Towards Understanding the Paleocean

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Abstract

A comparison is made between some of the framework used to discuss paleoceanography and parallel situations in modern physical oceanography. A main inference is that too often the paleo literature aims to rationalize why a particular hypothesis remains appropriate, rather than undertaking to deliberately test that hypothesis.

> "Too much of the theory [of the ocean circulation] has depended upon purely hypothetical physical processes. Many of the hypotheses suggested have a peculiar dreamlike quality, and it behooves us to submit them to especial scrutiny and to test them by observation." H. Stommel (1954).

> "Allow people to make assumptions and they will come away absolutely convinced that assumption was correct and that it represents fact." James Randi (Quoted by George Johnson in NY Times 22 August 2007).

1 Introduction

The Editors of QSR suggested that some perspective would be useful on the differences between modern understanding of the ocean circulation and climate more generally, and the very much simplified models, conceptual and numerical, commonly used in discussing the paleoclimatic record. I have written previously at some length about some of this contrast (including Wunsch, 2006, 2007; Huybers and Wunsch, 2010) to which I refer the interested reader, and repeating that material would not be very productive. Instead, I will take the opportunity to discuss some of the less technical, more general, aspects of the problems of understanding the ocean circulation of the past.

Anyone coming from the outside to the study of paleoceanography and paleoclimate has to be struck by the general, extreme, lack of data as compared to the modern world—but where we still justifiably complain about undersampling. Although there are many proxy data of many types (speleothems, tree rings, banded iron formations, terraces, etc.; e.g. Cronin, 2010) proxy data in ice cores provide much of the time series information about the climate system over roughly the last 100,000 to 1 million years. These are obtained from Greenland and Antarctica—regions hardly typical of the global climate, but nonetheless the records are usually interpreted as being at least representative of the hemispheric state and commonly the entire globe. Marine cores carry one back some tens of millions of years, but they are available only in narrow strips around the ocean where thick sediment layers exist (e.g., Divins, 2002). Beyond 100 million years, one is reduced largely to inferences from the geochemical nature of scattered rock deposits with even poorer age controls in a system evolving over some 3.5GY. Thousands of papers do document regional changes in proxy concentrations, but almost everything is subject to debate including, particularly, the age models, the geographical representativeness of the regional data, and the meaning of the apparent signals—often transformed in complicated ways enroute through the atmosphere and ocean to the sediments.

From one point of view, scientific communities without adequate data have a distinct advantage: one can construct interesting and exciting stories and rationalizations with little or no risk of observational refutation. Colorful, sometimes charismatic, characters come to dominate the field, constructing their interpretations of a few intriguing, but indefinite observations that appeal to their followers, and which eventually emerge as “textbook truths.”

Consider the following characteristics ascribed to one particular, notoriously data-poor, field (Smolin, 2006, P. 284), as having:

> 1. *Tremendous self confidence,* leading to a sense of entitlement and of belonging to an elite community of experts.
> 2. *An unusually monolithic community,* with a strong sense of consensus, whether driven by the evidence or not, and an unusual uniformity of views on open questions. These views seem related to the existence of a hierarchical structure in which the ideas of a few leaders dictate the viewpoint, strategy, and direction of the field.
> 3. In some cases a *sense of identification with the group,* akin to identification with a religious faith or political platform.
> 4. A strong sense of the *boundary between the group and other experts.*
> 5. A *disregard for and disinterest* in the ideas, opinions, and work of experts who are not part of the group, and a preference for talking only with other members of the community.
> 6. A tendency to *interpret evidence optimistically,* to believe exaggerated or incorrect statements of results and to disregard the possibility that the theory might be wrong. This is coupled with a tendency to *believe results are true because they are 'widely believed,'* even if one has not checked (or even seen) the proof oneself.
> 7. A lack of appreciation for the extent to which a research program ought to involve risk."

(Emphasis in the original.)

Smolin (2006) was writing about string theory in physics, and I have no basis for judging the validity of his description (Woit, 2006, expresses much the same view). Nonetheless, observers of the paleoclimate scene might recognize some common characteristics, even though paleoclimate may have better prospects for ultimately obtaining observational tests of its fundamental tenets. The group identification Smolin refers to, clearly exists in paleoclimate, exemplified by the hagiographic title of one recent paper: “Wally was right...”

Smolin’s (7) is perhaps the most important in his list. Good scientists seek constantly to test the basic tenets of their field—not work hard to buttress them. Routine science usually adds a trifling piece of support to everyone’s assumptions. Exciting, novel, important, science examines the basic underpinnings of those assumptions and either reports no conflict or, the contrary—that maybe it isn't true. Imagine Darwin working hard to fit all of his observational data into the framework of Genesis (today we laugh at the so-called intelligent design community for doing just that).

