Where We Started, Where We Are (Maybe) Going

A Personal View—The Ocean Problem

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June 2023

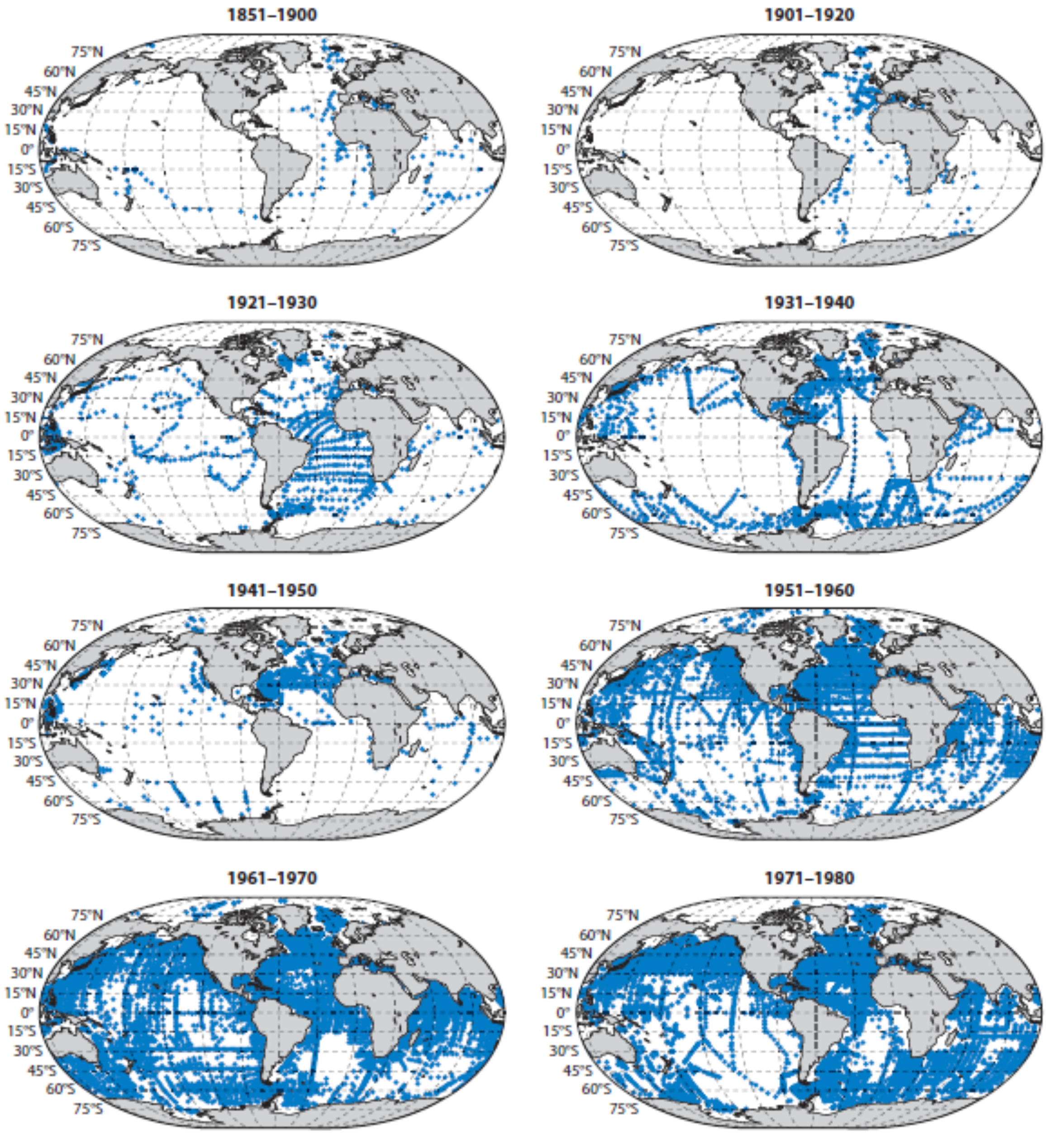
"Don't look back---something may be gaining on you." Satchel Paige

Circa 1970:

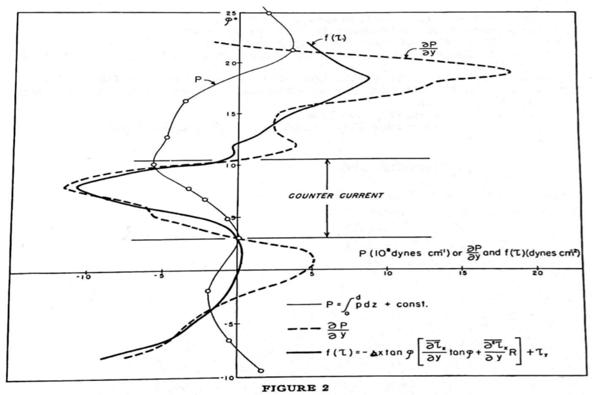
Main observational tool was still hydrography from ships. Coarse horizontal spacing.

Sporadic (space/time) Swallow float data, moored current meters running a few weeks.

Interpretation as a steady-state with a bit of "noise."



Some very elegant theory was developed beginning around 1900: Ekman layers, Sverdrup balance, western/eastern boundary currents, Stommel-Arons flow, abyssal recipes, equatorial undercurrents, thermocline theories, ….Some numerical models mainly for low-Reynolds number steady flows.



