Decadal Climate Variability and the Thermohaline Circulation

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1. Introduction

I start from the premise that research on decadal (including interdecadal) climate variability is critically important for at least two distinct if related reasons, each of which stems directly from the two major objectives of the WCRP.

a. ***to determine the predictability of climate*** – as stated in the COPES strategic framework, investigating the predictability of climate up to a decade ahead is one of the major immediate challenges faced by climate research. So far, climate predictions – in contrast to projections – have almost exclusively been performed for the seasonal-to-interannual timeframe, but the decadal scale is now beginning to be tackled.

b. ***to determine the effect of human activities on climate*** – for attributing observed change to human activity, for testing the realism of models of anthropogenic climate change, or for scientific advice on international negotiations and compliance with treaties, the natural variability needs to be filtered out of the observed climate records. This is particularly important in the decadal band, where the two signals overlap.

I will frame my discussion predominantly in the context of the climate predictability objective, **a.**, to focus more clearly on decadal variability *per se*, rather than viewing it as noise to be eliminated. Decadal predictability would largely be expected to arise from the slow components of the climate system, most notably the ocean and land surfaces. Restricting the discussion to the ocean, ocean predictability would be a necessary, though not sufficient, condition for climate predictability. Ocean predictability in turn is aided if ocean variability arises predominantly from inherently oceanic or coupled ocean-atmosphere processes, rather than from integrating atmospherically-induced noise (Hasselmann 1976). The latter would create SST predictability solely through thermal inertia of the ocean mixed layer. In contrast, a significant influence of ocean dynamical processes at least gives the possibility of longer timescales being involved (unless the ocean processes are predominantly chaotic on too short timescales, a possibility we need to consider eventually but will ignore here for sake of argument). This logic brings us to the thermohaline circulation (THC) as a potential origin of decadal predictability, as it is more likely to be governed or at least influenced by internal ocean processes on relatively long timescales. In contrast, the wind-driven circulation is more likely to reflect immediately the predictability of the wind, with limited possibility of adding predictability. This heuristic reasoning leads us to the conclusion that decadal variability of the THC must play a central role in investigating decadal variability *and predictability* of climate.

The purpose of this note is to identify research needs, from a personal perspective, thus serving as the starting point of a discussion. While I do not even attempt to give a comprehensive overview of the literature, I will attempt to refer to the original papers of a particular point where known to me. In what follows, I draw significantly on the excellent “student review paper” by (Pohlmann and Keenlyside 2004), presented at the 2004 CRCES-IPRC Workshop on Decadal Climate Variability, which also contains many additional references.

2. Mechanisms of THC variability

I start with a discussion of mechanisms of THC variability, as this sub-field is far more mature than the observational side. Right away, this order points to a huge difficulty in understanding decadal variability of the THC: We cannot follow the classical route favoured by the scientific process, of starting from some observed phenomenon to using theory and models to understand what we measured. This conundrum implies that we cannot “simply” refer to observations to decide which of the competing mechanisms dominates.

Fifteen years after decadal-interdecadal variability of the THC was first discovered in models, both uncoupled (Marotzke 1990; Weaver and Sarachik 1991) and coupled (Delworth et al. 1993), there is still no agreement as to whether it constitutes a true coupled ocean-atmosphere mode of variability (Timmermann et al. 1998) or a response to quasi-random low-frequency atmospheric variability. The latter possibility comes in two flavours, the “pure” random walk, integrating atmospheric perturbations (Hasselmann 1976; Frankignoul et al. 1997), or including an enhancement through a self-sustained or damped oceanic internal mode (Griffies and Tziperman 1995). Recent coupled model simulations indicate a dominant role of heat flux forcing in inducing decadal THC variability, and the possibility that even within the same model (here, ECHAM5/MPI-OM) the mechanisms differ depending on whether one considers interdecadal (ca. 20 yrs) or multidecadal (ca. 70 yrs) timescale (X. Zhu and J. Jungclaus, 2006, pers. comm.). In summary, there is significant research yet to be done on why the model results differ, including the potential for stating criteria for when one of the two major candidate mechanisms dominates. This dichotomy – and resulting research need – is reminiscent of the debate during the 1990s about the dominating mechanism governing El Niño (e.g., Neelin et al. 1998). Notice that this quest does not even address the bigger question – what is the mechanism in reality? – which can only be answered with a sufficient observational base.

After long periods of doubt, there is now some evidence that variations in the North Atlantic sea surface temperature (SST), themselves possibly caused by THC variations, leave a non-dominant yet significant imprint on the atmosphere. Evidence comes in large part from models (e.g., Pohlmann and Keenlyside 2004; Pohlmann et al. 2004; Sutton and Hodson 2005) but also from observations (Czaja and Frankignoul 2002).