The Hope for a Simple World

As both human beings and scientists, we always hope for explanations of the world that are conceptually simple yet with important predictive skills (in the wide sense of that term). Thus the strong desire that box models should explain climate change, or that simple orbital kinematics can explain the glacial cycles, or that climate change is periodic, is understandable. But some natural phenomena are intrinsically complex and attempts to represent them in over-simplified fashion are disastrous. (Analogues might be the use of a 10-box model to describe and predict the world economy, or of a five-degree-of-freedom representation to teach pilots the dynamics of a flying helicopter, or depicting internet connections with a mere 100 links in studies of its stability. "Everything should be made as simple as possible, but not simpler." Usually attributed to A. Einstein.)

In the climate context, one underlying question is "Under what circumstances can a three-dimensional, time-dependent, turbulent, flow of the atmosphere and ocean be reproduced usefully by a one- or two-dimensional steady circulation?" If it can be done, and understood, the result would be a most remarkable achievement in fluid dynamics, one that has eluded some of the most important mathematicians and physicists of the last three centuries. Yet the assumption that such a representation has been achieved, and even more remarkably, can be used to predict what would happen if the external parameters were disturbed (e.g., a change in insolation), underlies the great majority of discussions of the paleoclimate (and future climate) system. Under what circumstances, might the assumption be basically correct?

Until recently (circa 1975), the ocean circulation was almost universally represented as a large-scale, almost unchanging, system, one that was best described as "laminar", and being more nearly geological than fluid-mechanical in nature. This picture was a necessary and inevitable consequence of the observational data available to oceanographers—almost solely temperatures and salinities as a function of position as compiled by hydrographers working on ships over many decades. They pieced together a data set leading to the now ubiquitous hydrographic sections. Fortuitously, it was found that the bulk thermohaline and related chemical properties of the ocean, occupying volumes spanning thousands of kilometers, were quasi-steady, and contourable. It was inferred from these pictures that thousands of years would be required to communicate properties from the surface to and from the abyssal ocean. That one's perception of a problem can be gravely distorted by the accident of which observations are available is plain.

The Stommel quotation at the beginning of this paper was a product of this era..

The study of what came to be called "geophysical fluid dynamics" is directed at understanding the processes underlying real flow fields by reducing the systems to the most basic-barebones elements—thus exposing the essential ingredients. Much progress has been made that way. The pitfall, which has not always been avoided, is in claiming that because an essential element has been understood, that it necessarily explains what is seen in nature. An attractive theory of the simplified system is then applied far outside any plausible range of validity. Thus the rather beautiful Stommel and Arons abyssal circulation theory (e.g., Stommel, 1958) is a good example. This theory is particularly beguiling because, (1) the mathematics are extremely simple (the linearized geostrophic balance equations plus mass conservation) and, (2) the result is counter-intuitive (implying e.g„ that abyssal flows must be *toward* their sources).

One sees published papers flatly asserting that the ocean abyssal circulation is what was described by Stommel-Arons. But there is essentially *no* evidence that the theory describes very much of the volume of the ocean (it does predict, qualitatively, the *existence* of deep western boundary currents—a triumph of GFD—but not always their average direction of flow); the inferred meridional flows are nowhere to be seen, however (See Fig. 1). The theory applies to a fluid flow that is in a steady-state, very weak and linear, fed by a small number of isolated convective regions, on a flat-bottomed-ocean, with a vertical return flow that is globally uniform, undisturbed by any other forces. Given the many assumptions, it is no surprise that one does not observe flows implied by the picture constructed by Stommel (1958; see for example, Fig. 1). The physical insight—that interior geostrophic balance and the implied vorticity balance dominate—is truly fundamental to any understanding of the ocean circulation, and it is difficult to over-emphasize the importance of this simple model. But when it is claimed to describe the dominant flow field of the real ocean, the wish for beauty and simplicity are trumping the reality of observations. Extension of a simplified description or explanation outside of its domain of applicability is of little or no concern to anyone outside the academic community—unless it begins to control observational strategies or be used to make predictions about future behavior under disturbed conditions.

One notes, for example, that there were essentially no measurements below 1000m of the hydrography of the Pacific Ocean until the middle 1960s, because "everyone knew" that the flows there were inconsequential. Meteorologists who assumed that the abyssal ocean was slow and steady, or accepted that the Sverdup et al. (1942) inference that the ocean could only carry about 10% of the meridional heat transport toward the poles (see e.g., Wunsch, 2005) , etc., took a very long time to move away from their "swamp models" of the ocean for studying climate—models that have still not disappeared.

2 Conveyor Belts

Broecker (1991, and many other papers), building on a sketch of Gordon (1986), reduced the discussion of the paleocean circulation to that of a one-dimensional ribbon that he called the "great global conveyor." Its rendering in color cartoon form in Natural History magazine has captured the imagination of a generation of scientists and non-technical writers alike. It is a vivid example of the power of a great graphic, having been used in at least two Hollywood films, and has found its way into essentially every existing textbook on climate, including those at a very elementary level. It is thus now a "fact" of oceanography and climate. (Broecker, 1991, himself originally referred to it as a "logo," and it would be well to retain that label.)

