pasadena, California 91109  
USA

Tel.: (213) 354-5607

Nobukumi, T.  
Gotogaoka Junior High School  
350-8 Nakashima Yonago-Shi  
Tottori-Ken  
JAPAN 683

Tel.: (085) 933-2918

Oakesy, Neil S.  
Atlantic Oceanographic Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA

Tel.: (902) 426-3147

O'Brien, Jim  
Florida State University  
Meteorology Annex  
Tallahassee, Florida  
USA

Ochman, Stefan  
212 Dresden  
Ville Mont Royal, Quebec H3P 2B8  
CANADA

Tel.: (514) 739-8736

O'Dor, Ron  
Biology Department  
Dalhousie University  
Halifax, Nova Scotia B3H 4J1  
CANADA

Tel.: (902) 424-2357

Okabe, Shiro  
Tokai University  
1000 Orito Shimizu-Shi  
Shizuoka-Ken  
JAPAN

Tel.: (054) 334-0411

Okuda, Taizo  
Instituto Oceanografico  
Universidad de Oriente  
Apartado 338  
Cumana  
VENEZUELA
Olbers, Dirk J.
Max Planck Institut Fur Meteorologie
Bundesstrasse 55
Hamburg 13
FEDERAL REPUBLIC OF GERMANY

Olivier, Santiago Raul
United Nations Development Program (CUBA)
One United Nations Plaza
New York, New York 10017
USA

Ong, Jin-Eong
School of Biological Sciences
University Sains Malaysia
Penang
MALAYSIA

Oonishi, Y.
Lake Biwa Research Institute
4–1–1 Kyomachi
Otsu-Shi
JAPAN
Tel.: (077) 524-1121

O’Quinn, Leo D.
Canadian Committee on Oceanography
240 Sparks Street
7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-5659

Orekoya, T’Oluwalope
Nigerian Institute for Oceanography
Victoria Island
P.M.B. 12729
Lagos
NIGERIA

Orzech, James
Scripps Institution of Oceanography
(A–008), La Jolla, California 92039
USA

Orme, G.R.
Department of Geology & Mineralogy
University of Queensland
St. Lucia
Queensland 4067
AUSTRALIA

Orsolini, Patrick
Société Elf Aquitaine
Tour Générale la Défense 9
5, Place de la Pyramide
92088 Paris
FRANCE

Osborn, Thomas
U.S. Naval Postgraduate School
Monterey, California 93940
USA
Tel.: (408) 646-3237

Ostenso, Ned A.
2871 Audubon Terrace N.W.
Washington, D.C. 20008
USA
Tel.: (202) 244-5424

Osterroth, Christopher
Institut fur Meereskunde

Universitaet Kiel
Duestenbrooker Weg 20
D–2300 Kiel
FEDERAL REPUBLIC OF GERMANY
Tel.: 597-2744

O’Toole, Michael
National Board for Science and Technology
Shelbourne House
Shelbourne Road
Dublin
IRELAND

Ouellet, Guy
Université du Québec à Rimouski
310, des Ursulines
Rimouski, Québec G5L 3A1
CANADA
Tel.: (418) 724-1747

Owens, Anne
1196 Morrison Drive
Ottawa, Ontario K2H 7L3
CANADA

Palomo, Carlos
Instituto Espanol de Oceanografia
Departamento de Geologia Marina
Alcalá 27–4
Madrid 14
ESPANA

Pannier, Federico
Universidad Central de Venezuela
Escuela de Biologia
Departamento de Botanica
Apartado 80, 390
Caracas 1080–A
VENEZUELA

Parker, Llewellyn
Zoology Department
Rhodes University
Grahamstown R140
SOUTH AFRICA

Parish, Christopher
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3671

Parsons, Alan Dean
9101 Gue Road
Damascus, Maryland 20872
USA
Tel.: (202) 634-7490

Parsons, T.R.
Department of Oceanography
University of British Columbia
Vancouver, British Columbia V6T 1W5
CANADA
Tel.: (604) 228-4273