Sverdrup original, 1947

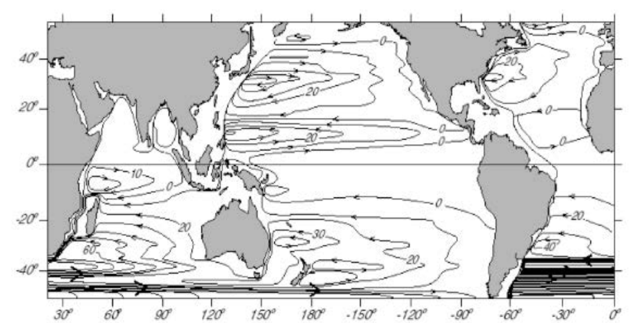
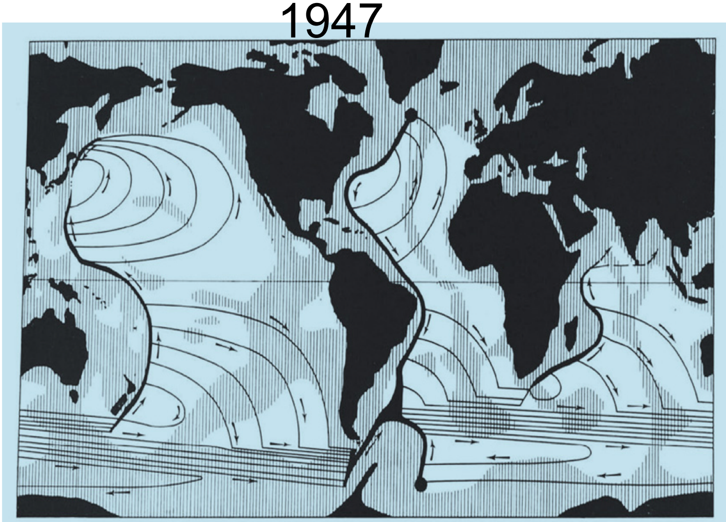
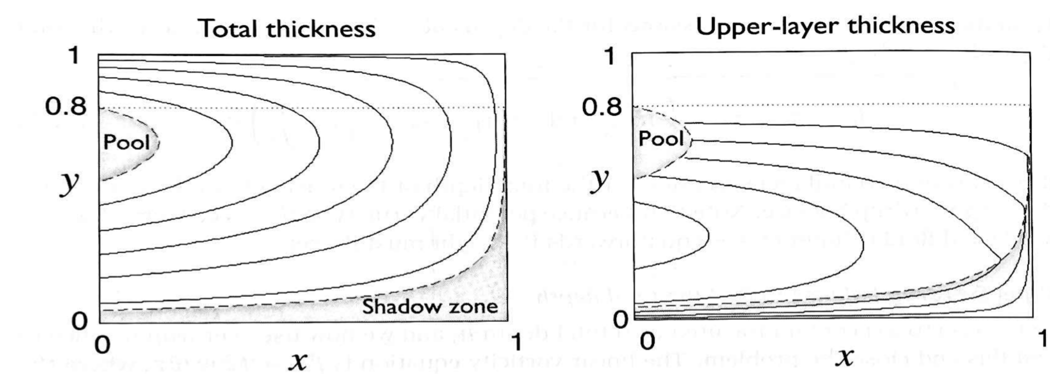


Figure 11.3 Depth-integrated Sverdrup transport applied globally using the wind stress from Hellerman and Rosenstein (1983). Contour interval is 10 Sverdrups. After Tomczak and Godfrey (1994: 46).

Sverdrup from Tomczak & Godfrey 2003



Stommel-Arons flow



LPS (Luyten-Pedlosky-Stommel, 1983)

Also, Gulf Stream(s), recirculations, various thermocline theories...

By inference, these applied to a steady-state ocean. But...suspicions existed that the ocean was turbulent in the sense Victor Starr (and Harold Jeffreys long before) had demonstrated for the atmosphere.

Stommel (1948, Yale Scientific Magazine): "...there is a more profound difficulty ...which completely overwhelms all others, for the motion of the oceans is highly turbulent..." He also wrote a once famous unpublished paper called, "Why do our ideas about the ocean circulation have such a peculiar dreamlike quality?" He then set all that aside for 20+ years.

By about 1970, Stommel had concluded the time had come to face up to the question of what was the nature of the turbulence? He proposed a joint UK-US "experiment" to last a few months not far from Bermuda (logistical considerations).

Some very early time-dependent models existed:

