WOCE
World Ocean Circulation Experiment

NEWSLETTER

Number 1 October 1985

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The World Ocean Circulation Experiment

The first issue of WOCE Newsletter coincides with the publication of the ”Scientific Plan for WOCE”, which details the strategy developed by the Scientific Steering Group during the past three years. It provides the framework for assessing priorities and for experimental design in the five years remaining before the Intensive Observing Phase. The experiment will be a major step in ocean exploration, with applications in biological, chemical and geological, as well as in physical oceanography. The global perspective provided by satellite altimetry lies at the heart of WOCE. It will help to resolve fundamental problems in planetary climatology. But the sense of urgency that led to proceeding as soon as technically feasible comes from the need to predict decadal climate change, and in particular the changes that will be provoked by CO_2 pollution of the atmosphere. The scientific background to WOCE has been reviewed in Nature (314, 501-511, 11 April 1985).

We hope all concerned will use this Newsletter to present ideas for experimental design, the results of related studies, plans for future experimental work, etc. It will serve participants in WOCE as ‘MODE NEWS’ and ’POLYMODE NEWS’ did those involved with the investigation of ocean eddies in the last decade, and as ‘TO-AN’ serves those active in TOGA today. The ‘Ocean Modelling’ Newsletter will, we hope, continue to provide a vehicle for trying out ideas and announcing results in modelling including WOCE-related activities. This does not exclude submission of modelling contributions to ’WOCE Newsletter’.

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Published by: WOCE-IPO, Institute of Oceanographic Sciences, Brook Road, Wormley, Godalming,
Surrey, UK Tel.: 042879 4141 Telex: 858833 OCEANS G

The International Origins of WOCE

The international history of WOCE can be traced back to the Global Atmospheric Research Programme (GARP). There are roots in both the first and second objectives of GARP. Oceanographic activities featured (or were tolerated) in all GARP experiments. And early considerations of a strategy for climate predictions showed that research into the global oceanic circulation required high priority.

The Global Atmospheric Research Programme

The second objective of GARP

The second objective of GARP was to develop a research strategy for climate research, with priority for those elements of the planetary climate system that are expected to have the biggest impact on the accuracy of climate forecasts. The Joint Organizing Committee (JOC) for GARP opened a debate on the subject in 1974 with an international Study Conference in Stockholm. The resulting publication, ”The Physical Basis of Climate and Climate Modelling” (GARP 16) had to be reprinted several times to meet the demand. Most of the key ideas were discussed at that conference. In 1978 the JOC published the first elements of a strategy for climate research. It was decided that the emphasis should be on learning how to predict climate on time scales of several weeks to several decades”. Those are the lead times for decisions in agriculture, industry and government involving such large investments that even a marginal ability to predict climate would offer significant economic and social benefits.

The decision at JOC XV set a course quite different from that followed by academic climatologists who had concentrated on documenting past climate from historical to geological time scales. It was a brave decision. Many meteorologists thought the climate system might prove to be unpredictable on those time scales, and most were sceptical about the timeliness of embarking on research aimed at predicting it. But the JOC strategy statement had the effect of concentrating minds on a new problem. What would have to be done to make climate prediction possible on those time scales? A very long list could be drawn up from themes discussed at the Stockholm Conference. What criterion would be appropriate for establishing priority in that list?

By 1979 the JOC had identified two problems that deserved absolute priority, namely CLOUD-RADIATION INTERACTION and OCEAN CIRCULATION. Models designed to predict climate on the chosen time scales were expected to be especially sensitive to errors in those aspects of the planetary climate system. Furthermore, the present state of knowledge about them was inadequate and it would take a long time and major research investment to improve the situation.

Later the Cage criterion was adopted as a touchstone to establish the relative importance of different aspects of the climate system. Each was assessed in terms of the uncertainty it produces in the atmospheric heating rate computed by climate models. Those producing an error below 10 Wm^{-2} were deemed to be of minor importance. The Cage group estimated the uncertainty in the sea surface heat flux to be about 50 Wm^{-2}. It therefore deserved higher priority than, for example, stratospheric aerosols, for which the uncertainty in heating rate is much lower. This emphasis on accuracy of measurement and parameterization in models contrasts strongly with the more qualitative methods of traditional climatology. It led to the aphorism that climate prediction research is more about metrology than meteorology. The Cage criterion is equally relevant to assessing priorities in WOCE.

Oceanography in GARP

Oceanography played a central role in only one of the GARP experiments, the Joint Air-Sea Interaction (JASIN) project in 1978. The experience gained during JASIN has proved to be particularly relevant to WOCE, largely because of the opportunity it provided to test the new instruments carried by Seasat-A. The encouraging results of those tests helped to establish the altimeter and scatterometer as serious candidates for a major survey of the World Ocean.

Significant oceanographic programmes were also carried out (on a non-interference basis) during the other major GARP experiments: AMTEX (1974/5), GATE (1974), FGGE (1979) and ALPEX (1982). They influenced the development of the TOGA programme, but have been less important for WOCE.

The policy of the JOC for GARP was to leave the organization of such activities to the international oceanographic community, supported by SCOR and IOC. Oceanographers on the committee (Stewart, Hasselmann, Woods) kept the JOC informed of developments, and wrote the ocean sections of successive JOC reports. There was no serious attempt to bring the meteorological and oceanographic communities together. That policy established the pattern for the WCRP, and led to the formation of the SCOR-IOC Committee for Climate Change and the Ocean (CCCO), which now works with the JSC on oceanographic aspects of the WCRP. The chairman of the JSC-CCCO Liaison committee attends the annual meetings of both committees. The JSC and CCCO jointly organize all major meetings relating to the oceanographic component of the WCRP, and they jointly sponsor the Scientific Steering Group (SSG) for WOCE.

The World Climate Research Programme

The strategy developed by the JOC for GARP was developed further by the committee that succeeded them in 1980: the JSC for the WCRP. The JSC separated the research into three Streams according to the time scale of the forecast. Stream one is concerned with periods of months, Stream two with years, and Stream three with decades. Following Richardson’s principle of simplifying the ocean part of the climate model as far as possible, it was proposed that for stream one it might be sufficient to treat the ocean in terms of the climatological mean cycle of sea surface temperature plus anomalies observed at the start of the forecast, which are assumed to decay according to fixed rules, to be determined from climatological data. An alternative to that statistical approach would be to incorporate a model of the seasonal boundary layer of the ocean into the atmospheric GCM, with a climatological mean geostrophic circulation. The strategy for steam two is based on the assumption that it is necessary to treat the interannual variation of the circulation in the upper tropical ocean explicitly with a coupled ocean-atmosphere model, but that the rest of the ocean can be treated statistically as in stream one. The TOGA project is designed to test that hypothesis. The strategy for stream three is based on the assumption that decadal climate prediction will require a comprehensive oceanic GCM coupled to an atmospheric GCM. Simplifications may be possible, but the present state of knowledge about the ocean circulation does not permit us to say what they are a priori.

An oceanographic strategy for the WCRP

The first meeting organized to develop the oceanographic component of the climate programme was held in Kiel, in November 1978. The meeting discussed the need for improved observations of the seasonal cycle as a pre-requisite for any discussion of climate anomalies. It also took the first steps towards a strategy for long-term monitoring of the ocean, under the title “Pilot Ocean Monitoring System (POMS)”. That was the start of an international programme that led to the IOC (1985) report on an “Ocean Observing System Development Programme” (OOSDP), and to Goal Two of WOCE.

