

2.1

Global Problems and Global Observations

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2.1.1 Different views of the ocean

Some readers of the wider literature on the general circulation of the ocean come to recognize that the writing suffers from a kind of multiple personality disorder. As with the actual psychiatric situation, the different personalities do not always recognize the existence of the other individuals. One can identify several of these personalities:

1 *The descriptive oceanographers' classical ocean.*

This ocean circulation is large scale, steady and laminar. Its origins lie with the technology of the mid-nineteenth to late-twentieth century, which was almost entirely ship-based. To obtain a description of the ocean, one had to lump together observations spanning many decades and treat them as though they were simultaneous. Out of 100 years of observations arose the many attempts to depict 'the' oceanic general circulation as in the diagrams of Defant (1941), Wüst (1935), Sverdrup *et al.* (1942) and many, many others. A recent example is Schmitz and McCartney (1993). The extreme version of this approach is the Broecker (1991) 'global conveyor belt', in which the caricature of the circulation renders it as a geological structure. It is an important corollary of this view that there is a unique general circulation – usually given in terms of mass fluxes – with all other property fluxes such as temperature or nutrients computed as the product of the mean flows and the local, supposed smoothly varying, property distributions.

2 *The analytical theorists' ocean.* The beginnings are often traced to Sverdrup (1947), Stommel

(1948) and the large literature that followed. A recent depiction of this ocean is in Pedlosky (1996). This impressive theoretical construct is very similar in its framework to the classical descriptive one. The ocean is thought of as essentially quasi-steady, in which features such as the Gulf Stream have nearly fixed paths. Any time variability is, as in **1**, thought of as 'noise'. There are many similarities between the pictures derived from **1** and the theory as it emerged in **2**, although the resemblance is rarely made quantitative; usually appeal is made only to pictorial similarity. (For a crude example of a quantitative test, that of the Sverdrup relationship, see Wunsch and Roemmich, 1985; Hautala *et al.*, 1994.)

There is an important field called 'geophysical fluid dynamics', whose goal might be described as the reduction of geophysical systems to terms sufficiently simple so as to produce a deep understanding of the various dynamical processes. To some degree, the analytical theorists' ocean (**2**) is that of geophysical fluid dynamics. Sometimes, however, the original simplifications are forgotten and the reduced models are then confused with the real system.

3 *The observers' highly variable ocean.* Here the focus is on elements of the ocean circulation that are time variable and that often also exhibit very strong spatial structures over very short distances (the lowest baroclinic Rossby radius of order 30 km or less). The emphasis, again for reasons of technological capability, has until very recently been on regionally varying elements. This view of the ocean emerged during

the early 1970s with programmes such as the Mid-Ocean Dynamics Experiment (MODE), the Coastal Upwelling Experiment (CUEA), POLY-MODE (an amalgam of the Russian POLYGON with the Mid-Ocean Dynamics Experiment) and International Southern Ocean Studies (ISOS). In this view, there are multiple, local, general circulations. Mean mass fluxes are perceived to be complex spatially varying fields $\langle \rho(x, y, z) \mathbf{v}(x, y, z) \rangle$ where x, y, z are three space coordinates, $\mathbf{v} = [u, v, w]$, ρ is the fluid density, and the brackets $\langle \cdot \rangle$ denote the true time average. But the circulation of any scalar field, C (e.g. temperature, carbon, or potential vorticity), must be computed as $\langle \rho(x, y, z, t) \mathbf{v}(x, y, z, t) C(x, y, z, t) \rangle$, where t is time. Unfortunately,

$$\begin{aligned} \langle \rho(x, y, z) \mathbf{v}(x, y, z) \rangle & \langle C(x, y, z) \rangle \\ & \neq \langle \rho(x, y, z, t) \mathbf{v}(x, y, z, t) C(x, y, z, t) \rangle \end{aligned} \quad (2.1.1)$$

where the failure of the equality can be at zero-order. An extreme case of such a failure would be the attempt to calculate the time-average temperature transport of the Gulf Stream by multiplying the time-average mass transport (a slow, broad flow) by the time-average temperature field, also a broad, slowly varying field. The true temperature transport involves averaging the product of a very narrow intense jet with a field having a narrow, very large instantaneous temperature maximum. In such a situation, the two sides of inequality (2.1.1) can and do differ by an order of magnitude. Because the covariances between the mass flux fields and C will be different for differing fields C , the circulation of each will be different, and often radically so.

Because of the regional focus, these differing general circulations are not usually pieced together into any sort of global picture. They remain rather, as isolated sub-basin scale, stand-alone pictures.

- 4 *The high-resolution numerical modellers' ocean.* Beginning in about 1990, basin-to-global scale models emerged that contained variability resembling the oceanic mesoscale eddy field. The character of this newly emergent view of the ocean can be seen, e.g. in the results of Semtner and Chervin (1992), Böning *et al.* (1991), Cox (1985), Smith *et al.* (2000) and others. These results, when heavily averaged,

have a qualitative resemblance to some elements of all of 1–3, but differ quantitatively from all of them. The general circulation property fluxes differ, as in 3, from those of mass alone, because as in 3, the mass flux/property covariances are all different, but the scope is global.

Little communication between the apostles of these different personalities appears to exist; nearly disjoint literatures continue to flourish.