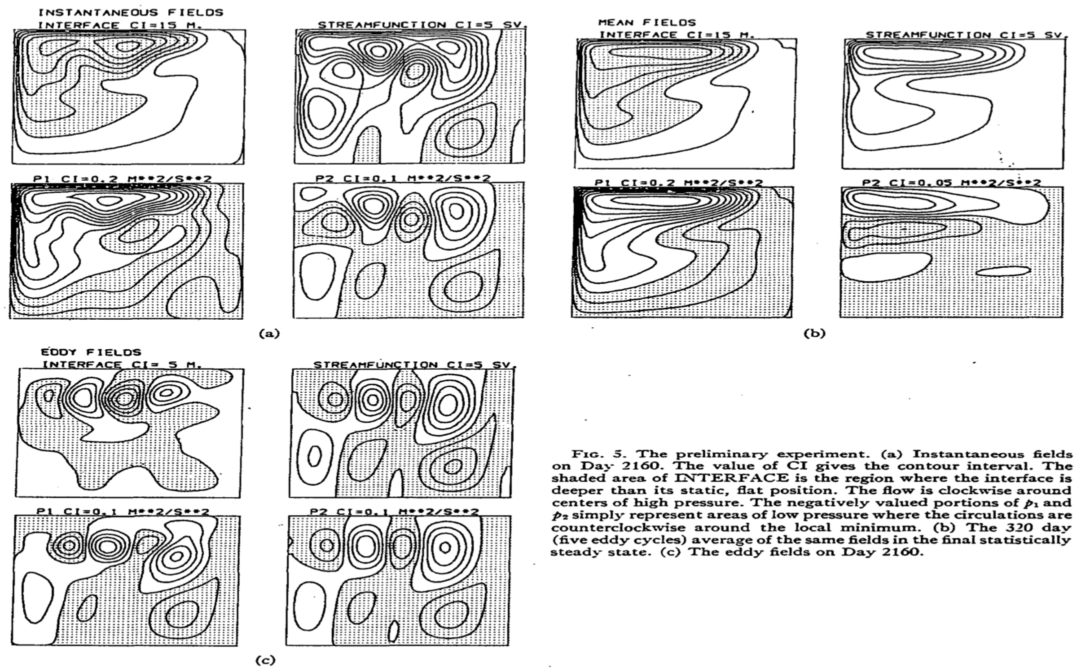


FIG. 5. The preliminary experiment. (a) Instantaneous fields on Day 2160. The value of CI gives the contour interval. The shaded area of INTERFACE is the region where the interface is deeper than its static, flat position. The flow is clockwise around centers of high pressure. The negatively valued portions of p_1 and p_2 simply represent areas of low pressure where the circulations are counterclockwise around the local minimum. (b) The 320 day (five eddy cycles) average of the same fields in the final statistically steady state. (c) The eddy fields on Day 2160.

Hints in data that strong transients existed, e.g. Swallow and Crease float measurements.

Theoretical description by Norman Phillips.

Holland and Lin, JPO, 1975.

Bryan and Manabe, 1975, had produced what might be the first coupled model.