Soon after the Kiel meeting, SCOR established the CCCO to provide international support for all aspects of oceanographic research relating to climate change, and to take responsibility for the oceanographic component of the WCRP in collaboration with the newly formed JSC. (Shortly afterwards IOC joined SCOR as co-sponsor of CCCO). The JSC-CCCO Liaison Committee was created. The first act of the CCCO was to sponsor a meeting in Miami (October 1979), to continue the discussion started in Kiel. The oceanographic strategy for the WCRP was established at that Miami meeting. It contained two projects: Cage and WOCE. Cage was concerned with improving measurements of the surface fluxes. WOCE was to undertake a global survey of the ocean circulation exploiting the satellite radar methods that had been successfully tested by Seasat. This survey was seen as a long-term investment with a wide range of applications, of which the climate response to atmospheric CO pollution was the most pressing. (In 1992, the JSC and CCCO added a third project, TOGA, in response to the USA El Nino - Southern Oscillation initiative.)

The JSC-CCCO Liaison committee was charged with commissioning feasibility studies for Cage and WOCE, and with arranging a Study Conference to present the new strategy to leading members of the international oceanographic community. That meeting was held in Tokyo in May 1982. The Cage and WOCE feasibility groups presented interim reports. These and the commissioned review papers and working group reports were published as the first volume in the new WCRP Publication Series. Those of us who had been involved in developing the strategy during the previous five years were encouraged by the positive response to the Cage-WOCE proposal. The way was clear to proceed further along the same lines. The next landmark came with the final reports of the Cage and WOCE feasibility groups.

The Cage Experiment

The Cage group (Dobson et al., 1982) presented a review of errors in the main methods of mapping the heat flux between the ocean and atmosphere, which has since been widely quoted. They concluded that the errors are an order of magnitude larger than the change due to doubling atmospheric CO_2 and are therefore unacceptably large for climate prediction. Nevertheless they felt it was not timely to embark on a major experiment to compare the different methods in a single basin. Their advice was accepted by the JSC and CCCO. The decision was made not to proceed with Cage as a separate project, but to encourage efforts to improve the accuracy of the separate methods piecemeal. This policy has already begun to show results. One of the pointers made by the Cage group was that annual surface heat fluxes computed from atmospheric GCMs contained errors that exceeded ± 100 Wm^{-2} on the continents. Such errors had not previously preoccupied climate modellers, but they are now perceived as significant when measured against the WCRP criterion of ± 10 Wm^{-2}, and a major attack on the problem is being made by atmospheric modelling groups around the world. Other aspects of the Cage project were to be incorporated into WOCE.

The World Ocean Circulation Experiment

The WOCE group concluded that the experiment was feasible, but that it depended critically on altimeter and scatterometer satellite missions (Bretherton et al., 1982). The JSC and CCCO accepted the group’s recommendation and established a Scientific Steering Group (SSG) to plan WOCE. They also invited the heads of all space agencies (NASA, ESA, JASA, etc.) to a briefing session on the satellite requirements for the WCRP and in particular for WOCE. This meeting was held in Vienna in August 1982. It is believed to be the only occasion when the space agencies had received such a joint briefing. The list of oceanographic satellites presented at the meeting was considered sufficient to justify proceeding with planning WOCE. International workshops were organized on the altimeter (San Miniato, Italy, April 1983) and on the scatterometer and radiometers (Corsica, October 1983), and a permanent working group was established by the JSC to further develop satellite requirements for the WCRP and to keep the space agencies informed.

WOCE timetable

The SSG for WOCE began its series of biannual meetings at Woods Hole (August 1983) and has since met at Wormley (February 1984), Venice (November 1984) and Wormley (April 1985). The first two meetings were devoted to developing a strategy for the experiment, which now appears in the Scientific Plan as the Goals and Objectives of WOCE. The procedure for converting this strategy into experimental design was established at the third meeting with the specification of three Core Projects. At the same time the SSG started to identify the resources needed to undertake the Core Projects. It had been recognized from the start that WOCE depends critically on satellites, and in particular on altimeter missions planned by NASA, CNES and ESA. Many of the in situ oceanographic measurements will have to be timed to coincide with those missions, so the SSG introduced the concept of an Intensive Observing Period of Five Years during the satellite missions, which were planned to start in 1989. It was agreed that the dates of the WOCE-IPO would be adjusted if the satellite launch dates changed. The timing of other in situ measurements was more flexible. In particular, the SSG identified a global chemical tracer sub-programme (the natural extension of the Geosecs and TTO programmes) as a component of WOCE that should proceed as soon as possible.

The WOCE Scientific Director and International Planning Office

It was recognized from the start that planning WOCE would be a task comparable with that of planning FGGE, and would require an International Planning Office (IPO), led by a full-time Scientific Director. This was achieved in 1985 when JSC and CCCO formally accepted offers from the UK Natural Environment Research Council to host the WOCE-IPO at the Institute of Oceanographic Sciences (Wormley) and from Canada to second Dr George Needler to the WOCE-IPO as Scientific Director.

Implementation

As the planning for WOCE settles down to the design of each Core Project, and detailed technical studies of observing systems, the contributions of specialist working groups sponsored by the SSG or by its parent bodies (CCCO and JSC, IOC and SCOR, WMO and ICSU) have become increasingly important and the first steps have been made to develop national plans for contributions to WOCE. National WOCE committees already exist in the USA, France and Germany and are being planned elsewhere. The venturi effect has begun. There are already examples of principal investigators referring to the relevance of their work to WOCE in proposals for funding. (Even before the Scientific Plan for WOCE was published!). The task for the SSG is no longer to introduce WOCE to the scientific community: it is now to ensure that the strategy laid down in the Scientific Plan is achieved.

The next step will be to publish a WOCE Implementation Plan and to organize an International WOCE Conference. Those are both scheduled for 1987. Meanwhile, the WOCE requirements are included in the WCRP Implementation Plan to be released early in 1986, and will be represented at the WCRP inter-governmental meeting to be held later that year. After satellites, the most urgent requirement is for a dedicated research ship (provisionally named R.V. “WOCE”) equipped and manned for the highest quality hydrographic and chemical tracer work during trans-ocean sections in the Southern Hemisphere. The list of major items needed for WOCE but too expensive to be funded by the normal methods available to individual scientists comprises: satellites, R.V. “WOCE”, facilities for data management, computers for ocean modelling, a tide gauge network, ship-of-opportunity oceanographic measurements, surface drifters and deep floats, and upgraded meteorological observations from voluntary observing ships.

The Future

The World Ocean Circulation Experiment is a major step for oceanography, comparable with the 1979 Global Weather Experiment FGGE) for meteorology. It is taking about ten years to plan WOCE, as it did FGGE. We are now rapidly approaching the field phase, and must soon begin to book ship time for the major hydrographic and chemical tracer sections. The implementation of WOCE lies in the hands of individual scientists, their institutes and national agencies.

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The WOCE Scientific Plan

The International Scientific Plan for WOCE has been developed by the SSG in consultation with oceanographers throughout the world. A final draft has been prepared by the WOCE International Planning Office and distributed to the SSG and National WOCE Committees for comment. After final approval at the 5th meeting of the SSG in October, an illustrated version will be widely distributed throughout the international oceanographic community. The intention of the SSG is that the Scientific Plan should serve as the framework for WOCE which will stimulate the interest of both nations and individual scientists and which will lead to programmes focused on the objectives of WOCE. Only by cooperating within such a framework will it be possible to concentrate the necessary resources globally in a limited number of programmes that can really increase our ability to predict decadal climate change.