2.1.2 The origins of WOCE

By the late 1970s, a few scientists had begun to recognize that the various circulation personalities listed above could be thought of as originating in two conflicting paradigms (to use the terminology of Kuhn, 1962), although no one seems to have described it that way at the time. The two paradigms are:

- 1 The ocean is a quasi-steady, large-scale equilibrium system. I will refer to this as the 'historical' paradigm.
- 2 The ocean is a fundamentally turbulent, constantly changing, non-equilibrium system in which no element is actually fully steady. I will call this the 'WOCE' paradigm, although it would be wrong to claim that all those who formulated and carried out WOCE were subscribers to it.

2.1.2.1 The historical paradigm

The ocean is opaque to all forms of electromagnetic radiation at usable wavelengths, hence modern oceanography, from its beginnings in the late nineteenth century, has been built upon an observational base acquired by physically placing an instrument at particular positions and depths of interest. For other technical reasons, most such measurements have been not of the velocity field in the ocean, but rather of scalar quantities such as temperature, salinity, oxygen content, etc. Because ships provided the only platform for reaching mid-ocean locations, it took many decades to acquire observations adequate to delineate the bulk scalar properties of the ocean.

By great good fortune, the ocean was early on perceived to display large-scale, temporally stable, contourable fields of these tracers ('tongues', etc.; see particularly Wüst, 1935; some of these pictures are reproduced in Wunsch, 1996). It was thus possible to combine the scalar observations spanning

multiple decades into a qualitatively consistent picture. This distribution of properties was interpreted in terms of a corresponding large-scale, steady, flow pattern. Theories (wind-driven, thermocline, etc.) were then constructed that produced flows resembling the required circulation patterns.

It was inferred from this picture of very slow, creeping or 'spreading' flows, that the relevant time scales for serious change in the ocean circulation had to be of order 1000 years and longer, with the abyssal ocean acting only as a passive reservoir responding to the divergences of the upper ocean.

Until about 1975, available computing power permitted only coarse-resolution, extremely viscous numerical models of the ocean. These models qualitatively mimicked the available analytical solutions and this close agreement tended to confirm the historical paradigm. Almost all textbooks – oceanographic, meteorological and climate – as well as many current research papers, still reflect this view of the ocean circulation.

2.1.2.2 The WOCE paradigm

At the time WOCE was conceived, in the late 1970s, use of the new technologies, especially those able to produce time series, such as moored current meters and drifting floats, had begun to make it clear that the flow field in the ocean was very different from the steady, large-scale simple flow fields that had emerged out of the tongue depiction by Wüst and others, and from the analytical and numerical models. Rather it showed a flow field dominated by what has come to be called 'mesoscale eddies', but which is actually much more complex than any single, mesoscale¹ phenomenon. The kinetic energy of the variability was seen to be roughly two orders of magnitude larger than that associated with the quasi-steady basin scales. Indeed, a major supposition behind much of the WOCE design was that the ocean changed on all space scales from the sub-Rossby radius to the entire global circulation, and on all time scales out to the oceanic lifetime.

The presence of an intense variability does not necessarily mean that it has any dynamical or kinematical consequences: it could be a purely passive 'noise' phenomenon, causing sampling (aliasing) difficulties, but of no further consequence. But it is also true that it could be of enormous kinematical and dynamical consequence with, for example, the

tongues being nothing but the integrated fields obtained from an extremely complex small-scale structure, rather than implying large-scale steady mean flows (for an example, see Hogg and Owens, 1999, or any of the high-resolution model calculations, such as that of Smith *et al.*, 2000).

Within the group that designed WOCE there were two other overlapping, but nonetheless conflicting, views as to how best to understand the oceanic role in climate. These two views were that ultimate insight would be best obtained by:

- 1 regional and process-focused studies; or
- 2 observing where and how the ocean is changing globally.

Finally, of course, WOCE followed both these strategies to some degree. Regional programmes such as the Subduction Experiment, Brazil Basin Experiment and the Purposeful Tracer Experiment were central to what came to be called WOCE 'Core Project 3 – Gyre Dynamics'.

In the end, however, the major activities in WOCE were on the global scale. Achieving this global strategy was not so easy. The decade of the 1970s, beginning with, among others, the Mid-Ocean Dynamics Experiment (MODE Group, 1978), can be called the 'decade of the mesoscale'. Classical hydrographic work, which was often used to depict the global-scale ocean of the steady paradigm, had come to many to seem less 'scientific' than did the study of physical processes. The latter became accessible through powerful new technologies, including moored current meters, neutrally buoyant floats, Current-Temperature-Depth probes (CTDs), bottom pressure gauges, etc. In particular, the occupation of long hydrographic lines had almost ceased. Figure 2.1.1 shows most of the transoceanic hydrographic lines that had been obtained following the last International Geophysical Year (IGY) lines of 1959, and prior to the first WOCE discussions of 1979. The Eltanin Survey (see Gordon and Molinelli, 1982) was an exceptional large-scale survey of the Southern Ocean. Furthermore, numerical models (e.g. Holland, 1978) were beginning to show intense variability that seemed to confirm an entire physical realm of oceanography previously completely unexplored. There was a clear sense that the scientific future lay with regional studies: much of the community was focused on processes and the 'physics' of internal waves, the mesoscale, upwelling, tropical waves and the like.

¹ 'Synoptic' scale to a meteorologist.