Patel, Bhupendra
Bhabha Atomic Research Centre
P.O. Box
Bombay–400, 085
INDIA
Tel.: (523) 321-2330
Peacock, Keith
Applied Physics Laboratory
Johns Hopkins University
Johns Hopkins Road
Laurel, Maryland 20707
USA
Tel.: (301) 953-7100

Pearce, John B.
National Marine Fisheries Service
Woods Hole Laboratory
Woods Hole, Massachusetts 02543
USA

Pearcy, William
School of Oceanography
Oregon State University
Corvallis, Oregon 97331
USA
Tel.: (503) 754-4666

Pearse, Siford
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-7061

Peck, Stephen
Centre Champlain des sciences
B.P. 15500-901 Cap Diamant
Quebec, Quebec G1K 7Y7
 CANADA
Tel.: (418) 694-7781

Pederson, Tom
Department of Oceanography
University of British Columbia
Vancouver, British Columbia V6T 1W5
CANADA
Tel.: (604) 228-5984

Pempkowski, Janusz
Chemical Oceanography Division
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3978

Percy, J.A.
Arctic Biological Station
555, rue St. Pierre
St. Anne de Belleview, Quebec H9X 3R4
CANADA

Pereira, Christopher P.G.
Centre for Cold Ocean Resources Engineering
Memorial University of Newfoundland
St. John's, Newfoundland A1B 3X5
CANADA
Tel.: (709) 737-7579

Perkins, Henry
Naval Ocean Research and Development Activity
Code 331
NSTL Station
Mississippi 39524
USA
Tel.: (601) 688-4733

Perry, R. Ian
Department of Oceanography
University of British Columbia
Vancouver, British Columbia V6T 1W5
CANADA
Tel.: (604) 228-4834

Peterson, William
Rosenstiel School of Marine and Atmospheric Science
University of Miami
4600 Rickenbacker Causeway
Miami, Florida 33149
USA
Tel.: (305) 233-7607

Petrie, Brian
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3809

Philippon-Turloch, Natalie
IOC UNESCO
7, Place de Fontenoy
75700 Paris
FRANCE

Pichel, William Gregory
Nova Earth Satellite Service
FOB #4, Room 3057 SDDS23
Washington, D.C. 20233
USA
Tel.: (301) 763-2700

Pierrot-Bults, A.C.
Institute of Taxonomic Zoology
University of Amsterdam
P.O. Box 20125
Amsterdam
NETHERLANDS

Pilkington, G. Roger
Dome Petroleum
P.O. Box 200
Calgary, Alberta
CANADA

Pilson, Michael E.Q.
Graduate School of Oceanography
University of Rhode Island
Narragansett, Rhode Island 02882
USA
Tel.: (401) 792-6104

Pindam, M.
Department of Environment
P.O. Box 15, Site 9
Mira Road, RR1
Porters Lake, Nova Scotia B0J 2S0
CANADA
Tel.: (902) 426-4286

Pinkel, Robert
Scripps Institution of Oceanography
Mail Code A030
La Jolla, California 92039
USA
Tel.: (714) 452-2056

Pitman, Walter C.
Lamont-Doherty Geological Observatory
Palisades, New York 10964
USA
Piuze, Jean  
Sciences et levés océaniques  
Sciences de la mer  
B.P. 15500  
901, Cap Diamant, Québec G1K 7Y7  
CANADA  
Tel.: (416) 694-7781

Piyakarnchana, Twesukdi  
Department of Marine Science  
Chulalongkorn University  
Bangkok 10500  
THAILAND

Platt, Trevor  
Marine Ecology Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3793

Pocklington, Roger  
Chemical Oceanography Division  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-8880

Pollard, Raymond  
Institute of Oceanographic Sciences  
Wormley  
Godalming, Surrey GU8 5UB  
UNITED KINGDOM

Pongsapipatt, Tavorn  
Hydrographic Department  
Royal Thai Navy  
Dhonburi 10605  
THAILAND

Postma, Hendrik  
Netherlands Institute for Sea Research  
P.O. Box 59  
Texel  
THE NETHERLANDS