3. Observations of THC variability

There has been enormous confusion – both within the oceanographic community and without – concerning whether change in the THC has been observed or not. To avoid that confusion, it is critical to define precisely what is being discussed. First, it is preferable not to use the term THC, but instead the Meridional Overturning Circulation (MOC). The MOC is a purely kinematic quantity and reflects the total northward or southward flow, integrated over longitude and depth. The MOC is thus the ocean analogue to the Mean Meridional Circulation in the atmosphere (assuming that “mean” does not refer to time-mean). The THC can be defined as the part of the MOC driven by heat and water exchange with the atmosphere, using the term “driven” loosely and leaving aside questions of the mechanical energy balance, (e.g., Weyl 1968; Munk and Wunsch 1998). Thus, the MOC has both thermohaline and wind-driven components, the latter most prominently in the shallow subtropical and tropical cells (McCreary and Lu 1994). Usage of the term MOC is preferable since the MOC, in principle, is an observable quantity, whereas THC implies an interpretation, which may or may not be correct. Often the two are used synonymously, but one should use THC only when one is confident of the interpretation and MOC when rigour is required.

Arguably the larger part of the confusion has arisen because change has been observed in quantities thought to influence the MOC, but not in the MOC itself. Among these quantities are a freshening of northern Atlantic surface and deep waters (Curry et al. 2003; Dickson et al. 2002) during the latter decades of the 20th century, variations in the overflows in the northern North Atlantic (Hansen et al. 2001), and an observed weakening in the subpolar gyre during the 1990s (Häkkinen and Rhines 2004). However, none of these observations, important though they have been, allows us to infer directly a change in the MOC, and none of these quantities has been shown to be a valid proxy for MOC change. On the contrary, results from state-of-the-art coupled climate models have indicated that freshening may instead coincide with stronger MOC (Wu et al. 2004) and that there is no significant correlation between subpolar gyre strength and the MOC, at least on timescales shorter than 20 years (Landerer et al. 2006).

Therefore, until a few months ago it was correct to state that no robust analysis of direct measurements of the MOC, which has had to rely on full-depth, coast-to-coast hydrographic sections, had shown a change in the MOC (e.g., Macdonald and Wunsch 1996). But the analysis of five "snapshot" measurements over the past 50 years, taken at 25°N on Oct 1957, Jul/Aug 1981, Jul/Aug 1992, Feb 1998 and April 2004, showed for the first time a different MOC estimate, for 1998 and 2004, compared against the earlier observations (Bryden et al. 2005). Ostensibly, the measurements suggested that the MOC had slowed by 30%. The southward transport of lower North Atlantic Deepwater was reported to have halved and the southward recirculation of upper waters in the subtropical gyre doubled.

The publication of (Bryden et al. 2005) was followed by an intense debate. Two main criticisms were levied against the conclusion, apparently supported by the paper though not by the press conference accompanying publication, that a long-term change in MOC had been observed. First, a 30% slowdown should have left an imprint on SST, through a reduction of maybe 1°C (R. Wood, as quoted by Kerr 2005), whereas coupled models indicate that the actually observed higher SST around the turn of the millennium should coincide with stronger MOC, at least on an interdecadal timescale (Latif et al. 2006). Second, model analyses suggest that the subannual variability of the MOC is significantly larger than previously thought, especially in its baroclinic component (e.g., Hirschi et al. 2003, Fig. 1 here). Thus, the five snapshots used by (Bryden et al. 2005) might have subsampled intense high-frequency variability. The conflicting evidence for and against a recent weakening of the Atlantic MOC underscores the crucial importance of observing it continuously, such as started two years ago by the UK-led array at 26.5°N (Marotzke et al. 2002). This has been the consensus in the recent debate, as summarised in a *Nature* (2006) editorial.