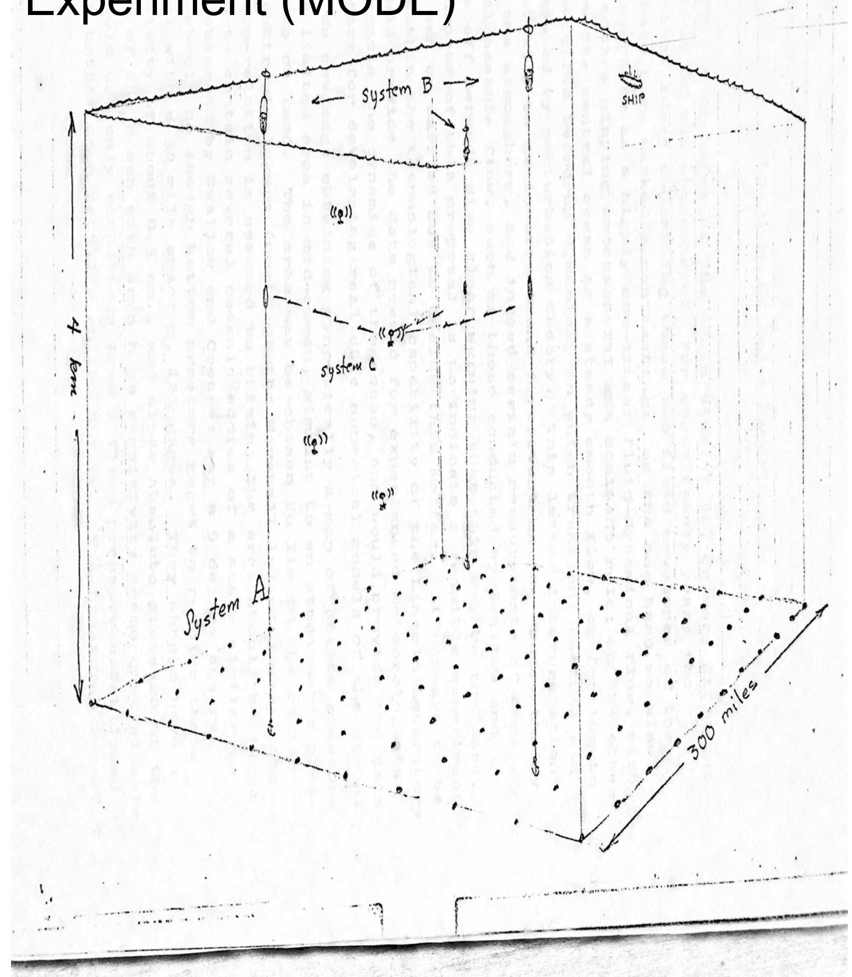
Mid-Ocean Dynamics Experiment

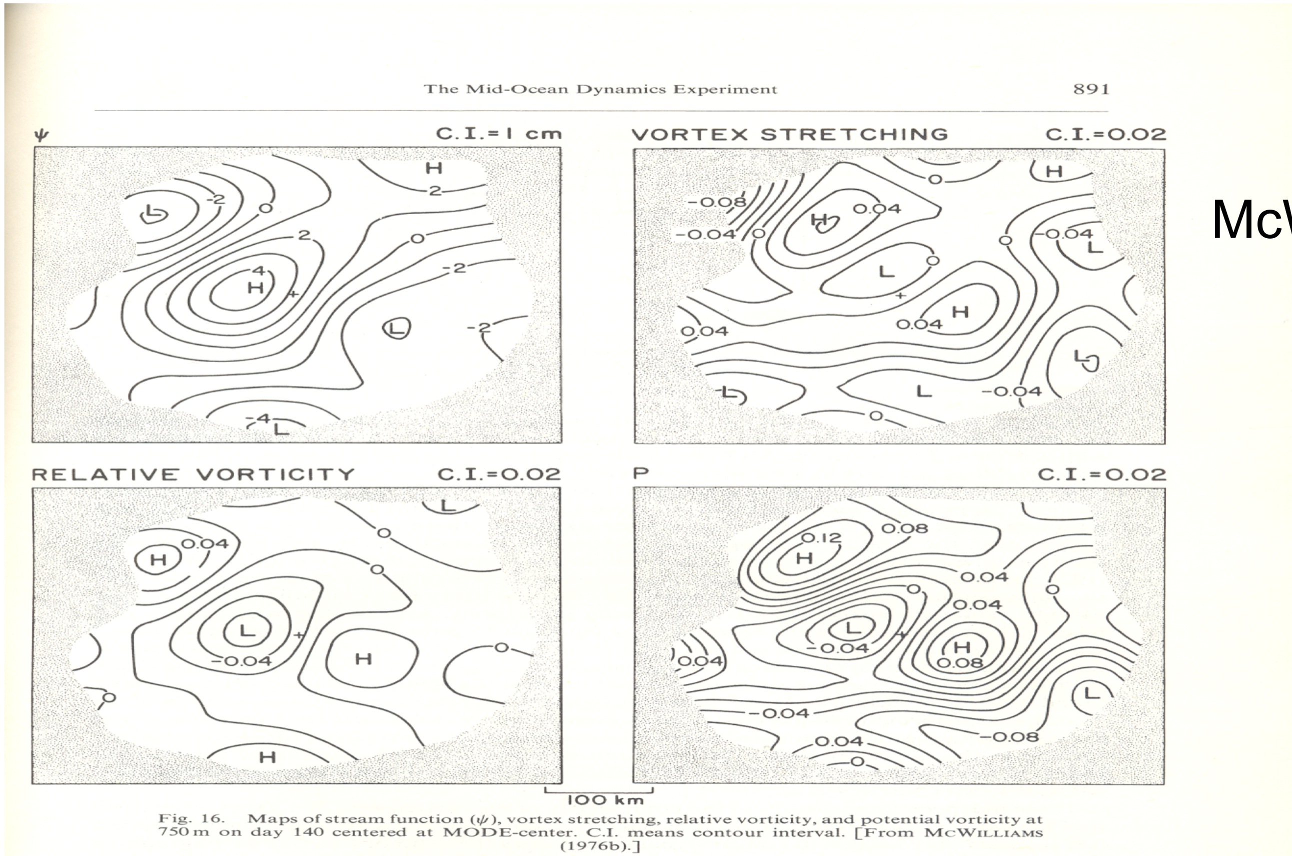
Is the dynamics of the ocean similar to, or very different from, that of the atmosphere? For about twenty years the theoretical ideas concerning these two fluid envelopes of the earth have been markedly contrasting: on the one hand we view the atmosphere as a highly non-linear fluid-dynamical flow, with large eddies playing an essential and dominant role; on the other we view the central ocean as a steady smooth flow, conforming to the so-called Sverdrup dynamics, in which transient motions can be computed by perturbation theory. This latter interpretation may actually be as incorrect and irrelevant as the Hadley theory is to the atmosphere, and indeed certain measurements of deep central oceanic flow, such as those conducted by Swallow and Crease off Bermuda give clear warning that this may be the case. The purpose of this proposal is to indicate a feasible experiment which can be carried out to decide this matter. It appears to be well within the technological capability of the Lincoln Laboratory. It would provide the data needed for experienced meteorologists to diagnose the dynamics of the ocean, and would provide the data necessary for developing realistic numerical models of the ocean.

We envisage obtaining synoptically a map of bottom pressure over a limited area in mid-ocean, similar to an atmospheric pressure map on land. The area may be chosen to lie in the center of a subtropical gyre (north of the Marshall Islands) where the Sverdrup-relation is assumed to obtain; the area should be large enough to contain several oceanic eddies of a scale similar to those observed by Swallow and Crease: say a 5 degree square. There should be enough bottom pressure gages to resolve these eddies: with a 30 mile spacing, 121 gages. They should have a sensitivity of about 0.3 cm., and since absolute pressure at the bottom of the ocean with such high sensitivity seems unattainable, we should use only variations from a fixed pressure established at the bottom upon settling on the bottom. The pressures recorded

Was directed at MIT Lincoln Laboratory.

What became the Mid-Ocean Dynamics
Experiment (MODE)





McWilliams, 1976

Eddies did exist!

Some effort, e.g. by Bill Schmitz, to deploy moorings around the world to last a year. Would be decades before global coverage could be obtained.