The internationally agreed goals and objectives of WOCE are:

Goal 1: To develop models useful for predicting climate change and to collect the data necessary to test them.

Within Goal 1 the specific objectives are:

To determine and understand on a global basis the following aspects of the World ocean circulation and their relation to climate:

1. The large-scale fluxes of heat and fresh water, their divergences over 5 years, and their annual and interannual variability.
2. The dynamical balance of the World ocean circulation and its response to changing surface fluxes.
3. Components of ocean variability on months to years, megameters to global scale, and the statistics on smaller scales.
4. The rates and nature of formation, ventilation and circulation of water masses that influence the climate system on time scales from ten to one hundred years.

Goal 2: To determine the representativeness of the specific WOCE data sets for the long-term behaviour of the ocean, and to find methods of determining long-term changes in the ocean circulation.

Within Goal 2 the specific objectives are:

1. To determine the representativeness of the specific WOCE data sets.
2. To identify those oceanographic parameters, indices and fields that are essential for continuing measurements in a climate observing system on decadal time scales.
3. To develop cost effective techniques suitable for deployment in an ongoing climate observing system.

Any experiment designed to meet Goal 1 of WOCE should recognize the fact that the ocean basins are parts of an interconnected system in which rather different dynamical problems can arise. In order to achieve a balance between the potentially conflicting needs of uniform global coverage, on one hand, and concentration on more regional problems, on the other, the field component of WOCE is based on three Core Projects each of which serves to meet the specific objectives of Goal 1 in a different way.

The three Core Projects are:

Core Project 1 The Global Description. This is concerned with obtaining data that can be used to provide quantitative global descriptions of the circulation of heat, fresh water and chemicals and of the statistics of eddies. These constitute the zeroth order description of the role of the ocean in the planetary climate system. Comparison with model simulations based on surface fluxes observed at the same time will provide a powerful test of the models of decadal climate change.

Core Project 2 The Southern Ocean. The Antarctic circumpolar current, by linking the circulations of the Pacific, Atlantic and Indian Oceans, provides the connection that transforms the oceanic heat flux from a regional into a global phenomena. South of the circumpolar current large quantities of heat supplied at low latitudes are lost to the atmosphere with the resulting formation of deep waters; to the north there are regions of mode water formation. Model predictions are expected to be particularly sensitive to the way these are represented.

Much more is known about the circulations within ocean basins, especially in the North Atlantic, which is the best observed. Nevertheless, recent developments have posed fundamental questions about various important processes and their proper representation in models of the oceans circulation. The improved resolution permitted by more powerful computers will help, but it would be unwise to assume that model predictions will be insensitive to the method of parameterizing motions that models do not resolve. The ultimate aim of developing models suitable for decadal climate prediction poses challenging specifications for the accuracy to which ocean circulation must be simulated.

Core Project 3 The gyre dynamics experiment will study one ocean basin in sufficient detail so that major advances can be made in the models for that basin which can be later extrapolated with some confidence to other ocean basins and to the global circulation. Concentrating on these processes in one ocean basin has many practical advantages.

The experimental design of the three Core Projects is far from clear at this time. Steps will be taken to involve the oceanographic community in constructing a practical and more detailed experimental plan to meet the goals of WOCE and to which nations and individual oceanographers will wish to contribute. This and the preparation of the associated implementation plans are major tasks facing WOCE in the near future.

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Technology Issues in WOCE

A number of technology issues are important in the planning of WOCE. To provide some oversight in these areas a Technology Working Group has been formed under the chairmanship of the author. This W.G., while nominally a U.S. group, has international participation. Some of the issues involve the development of new instruments or systems, others are more in the nature of coordination problems. It is not possible to cover in any detail here the full range of technology that may affect WOCE. Instead, I will outline some aspects of the more important areas.

Profilers

A family of new generation density profiling instruments which could be ready to play a role before the end of WOCE is as follows:

- An “Advanced CTD”, with capabilities exceeding that achieved today with off-the-shelf CTDs.
- A “Smarter CTD”, with the same capabilities as today’s CTDs but simpler and less expensive to maintain and operate.
- A “CTD of Opportunity”, with perhaps somewhat lower specifications than today’s instruments, but capable of collecting usable data from non-dedicated research vessels.

The “Advanced CTD” has perhaps the longest lead time. It would be an advance on the state of the art of ocean profiling, and would represent technology that would be in use well into the 21st century, given the fifteen-year life cycle of oceanographic instrumentation. It seems unlikely that such an instrument can be developed in time to play a role in WOCE.

The “Smarter CTD”, however, could be developed in three to five years. Indeed many of the components for it exist already. This instrument would take advantage of advances in software and hardware to require less support resources, both at sea and in the laboratory.

The “CTD of Opportunity”, similarly, could be in place in time for

WOCE. It could be used on non-dedicated research vessels, with less highly-trained support crews.

Three meetings have been held in the U.S. during which discussions centered around specifications for such instruments and technical feasibility. A new WOCE scientific group, chaired by Terry Joyce, will now attempt to develop plans for coordination between scientists and engineers for development of such instruments.

A related development is the Fast Fish profiling vehicle under development by Albert Bradley at Woods Hole Oceanographic Institution. This will be a free-fall vehicle which can carry a CTD or other instrumentation, descending to 5000 meters and returning in less than 30 minutes, homing in on the ship acoustically on its return. The Fast Fish could extend the use of CTDs to vessels with no deep-sea winch capability, and greatly increase the number of stations per ship day for some density programmes.

Tracer Sampling

A number of advances in tracer sampling, both in the areas of collection and processing, are on the horizon. A working group under Ray Weiss at Scripps Institute of Oceanography is beginning to consider a wide range of options, including the ability to use smaller samples, faster collection techniques, improved shipboard analysis systems, and an accelerator facility for radiocarbon processing.

Drifting Buoys

A number of new drifting buoy systems are under development. Several hold promise for WOCE. Most are now approaching the early stages of at-sea testing.

A new class of drifters are the vertical cycling deep drifting buoys. There are three of these in various stages of development.

- Diogene, under active development by Jean Claude Gascard, which will drift at depth but surface about ten times in the course of up to a three-year deployment to report its data. Vertical cycling is accomplished by alternatively releasing ballast and buoyancy.
- The Global Circulation Drifter (GCD) being developed by Doug Webb for Russ Davis. This deep drifter may have up to a five-year lifetime. It will surface up to 50 times to report its data, using a variable displacement system.
- RAFOS is a drifting buoy system being deployed in the Gulf Stream by Tom Rossby. It records time of arrival signals from moored acoustic sources, as well as in-situ data such as temperatures. It drifts for six months to a year, then surfaces and reports its data before reaching the end of its life as its batteries die.

Each of these buoys, as well as “traditional” SOFAR floats with moored listening stations, is expected to be available during WOCE.

A second category is the low-cost surface circulation drifter. A number of buoys built commercially and in government and academic labs are, of course, already available. In addition, a new buoy is under development by John Dahlen. This is the Low Cost Drifter (LCD), which is aimed at being available for between $1,000 to $1,500 in large production quantities. This buoy will carry only a minimum of sensors (probably only for SST), but is being designed for low cost, long lifetime, and calibrated Lagrangian performance.

Lastly, it is expected that various developments in sensor technology will make possible a modular surface flux and heat content drifter. The Lagrangian characteristics of this buoy will be secondary, but, it will be able to carry sensors for wind stress, Tz, SST, air temperature, humidity, and possibly precipitation. Bill Large is currently overseeing development of this system.