Prell, Warren  
Department of Geological Sciences  
Brown University  
P.O. Box 1846  
Providence, Rhode Island 02912  
USA  
Tel.: (401) 863-3221

Press, Murray  
Department of Physics  
Royal Roads Military College  
FMO Victoria, British Columbia  
CANADA  
Tel.: (604) 388-1717

Prinsenberg, S.  
Ocean Division, Bayfield Laboratory  
Canada Centre for Inland Waters  
P.O. Box 5050  
Burlington, Ontario L7R 4A6  
CANADA

Prospero, Joseph M.  
Rosenstiel School of Marine and Atmospheric Sciences  
University of Miami  
4600 Rickenbacker Causeway  
Miami, Florida 33149  
USA  
Tel.: (305) 350-7440

Pu, Shuzhen  
Institute of Oceanographic Sciences  
Wormley  
Godalming  
Surrey GU8 5UB  
UNITED KINGDOM

Pugh, D. T.  
Institute of Oceanographic Sciences  
Bidston Observatory  
Birkenhead, Merseyside  
UNITED KINGDOM  
Tel.: (051) 653-8633

Puri, Kewal K.  
Department of Mathematics  
University of Maine  
Orono, Maine 04469  
USA  
Tel.: (207) 581-2783

Qui, Shu-Yuan  
Department of Oceanography  
Amoy University  
Xiamen, Fujian  
CHINA

Quon, Charles  
Atlantic Oceanographic Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA

Quraish, J-hole Salamuddin  
National Institute of Oceanography  
37-KM, Block 6  
Peaches, Karachi  
Pakistan  
Tel.: 043-4308

Ragan, Mark A.  
National Research Council  
Atlantic Research Laboratory  
1411 Oxford Street  
Halifax, Nova Scotia B3H 3Z1  
CANADA  
Tel.: (902) 426-8282

Ramamurthy, V.D.  
Marine Products Export Development Authority  
P.B. No. 1708, M.G. Road  
Cochin 16  
INDIA  
Tel.: (064) 113-1979

Ramsaroop, Doon  
Institute of Marine Affairs  
Hilltop Lane  
Chaquaramas, Trinidad  
WEST INDIES

Rassoulzadehag, Fereidoun  
Station zoologique  
06230 Villefranche sur Mer  
FRANCE
Rayner, Ralph Frank
Wimполь Ltd., Groundwell Industrial Estate
Hargreaves Road, Swindon
Wiltshire, SN2 5A2
UNITED KINGDOM
Tel.: (079) 372-3014

Rebeiro, Angela T.
Dalhousie University
Killam Library
Halifax, Nova Scotia
CANADA
Tel.: (902) 424-3410

Reed, Mark
Applied Science Associates
P.O. Box 337
Wakefield, Rhode Island 02879
USA
Tel.: (401) 789-6224

Reid, Allan T.
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 996-5783

Reid, Joseph L.
Marine Life Research Group (A-030)
Scripps Institution of Oceanography
La Jolla, California 92093
USA
Tel.: (714) 452-2055

Reiffers, C.
171, chemin Royal
St. François, Île d’Orléans
Québec G0A 3S0
CANADA
Tel.: (418) 829-3586

Requejo, Adolfo
Graduate School of Oceanography
University of Rhode Island
Narragansett, Rhode Island 02882
USA
Tel.: (401) 792-6152

Revelle, Roger
7348 Vista Del Mar
La Jolla, California 92037
USA

Rey, Jorge
Laboratorio Oceanografico
Paseo de la Farola 27
Malaga 16
SPAIN
Tel.: (095) 221-2810

Rice, Gregg
Department of Biological Sciences
Fairleigh Dickinson University
Teaneck, New Jersey 07666
USA
Tel.: (201) 692-2297

Rice, Jake
Northwest Atlantic Fisheries Centre
Box 5667
St. John’s, Newfoundland A1C 5X1

CANADA
Tel.: (709) 772-2051

Richards, Francis A.
Office of Naval Research
223 Old Marylebone Road
London NW1 5TH
UNITED KINGDOM