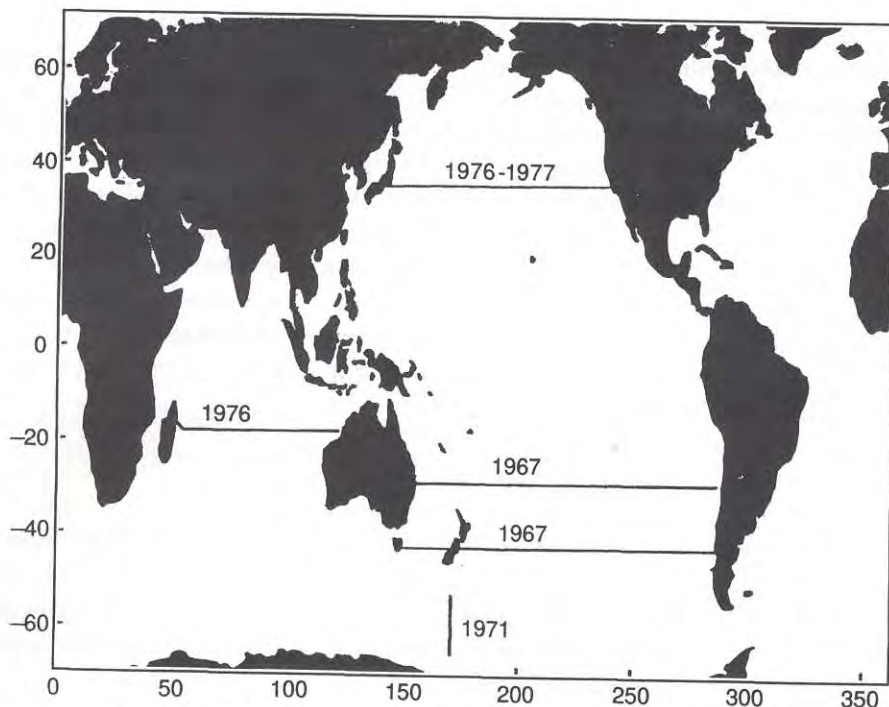


Fig. 2.1.1 The very few trans-oceanic hydrographic sections obtained during the period following the IGY (1958–59) and preceding the WOCE design period. This latter coincided with the resurvey of the North Atlantic (early 1980s), which to some extent marked a return to large-scale physical oceanography following the mesoscale, and other process programmes that dominated the 1960s and 1970s.

Much of the debate that took place in the WOCE formulation dealt with the desirability, and feasibility, of obtaining global coverage, not only by hydrography, but also with satellites, expendable bathythermographs (XBTs), etc., and the extent to which these were possible (the satellite technologies were very new; see, e.g. Born *et al.*, 1979).

As we finish WOCE, I am concerned that we are emerging with the same two paradigms (and the various conflicting personalities) largely intact and almost undiscussed. Ocean science following WOCE is emerging as having a major focus on climate problems, including those involving time scales of decades and longer. WOCE was intended to provide a mechanism by which this transition to climate programmes could take place. But given the long time scales of climate change, the possibilities for self-delusion grow abundantly, and the question of what observations are required so that we come to understand the system are of very great importance.

It is the import of this chapter that more than ever one requires an adequate, long-term observational system: there is a tendency to think that models will come to substitute for observations. The basis of the hope is the understandable wish that one can evade the necessity of waiting the decades required to observe the actual climate system by modelling it instead.² Although models are extremely important elements of the arsenal for understanding climate change, and will become increasingly so, the entire history of fluid mechanics (and it is worth recalling that physical oceanography is a branch of fluid dynamics) shows that without observations one generally goes seriously astray. A famous example in classical fluid dynamics is the discrepancy between the Stokes solution for flow around a sphere and the flow actually observed in the laboratory.

2.1.2.3 Consequences

For anyone attempting to understand climate, the consequences of the application of the two views is

² A reviewer thinks I am beating a dead horse here: that no one believes models are a substitute for observations. But at least one very influential meteorologist has publicly and repeatedly asserted that because there is no 'physics' in the ocean (in the peculiar meteorological sense of that term), one can model everything adequately without data.

Table 2.1.1 Frameworks (paradigms) of the ocean circulation

Historical	WOCE
<p>Ocean is nearly steady, fundamentally large-scale, and laminar</p> <p>Modelling consequences</p> <ul style="list-style-type: none"> • Coarse resolution adequate • Quasi-steady adequate • Integration over very long periods is accurate <p>Observation consequences</p> <ul style="list-style-type: none"> • Greatly simplified: keep coarse track of upper branch of 'global conveyor belt' 	<p>Ocean is unsteady on all space and time scales and fundamentally turbulent</p> <ul style="list-style-type: none"> • Extremely high resolution probably required (<1/12° in boundary currents) • Highly non-steady (turbulent fluid flow) • Systemic errors in physics may well accumulate over long periods of time and swamp the results <ul style="list-style-type: none"> • Very serious and demanding problem • Small spatial scales and long temporal scales must be observed • Not possible to rule out any oceanic depth or region as irrelevant
<p>Schematic of the consequences of the two different views of the ocean circulation – ranging from the 'historical' one of a basically steady, laminar system, and the view that one is dealing with a constantly changing intrinsically turbulent fluid flow. If either view were completely correct, the modelling and observational consequences would be quite extreme. In practice, both are partially valid, but the existence of any turbulent component forces one towards the consequences of the fully turbulent system.</p>	

very important. A 'telegraphic' summary would be as listed in Table 2.1.1.

I am acutely aware that the debate as to the oceanographic future is ongoing, and that many organizational developments are taking place. Here I run the risk of preaching to the choir; perhaps by the time this book appears, the community will regard what I say here as obvious, and will have moved to accommodate these views. I hope that this will be true.