I have written elsewhere (Wunsch, 2002) about the long-list of ways in which the conveyor contradicts known ocean physics. Most insidious, however, is the implication, from its wide acceptance, that the ocean circulation is intrinsically so simple that one can predict its behavior from what a one-dimensional ribbon flow would do. Rather than repeat that earlier discussion, let me confine myself here to three recent examples of the way in which the complexity of the actual circulation is qualitatively at odds with the ribbon picture.

Fig. 2, taken from Bower et al. (2009) shows the trajectories of neutrally buoyant floats deployed in the western sub-polar gyre, and where the expectations from the conveyor, and those of the authors, was that the floats would largely move along the continental margin entering the subtropical gyre in the deep western boundary current. As is apparent, of the 40 floats deployed, only a single one (!) followed the conveyor pathway—the remainder moved into the interior of the subpolar gyre to undergo a subsequent set of complex pathways. How they ultimately (when?, if?) enter the ocean further south is far from apparent.

Similarly, Fig. 3 (from Brambilla and Talley, 2006) shows surface drifters deployed in the subtropical gyre over a period of 12 years. These drifters apparently do not "know" that they were meant to move into the subpolar gyre as part of the conveyor. (The simplest interpretation is probably that their trajectories are governed by the surface Ekman layer whose net transport is southward in this region—an important flow structure entirely missing from the conveyor.) Most paleoclimate discussions of the North Atlantic circulation fail to even acknowledge the existence of such conflicting data sets.

The conveyor postulates one region, the northern North Atlantic, where water sinks and fills the deep ocean, although even its partisans would likely agree that the Weddell and Ross Seas also contribute. But water that is at the surface *anywhere* in the ocean, ultimately moves elsewhere in the three-dimensional volume. Fig. 4 shows the estimate by Gebbie and Huybers (2010) of the fraction of the volume of the ocean that last was at the surface in each of all 4×4

degree boxes. Although some regions do make a higher than average contribution, none actually vanishes, and even the high latitude contributions are much more widespread than one might have inferred from the obsession with the Labrador or Greenland Seas, or the Weddell or Ross Seas in the south.

One might argue that the conveyor is a useful simplification employed mainly as a framework for discussing complex proxy data. The idea that the ocean transports mass, enthalpy, etc. around the world ocean is indeed incontrovertible, as is the inference that heat, in particular, is "conveyed" from the tropics to high latitudes. But when the cartoon (the logo) becomes a substitute for the reality, and is no longer the subject of questions and tests, it is time to raise the alarm. For example, one eminent, and sophisticated, meteorologist once assured me that global ocean observations were unnecessary—as keeping track of the entire system could be done very simply and cheaply with expendable bathythermograph data in the North Atlantic, high latitude, branch of the "conveyor". The large field programs now underway, intended to measure primarily the North Atlantic circulation, are a direct consequence of this notion, and the conviction that this ribbon flow is reality, has clearly led to the extreme emphasis on supposed control of global climate by the North Atlantic Ocean. This narrow approach to the science is perhaps personified by the notorious "hosing" experiments discussed in the next section.

3 The Hosing Scenario

Myriad hypotheses have been put forward as rationalizing some elements of the oceanic role in influencing climate—ranging over essentially all possible time scales out to the age of the ocean. One cannot begin to discuss all of these, and so I will here take as a not untypical example, the hypothesis that the North Atlantic circulation largely controls the climate system, and in particular, the notion that the surface salinity concentration is the determining influence.

Using the putative conveyor as a framework, Broecker (1990) and others have suggested that a meltwater pulse onto the North Atlantic would have had a major climate impact. The origin of this idea is not so clear. Berger and Killingley (1981), attribute it to Worthington (1968) and there clearly is a connection with Stommel's (1961) one-dimensional fluid model displaying two stable states. Initially, the focus was on explaining the Younger Dryas, and it was later extended to numerous other events in the paleoclimate record, and then to predictions of what future global warming will bring.

The suggestion is both a plausible and interesting one (see e.g., Bryan, 1987), and it was picked up by Manabe and Stouffer (1995) who showed in coupled climate GCM that they could produce a marked disturbance in the North Atlantic circulation by imposing a "massive surface flux" of fresh water.[1] As a geophysical fluid dynamics (GFD) hypothesis, it is a sensible avenue to explore. Despite the hundreds of papers discussing the idea, however, only a tiny minority has attempted to better understand the underlying physics, and just as important, to analyze the possible conflicting evidence. Indeed, in the 15 years since their paper appeared, this hosing story has become essentially another "fact," with most papers on the subject repeating variants of the initial story.