Richardson, Jacques G.
UNESCO
7, Place de Fontenoy
75700 Paris
FRANCE
Tel.: (331) 577-1610

Richardson, Phillip L.
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543
USA
Tel.: (617) 548-1900

Robinson, G.A.
National Parks Board
P.O. Box 787
Pretoria
SOUTH AFRICA
Tel.: (001) 244-1171

Robinson, Ian Stuart
Southampton University
Department of Oceanography
Southampton S09 5NH
UNITED KINGDOM

Rochford, D.J.
CSIRO Division of Fisheries and Oceanography
P.O. Box 21
Cronulla, N.S.W. 2230
AUSTRALIA

Rocque, Susan M.
Department of Fisheries and Oceans
240 Sparks Street, 7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2197

Rodhe, Nils Johan
Institute of Oceanography
P.O. Box 4038
S-40040 Goteborg
SWEDEN
Tel.: (003) 112-8014

Roether, Wolfgang
Institut fur Umwelt Physik
Im Neuenheimer Feld 366
D-6900 Heidelberg
FEDERAL REPUBLIC OF GERMANY
Tel.: (622) 156-3350

Roll, Hans A.
German Committee for Marine Research
Roegenfeld 34
2000 Hamburg 67
FEDERAL REPUBLIC OF GERMANY
Tel.: (040) 603-8882

Rona, Peter A.
Atlantic Oceanographic and Meteorological Laboratories (NOAA)
15 Rickenbacker Causeway
Miami, Florida 33149
Sarachik, Edward S.
Center for Earth and Planetary Physics
Pierce Hall
Harvard University
Cambridge, Massachusetts 02138
USA
Tel.: (617) 445-4551

Sarkisyan, Artem
Department of Numerical Mathematics
USSR Academy of Sciences
11 Gorky Stz.
103009 Moscow
USSR

Sarmiento, Jorge L.
Geophysical Fluid Dynamics Program
Princeton University
Box 308
Princeton, New Jersey 08540
USA
Tel.: (609) 452-6585

Sarmthein, Michael
Geologisch Institut
Universitaet Kiel
Olshausenstrasse 40-60
D-2300 Kiel
FEDERAL REPUBLIC OF GERMANY

Satyanarayana, Devalraju
Department of Chemistry
Andhra University
Waltair
Vishakhapatnam
INDIA 530003

Savard, Jean Pierre
Université McGill
505, Maisonneuve ouest, Ste. 1000
Montréal, Québec H3A 3C2
CANADA
Tel.: (514) 281-1866

Scale, Susan D.
Department of Fisheries and Oceans
7th Floor West
240 Sparks Street
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-9119

Schafer, Charles T.
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-7734

Scheibling, Robert
Biology Department
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3514

Schott, Friedrich A.
Rosenstiel School of Marine and Atmospheric Sciences
University of Miami
4600 Rickenbacker Causeway
Miami, Florida 33149
USA
Tel.: (305) 350-7462

Schreiber, B. Charlotte
P.O. Box 568
Palisades, New York 10964
USA
Tel.: (914) 359-2900

Schumann, E.H.
NIRO
P.O. Box 320
7600 Stellenbosch
SOUTH AFRICA

Schwinghamer, Peter
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3791

Scott, Desmond P.D.
St. Faiths House
The Close
Chichester, West Sussex
UNITED KINGDOM

Sears, Jonathan
Cambridge Scientific Abstracts
5161 River Road
Bethesda, Maryland 20816
USA
Tel.: (301) 951-1408

Seibold, Eugen
Deutsche Forschungsgemeinschaft
Kennedyallee 40
5300 Bonn 2
FEDERAL REPUBLIC OF GERMANY

Selier, Wolfgang
Max-Planck Institut fur Chimie
Postfach 3060
Mainz 0–6500
FEDERAL REPUBLIC OF GERMANY

Sekiguchi, Hideo
Faculty of Fisheries
Mei University
2–80 Edobashi
Tsu, Mei 514
JAPAN
Tel.: (059) 232-1211