Platform Location

Effective use of the buoy systems described above will require an effective system of platform location via satellite. The ARGOS system is expected to provide this for the duration of WOCE. However, the scale and sampling schemes of WOCE drifting buoy programmes pose some issues for ARGOS.

We must determine soon the approximate numbers of buoys to be deployed in WOCE. In addition, we need estimates of other users of platform location technology for instance ships-of-opportunity. It is likely that the needs of WOCE will Put pressure on the capacity of the ARGOS system.

There are solutions to the problem. For instance, the deep drifting surface cycling buoys such as the CTD could share identifiers. Since a given buoy is on the surface for only a few days a month, perhaps as many as 10 buoys might use the same ID. One might include a “secondary ID” in the data word to identify each individual buoy to the scientist’s data system. This approach has implications for ARGOS’s own processing procedures.

A second issue in this area is the price of the available Platform Transmit Terminals (PTT’s). Given the numbers of these likely to be needed in WOCE, a reduction of the price through volume purchases would save substantial amounts of programme money. Bulk purchases imply, first, agreement among the various researchers as to the design, and second, a mechanism for issuance of joint bid requests and purchase orders. Towards this end, a group of U.S. engineers led by Ken Peal has circulated a draft description of a simple but adequate PTT. This document has been sent to a wide variety of PTT users. If there is sufficient agreement on a description, we will investigate a mechanism for joint purchases for WOCE. (Participation would, of course, be open to non-WOCE researchers).

Telemetry

Increasingly, telemetry is being included in plans for field work in physical oceanography. This will become even more so in the context of the five-year lifetime of WOCE. Indeed, telemetry is an integral part of several projected measurement programmes. And, of course, telemetry is essential in any drift buoy programme, along with platform location.

It is not clear, however, that we will have the satellite bandwidth available to us to pass the amount of data that is desired during WOCE. The satellite systems that we use are very vulnerable to political factors and to pressure from other communities of users. It is important that the possible needs of WOCE in this area be evaluated soon.

Ships-of-Opportunity

The prospect of a large scale ship-of-opportunity programme within WOCE opens up the possibility of utilizing a range of instruments and sensors in a cost-effective way. Once the logistical costs of setting up and maintaining such a programme are provided for, careful consideration should be given to the incremental costs of increasing and optimizing the sensor packages deployed.

For instance, in addition to XBTs, it is expected that an Expendable CTD (XCTD) will be available in the next few years (Sippican Ocean Systems). Similarly, an automated, meteorological packages developed for the U.S. National Ocean Service is already available with an interface for XBT data and telemetry built-in.

Other technologies are under development that might be included in a ship-of-opportunity programme. These include wind stress from radar backscatter (Bill Woodward), doppler current profiles through the ship’s doppler log (Dave Cutchin), and underway electromagnetic current measurements (Tom Sanford).

Lastly, consideration should be given to the cost-effectiveness of data telemetry from the participating vessels in any large scale ship-of-opportunity.

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Experiments on the Sensitivity of Ocean Circulation Estimates to Potential WOCE Observations

Many of the stated goals for the WOCE programme are directed at improving estimates of the ocean circulation and its properties (e.g. the heat and potential vorticity fluxes). The tools possibly available for observations to meet these goals run the gamut from satellites to conventional in situ instruments such as current meters and floats. To the extent that one has the luxury of being able to choose to place resources where they will have the greatest impact, one needs to be able to estimate that impact well-ahead of any field programme. Should WOCE try to place 10,000 SOFAR floats in the ocean, or would it be more fruitful to expend the resources (financial and human) on obtaining 50,000 fluorocarbon measurements? I recognize that experimental design is not this tidy, even if clear answers were available; design is an amalgam of what one can do, combined with dire necessity (in particular, planning for satellite missions has to take place 10-15 years prior to their actual appearance when much of the scientific impact is determinable only through guess work). Nevertheless, in approaching programmes with such ambitious goals as WOCE, with such long lead-times, and consequent political hazards, it is useful to try and make some prior estimates of where and how one will deploy whatever resources are available, if only to try and provide answers to the question “just how well will you do?” that arise both from those who must provide the resources, and those who are simply skeptics by nature.

Because much <u>is</u> already known about the ocean circulation, mere statements about accuracies and precision required to measure the circulation are inadequate. The measurement of components of the circulation already known by conventional means is much less useful and compelling than determination of poorly know components. But how is one to make estimates of how well we might do with new (but as yet non-existent) observation systems?

Any study of the usefulness of observational systems for understanding dynamics and kinematics is necessarily (inevitably) dependent upon a model framework. Dependency of conclusions upon the model used is often regarded pejoratively, as though model-based conclusions were somehow of lesser value than supposed “pure” or “model-independent” ones. But to the extent that models represent a synthesis of known physics and chemistry, they bring to bear information that must not be ignored. Furthermore, I am unaware of any scientific inference drawn from observations that is truly independent of some construct (whether explicit or not) of how the physical world operates.

As an example of the type of testing that can be done, I will use here the quasigeostrophic synthesis of the existing data base of the Atlantic Ocean into an ‘eclectic’ model, as reported by Wunsch (1984a, b and referred to as papers A, B). These two papers were an attempt to incorporate into one internally consistent model much of the information available today about the circulation. Such an effort is always incomplete; but as described in the papers, the large-scale (IGY-era) hydrography was combined into a geostrophic plus Ekman layer balance model along with a variety of assertions about water mass movement, deep velocity statistics, directions of ventilation for deep basins, etc. The model (as used here) contains the tropical radiocarbon (carbon-14) balance equations described in B, but neither the tritium balances nor a number of direct current meter and float trajectory estimates, these will be inserted in a later model (Wunsch, in preparation).

Here I wish to regard this model as at least a rough statement about what we know about the average circulation of the northern Atlantic Ocean (admitting all the while that it necessarily falls short of being complete). Figure 1 shows the defining hydrography of the model. The complete set of formal constraints on the model is quite lengthy (reflecting how much really is already known or believed) and is not repeated (see the two papers cited).

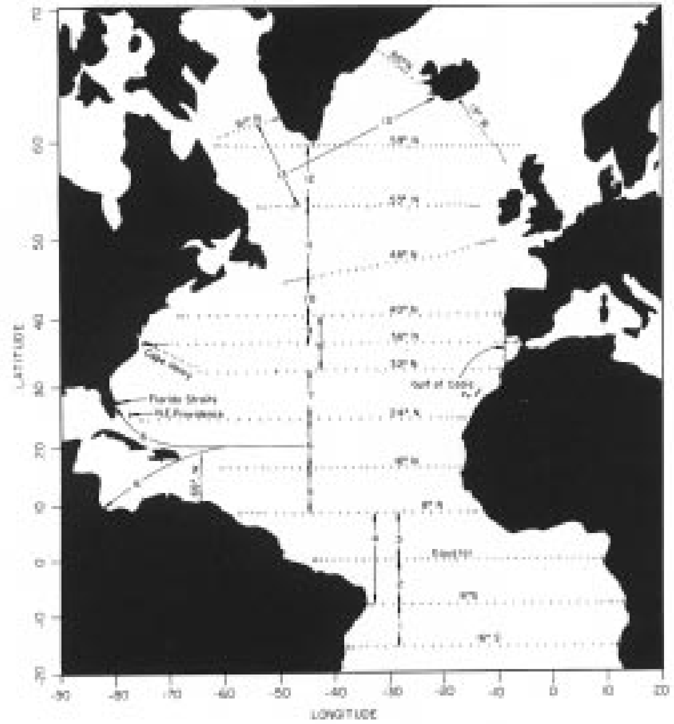

Figure 1. Defining hydrography of the ‘eclectic’ model of Wunsch (1984a,b).