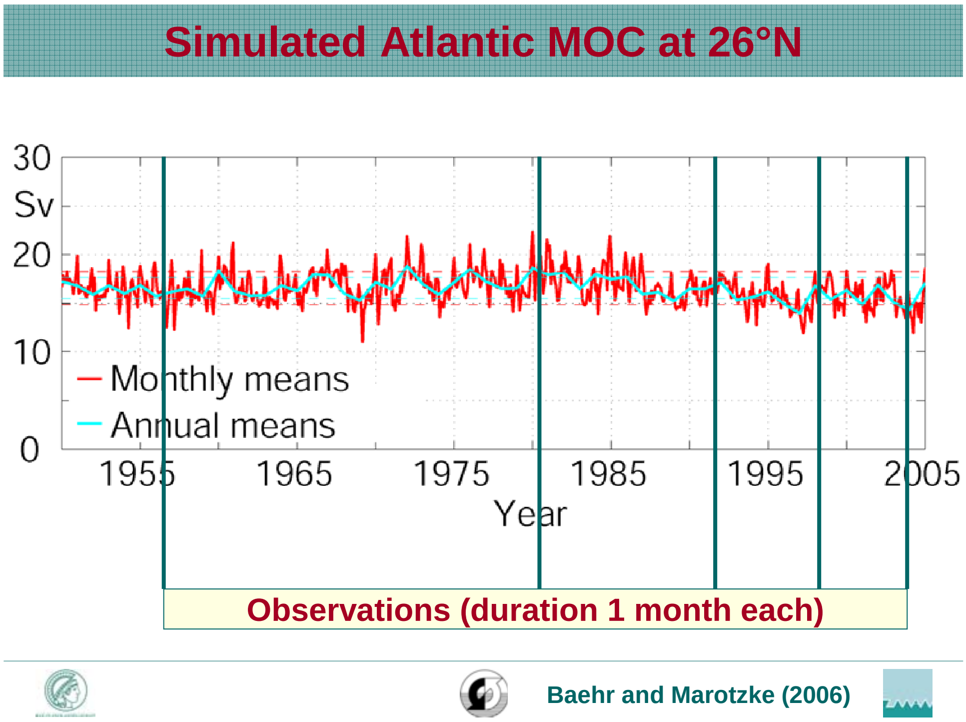

Fig. 1: Atlantic MOC strength at 26°N, as simulated in the IPCC 20th-century runs with ECHAM5/MPI-OM, with the monthly means of the directly wind-forced MOC contributions subtracted. This treatment gives the month-to-month model counterpart to the standard analysis of hydrographic sections. The level of subannual variability is so high that five arbitrarily placed "snapshots" (green vertical lines marking the times of the five hydrographic occupations) easily sample variability of order 5-8 Sv. Preliminary analysis of the MOC monitoring array at 26.5°N indicates MOC variability of similar magnitude.

4. Research needs and the role of CLIVAR

One can collect the research needs under four broad headings:

a. MOC time series

The number one priority to me is the continuation of the single existing MOC time series at 26.5°N in the Atlantic, augmented by time series at other locations. The debate around the (Bryden et al. 2005) result, but also the original report (McPhaden and Zhang 2002) and later rescindment (McPhaden and Zhang 2004) of a slowdown of the shallow Pacific overturning cell indicate that there is no substitute for as direct as possible, continuous observation of the MOC. The UK, through NERC, seems prepared to fund the RAPID MOC array until 2014; a proposal has just been submitted. The exact long-term observational strategy needs to be worked out, both in terms of technique at 26.5°N and in terms of additional locations. Using full-depth gliders looks like a powerful substitute for the current approach of a moored array and might be feasible within a decade. Whether the approach at 26.5°N can simply be transferred to other latitudes is unclear (Baehr et al. 2004); a serious discussion about design and strategy is required.

CLIVAR role: CLIVAR can and should co-ordinate a discussion about

- observing locations complementary to 26.5°N
- alternative observing systems
- development of cheaper technologies
- transfer to operational agencies

b. Development of MOC proxies

The technical challenge and expense of directly measuring the MOC motivates the discussion of efficient proxies for the MOC. Moreover, having proxies available would enable us to extend the time series backward in time, prior to 2004. While simple proxies (meridional SST differences) have been suggested, based on coupled model simulations (Latif et al. 2004), validation against actual MOC observations is required. Results from data assimilation activities (e.g., Stammer et al. 2003; Carton et al. 2005) can be regarded as the ultimate “multi-proxy” approach – if a consistent solution is found, the MOC can be diagnosed from the model solution although it might not have been observed directly. But, again, independent confirmation of the MOC derived from data assimilation is required; experience from the first-generation general circulation inverse models suggests this confirmation is nontrivial (Marotzke and Willebrand 1996).

CLIVAR role: Co-ordinating systematic development of MOC proxies

c. Decadal predictability

Studies of decadal predictability, whether of the MOC, SST, or surface air temperature (SAT), have so far almost exclusively been limited to pure modelling studies (e.g., Griffies and Bryan 1997; Pohlmann and Keenlyside 2004), without the inclusion of observations to define the initial conditions of climate simulations. Notable exceptions are the efforts to initialise coupled models with observed anomalies (Pierce et al. 2004); this approach has also been used by the Hadley Centre (I have, however, not been able to find any detailed information beyond a brief summary in Pohlmann and Keenlyside 2004). But using observations systematically in the initialisation of decadal-timescale climate prediction systems is at the heart of the “seamless prediction” strategy of the WCRP strategic framework COPES. Measurements of the MOC itself, possible proxies, and quantities possibly influences by MOC changes should play a crucial role.

CLIVAR role: CLIVAR can and should take the lead within WCRP by co-ordinating a discussion about MOC predictability and end-to-end prototype prediction systems.

d. Mechanisms of decadal-interdecadal MOC or THC variability

This is a thriving enterprise, and it is less clear that action by CLIVAR is needed, apart from endorsement of and through its various modelling panels. The possible exception might be an initiative to expose, systematically, the proponent models of competing mechanisms to long-term observations. Of course, such an initiative might be ongoing already, unbeknownst to me.

CLIVAR role: Encourage a greater role of observations in the analysis and comparison of decadal variability model results.

5. References

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