Serpoianu, Gheorghe
Institutul Roman de Cercetari Marine
B-Dul Lenin 300
Constanta
ROMANIA
Tel.: 004-3288

Servain, Jacques
Laboratoire d’océanographie physique
29283, Brest Cedex
FRANCE

Shackleton, Nicholas J.
Godwin Laboratory for Quaternary Research
Free School Lane
Cambridge CB2 3RS
UNITED KINGDOM

Shankar, Rajasekhariah
Marine Geology
Mangalore University
Mangalgangotri (DK)
Karnataka
INDIA

Shannon, L. V.
Sea Fisheries Institute
Private Bag X2
Roggebaai
Capetown
SOUTH AFRICA

Sharafeldin, Sayed Hassan
Faculty of Science
Alexandria University
Alexandria
EGYPT

Sharp, Gary D.
FAO Fisheries Department
Via delle Terme di Caracalla
Rome 00100
ITALY
Tel.: (005) 797-6656

Sharp, Jonathan
College of Marine Studies
University of Delaware
Lewes, Delaware 19958
USA
Tel.: (302) 645-4259

Shaw, John
Bayview Laboratories
Burlington, Ontario
CANADA

Shen, Colin
School of Oceanography WB-10
University of Washington
Seattle, Washington 98195
USA
Tel.: (206) 543-5129

Shepherd, John Graham
Fisheries Laboratory
Pakefield Road
Lowestoft, Suffolk NR33 0HT
UNITED KINGDOM
Tel.: (005) 026-2244

Sherman, Kenneth
National Marine Fisheries Service
Narragansett Laboratory
Narragansett, Rhode Island
USA
Tel.: (401) 789-9326

Shih, Chang-Tai
National Museums of Canada
Ottawa, Ontario K1A 0M8
CANADA
Tel.: (613) 996-1690

Short, Andrew D.
Coastal Studies Unit
University of Sydney
Sydney, N.S.W.
AUSTRALIA

Sibuet, Jean Claude
Centre océanologique de Bretagne
29273 Brest, Cedex
FRANCE

Siedler, Gerold
Institut Fur Meereskunde
Universitaet Kiel
Dueskernbrooker Weg 20
2300 Kiel
FEDERAL REPUBLIC OF GERMANY

Simpson, E. S. W.
Department of Oceanography
University of Capetown
Rondebosch 7700
SOUTH AFRICA
Tel.: 065-4754

Simpson, John H.
Marine Science Laboratories
University of North Wales
Menai Bridge
Anglesey, Gwynedd
UNITED KINGDOM
Tel.: (024) 871-2641

Sinclair, Michael
Department of Fisheries and Oceans
Invertebrates and Marine Plants Division
P.O. Box 350
Halifax, Nova Scotia
CANADA
Tel.: (902) 426-8390

Skinner, L. M.
NERC Research Vessel Services
No. 1 Dock
Barry, South Glamorgan
South Wales
UNITED KINGDOM

Sladkov, Vitaly
Moscow K-9
Gorky Sts 11
Moscow
USSR

Smirnov, Boris
USSR Embassy
285 Charlotte Street
Ottawa, Ontario KIN 8L5
CANADA
Tel.: (613) 235-4341

Smith, J. Dungan
School of Oceanography WB–10
University of Washington
Seattle, Washington 98195
USA
Tel.: (206) 543-9279

Smith, John N.B.
Chemical Oceanography Division
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3865

Smith, Peter C.
Coastal Oceanographic Division
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3474
Smith, Ralph  
Marine Ecology Laboratory  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-3255

Smith, Stuart D.  
Bedford Institute of Oceanography  
P.O. Box 1006  
Dartmouth, Nova Scotia B2Y 4A2  
CANADA  
Tel.: (902) 426-2558

Smith, William R.  
General Delivery  
River John, Nova Scotia B0K 1N0  
CANADA  
Tel.: (902) 351-2445

Snedeker, Samuel  
Rosenstiel School of Marine and Atmospheric Science  
University of Miami  
4600 Rickenbacker Causeway  
Miami, Florida  
USA  
Tel.: (305) 350-7485