2.1.3 What do we know?

WOCE has completed its field phase, and the serious analysis of the data has only begun. It is nonetheless possible to draw some immediate conclusions. Much of the data, but especially the TOPEX/POSEIDON altimetric data, now 7+ years long (as of early 2000), shows the extremely active time dependence of the oceanic circulation. (Wunsch and Stammer (1998) review the altimetric results.) Other widespread data sets, e.g. the neutrally buoyant floats (Davis, 1998a,b; Hogg and Owens, 1999), current meter records, surface drifters (e.g. Niiler and Paduan, 1995), etc., all confirm the turbulent nature of the flow. This extreme variability is confirmed by the high-resolution general circulation models that have been developed as part of and alongside WOCE (Semtner and Chervin, 1992;

Stammer *et al.*, 1996; Smith *et al.*, 2000). In parallel with WOCE and its preparations, it was shown that tracers such as tritium and chlorofluorocarbons penetrated to the abyssal seafloor from the surface, on time scales of 10 years, rather than the hundreds to thousands of years (e.g. Östlund and Rooth, 1990) suggested by the 'historical' view. The palaeoceanography core records show that major climate shifts occurred in the ocean on time scales of order a decade and even less (e.g. Boyle, 1990). Furthermore, even the coarse-resolution models were suggesting that the ocean circulation could undergo dramatic shifts on very short time scales (e.g. Manabe and Stouffer, 1994; Marotzke and Willebrand, 1991; Weaver *et al.*, 1993), much shorter than the historical view would lead one to expect.

A major component of the climate role of the ocean involves its transports in three dimensions of the scalar fields of heat (temperature), fresh water (salt), carbon, etc., whose large-scale structures have pervaded the discussion of the ocean circulation for more than a century. It is too soon in the WOCE analysis phase to quantify the degree to which these property transports and their variability through space and time, seasonally to inter-annually to decadal and beyond, are the result of (a) a simple time-dependent version of a laminar conveyor belt; (b) the result of a complex integration

over fully turbulent elements; or as seems likely, (c) some complex combination of both. (A particularly striking example of the complexity of the processes that maintain the tongue-like features can be seen in the Brazil Basin float and tracer results of Hogg and Owens (1999), where some large-scale, laminar flow may well coexist, but which is almost imperceptible in the data.)

But given what we see in the WOCE data and elsewhere, and what the high-resolution models are telling us, it seems a vast leap of faith to assert that one can simply integrate coarse, laminar, representations of the ocean circulation for hundreds to thousands of years and to expect that the Lagrangian property transports of heat, fresh water, carbon, etc., are being computed with useful skill. If one can do so, it is either a result of extraordinary good fortune that for this particular fluid, the turbulent fields are purely passive, or that the General Circulation Model (GCM) builders are so clever they have solved the problem of fully parameterizing the turbulent elements, boundary jets and other unresolved elements of a three-dimensional rotating, stratified fluid in a complex geometry. If the latter is actually true, it is a remarkable achievement of computational skills, a landmark in the history of fluid dynamics.

2.1.4 The need for global-scale observations

2.1.4.1 Unobserved regions

When adequate observations have been unavailable to depict the world, the human race has generally reacted in one of two extreme forms. The first form is represented by the map of New Guinea shown in Fig. 2.1.2. With no observations, the cartographer felt the need to show non-existent mountains and monsters, providing an exciting metaphor for the general unknown. The opposite extreme is to assume that ‘the absence of evidence is evidence of absence’ and to infer that nothing interesting exists where nothing has been observed. Examples are the nineteenth century inference of a biological desert in the abyss (Mills, 1983), the modern (until about 1965) assumption that the deep Pacific Ocean was devoid of any significant flow, and the inference of the existence of oceanic abyssal plains in the absence of sounding data. It is easy to make a list of phenomena that were so widely and plausibly believed to be absent

or irrelevant that little effort was made to test the hypothesis by observation:

- *Things everyone ‘knew’*:
 - No interesting currents at depth on the equator (1950)
 - Vertical temperature and salinity profiles are smooth (1965)
 - No interesting abyssal flow structures in the Pacific Ocean (1965)
 - Ocean heat transport is negligible compared to the atmosphere (1970)
 - The deep interior ocean moves at climatological speeds (1958)
 - Ocean mixing is geographically uniform (1970).

The date given is (very approximately) when the hypothesis started to become untenable because someone became convinced it didn’t have to be correct, and set out to test it.

There is a parallel problem in the inference of overly simplified depictions of the ocean circulation, a depiction with its roots in personality 1 described in Section 2.1.1 above. Broecker’s (1991) well-known ‘conveyor belt’ has often been used to describe the role of the ocean in climate. The picture is a visually powerful metaphor for the circulation; trouble occurs, however, when scientists begin to take it literally, forgetting its metaphoric origin. The conveyor belt has been invoked to claim that observing the ocean climate state is cheap and easy – because observations can be confined to a few upper ocean XBTs in the northern North Atlantic. As one eminent meteorologist once insisted to me, one ‘need only keep track of the upper branch to know what the whole ocean is doing’. The picture has also been invoked frequently to confirm the old prejudice that the deep ocean does nothing of interest; it has kept alive the completely incorrect view that water flowing through the Indonesian Passages connects immediately and directly across the Indian Ocean to flow around the Cape of Good Hope, among other myths.