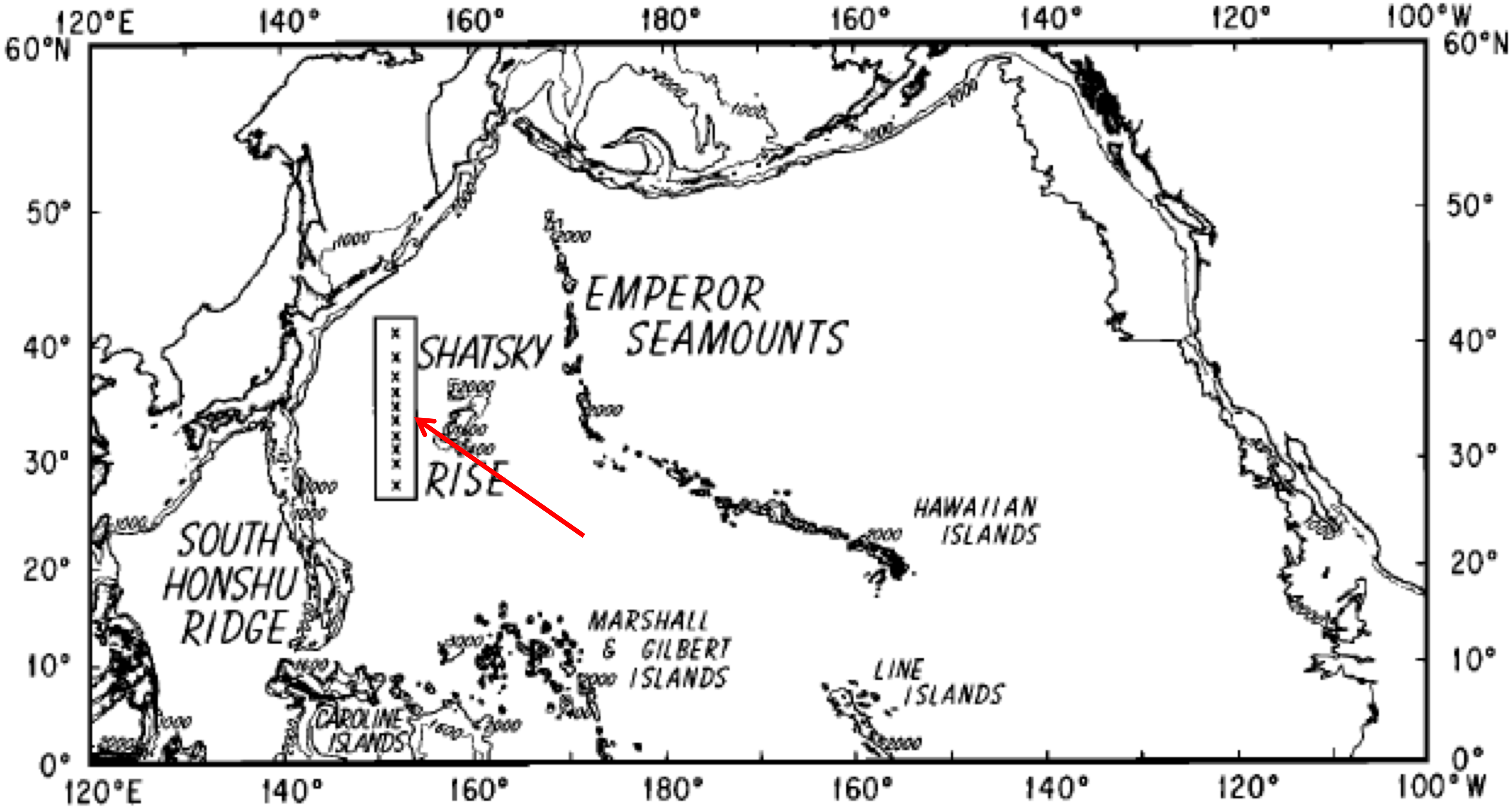


Fig. 1. Mooring locations along 152°E, denoted by crosses enclosed in a rectangular box. Depth contours in fathoms (1 fathom = 1.8 m) and feature names taken from *Chase et al.* [1977].

Years later: 2-year current meter records: Schmitz, Niiler, Koblinsky, 1987

1979:

Carbon Dioxide and Climate: A Scientific Assessment

Report of an Ad Hoc Study Group on Carbon Dioxide and Climate
Woods Hole, Massachusetts
July 23-27, 1979
to the
Climate Research Board
Assembly of Mathematical and Physical Sciences
National Research Council

Oceanographers on the Committee could say little more than that the ocean would likely take up some unknown fraction of the heat and carbon dioxide on an unknown time-scale.

Ocean was being treated as completely passive in the new coupled models. (Sensible)

NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1979

Ad Hoc Study Group on Carbon Dioxide and Climate

Jule G. Charney, Massachusetts Institute of Technology, *Chairman*
Akio Arakawa, University of California, Los Angeles
D. James Baker, University of Washington
Bert Bolin, University of Stockholm
Robert E. Dickinson, National Center for Atmospheric Research
Richard M. Goody, Harvard University
Cecil E. Leith, National Center for Atmospheric Research
Henry M. Stommel, Woods Hole Oceanographic Institution
Carl I. Wunsch, Massachusetts Institute of Technology