Snider, Jean E.  
Office of Ocean Minerals & Energy  
2001 Wisconsin Avenue N.W.  
Suite 410  
Washington, D.C. 20235  
USA  
Tel.: (202) 653-8257

Soto-Gonzalez, Luis  
Instituto de Ciencias Del Mar  
Universidad Nacional Autonoma de Mexico  
Appdo. Postal 70-305  
Mexico 20, D.F. 04510  
MEXICO

Southam, John R.  
Rosenstiel School of Marine and Atmospheric Sciences  
4600 Rickenbacker Causeway  
Miami, Florida  
USA

Soyer, Jacques  
Laboratoire Arago  
66650 Banyuls-sur-Mer  
FRANCE  
Tel.: (166) 888-0040

Spence, Tom  
4311 24th Street North  
Arlington, Virginia 22207  
USA  
Tel.: (703) 276-1332

Speth, P.  
Institut fur Geophysik  
Kerpener Str. 13  
D-5000 Köln 41  
FEDERAL REPUBLIC OF GERMANY  
Tel.: (022) 247-0367

Spitzer, Daniel  
Netherlands Institute for Sea Research  
P.O. Box 59  
1790 Ab Den Burg

Texel  
NETHERLANDS

Steele, John  
Woods Hole Oceanographic Institution  
Woods Hole, Massachusetts 02543  
USA  
Tel.: (617) 548-1400

Stefanon, Antonio  
Instituto di Biologia del Mare  
Riva 7 Martiri  
Venezia 30172  
ITALY  
Tel.: (04) 970-7622

Stein, Rudiger  
Geolisch Paleontologisches Institut Kiel  
Ohlshausenstr 40-60  
2300 Kiel  
FEDERAL REPUBLIC OF GERMANY

Stel, J.H.  
Nederlands Raad Voor Zee-Onderzoek  
Postbus 19121  
1000 GC Amsterdam  
NETHERLANDS

Stern, Melvin A.  
Graduate School of Oceanography  
University of Rhode Island  
Kingston, Rhode Island  
USA  
Tel.: (401) 792-6143

Sternberg, Richard  
School of Oceanography WB-10  
University of Washington  
Seattle, Washington 98195  
USA  
Tel.: (206) 543-9310

Stewart, Robert W.  
Room 109  
Parliament Buildings  
Victoria, British Columbia V8V 1X4  
CANADA  
Tel.: (604) 387-6008

Stiller, Mariana  
Weizmann Institute of Science  
Isotope Department  
Rehovot 76100  
ISRAEL

Stirling, James  
370 Olive Street  
Morro Bay, California 93442  
USA  
Tel.: (805) 772-7636

St-Jacques, Yolaine  
Environment Canada  
1550 de Maisonneuve West  
Room 410  
Montreal, Quebec H3G 1N2  
CANADA  
Tel.: (514) 283-7307

Stone, David Phillip  
Northern Environment and Indian Affairs  
Les Terrasses de la Chaudière  
Ottawa, Ontario K1A 0H4
Turner, J.S.
Research School of Earth Science
Australian National University
P.O. Box 4
Canberra, A.C.T. 2600
AUSTRALIA

Tutuwan, Enil Jacob Bambodt
Department of Organic Chemistry
University of Yaounde
B.P. 812 Yaounde
CAMEROUN
Tel.: 022-3501

Untersteiner, N.
Applied Physics Laboratory
University of Washington
Seattle, Washington
USA

Uyeda, Seiya
Earthquake Research Institute
University of Tokyo
Yayoi Bunkyo-Ku
Tokyo 113
JAPAN
Tel.: (003) 812-2111

Vaisserie, R.
Centre scientifique de Monaco
16, boulevard de Suisse
Monte Carlo
MONACO

Van der Land, Jacob
Rijksmuseum van Natuurlijke
P.O. Box 9517
2000 RA Leiden
THE NETHERLANDS