Schematic pictures of the circulation are of course, very helpful, both as mnemonics and as aids in conveying to the non-scientific community how the system ‘works’. But when employed out of context, they can be extremely misleading. Consider the schematic in Fig. 2.1.3a (see Plate 2.1.3a, p. 76) of communication links between the



Fig. 2.1.2 Map (de Jode, 1593) showing the interesting fauna and non-existent landscape (an entire mountain range) on the coast of New Guinea and Australia placed there by mapmakers who felt compelled to show something where nothing was known. Modern scientists would leave blank such unexplored areas; sometimes these blanks are reinterpreted to imply that nothing of interest lies there – an equally unfounded hypothesis.

US and Europe. Information flows along the lines indicated and the picture would make a sensible cartoon for a public discussion of volume and fluctuations of information traffic. The picture has the great virtue of extreme simplicity. But if one looks at one indicator of the routes of information flow on modern computer links (Fig. 2.1.3b, see Plate 2.1.3b, p. 76), one's understanding of the situation, and consequent policy decisions, are likely to be quite different.

Gradually it has become clear that climate is a global phenomenon. Nonetheless, in parallel with the listing above of historical failure of some

simplifying hypotheses, there exist today a number of hypotheses that are often invoked in discussions of the ocean observation problem. They include:

- *Things many (some?) people 'know' now:*
 - There is a global conveyor belt whose strength controls the climate state through thousands of years.
 - Only the upper ocean is relevant to 'climate'.
 - Coarse resolution ocean models can be run for 1000+ years with climate forecast skill.
 - The strength of the oceanic heat transport is controlled by the surface density gradient alone.

- Only the upper tropical ocean influences the El Niño-Southern Oscillation phenomenon (ENSO).
- All changes in the circulation have deterministic causes.
- The ocean is in equilibrium with the atmosphere (the present-day ocean state is caused by interaction with the present-day atmosphere).

Few meteorologists would argue that large regions of the atmosphere can be omitted from their models without degrading them: there is no argument known to me that any part of the atmosphere is decoupled, laterally, from the rest on time scales exceeding a day or two. Similarly, it seems unlikely that any part of the ocean can safely be assumed to be unchanging, and passive, in attempts to model the system over periods from a few to thousands of years. Thus ocean modellers have gravitated towards making their models truly global. But the more sophisticated a model is, the higher is the data quality required to test it (a crude model only requires crude tests). Such models have to be tested and calibrated everywhere – an error in high latitudes will eventually ‘bite’ one in the tropics after the fluid has carried that erroneous information through the system. That transmission may take a long time, but eventually, on some climate time scale, it will probably happen. One must have observations everywhere capable of delineating all space and time scales.

2.1.4.2 Too short records

We know from the geological record that climate has been changing ever since the time when it makes sense to speak of such a phenomenon, i.e. since about 3 billion years ago. Within this enormous span of time, changes have been taking place on every time scale definable as ‘climate’, i.e. from a year or two, to millions and billions of years.

There is an enormous temptation to interpret the record of change as seen in one’s lifetime, or even in human history, as being representative of the climate system as a whole. Such a temptation is fraught with grave dangers. Systems with memory, and the climate system, particularly the ocean (and cryosphere), have long-lived and multiple time-scale memories, undergo often highly unintuitive random walks. This type of behaviour is too readily converted into interesting deterministic stories. The tendency to long random walks is the

central element of Hasselmann’s (1976) stochastic theory of climate. It is easy to give examples where one’s intuition can fail very badly in these circumstances, and I have written about this phenomenon elsewhere (Wunsch, 1992, 1999a). Consider by way of example, the ‘temperature’ record in Fig. 2.1.4a (Wunsch, 1992). Taken by itself, it resembles many oceanographic time series. One might be tempted to think of the maximum near time 2500 as representing a climate extreme. But this record was generated simply as the accumulating sum of that shown in Fig. 2.1.4b – which is pure white noise. The existence of a maximum near 2500, a strong minimum near 1800, and the apparent long-term trend in between, has no ‘cause’ other than the stochastic accumulation of a few excess positive or negative values in a long run of a purely random sequence.

Another example can be seen in Fig. 2.1.5; here the transport of a western boundary current and its spectral density are displayed. Again, the variability is not visually very different from that seen in practice (e.g. Schott *et al.*, 1988). In this particular case, however, the variability was generated in a theoretical Stommel-gyre by a purely stochastic variability (white noise in both space and time) in the interior wind forcing.

It is often argued (as in the WOCE design period), that large-scale observation systems are not required – that it suffices to instrument so-called choke points of the system and to monitor the behaviour of the flow through them. The most commonly described choke points include the Drake Passage and the Florida Straits, as well as the Indonesian Passages, and sometimes the region south of the Cape of Good Hope. But Fig. 2.1.5, and others like it, show this type of strategy to be fallacious: choke points are regions where any stochastic forcing and response for the entirety of the ocean sum together, and where one *expects* to see large-scale positive and negative excursions like that in the figure. The temptation to ascribe such fluctuations to large-scale deterministic changes, e.g. in the wind field, has not often been resisted (see the literature particularly on the Florida Current). The circulation certainly contains *both* stochastic and deterministic elements. Separating them can be quite challenging, requiring adequate observations to depict the response of the complete ocean interior. Often, the random walk explanation will prove the simplest