To set the scene, consider first some descriptive numbers. Table 1 lists approximate values characterizing freshwater input into the present-day world ocean, as best as we can determine them. By far the largest component is over-ocean precipitation, producing about 12Sv (1 Sverdrup=$10^6\text{m}^3/\text{s} \approx 10^9\text{kg/s}$) of fresh water. Next is river-runoff of about 1Sv and possibly (Moore, 2010) another 0.1 Sverdrup from subsurface percolation. Of the runoff, modern Greenland is supposed to account for about 0.01Sv (Box et al., 2004), with a possible *increment* of 0.01Sv from recent excess ice loss (e.g., Velicogna, 2009). The equivalent values for Antarctica are (very roughly) 0.1Sv background with perhaps 0.01 Sv of recent excess net melting. Almost all of this injection of freshwater is balanced by net evaporation—but in a different regional pattern and with a different atmospheric physics; the residual is a global sea level rise of order of magnitude of 1mm/y (an excess of about 0.01Sv more freshwater entering than leaving).

For an example, consider that Stanford et al. (2006) suggest that Meltwater Pulse 1a (MWP1a), occurring at approximately -14ky, reached a peak as large as 40mm/y (about 10 times the estimated recent sea level rise rate), superimposed on a background deglaciation rate of about 20mm/y. So the peak melting-ice value corresponds to about 0.2Sv on top of a larger background value of about 0.2Sv. How much of this represents northern rather than southern sources is the subject of some controversy. Evaluating the response of the ocean circulation to such an input disturbance raises a whole series of interesting questions that would need to be answered before one could claim understanding adequate to predict oceanic and climate behavior, be it past or future.

In that list one would necessarily ask whether, given the relatively enormous modern precipitation rates, did the precipitation pattern shift, and if so, was the change small compared to 0.4Sv? If the background melt rate shifted for thousands of years from the estimated modern value of 1-3mm/y (0.01-0.03Sv) to 20mm/y (0.2Sv), how was the resulting circulation different from today's—prior to MWP1a? How did the sea ice cover change with that excess of freshwater? How does that sea ice cover change influence the resulting circulation (attention is called to the paper of Våge et al., 2009, who showed, in the modern world, that an increase in near-coastal ice cover in the Labrador and Irminger Seas, led to an *increased* convective response in

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[1] This account is not intended to be a history of either the "hosing" hypothesis nor of the conveyor idea.

the ocean—because the atmosphere was much colder when it finally reached open water).

Any important climate shift implies a wind-field change. As discussed by Huybers and Wunsch (2010), the overall strength of the ocean circulation is set by the magnitudes and patterns of the curl of the wind-stress. How did these change with the changing sea ice cover? With the changes in height and albedo of the continental ice sheet? With the changes in sea surface and land temperatures? In the modern world, the high latitude North Atlantic meridional Ekman transport exceeds 1Sv in magnitude (e.g., Josey et al., 2002). Thus a mere 10% change in the magnitude of the wind stress (not its curl) would change the surface layer transport by 0.1Sv. It is difficult to understand how such a potentially rapid and efficient mechanism for changing the transports of surface waters (fresh water and ice) can be ignored. (And ice cover directly influences the transmission of stress from atmosphere to ocean.) At lower latitudes (e.g. the latitude of putative fresh water injection into the Gulf of Mexico through the Mississippi system) the Ekman transports are more than an order of magnitude larger—with consequent very large potential for moving and diverting surface waters.

Supposing that one does determine where (the Arctic, Greenland, the St. Lawrence Valley, the Mississippi, Antarctica,...) an excess of fresh water enters the ocean, a series of dynamical issues occur that will be peculiar to the particular region. Fresh water injection from the continents enters the ocean in some of the most complex of all oceanic regions—the continental margins, subject to strong tides, wind forcing, the local ambient circulation and in high latitudes, to seasonal ice formation. If winds are downwelling-favorable at the point of entry, one expects a very different distribution of salinity than if they are upwelling-favorable. Consider as perhaps the simplest example, fresh water input along a straight coastline (Fig. 5). As discussed in Wunsch (2010, unpublished ms.) this problem is an example of the "Rossby adjustment problem." The main result, known to all dynamicists, is that rotation tends to trap the fresh water near the coastline, over a distance dependent upon the rotation rate, the water depth, and the contrasting densities, but normally much less than 10km distance at high latitudes (the baroclinic Rossby radius of deformation). Although global sea level (or bottom pressure) initially adjusts extremely rapidly, it can take many decades and longer for the freshwater to escape from the coastal area, depending upon the winds, the larger-scale general circulation, the water depth along and normal to the shore, the intensity of the oceanic eddy field, and the behavior of coastal ice, if any. A rich literature exists on the influence of freshwater on the coastal circulation (e.g., Garvine and Whitney, 2006), yet almost none of the many papers on the paleoceanographic influence of fresh water sees fit to notice the possibility that it may be very difficult to overlay most of the subpolar gyre with freshwater. Many authors seem intent primarily on bolstering the assumption that freshwater will simply overrun it, giving rise to weakening or "shut-down" of the meridional overturning circulation.