Van der Linden, Willem Jan Marie
Institute of Earth Sciences
University of Utrecht
Budapestlaan 4
3508 Ts Utrecht
THE NETHERLANDS

Vandermulen, John
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2479

Van der Westhuysen, O.A.
Council for Scientific and Industrial Research
P.O. Box 395
Pretoria 0001
REPUBLIC OF SOUTH AFRICA

Van Foreest, D.
University of Capetown
Rondebosch 7700
SOUTH AFRICA

Vasilikiotis, George
The University
Thessalomikis
GREECE

Verstraete, Jean Marc
ORSTOM
24, rue Bayard
75008 Paris
FRANCE

Vetter, Richard C.
National Academy of Sciences
2101 Constitution Avenue N.W.
Washington, D.C. 20418
USA
Tel.: (202) 334-3141

Vincent, Christopher E.
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
UNITED KINGDOM

Volckaert, Filip
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3675

Von Bodungen, Bodo
University of Kiel
Olshausenstrasse 40-60
D2300 Kiel
FEDERAL REPUBLIC OF GERMANY

Vukovich, Fred M.
Research Triangle Institute
P.O. Box 12194
Research Triangle Park
North Carolina 27709
USA
Tel.: (919) 541-5813

Wakefield, Simon John
Department of Oceanography
University College of Swansea
Singleton Park
Swansea SA2 8PP
UNITED KINGDOM
Tel.: (079) 220-5675

Wakeham, Stuart G.
Chemistry Department
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543
USA
Tel.: (617) 548-1400

Walder, Hans
Tinsdaler Kirchenweg 233A
D-2000 Hamburg 56
FEDERAL REPUBLIC OF GERMANY

Waldichuk, Michael
West Vancouver Laboratory
4160 Marine Drive
West Vancouver, British Columbia V7V 1N6
CANADA
Tel.: (604) 926-4112

Wallace, Douglas
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3671

Wallentinus, Inger
Asko Laboratory, Institute of Marine Ecology
University of Stockholm
P.O. Box 6801
S-11386 Stockholm
SWEDEN

Walsh, John G.
Department of Atmospheric Sciences
University of Illinois
1101 W. Springfield Avenue
Urbana, Illinois 61801
USA
Tel.: (217) 333-7521

Walter, Mary Ann
Biogeochemistry and Living Resources
University of Miami
4600 Rickenbacker Causeway
Miami, Florida 33149
USA
Tel.: (305) 350-7342

Walton, Alan
International Laboratory of Marine Radioactivity
International Atomic Energy Agency
Musée oceanographique
MONACO

Wang, Longwen
Second Institute of Oceanography
Hangzhou
CHINA

Wang, R.
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-2257

Wang, Zhongsheng
First Institute of Oceanography
Quingdao
CHINA

Wangersonsky, Peter J.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3674

Wannamaker, Brian
NATO Saclantcen
Vale San Bartolomeo 400
La Spezia
ITALY 19026

Warner, James L.
Martec Ltd.
5670 Spring Garden Road
Halifax, Nova Scotia
CANADA
Tel.: (902) 425-5101

Watson, Jeffrey
Department of Fisheries and Oceans
7th Floor West
240 Sparks Street
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 996-1830
Watson, Nelson H.F.
Bayfield Laboratories
Canada Centre for Inland Waters
P.O. Box 5050
Burlington, Ontario L7R 4A6
CANADA
Tel.: (416) 637-4395

Weatherly, Georges L.
Department of Oceanography
Florida State University
Tallahassee, Florida 32306
USA
Tel.: (904) 644-6700

Wefer, Gerold
Geologisch-Paleontologisches Institut
Universitaet Kiel
Olshausenstrasse 40-60
2300 Kiel
FEDERAL REPUBLIC OF GERMANY

Weiss, Edward
4 Merrimac Court
Dix Hills, New York 11746
USA
Tel.: (516) 643-5418

Weller, G.
Geophysical Institute
University of Alaska
Fairbanks, Alaska 248664
USA
Tel.: (907) 474-7371

Wells, Peter
Marine Ecology Laboratory
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3276