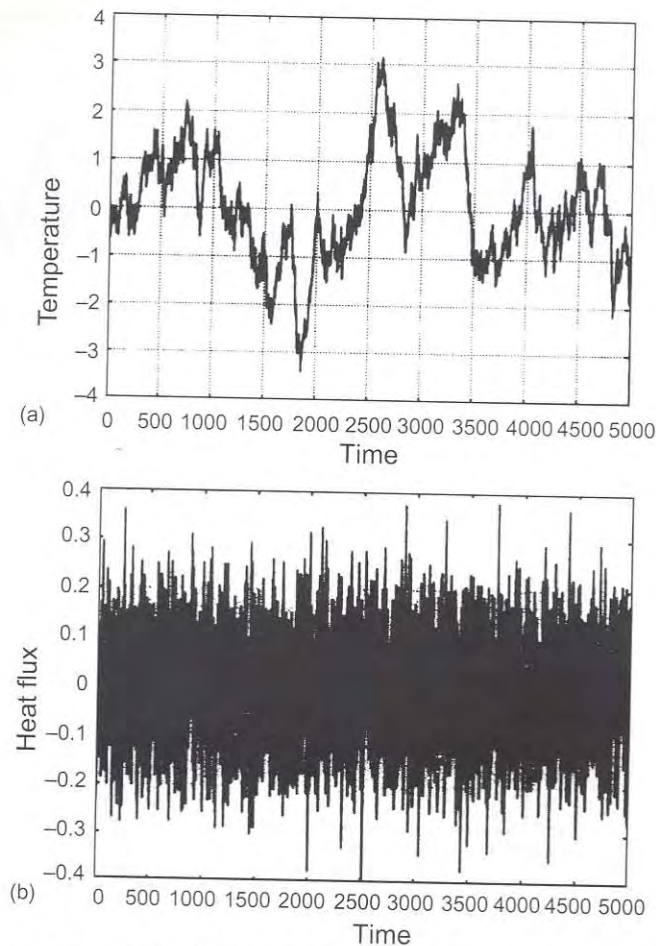


Fig. 2.1.4 (a) A pseudo-temperature record whose visual structure is much like many real oceanographic time series (it is a 'red noise' process). The extreme values, and apparent trends between them, lend themselves readily to rationalizations in terms of deterministic causes (Wunsch, 1992), but the record was generated from the 'forcing' in part (b).

(b) A pseudo-random number generator was used to generate this white noise sequence, whose running (accumulating sum) is depicted in (a). Excursions in (a) are nothing but chance runs of excess numbers of negative or positive values (see discussions in Feller, 1957; or Vanmarcke, 1983).

and could be regarded as the most compelling null hypothesis, although it is the deterministic cause that is usually assumed to be correct.

A third example is perhaps even more troubling: Fig. 2.1.6a shows Brooks's (1923) demonstration that central African lake levels appear to follow the 11-year sunspot cycle. Figure 2.1.6b shows what happened subsequently. The reader is referred to Pittock (1978) for this and many other examples where short-record visual correlations suggested a non-existent causal relationship. Similar published examples of misleading short-record correlations can be replicated indefinitely. The problem is greatly compounded when

investigators start to shift in time the relative positions of two or more records so as to determine time lags. The probability of 'false positives' then grows enormously.

None of these examples just cited can be used to refute the possibility that climate change is simply causal, and so simply interpretable, and possibly even predictable. All they do is suggest that, for some purposes, there is no substitute for very long records over the entire global ocean. Climate will never be properly understood until the records encompass many realizations of the time scales one is examining (as scientific history seems to show to be true for all physical phenomena).