Freshwater certainly does enter the ocean and convective mixing is a delicate process balanced between having the water freeze, and having it become dense enough to sink. But even if it does sink, it is far from obvious what the influence is on the larger-scale circulation. Using a model, Nilsson, et al. (2003) show that the addition of fresh water to the ocean can *increase* the meridional overturning . In another modeling result, de Boer et al. (2010) question whether the meridional density gradient is a determinant of the circulation rate, and there are other, similar suggestions that the situation is hardly as simple as one might infer from the bulk of the literature.

To my knowledge, only the very recent paper of Eisenman et al. (2009) notices that variations in precipitation (*mutatis mutanidis,* evaporation) might be considered as potential major influences on the circulation. Furthermore precipitation, unlike runoff, is injected in the open ocean more or less as the hosing story has it.

The hosing experiments often lead to shifts in the climate of the North Atlantic region, most commonly, apparently, because the meridional oceanic heat transport is diminished. What is also surprising is that one rarely if ever sees the question raised as to how the global heat budget is then maintained? Does the atmosphere respond by *increasing* its transport—getting warmer and/or wetter–as in Bjerknes (1964) compensation? See for example, Shaffrey and Sutton (2006). Does the Pacific meridional enthalpy transport increase? Perhaps the tropical albedo increases? Or more heat is transported poleward in the southern hemisphere? Questions such as these would lead to greater insights than merely rationalizing yet another data set in terms of "shutdown."

It is of course, possible that ice melt *does* control the major features of the North Atlantic circulation, and none of the complications listed above (surely there are others) has any significant impact on that inference. But strikingly little attention has been paid to examining the basic physical elements of "what everyone knows." (The original hosing story, of control of the Younger Dryas by the abrupt drainage of glacial Lake Agassiz into the St. Lawrence valley, seems finally on the way to abandonment because of the absence of any supporting geomorphological structure (e.g., Murton et al., 2010). It might have been regarded as suspect much earlier—had the physics of the circulation been examined at the outset. Drainage through the now-favored Arctic Sea route will affect the wider ocean circulation very differently from the supposed St. Lawrence pathway.)

4 The Model Problem

Hosing experiments and many other climate discussions rely on complicated ocean general circulation models (GCMs) and their even more complex use as sub-components in coupled models involving, in addition, the atmosphere, cryosphere, and biosphere. Such models now dominate discussions of the behavior of the climate system. As with future climate, where no data exist at all, the models promise descriptions of climate change—past and future—without the painful necessity of obtaining supporting observations. The apparent weight given to model behavior in discussions of paleoclimate arises also sometimes simply because they are "sophisticated" and difficult to understand, as well as appearing to substitute for missing data. Huybers and Wunsch (2010) have discussed the issue of model credibility at some length. Here I note only that fully-coupled climate models are among the most complicated pieces of machinery ever assembled, with upwards of a million lines of code (the computer equivalent of "moving parts.") A machine that was fully realistic would be as complicated as the real system, and so the great power of models is their ability to simplify—so that one can come to understanding. But understanding a machine with "only" hundreds of thousands of interlinked elements is not so easy either.

That models are *incomplete* representations of reality is their great power. But they should never be mistaken for the real world. At every time-step, a model integration generates erroneous results, with those errors arising from a whole suite of approximations and omissions from uncertain or erroneous: initial conditions, boundary values, lack of resolution, missing physics, numerical representation of continuous differential operators, and ordinary coding errors. It is extremely rare to read any discussion at all of the error growth in models (which is inevitable). Most errors *are* bounded in some way: the ocean is not permitted to boil or freeze over—limiting any temperature errors, and lateral displacement errors cannot exceed half-the Earth's circumference; diffusion ultimately removes the effects of small initial condition errors—albeit the time required to do so may be many thousands of years. A stopped clock never has an error exceeding six hours (on a twelve-hour system), but few would argue that it is a particularly useful model of the passage of time. An oceanic model run for five years might, with impunity, ignore errors tending to underestimate the amplitude of the annual sea ice cover change. But in a model run for 100+ years, those errors could dominate important aspects of the model-climate. Thus if one simulates with e.g., a coarse horizontal resolution, 20-layer vertical resolution, model for extended periods of time, one is implying (usually without mention), that the turbulence closure problems of the ocean circulation have been solved such that residual errors incurred are negligible after 100, 1000, or 1 million years. If that is correct, it is a truly remarkable breakthrough in fluid dynamics—one that should be celebrated everywhere as one of the major fluid dynamics accomplishments of the last 100 years. Has such a breakthrough been achieved?