The model is an inverse one, with the formal unknowns being the reference level velocities (mostly 1,000 decibar level), cross-isopycnal transfers, and various properties of the flow such as the meridional heat flux. Our goal here is to ask to what extent introduction of new information would reduce the uncertainty in the heat flux calculations. Out of the long list of possible candidate new measurements, we will use as convenient examples, the existing measurements of carbon-14 and a hypothetical measurement of the absolute slope of the sea surface relative to the geoid. A decision to focus upon the heat flux is somewhat arbitrary. There are many questions about the general circulation with at least as much claim to being central as the meridional heat flux. But it is vital to understanding the role of the ocean in the climate goals of WOCE and is a convenient example of the issue which will arise when attention does turn to other aspects. Ultimate experimental design will be based upon examination of many of these other issues, a weighing of the combined results, and general establishment of priorities. The combination of a tracer and an altimeter were deliberately chosen as two extremely different types of observation, normally treated in isolation, but exemplifying the combinations that will be necessary in WOCE.

(a) Radiocarbon

Figure 2 shows the bounds of heat flux computed in paper A (correcting a plotter error for the 36°N minimum). In B, bomb radiocarbon (carbon-14) constraints were added in the region lying between 16°S and 16°N in the top two layers to study the upwelling there.

Here these carbon-14 constraints are employed instead to determine new values of the bounds on the meridional heat flux. Additional constraints can lead to one of three outcomes: (i) a

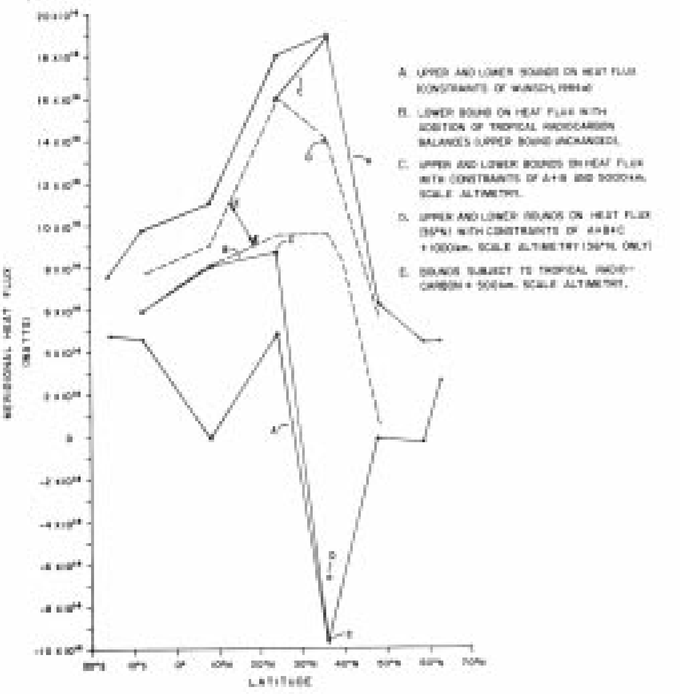

Figure 2. Meridional heat flux bounds on the Atlantic circulation under various assumptions. The outermost curves are the maximum and minimum from paper A. At low latitudes, the minimum is strongly affected by the demand for consistency with C-14 constraints of paper B. At mid- and higher latitudes, simulated sea level constraints make a significant impact on the bounds.

contradiction with the existing constraints, (ii) redundancy with the existing constraints so that the upper and lower bounds are unchanged, (iii) provide new information so that the upper or lower bounds or both are modified (bounds narrowed). In B, it was shown that (i) did not occur. The determination of the new heat flux bounds with the carbon-14 constraints added is displayed in Figure 2. The upper bound is unchanged - these additional constraints added no new information to the previous values. But the lower bound equatorward of 24°N is substantially raised above the previous value. Thus the carbon-14 has constrained the minimum value. This result is entirely equivalent to the result of paper B - where the maximum equatorial upwelling was found unaffected by the radiocarbon distribution, but the minimum was greatly modified; the water upwelled was forced into the northern hemisphere, carrying heat which had to be lost there. Poleward of 24°N, the minimum heat flux is essentially unaffected by use of tropical carbon-14 constraints.

One may draw the conclusion from this exercise that zonal average tracer information (which is all that was employed) has a very strong impact upon the meridional heat budget, and that an appropriate observational strategy would be to improve the error bounds on the radiocarbon.

(b) Altimetric constraints

To explore the impact of sea level slope measurements on the heat flux problem, the slope of the sea surface was simulated for some of the long, nearly trans-oceanic segments of the sections. Figure 3a shows the sea surface elevation (arbitrary zero on the western side) for one of the sections of Figure 1. Figure 3b, shows the extreme change in elevation across the ocean that is possible consistent with the constraints of paper A (determined by maximizing and minimizing the trans-Atlantic sea-level differences).

Experiments were performed by fixing the sea surface elevation along several of the lines on spatial scales ranging from 5000 km to 500 km. Figure 2 shows some of the results. (These results are extremely conservative as we anticipate that WOCE-related satellite missions will provide sea surface slopes on all lines simultaneously and with absolute slope estimates at the centimetre level down to spatial scales of 30 km; this latter point is taken up below).

Despite their preliminary nature, from these computations (and many not displayed here), one can draw a variety of conclusions. For example, on the 5000 km scale in the tropics, trans-Atlantic absolute slopes would need to be known at the 1 cm level of accuracy to improve what we already know (under the important assumption in the model that the hydrography adequately represents the annual mean, probably a poor assumption, but again a conservative one for evaluating altimetry and tracers). But at mid- and higher latitudes, even 10 cm trans-Atlantic accuracy improves on existing knowledge.

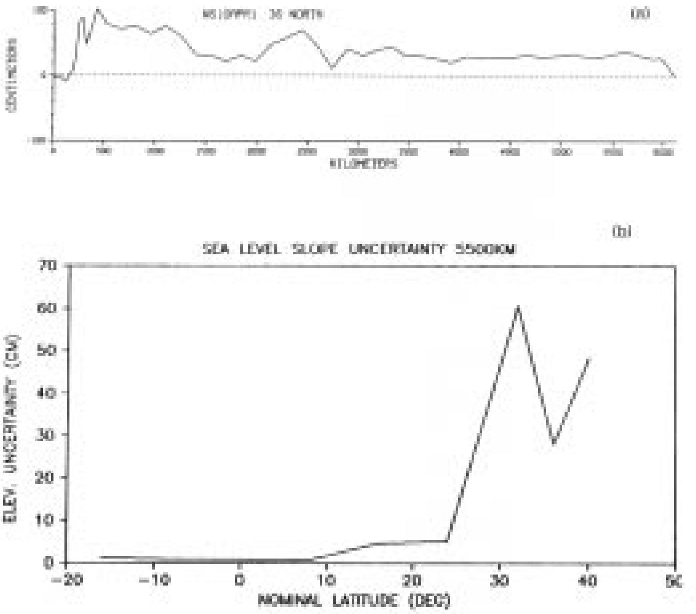

Figure 3. Trans-Atlantic difference in sea level as a function of latitude permitted when circulation is forced to be consistent with constraints of paper A. At tropical latitudes, the difference (interpretable as an uncertainty) is very small, but grows to a maximum of 32°N.