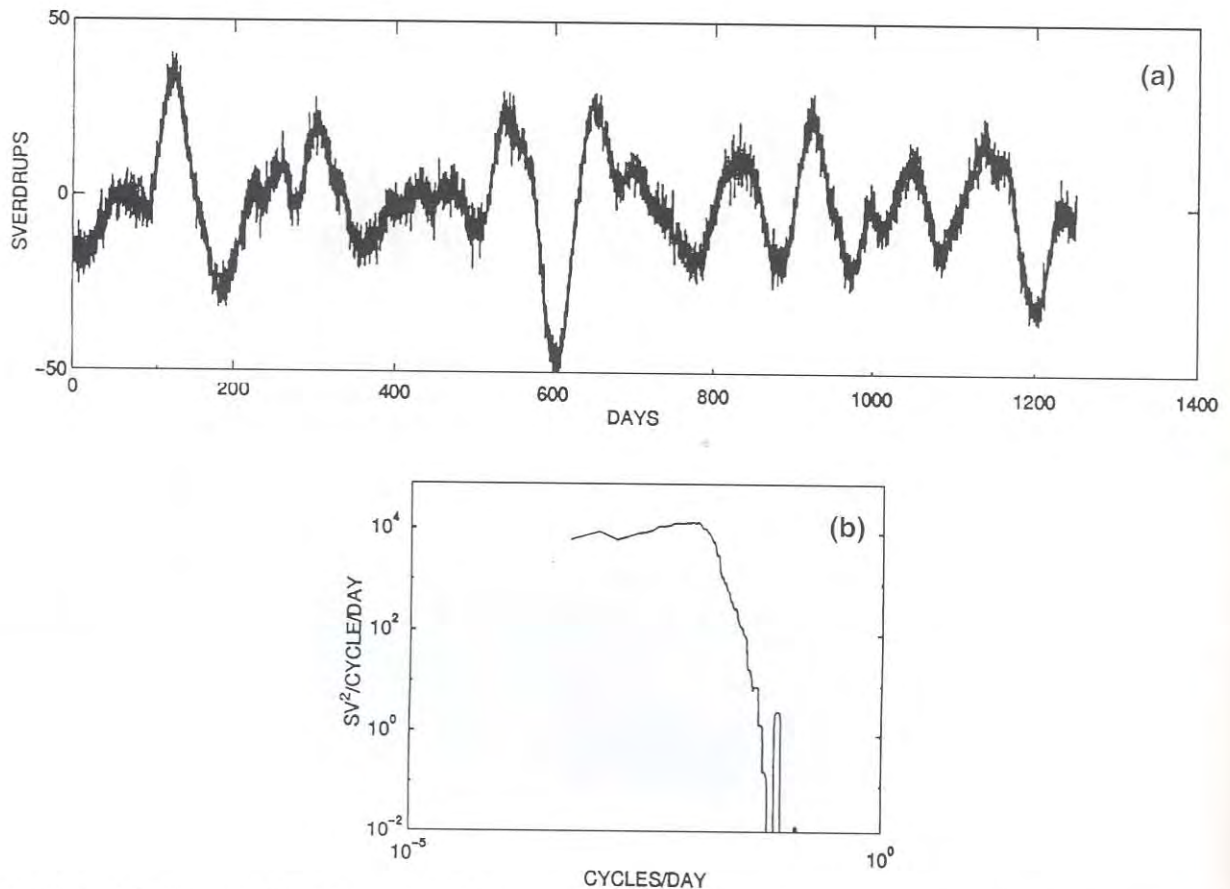


Fig. 2.1.5 (a) 'Transport' of the Stommel Gulf Stream when the interior wind field has a purely white noise (space and time) curl. The transport excursions are the result of the random summations of the responses over the entire domain of the model ocean.

(b) Power density spectrum of the record in (a) – a result similar to spectral estimates of real time series (the too rapid drop at high frequencies does not occur in the natural world – the model contains no internal waves or other noise processes).

2.1.5 Where do we go from here?

History tells us that conflicting ideas about the nature of a fluid flow can only be finally resolved by observations. We already have adequate data to demonstrate that we are dealing with a flow that has powerful turbulent elements, and that the historical picture must be abandoned as an adequate description. Something like it could conceivably ultimately re-emerge as the result of multi-decadal averaging, but such an outcome is far from assured.

The major issue for us concerns understanding the ocean as it pertains to climate prediction. Only an adequate observation base will permit us to determine the present state of the ocean, to understand which elements are undergoing secular shifts, the extent to which the ocean is predictable

beyond a year or two and, to the extent that we find predictability, to actually make forecasts.

A rational observing system is inevitably a series of compromises and tradeoffs. There is insufficient space here to discuss all of the various issues, but a few simple points are perhaps worthwhile. Our only extant true global-scale observations come from space. These, however, are currently restricted to surface properties; this restriction in turn means that only a small number of the possible measurements are really of interest, including altimetry and roughness (for the wind field). It is difficult to imagine any future ocean observations not requiring spacecraft of these types. One also needs global *in-situ* measurements. Here the tradeoffs tend to be of accuracy and precision versus the need for large numbers of measurements.