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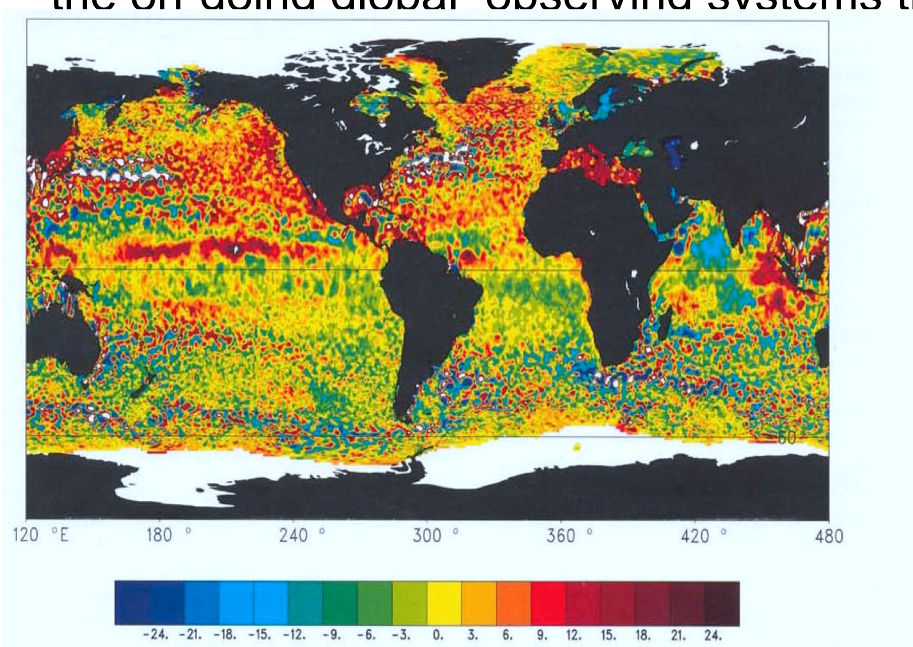
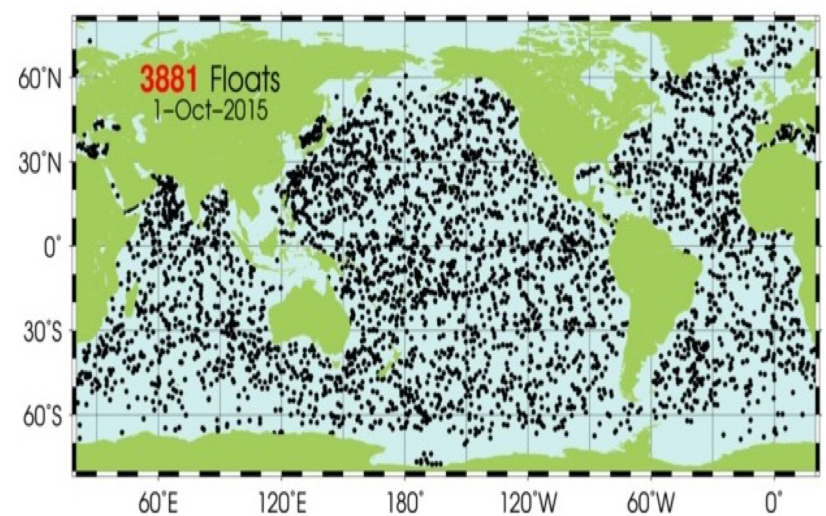
Theresa Fisher

To some of us, circa 1979, appeared that in climate terms, physical (and chemical and biological) oceanography were in grave danger of becoming intellectually irrelevant, with the dominant meteorological/climate community treating it as a swamp.

What to do?

Beginning about 1979, numerous discussion meetings led ultimately to international agreements---scientific and logistical and financial---for the World Ocean Circulation Experiment (WOCE, circa1992+). That in turn led to the on-going global observing systems that continue today:





Argo+

TOPEX/POSEIDON +

Issues remain for both these and other systems of aliasing in space and time.

Also GO-SHIP, gravity, winds,... et al.

DATA TYPE	Number of values	Duration
Altimetry: TOPEX/POSEIDON	4500/day	1993-2005
Altimetry: Jason	4500/day	2002-2005
Altimetry: Geosat-Follow-On	4300/day (gridded)	2001-2005
Altimetry: ERS-1/2, ENVISAT	3800/day	1995-2005
Hydrographic climatology	16 million	1950-2002 inhomogeneous average
Hydrographic climatology	included above	multidecadal average seasonal cycle
CTD synoptic section data	17 thousand	NA
XBTs	1.4 million	1992-2005
ARGO Float profiles	2.1 million	1997-2005
Sea Surface Temperature	5.3 million	1992-2005
Sea Surface Salinity	24,238	1992-1999
TMI	1.5 million	1998-2003
Elephant Seal Profiles	21,000 profiles	2005
Geoid (GRACE mission)	1 degree resol.	NA
Bottom Topography	1 degree resol.	NA
FORCING:		
Windstress-scatterometer	9.4 million	1992-99, 7/1999-2005
Windstress	192x94 Gaussian grid (approx. 1.875 deg) 6-hourly	1992-2005
Heat Flux	"	1992-2005
Freshwater Flux	"	1992-2005
Short/long Wave Radiation	"	1992-2005

WOCE produced extremely disparate data types with very different sampling properties.

How to put those together to create an understanding of what the ocean is doing over days to decades?

As of 2008

(Some withheld data:TOGA/TAO, drifter velocities, tomography,…)

By about 1992 was clear that a considerable part of WOCE was actually going to happen. (Notably little about high latitude oceans/ice.) Chemistry (particularly carbon measurements) were off-loaded to a different community.

Meteorologists were long experienced in using global scale data, particularly in on-going numerical weather prediction. They had a considerable governmental infrastructure to handle the data, develop and run models, interpret the results. Physical oceanographers had nothing like that.