(c) Gravity constraints

The accuracy of 1 cm for the slopes relative to the geoid at 500 km horizontal scales is believed realistic for the proposed Geopotential Research Mission (GRM) of NASA. If these accuracies can be obtained, then the meridional heat flux uncertainty can be reduced at least to that shown in Figure 3 (further reductions can in fact be made with the same data by using it to put bounds on the eddy variability, which were not further constrained over those used in paper A). But suppose there is no GRM? Is there no recourse for bounding heat flux by altimetry? This question was addressed by Joyce, Wunsch and Pierce (1985), who showed that the combination of hydrographic constraints with direct shipboard measurements of near-surface velocity by acoustic means (Joyce et al, 1982) could produce local absolute geostrophic flow estimates equivalent to slope errors relative to the geoid of less than 1 cm over 30 km or 3 cm over 400 km as shown in Figure 4.

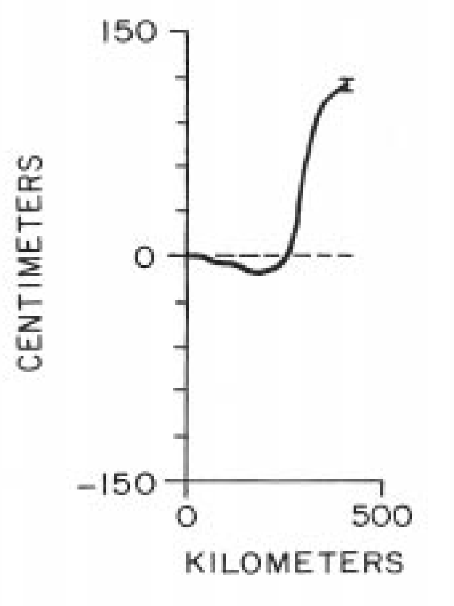

Figure 4. Sea level across the Gulf Stream as computed from results of Joyce et al (1985) from constraints of hydrography/oxygen and shipboard acoustic profiler. The formal uncertainty is very small, and would permit a very accurate estimate to be made of the geoid were an accurate altimeter operating.

Such estimates, if subtracted from contemporaneous altimetric measurements immediately produce geoid estimates of this same accuracy. A strategy of placing acoustic profiling instruments on all WOCE hydrographic ships is thus strongly suggested.

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Report on the NATO Advanced Research Workshop
‘Water Mass Formation in the Upper Ocean’
Venice, October 29 - November 2 1984

One of the goals of WOCE is to deduce the large-scale fluxes of heat and fresh water, and the volumes and locations of water masses with ventilation times of 10 to 100 years. To further this goal WOCE Working Group 6 was created in 1983 to advise on the problems of surface forcing, water mass transformation and mixed layers. Its aims were to bring together researchers in these fields to assess the state of knowledge and to provide input to WOCE on this difficult and fundamentally nonlinear problem.

Four main themes, all intertwined, emerged before and during the meeting. These were: tracers (both passive, such as chemicals, and active, for example heat and potential vorticity; surface forcing (both by wind stirring and by buoyancy effects); anomalies of heat and salt, both gyre-scale and small-scale; and thermohaline circulation. The degree of intertwining will be evident from the discussion below.

The first session was loosely tied to overviews of various topics. Bretherton discussed the existing WOCE plans within the context of the World Climate Research Programme. He stressed the role of the ocean in understanding climate changes due to increases in carbon dioxide content, and explored the modelling and observational backgrounds that would be necessary for the study. Broecker continued with a survey of the carbon-14 measurements. He showed how these measurements placed strong constraints on water mass transport between ocean basins, and noted that better data on carbon dioxide surface flux rates were needed to tighten the calculation still further.

The second session concerned surface oceanic mixed layers. Woods spoke on the detailed way in which the upper thermocline is ventilated by mixed layer processes, and specifically how the retreat of the spring seasonal thermocline leaves behind stratified water (hence also potential vorticity). By following drifter tracks in the Northeast Atlantic, one can see in this dynamically active region how in many cases the buoyancy and potential vorticity produced by this mechanism are swallowed again in the following winter downstream. Killworth showed preliminary results from a model of the same process, in which the ocean buoyancy field did not vary east-west (although flow in that direction was allowed) but two-dimensional advection was permitted. Features of the results included no obvious relationship between buoyancy and potential vorticity and the removal of vorticity by deep winter convection.

Kraus discussed advection in the mixed layer, from a different point of view. He showed how advection from an invariant inflow condition acted as a restoring force on anomalies produced, for example, by high frequency changes in air temperature. However, the nonlinearity of the model means that the effects of storms, for example, can be very high and must be included if we are to model observations correctly. Levitus looked at global coverage of the salinity cycle from his climatological atlas and computed sea-surface fluxes in salinity. He found strong variations, up to 0.5 salinity units, in the Northwest Atlantic in 40 years. Gaspar showed how Niiler-Kraus models, applied to Ocean Station Papa data, only give a realistic simulation provided the wind-mixing constant is tuned seasonally. He suggested a new formulation of dissipation which fitted the data much better (without tuning). Even here, though, errors rose of 0.5 degrees, with a seasonal signal. Inclusion of diurnal effects will probably lower this error significantly.

The third session examined deep convection regions. Gascard gave an overview of time and length scales associated with deep convection and drew attention to preconditioning, baroclinic instability, and surface forcing as ingredients in the process. He compared Labrador Sea observations with those in the Mediterranean, finding roughly similar hierarchies of scales from gyre scale (a few hundred km at most) to convective scale (1 km). Lazier asked why the Labrador Sea stays so fresh compared with the North Atlantic. He examined stations along and around a front in the Labrador Current, finding rapid variation in water properties between two closely spaced stations, and a divergence of eddy fluxes which fed energy into the mean flow.

Clarke discussed gas transfer between atmosphere and ocean during deep convective events. In particular, water involved in the Labrador Sea convection appeared significantly under-saturated in oxygen. He noted a similar effect in the Norwegian-Greenland Sea in 1982. Clarke found that it is difficult to transfer enough oxygen in or out of a deep convective chimney to make a significant change in its oxygen inventory.

The fourth session concerned shallower convection. K. Bryan used the GFDL Princeton general circulation model to find steady multiple equilibria in a bi-hemisphere model with zonal wind stress, a linear restoring condition on surface temperature, and with a flux condition on surface salinity. One solution was equatorially symmetric, the other (initialised with a positive salinity anomaly at high latitudes) had a pole-to-pole asymmetric circulation. He suggested that multiple solutions may play a role in long-term climate change. Hanawa and Toba surveyed water mass formation in the Japan Sea, and stressed its role as a ‘model sea’ for theory and observation, including deep convection to 400 m.

Young modelled thermally-driven circulation, by including a layer-to-layer transfer term in the Rhines and Young model. His solutions were quite realistic, but he stressed that (unlike wind-driven models) the precise details of the solution depended upon an intimate knowledge of the total convective effects, which would be difficult to measure experimentally. Observations by Pollard, using a batfish, showed that mixed layer depths vary by 100 m in 20 km horizontally, with some of the shallowest mixed layers at the coldest part of his section; one of the ‘quieter’ parts of the ocean. Mesoscale features and convection have a strong interaction in this area. Clarke also discussed the effects of mesoscale motions, while examining the region east of the Grand Banks, and showed how mixing of low-salinity water from inshore into the North Atlantic may be produced by small scale processes at specific locations, rather than a continuous stream of eddy processes occurring along the entire western boundary current.