Wheaton, Elmer P.
127 Solana Road
Portola Valley, California 94025
USA
Tel.: (415) 851-7466

White, Warren B.
Scripps Institution of Oceanography
La Jolla, California 92037
USA
Tel.: (714) 452-4826

Whitehouse, Brian G.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3671

Whitney, Leslie
Canada Centre for Remote Sensing
Dept. of Energy, Mines and Resources
2464 Sheffield Road
Ottawa, Ontario
CANADA

Wiesenburg, Denis A.
Naval Ocean Research and Development
Code 334-NSTL Station
Mississippi 39529

USA
Tel.: (601) 688-4600

Willis, David H.
Hydraulics Laboratory M-32
National Research Council
Ottawa, Ontario K1A 0R6
CANADA
Tel.: (613) 993-9201

Willmott, Andrew J.
Department of Oceanography
Code 68 WT
Naval Postgraduate School
Monterey, California 93940
USA
Tel.: (408) 646-3258

Wilson, J.R.
Marine Environmental Data Service
240 Sparks Street
7th Floor West
Ottawa, Ontario K1A 0E6
CANADA
Tel.: (613) 995-2007

Woelfle, W.E.
Canadian Hydrographic Service
615 Booth Street
Ottawa, Ontario
CANADA
Tel.: (613) 995-4351

Wolff, Torben
Zoologisk Museum
Universitetsparken 15
DK2100 Copenhagen
DENMARK

Wollast, Roland
Oceanography Laboratory
50 Avenue F. Roosevelt
1050 Brussels
BELGIUM

Woodall, Reuben F.A.
1922 Hidden Pt. Road
Annapolis, Maryland 21401
USA
Tel.: (301) 757-1118

Woooster, Warren
Marine Studies
University of Washington
Seattle, Washington 98205
USA
Tel.: (206) 543-7004

Wright, Brian
Gulf Canada
P.O. Box 130
Calgary, Alberta
CANADA

Wright, Daniel G.
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-4492

Wright, L.D.
University of Sydney
Sydney, N.S.W. 2006
AUSTRALIA
Tel.: (002) 692-3625

Wroblewski, J.
Department of Oceanography
Dalhousie University
Halifax, Nova Scotia B3H 4J1
CANADA
Tel.: (902) 424-3513

Yamaguchi, K.
Century Research Center Corporation
3-4-24 Tsuruhara
Izumisano City
Osaka
JAPAN
Tel.: (072) 462-1669

Yanez-Arancibia, Alejandro
Instituto de Ciencias Del Mar
Universidad Nacional
AP Postal 70-305
Mexico 04510 DF
MEXICO

Yang, Dequan
Bedford Institute of Oceanography
P.O. Box 1006
Dartmouth, Nova Scotia B2Y 4A2
CANADA
Tel.: (902) 426-3449

Yap, Nonita Tumulak
Department of Oceanography
Texas A&M University
College Station, Texas 77843

USA
Tel.: (713) 845-2959

Yoshimura, Hirozo
Hachioji-Shi
Tokyo-To 192
JAPAN
Tel.: (042) 642-0774

Youngbluth, Marsh J.
Harbour Branch Foundation
RR 1, P.O. Box 196
Fort Pierce, Florida 33450
USA
Tel.: (305) 465-2400

Yu, Yuancheng
National Bureau of Oceanography
Peking, Fuxingmen 100045
CHINA

Zafiriou, Oliver C.
Department of Chemistry
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543
USA
Tel.: (617) 548-1400

Zhang, Jinhao
Third Institute of Oceanography
Xiamen, Fujian
CHINA

Zimmerman, Joseph
Netherlands Institute for Sea Research
P.O. Box 59
Texel
NETHERLANDS
Mr. Ewing presenting JOA painting to the Hon. Roméo LeBlanc
(Minister of Fisheries and Oceans)

His Excellency Governor General Schreyer visits JOA
JOA Opening

Mr. Ewing, Dr. Simpson, Ayala-Castanares, Hon. R. LeBlanc
Dr. Simpson, President of SCOR

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