Some published model results indulge in a kind of psychological trick: the physics (and chemistry and biology) are highly over-simplified, but the geometry of the continents, oceans and ice sheets in maintained in detail, lending the results a spurious air of verisimilitude. Shouldn't the geometric effects, which can be exceedingly complicated (the real Labrador Sea, the real Philippine Sea, etc.), be simplified so as to permit understanding of what the governing elements really are? Would one willingly fly on an untested airplane designed using an aeronautical code of "intermediate complexity"—even if it sat, impressively, on the runway?

Models used for hosing experiments are particularly vulnerable to resolution errors. As was noted, the dominant spatial scale of freshwater input, under the influence of Earth rotation, is the Rossby radius of deformation, which is typically less than 7 km at high latitudes. Movement of the fresh water, once it has escaped the unresolved coastal regions, will largely be determined by the detailed physics of the near-surface boundary layers (Ekman and general mixed layers), and their interaction with the wind field, sea ice, and oceanic turbulence on all scales. Manabe and Stouffer (1995) used an oceanic model with resolution of 4.5° of longitude by 3.75° of latitude and 12 levels. If a model transports 0.1PW too much or too little heat meridionally, then after 100 years of integration, one has misplaced 3×10^{23}J of energy—enough to melt or form 10^{18}kg of ice, with all that implies. There is also a widespread notion that if errors are random that they "will average out." But the phenomenon of a random walk shows that the inference can be quite wrong. Hecht and Smith (2008) discuss some of the myriad ways in which model results depend upon their (still) inadequate resolution. They question, in particular, whether the sensitivity of adequately resolved models will be at all like that of the low resolution models—which raises doubts about the manifold claims that GCMs display the same multiple states as do Stommel's (1961) one-dimensional model and its kin.

If a model fails to replicate the climate system over a few decades, the assumption that it is therefore skillful over thousands or millions of years is a non sequitur. Models have thousands of tunable parameters and the ability to make them behave "reasonably" over long time intervals is not in doubt. That error estimates are not easy to make does not mean they are not necessary for model interpretation and use.

5 Abuse of Statistics

Much more could be said about many other issues. An important one, that I will only take enough space here to mention, is a widespread misuse of elementary staticstical tests. A simple listing would include: (1) Use of a priori correlation statistics on time series manipulated (wiggle-matched) to produce high correlations. (2) Inference using confidence limits (e.g., 80%) guaranteed to produce numerous false postives, which are then "explained." (3) Confusion of correlation with causality ("Antarctic temperatures lag northern hemisphere ones, ergo northern hemisphere insolation *caused* southern hemisphere climate changes"). (4) Use of implausible null hypotheses to demonstrate the existence of spectral peaks: e.g., assuming that climate is an AR(1) process—a two-parameter system. Estimated spectra are then claimed to have the wished-for "peaks", when the proper inference is the expected one that an AR(1) is an inadequate representation of an extremely complex system. Etc.

6 Concluding Remarks

The study of paleoclimate encompasses such a huge range of problems, methods, regions, phenomena, time and space scales, that no one has mastered it all. Sweeping generalizations, such as those I have made here, must be understood to perhaps apply to the very small portion of this vast enterprise that seems directly related to modern understanding of the oceans. Nonetheless, all sciences run the risk of becoming so abstract, or so devoted to particular stories, or both, that they lose relevance to the physical world. As Chamberlin (1890) pointed out, it is essential to always be alert to alternative hypotheses.

Some of the exaggeration of the degree of understanding, and of over-simplification is best understood as a combination of human psychology and the pressures of fund-raising. Anyone who has struggled for several years to make sense of a complicated data set, only to conclude that "the data proved inadequate for this purpose" is in a quandary. Publishing such an inference would be very difficult, and few would notice if it were published. As the outcome of a funded grant, it is at best disappointing and at worst a calamity for a renewal or promotion. A parallel problem would emerge from a model calculation that produced no "exciting" new behavior. Thus the temptation to over-interpret the data set is a very powerful one. Similarly, if the inference is that the data are best rationalized as an interaction of many factors of comparable amplitude described through the temporal and spatial evolution of a complicated fluid model, the story does not lend itself to a one-sentence, intriguing explanation ("carbon dioxide was trapped in the abyssal ocean for thousands of years;" "millennial variability is controlled by solar variations"; "climate change is a bipolar seesaw"), and the near-impossibility of publishing in the near-tabloid science media (*Science, Nature)* with their consequent press conferences and celebrity. Amplifying this tendency is the relentlessly increasing use by ignorant or lazy administrators and promotion committees of supposed "objective" measures of scientific quality such as publication rates, citation frequencies, and impact factors.[2] The pressures for "exciting" results, over-simplified stories, and notoriety, are evident throughout the climate and paleoclimate literature.