The next session was devoted to tracers. Roether used tritium, helium-3, and freons to model the eastern North Atlantic. The model was isopycnal, with specified inflow values at the edges of the model. He found that eddy coefficients had to be of order 500 m^2s^{-1} south of 40°N, and 3000 m^2s^{-1} north of this latitude, to fit the data, so that transport of tracers is predominantly advective. Broecker suggested that initial phosphate should be added to potential temperature and salinity as a tool for defining water mass circulation properties, because the oxygen content can be used to correct for biological effects on the phosphate. He used these data to show that the sources of water reaching the deep ocean are three: North Atlantic deep water, Weddell Sea deep water (both already known) and intermediate waters in the near-equatorial zones.

Jenkins continued by demonstrating the uses of tritium-helium-3 dating in the North Atlantic. A data set in the ‘beta triangle’ was used to assess the roles of mixing and advection in a quiet part of the ocean. Comparison with beta-spiral inverse methods of deriving the velocity field showed general agreement with those found from dating, but with a consistent bias; mixing effects were small. Oxygen distributions agreed with those of tritium. As a corollary, the global carbon flux may be three times larger than previously believed.

A session loosely devoted to buoyancy effects on gyre-scale flows followed. Rhines discussed various theoretical aspects of large scale circulation, looking at shear dispersal of active tracers like potential vorticity and propagation of density pulses. He showed how buoyancy forcing led to a ‘signature’ in the potential vorticity field which took the form of islands or ramps; such areas are numerous in the data. Needler talked about the resurgence of thermocline theory, and showed how recent papers by Rhines, Young, Luyten, Pedlosky and Stommel could perhaps be brought together by re-examining classical thermocline theory due to Welander. Pedlosky looked at two aspects of ventilated thermocline theory: the source for the water which does the ventilation (this source being Ekman layer pumping, not recirculating western boundary layer water), and how one might add a surface mixed layer to the essentially slab-like dynamics used in the theory, which introduces the possibility of potential vorticity minima.

Willebrand gave a survey of the use of beta-spiral methods for diagnosing ocean circulation from density fields. He showed how cross-isopycnal diffusivities and momentum mixing coefficients could be deduced; the distribution of such coefficients agreed well with Dantzler's maps of observed potential energy. The predominant difficulty was to allow for convection in the data, especially in the North Atlantic. Luyten's talk was also devoted to ventilated thermocline theory. He modelled the effects of buoyancy forcing in a manner similar to Young, by prescribing a mass flux between layers, and found two regimes depending on the speed of the flow produced. The results, when applied to the North Atlantic yielded a heat loss of 45 W m^{-2}.

The next session centred on anomalies. Dickson used an enormous variety of data sources to follow a large-scale salinity minimum around the North Atlantic during the 1970's. He argued that with any of three possible explanations of the data, a major change can occur in the ocean on time scales of a decade. If his preferred explanation holds, the anomaly moved as a connected mass over 12100 km at an average speed of over 3 cm s^{-1} Pollard also examined the anomaly, but further south, and showed its abrupt termination at 33°N. He argued that changes in surface precipitation and evaporation had to be involved to explain the salinity changes, and made the point that the five years of WOCE observations need not be 'typical' years because of anomalies like this.

Jenkins studied variations in ventilation rates using tracers and T-S data. He showed how interannual variability could significantly change local budgets: for example, on the Erika Dan repeat sections, the helium-3 content increased by about three years of production during years 'without ventilation'. He stressed that effects such as this must be included in the design of future observation systems. Talley looked at both observational and theoretical views of the North Pacific subtropical gyre. The salinity minimum was shallow because of high latitude ventilation. She extended the ventilated thermocline models to include more realistic winds, and showed how the shallow salinity minimum could be modelled without invoking mixing.

Bretherton discussed how one might define an 'equivalent steady circulation, with diffusion' to account for the long-time float displacements one might measure in the real ocean, as an aid toward experiment design for WOCE. Using simulated float tracks from an eddy-resolving circulation model, he found that the equivalent steady circulation had to include a 'pipe-line' component to model the rapid western boundary flow. Rooth surveyed how wind and buoyancy forcing interact in the ocean, from the point of view of energetics: closure of the meridional circulation with a deep diffusive flux involves a large potential energy increase of the system. A simple hydraulic model was used to discuss these points.

The use of eddy resolving general circulation models began the final session on WOCE modelling, with K. Bryan comparing results from model calculations with explicit eddies and coarser calculations which used eddy coefficients to achieve (apparently) the same ends. Although total poleward heat transport is almost identical in both cases, distribution of tracers like potential vorticity is very different. He also noted that estimates of heat flux could be in error due to anomalies on timescales up to 4 years. Sarmiento and F. Bryan's paper also used the GFDL model to examine water masses and heat transport. They found great sensitivity to the details of boundary conditions at the northern and southern extremes of the basins, which are largely open.

Killworth showed results from a two-level thermocline model, forced in the classical manner. Convection was shown to be vital for closure of the model, whose results were quite realistic given the simple geometry. Cross-gyre flow occurred as a result of ageostrophic and diffusive effects. Particle tracks showed that little of the deep subtropics is ventilated directly by surface advection. Talley concluded the presentations by examining Labrador Sea water and eighteen degree water variations in historical data, and showed how potential vorticity could be cut off by cessation of eighteen degree water formation.

The ensuing discussion identified several questions. Among these were:

- What can we learn from anomalies and tracers?
- What do we know about boundary conditions for tracers, including potential vorticity?
- Is there a way to average the patchiness in water mass formation and mixed layer depth?
- What should be the sampling strategies and/or interpretation?
- How should ventilation be monitored?
- Of the many atmospheric data possible, which ones are vital for an understanding of water mass formation?
- How can global satellite cover help us?
- Is there a better way to model gyre-scale mixing than eddy coefficients?
- How should inter-basin connections usefully be monitored, and what should one monitor there? In particular, what of cross-equatorial flow?
- What is the role of the southern ocean?

Finally, I should like to thank Dr L V da Cunha of NATO both for his support for and during the meeting, and for giving us a very interesting talk on NATO’s research support. Thanks also go to Dr R Frassetto, who acted as co-director for the meeting, for laying on such pleasant surroundings and extreme hospitality. Venice itself is to be thanked for providing seven days of uninterrupted blue sky at a most unseasonable time; thus I must also thank the participants not only for their talks but also for their restraint in turning up (and providing abstracts) in spite of the outside attractions! CIT Travel, and Ann Casa in particular, provided most efficient arrangements over travel, dinner, and accommodation. Members of WOCE working group 6, specifically J. Woods and R. Pollard, acted as organizing committee, and made my work a great deal easier. My thanks to all involved.

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News of National WOCE Programmes

From USA

Within the U.S. planning for WOCE is proceeding under the general direction of a U.S. WOCE Scientific Steering Committee (SSC) with the following members: D. James Baker, Jr. (Joint Oceanographic Institutions, Inc.,), Francis Bretherton (National Center for Atmospheric Research), Dudley Chelton (Oregon State University), Russ E. Davis (Scripps Institution of Oceanography), William J. Jenkins (Woods Hole Oceanographic Institution), Terrence Joyce (Woods Hole Oceanographic Institution), William G. Large (National Center for Atmospheric Research), James C. McWilliams (National Center for Atmospheric Research), Worth D. Nowlin (Co-Chairman, Texas A&M University), Ferris Webster (University of Delaware), Ray F. Weiss (Scripps Institution of Oceanography) and Carl Wunsch (Co-Chairman, Massachusetts Institute of Technology). The SSC, which meets three to four times a year, receives scientific and technical advice and plans from its Working Groups, from ad hoc groups and from individual scientists. Ongoing planning activities are the responsibility of the U.S. Planning Office for WOCE located at Texas A&M University under the direction of W.D. Nowlin, Jr. (Department of Oceanography, Texas A&M University, College Station, TX., U.S.A. 77843).