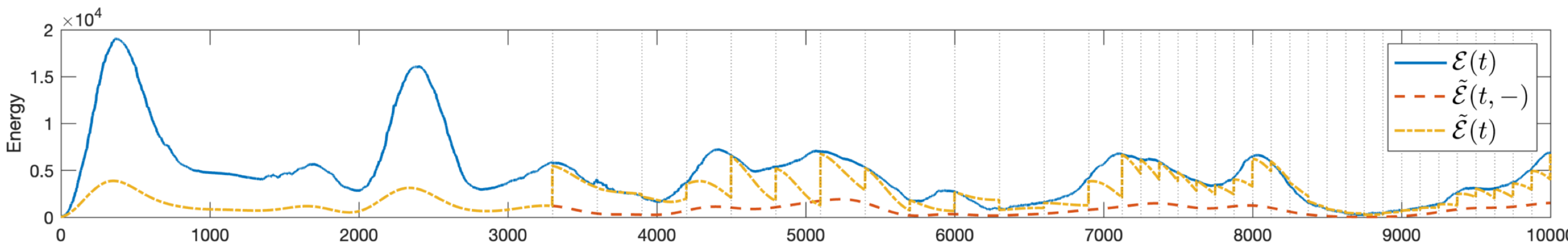
NWP represented a false analogy: As early as 1942, Norbert Wiener had divided the goals of time-series analysis into 3-components (1) Prediction (extrapolation), (2) filtering (now-casting) (3) smoothing (state estimation). He and many others developed the distinct tools for the 3 goals.

For NWP, if some method yields useful short-range forecasts, one should use it, even though it may not be fully understood (a bridge can be built without full understanding of stress-strain, etc. physics). But *WOCE and subsequent programs were directed at understanding of a highly time-dependent, turbulent ocean*---with climate-scale prediction postponed.

Getting funding for what eventually became ECCO was not so easy!

ECCO---Seeking estimates of the time-evolving ocean state that were physically interpretable:

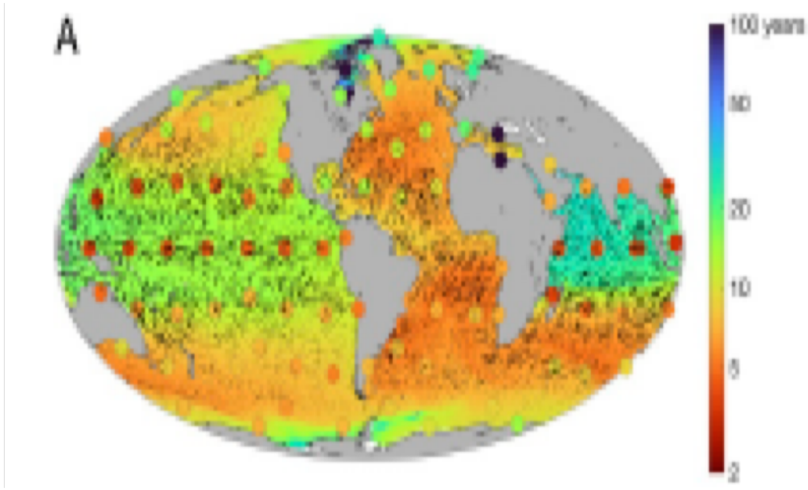
Nature, and time and space *distribution* of data can have a strong effect on the results. Requires quantitative understanding of *both* data and models---*a challenge to conventional educational programs!* (Toy models: Wunsch, Williamson, Heimbach, 2023).



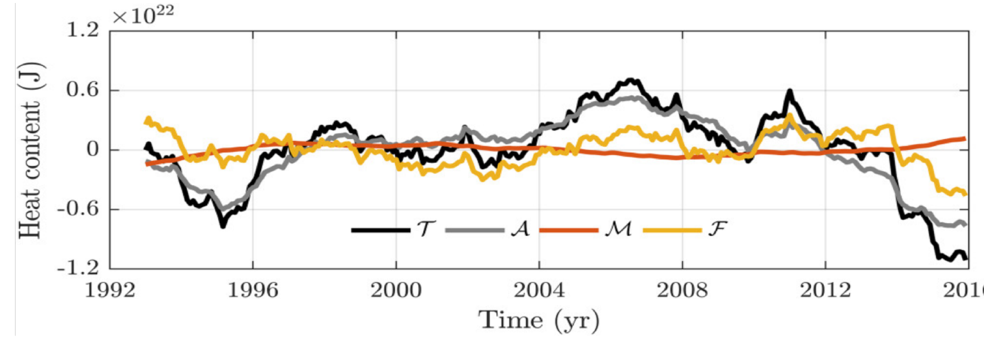
than NWP data assimilation.

Predictor-corrector methods underlying NWP and reanalyses generally fail to conserve mass, energy, vorticity, etc. The goal of what became ECCO was to produce truly global physically interpretable time-changes and mean states. Computationally challenging!