The price being paid is not a small one. Often important technical details are omitted, and alternative hypotheses arbitrarily suppressed in the interests of telling a simple story. Some of these papers would not pass peer-review in the more conventional professional journals, but lend themselves to headlines and simplistic stories written by non-scientist media people. One has the bizarre spectacle of technical discussions being carried on in the news columns of the New York Times and similar publications, not to speak of the dispiriting blog universe. In the long-term, this tabloid-like publication cannot be good for the science—which developed peer review in specialized journals over many decades beginning in the 17th Century—for very good reasons.

Paleoclimate reconstruction and understanding presents some of the most intriguing data and problems in all of science. Progress clearly requires combining the remarkable achievements in producing proxy data with similar achievements in understanding dynamics, and in this context, oceanic physics. This combination does represent a rare, truly interdisciplinary, field in which individuals must have at least a working grasp of the powers and pitfalls of the data, and of the models and dynamical theories. Paleoclimate studies emerged out of geology and geochemistry; these are fields which historically did not attempt large-scale quantitative syntheses using time-evolving partial differential equations. In contrast, general circulation modeling emerged out of geophysical fluid dynamics and computer science—during a period when oceanographic data were few and far between; comparisons of the sparse, poorly understood data, with clearly unrealistic numerical models led to a modeling community disconnected from understanding of the observational system. Paleoclimate study needs an open-minded, restrained, scientific community, one informed about both of these sub-fields—it is plainly primarily an issue of education.

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[2] Note, for example, that Stommel's now famous 1961 paper was apparently cited only once in the first 21 years after its publication —and that by Stommel himself. Many important scientific contributions took years to be understood and appreciated. Scientists have also learned how to "game" the citation system.

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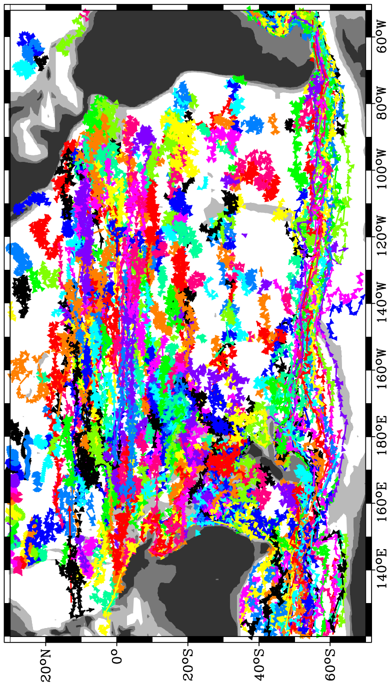


Figure 1: From Davis (2005) showing trajectories of neutrally buoyant floats deployed in the Pacific Ocean (mainly) at a nominal depth of 900m. The result shows little evidence of the large-scale meridional flows of the Stommel-Arons theory, nor does it suggest much in the way of a "conveyor belt" circulation. (Courtesy of R. Davis, 2010)

`{davis_all_pac`

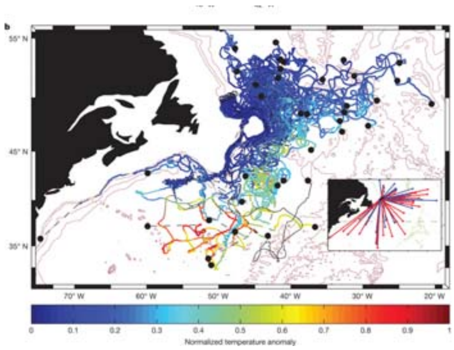


Figure 2: From Bower et al. (2009) showing two-year trajectories of floats released in the so-called Labrador Sea Water at 700 and 1500m depths. None of them enter the Deep Western Boundary Current in the sub-tropical gyre. There may well be issues here with exactly what floats do and do not measure that would permit one to reconcile this picture with the simplest conveyor belt-like stories. But how much more interesting and useful it is to ask whether these data are not telling a completely different story!

`{bower_floats_`

Figure 3: From Brambilla and Talley (2006) showing trajectories of surface drifters launched south of 45°N. With one exception, *none* of them enters the subpolar gyre. The nominal depth measured is 15m. Drifters were launched between 1990 and 2002

`{brambilla&tal`

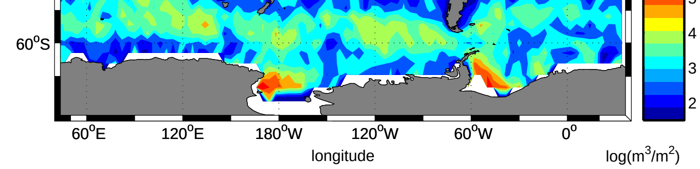


Figure 4: Ocean volume whose last contact with the surface occurred in each 4° × 4° square in $\mathrm{m}^3$of volume/$\mathrm{m}^2$of surface area. A logarithmic scale is used (Gebbie and Huybers, 2010, who show a higher resolution version of this plot). Courtesy of G. Gebbie.