The U.S. WOCE planning effort is financed by the National Science Foundation (NSF) with contributions from other agencies including the National Aeronautics and Space Administration and the National oceanic and Atmospheric Administration. As lead agency for this programme, the NSF has responsibility for interagency coordination of its planning.

WOCE planning within the U.S.A. began with a workshop on “Global observation and understanding of the general circulation of the oceans” attended by some 60 members of the U.S. oceanographic community. The weeklong workshop was organized under the auspices of the Board on Ocean Science and Policy of the National Research Council (NRC) and held during August 1983 in Woods Hole, Massachusetts. The workshop participants agreed that the WOCE concept is worthwhile and timely. They identified a provisional overall goal and objectives for the U.S. contribution and recommended that a U.S. planning committee and a number of working groups be established to address critical issues.

The workshop report received wide distribution and review within the U.S. oceanographic community. A panel for the U.S. WOCE was constituted within the NRC, sponsored jointly by panels of the Board on Ocean Science and Policy and the Board on Atmospheric Sciences and Climate. That panel reviewed the community’s comments and criticisms of the report. The Scientific Steering Committee was established with executive responsibility for planning a U.S. contribution to WOCE. The SSC has drafted a set of objectives for the U.S. contribution to WOCE and circulated these broadly within the oceanographic community for review and comment. To date, the following U.S. working groups have been formed to assist in planning WOCE activities: Numerical modelling, Geochemistry, Experimental design for measuring geostrophic circulation, Technology development, Atmosphere-Ocean exchanges, Ocean surface layer, and TOGA/WOCE data management. The working group structure will continue to evolve. In addition, other ad hoc group meetings and activities by interested scientists are being supported.

During April 1985, four ocean sector meetings were convened by the SSC to consider key problems in the general circulation and to recommend what could be done during WOCE to quantitatively change our understanding of the general circulation and its time variability of each sector. It was assumed that surface ‘altimetry’ winds would be available from satellites for at least part of the WOCE observational period. Participants were urged to focus on the in-situ observations that would be required to answer the fundamental general circulation issues.

The meetings focused on four ocean sectors: South Pacific, North Pacific, South Atlantic and Indian Oceans. The Antarctic sectors were included in the southern hemisphere oceans. So that equatorial circulation problems would not be split by a sector boundary, the equatorial regions were included in the South Pacific and South Atlantic sectors. The entire Indian Ocean with its Antarctic sector was considered in one meeting.

A meeting dealing with the North Atlantic sector was not conducted in this first round of meetings for several reasons:

- There has been good large scale coverage of hydrographic variables and transient tracers by recent sampling as part of the TTO programme and on numerous individual long lines of stations,
- There already exist major ongoing programmes by several nations in the North Atlantic (as reported in the records of SCOR Working Group 68),
- Planning for future experiments pertaining to elements of the North Atlantic general circulation are ongoing, for example the Greenland Sea Experiment or a programme of subduction experiments. We expect specific plans to emerge for the North Atlantic sector during the near future, just as we anticipate many refinements to the sampling plans for the other ocean sectors.

Based on the provisional scientific objectives, reports of the ocean sector meetings, and advice and reports from its Working Groups and ad hoc panels, the SSC is preparing a scientific plan for a U.S. contribution to WOCE. A draft is expected late in 1985.

As a next step in its planning, the SSC is sponsoring a follow-on series of meetings to address common elements which have been identified after careful study of the ocean sector meeting reports. The topics for consideration will include: interbasin exchanges and marginal sea ,outflows; cross-equatorial exchanges; deep circulation and topography; gyre interactions including boundary current effects and oceanic heat flux.

A series of U.S. WOCE Reports has been initiated. Report No. 1 contains documents on ocean sampling strategy and technology prepared as background for the ocean sector meetings; Number 2 presents records of the ocean sector meetings. Copies may be obtained from the U.S. WOCE Planning Office.

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From France

During 1984-85 three meetings have been held in Paris to determine a French contribution to WOCE. There was agreement that efforts should concentrate on a part of a gyre, which includes the western boundary and outcropping regions, with the aim of obtaining a picture of the large-scale circulation from top to bottom and statistical information at the mesoscale. The interaction of these two scales was judged to be crucial to the improvement of dynamical models. It was felt that several such gyre experiments should take place within WOCE in order to see how circulation and dynamics change with environmental parameters and rather tentatively at this point, the western South Atlantic has been considered as one possibility for a French effort.

The experimental strategy should be decided by considering explicitly the nature of the measurements (summarized below) and their ultimate synthesis within the framework of an inverse model. In order to help international discussion the list below summarizes the relation between the participants and their interests, the list being of course non-exhaustive:

Altimetry

M. Lefebvre, Y. Menard, P. de Mey (GRGS, Toulouse); J.F. Minster (IPG, Paris)

Tides

C. Le Provost, P. Vincent (IMG, Grenoble); A. Lamy (LOP Museum, Paris); D. Mazzega (GRGS, Toulouse)

Hydrology, Geochemical Tracers

Arhan (COB, Brest); J. Merle (LPCM, Paris) L. Merlivat, L. Memery (CEA, Saclay); J.F. Minster (IPG, Paris); C. Provost (LPCM, Paris)

Lagrangian floats/current meters

A. Colin de Verdiere, B.L. Hua, P. Tillier, M. Ollitrault (COB, Brest); J.C. Cascard (LOP Museum, Paris); J. Verron (IMG, Grenoble)

Acoustic tomography

Y. Desaubies, P. Tillier (COB, Brest); F. Gaillard (MIT, Cambridge)

Theoretical models

C. le Provost, J. Verron, B. Barnier (IMG, Grenoble); A. Colin de Verdiere, B.L. Hua (COB, Brest); C. Frankignoul, C. Provost (LPCO, Paris)

Inverse modelling

C. Provost (LPCM, Paris); M. Arhan (COB, Brest); H. Mercier (MIT, Cambridge); J.F. Minster, C. Perigaud (IPG, Paris)

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From F R Germany

A German WOCE Committee has been established as part of the national group of experts on climate research. This group had been set up earlier by the Ministry of Science and Technology of the Federal Republic of Germany. The WOCE Committee is expected to start its activities in late 1985. Members are: G. Siedler, Kiel (Chairman); E. Augstein, Bremerhaven; J. Duinker, Kiel; H. Grassl, Geesthacht/Hamburg; K. Hasselmann, Hamburg; J. Meincke, Hamburg; W. Roether, Heidelburg; J. Willebrand, Kiel; J. Woods, Kiel.

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Contributions should not be cited without the agreement of the author.

We hope that colleagues will see this Newsletter as a means of reporting work in progress related to the Goals of WOCE as described in this first issue and elaborated in the Scientific Plan. The SSG will use it also to report progress of working groups, and of experiment design and of models.

The editor will be pleased to send copies of the Newsletter to Institutes and Research Scientists with an interest in WOCE or related research.

The World Ocean Circulation Experiment (WOCE) was a component of the international World Climate Research Program, and aimed to establish the role of the World Ocean in the Earth's climate system. WOCE's field phase ran between 1990 and 1998, and was followed by an analysis and modeling phase that ran until 2002. When the WOCE was conceived, there were three main motivations for its creation. The first of these is the inadequate coverage of the World Ocean, specifically in the Southern Hemisphere