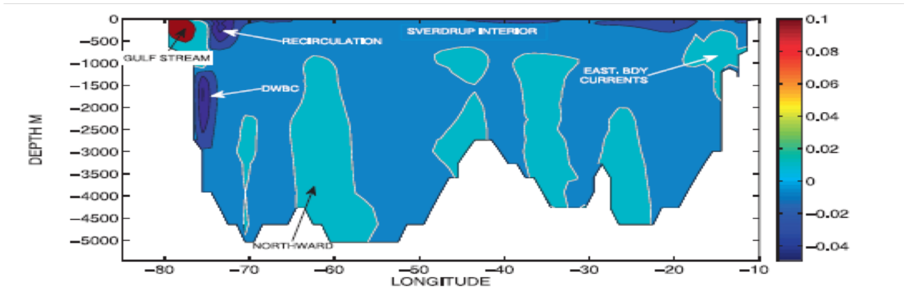
Lots of applications



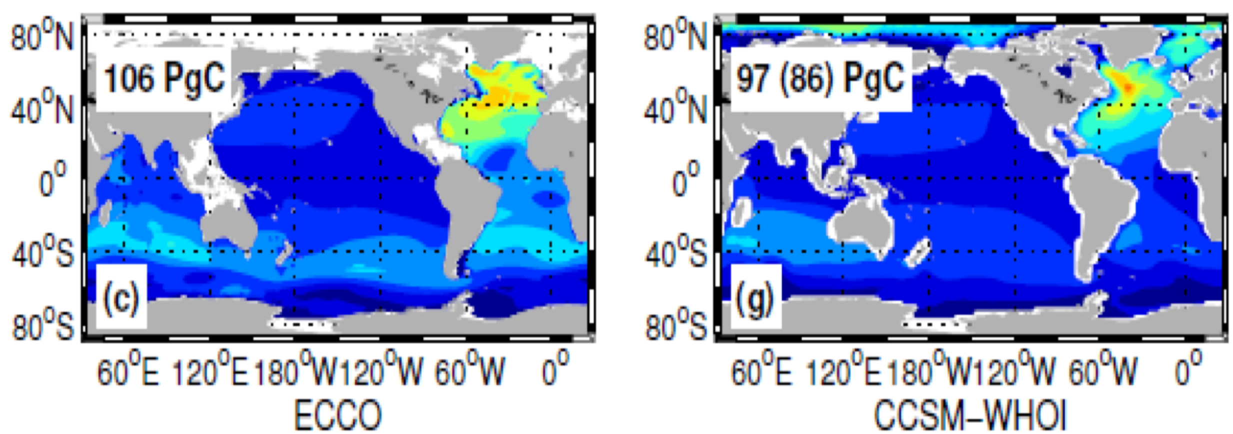
Plankton movements. Ward et al. PNAS 2021



Heat content. Desbruyeres et al. 2021



Flows,Wunsch&Heimbach 2013



Carbon uptake, Khatiwala et al. 2013

Also, ocean dynamics, earth rotation, sea level, ecology..

Where do we go from here?

Already ongoing:

Coupling to observed and realistically modelled atmosphere and ice components. Climate-active biological components. Numerous studies of the physics and other properties of the modern circulation and its changes over the ECCO period.

Ongoing, but needs more attention:

Much better estimates of uncertainty of the state and its controls and of derived products (e.g., carbon uptake).

Exploitation of the information flow content of the adjoint solution space.

Ongoing but must continue indefinitely:

Basic Science:
Maintenance of the global observing systems---including, especially, their calibration (essential to the uncertainty quantification), sometimes complex decisions about replacement technologies (e.g., swathe altimetry for nadir altimetry).

More fundamental understanding of the turbulence closures being used. To that end, ever-increasing spatial (and necessary temporal) resolution to test those closures. (Has Moore's law ceased to operate?)

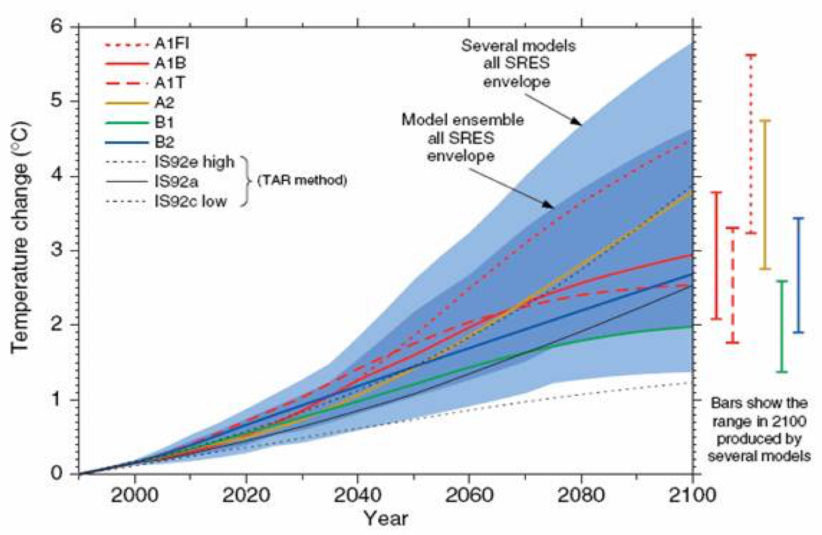
Understanding of the elements of the circulation (etc.) that are (1) globally applicable, (2) necessarily restricted to particular regions (is every grid point in the ocean unique in its physics?).

The long-term future:

Both basic science and applications:
Maintain and expand the observing systems. (Are there unexploited technologies, including e.g., new satellite measurement systems?)
Consider radically new model types (quantum?).
Other misfit measures (1-norm; infinity-norm are more robust)

Mainly applications but with strong basic science elements:
Climate system predictions with believable uncertainties, both global and regional. (Is there a predictability horizon? What controls it?)

Extend to quantitative risk assessment (probabilities).



IPCC. Why? Chaos? Systematic error? Stochastic error (random walk)? Missing elements (e.g., human intervention)?

Many interesting problems and applications to come!

Thank You