`{gebbie_sfcori`

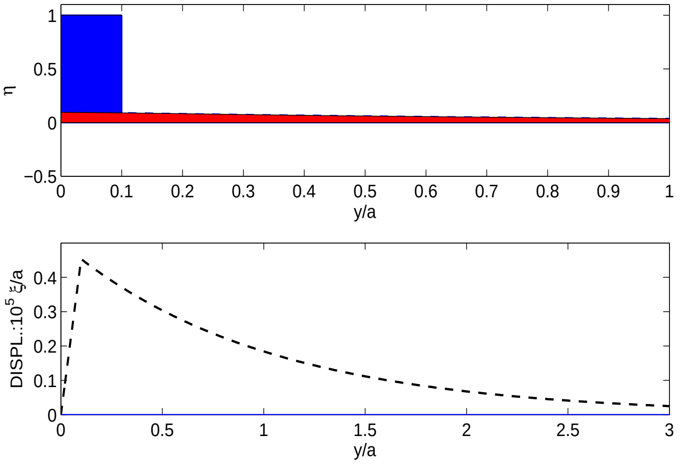
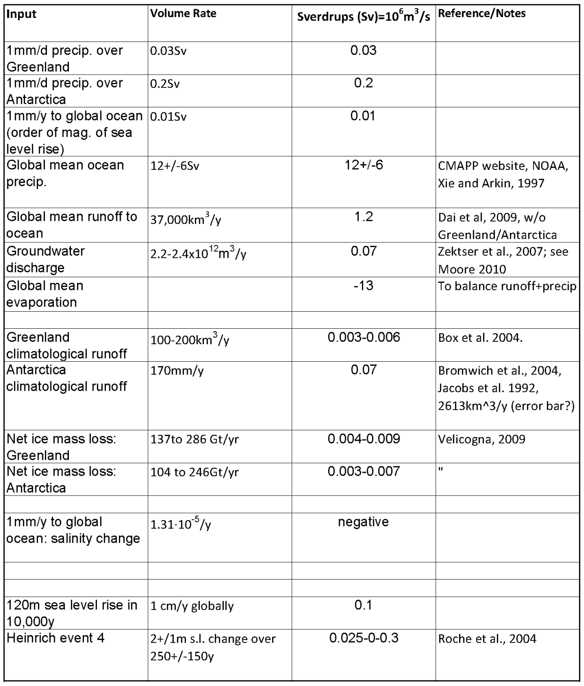


Figure 5: Upper panel. Initial surface elevation or bottom pressure anomaly (blue) for the special case $y_1 = a/10$, and after geostrophic adjustment. a is the barotropic deformation radius. Lower Panel. Non-dimensional (as a fraction of a) lateral isplacement of the fluid after adjustment, but which is a very small fraction of the distance disturbed, so that the fresh water distribution is little changed from its initial position, although it is assumed achieved local isostatic equilibrium. Note the differing horizontal scales. (Wunsch, 2010, unpublished ms.) `{displacement_`



Input	Volume Rate	Sverdrups (Sv)=$10^6 m^3/s$	Reference/Notes
1mm/d precip. over Greenland	0.03Sv	0.03	
1mm/d precip. over Antarctica	0.2Sv	0.2	
1mm/y to global ocean (order of mag. of sea level rise)	0.01Sv	0.01	
Global mean ocean precip.	12+/-6Sv	12+/-6	CMAPP website, NOAA, Xie and Arkin, 1997
Global mean runoff to ocean	37,000km^3/y	1.2	Dai et al, 2009, w/o Greenland/Antarctica
Groundwater discharge	2.2-2.4x$10^{12}m^3$/y	0.07	Zektser et al., 2007; see Moore 2010
Global mean evaporation		-13	To balance runoff+precip
Greenland climatological runoff	100-200km^3/y	0.003-0.006	Box et al. 2004.
Antarctica climatological runoff	170mm/y	0.07	Bromwich et al., 2004, Jacobs et al. 1992, 2613km^3/y (error bar?)
Net ice mass loss: Greenland	137to 286 Gt/yr	0.004-0.009	Velicogna, 2009
Net ice mass loss: Antarctica	104 to 246Gt/yr	0.003-0.007	"
1mm/y to global ocean: salinity change	$1.31 \cdot 10^{-5}$/y	negative	
120m sea level rise in 10,000y	1 cm/y globally	0.1	
Heinrich event 4	2+/1m s.l. change over 250+/-150y	0.025-0-0.3	Roche et al., 2004

Figure 6: Numerical values helpful for evaluating the context of ice melt rates. `{water_input_f`