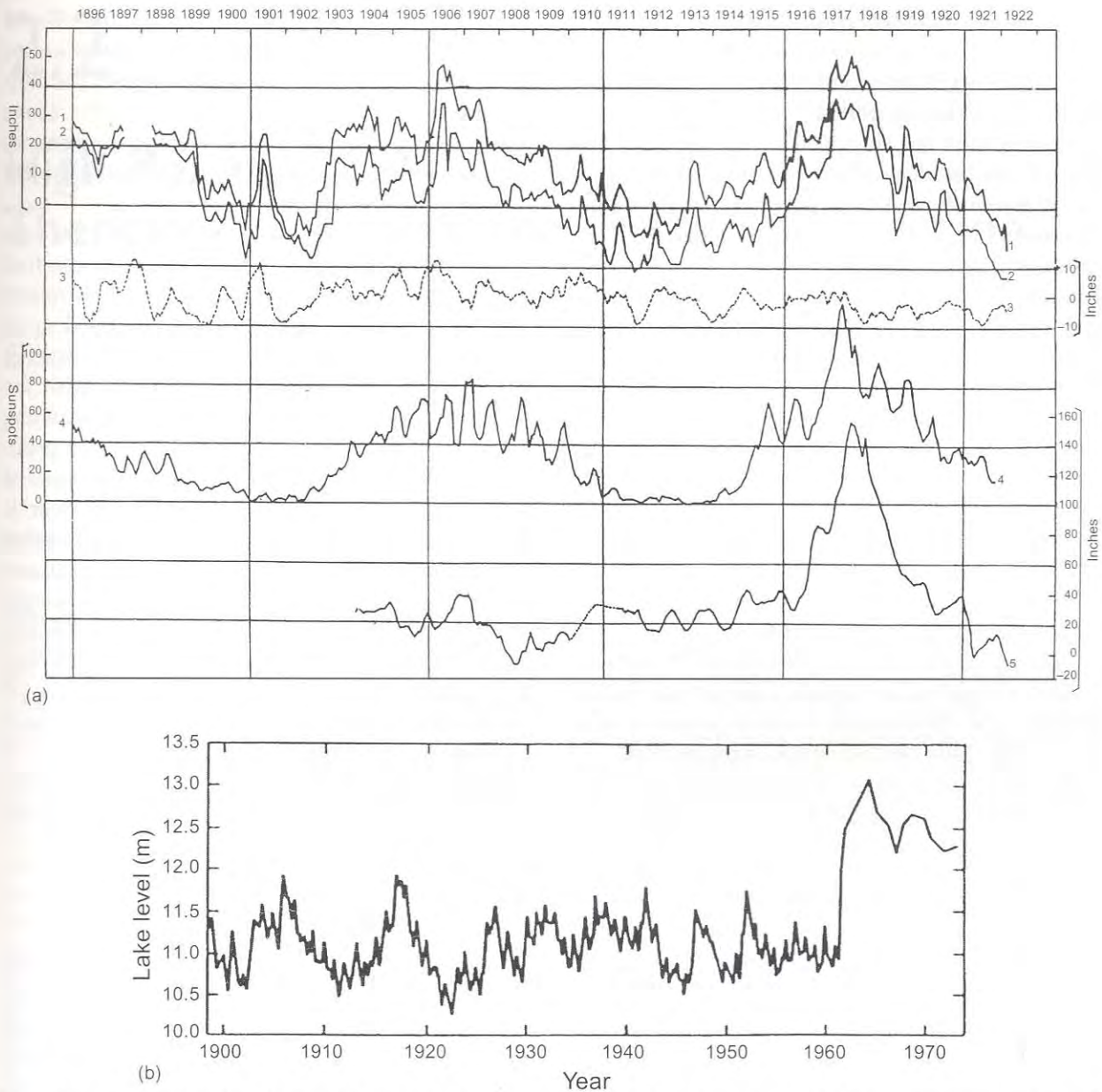


Fig. 2.1.6 (a) Brooks's (1923) graph of water levels in Central African lakes together with the simultaneous record of solar activity, which he suggested showed a possible cause and effect. Curves labelled 1 and 2 are the maximum and minimum values observed (in inches) in Lake Victoria. Curve 3 is a rainfall estimate as an anomaly from a climatology. Curve 4 denotes the monthly sunspot numbers and curve 5 is the mean level of Lake Albert.

(b) Much longer record (from Pittock, 1978) showing the subsequent evolution of the Central African lakes in which the apparent relationship to the solar cycles has broken down completely (noticed already in the mid-1930s) and would now be regarded as spurious. The abrupt change in character toward the end apparently has historical precedents prior to Brooks's record.

The Argo programme (to deploy several thousand profiling floats) is an outstanding example of what needs to be done: the production of thousands of adequate (not perfect or even the best one can do) measurements worldwide for long periods of time. A comparatively minor investment in technology

could produce a new generation of such expendable instruments of a variety of types (e.g. of long-duration expendable moorings and accompanying instruments). These and other possibilities (e.g. the entire panoply of acoustically based measurements), desperately call out for a systematic

programme of observational method tradeoffs, experimental trials, technological investment, and then sustained system evolution. No organization has emerged capable of doing these things.

WOCE itself has shown that, through the coalition of the academics, government laboratories, etc. that make up the worldwide oceanographic community, combined with modern oceanic observation and modelling technologies, the elements of a practical system already exist. This system could provide what is required, if it can be sustained and ultimately augmented. The existing system involves measurements from satellites, *in-situ* unmanned instruments such as profiling drifters, tomographic integrals, and long Eulerian time series, plus a judicious mix of shipboard measurements, all carefully carried out so as to evade the dominant aliasing effects of the variability. The modelling advances that have taken place over the past 15 years suggest that the combination of clever fluid dynamics and numerical methods, increased computing power, and the comparatively modest resources required to sustain such efforts would over the next 10–20 years become consistent with the need for fully describing the changing fluid.

But the community that put together the WOCE coalition is too small, too dependent upon year-to-year funding, too focused upon basic scientific issues, to sustain the required observations in the necessary open-ended fashion. If one seeks seriously to account properly for the ocean and its role in climate, special consideration must be given to maintaining the observation programme without destroying the underlying scientific community. Probably the central issue is that there are no operational agencies anywhere in the world that carry out large-scale systematic oceanic observations

analogous to those represented in the World Weather Watch.³ If oceanic climate issues are to be understood, the small oceanographic community will need resources, and probably the active assistance, of some operational agency or agencies to marshal the resources for an ongoing system capable of addressing the major issues.

This last statement is not a trivial one. Perhaps the greatest climate puzzle is whether one can find a way to study its 'slow physics' (and chemistry and biology) within funding systems based upon year-to-year budgets, high-frequency elections, short tenure deadlines, and the general wish for scientific results in the short term. Understanding the ocean in climate is surely a multi-decadal problem, at best. The observations and science required to solve it are fairly clear. What is less clear is whether we have the collective will, internationally, to produce the necessary effort.

It is, of course, possible to proceed by simply asserting that the historical paradigm remains valid. This assumption is enormously simplifying, and greatly reduces the costs and complexity of future climate programmes (see Table 2.1.1). Such a course is very appealing from many points of view, but seems dangerous to the ultimate understanding of climate should the assumption prove, as seems likely, false.

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³ Space agencies sometimes give the impression of being 'operational' agencies, simply because the planning and flight of spacecraft takes so long. But one of the great ironies of the US/France TOPEX/POSEIDON mission is that it has led the US National Aeronautics and Space Administration to declare its role in satellite altimetry to be near its end, and to seek actively to cease any further such measurements. The argument continues.