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Climate of the Past Discussions

Interactive comment on "Atmospheric multidecadal variations in the North Atlantic realm: proxy data, observations, and atmospheric circulation model studies" *by* K. Grosfeld et al.

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Review of ''Atmospheric multidecadal variations in the North Atlantic realm: proxy data, observations, and atmospheric circulation model studies" by K. Grosfeld, G. Lohmann, N. Rimbu, K. Fraedrich and F. Lunkeit.

Major Comments:

This paper is concerned with what is often called the Atlantic Multidecadal Oscillation (AMO), as seen in observations and models, including proxy data. A number of issues came to mind when reading the paper, as outlined below.

1. A recent paper, Mann and Emanuel(2006, EOS) has questioned the existence of the AMO, at least the influence of the AMO on tropical Atlantic SST, including the area of the sediment core discussed by the authors. In fact, Mann and Emanuel claim that the interdecadal SST fluctuations in the tropical Atlantic are radiatively forced. I am sure that the paper under review has bearing on Mann and Emanuel's work, and visa versa, an issue I think the authors should address. A complication that I see myself is that it is quite possible that the variability associated the AMO and the radiatively forced signal have actually been varying in phase during the 20th century. Whether or not this is chance is another, related issue.

2. Another paper, which takes the more usual view that the AMO is real, and associated with changes in the North Atlantic Meridional Overturning Circulation (MOC), is by Latif, B{\"o}ning, Willebrand, Biastoch, Dengg, Keenleyside, Madec and Schweckendiek with title ''Is the thermohaline circulation changing". As I understand the situation, the Latif et al. paper is in press in Journal of Climate, and I hope the authors can get hold of it, as it is certainly very relevant to the authors' paper. Latif et al take the view that interdecadal changes in the MOC are driven by the NAO through NAO-related modulation of the deep water formation in the Labrador Sea. The mechanism had earlier been demonstrated in a numerical model by Eden and Jung (2001, Journal of Climate), another paper that is very relevant to the authors' paper and should be referenced. The Eden and Jung paper provides convincing evidence that it is the NAO that drives multidecadal fluctuations in the MOC and the associated SST variability. I am a bit surprised that the authors claim that it is not the NAO itself that is associated with the multidecadal SST variability they discuss, but an NAO-like pattern that has its centres of action shifted from those of the NAO. I assume this is because the authors focus on the SLP pattern that varies in phase with the AMO, whereas the AMO actually lags the NAO by about 10 years. (In fact, there is no reason why the SLP that varies in phase with the AMO - if, indeed, there is such a pattern - should look like the NAO.) Anyway, we know from the spectrum of the winter NAO index that there is significant energy at the interdecadal time scales (see, for example, Wunsch(1999,

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Bull.Amer.Met.Soc.)) and the Eden and Jung paper shows that there is more than enough energy in the observed NAO index to drive significant interdecadal fluctuations in the North Atlantic MOC (see also Delworth and Greatbatch, 2000, Journal of Climate, who show that a white noise heat flux forcing derived from a coupled model can driven significant MOC variability at interdecadal times in the same coupled model). So, I do not agree with the statement on page 646, line 22, that ‘‘While the NAO is defined on an interannual time scale and is manifested in the NAO-index, AMO acts on longer time scales and modulates the Atlantic climate system...". Rather, I would argue that it is the NAO that drives the AMO. By the way, the SLP pressure pattern shown in Figure 4e surely looks like the winter NAO? - and presumably reflects the upward trend in the NAO index between 1970 and the mid-1990’s?

3. It is good to see that a simplified atmospheric model like PUMA can do such a nice job. But there is also no documentation referenced on the moist version of PUMA. Are there some other references that can be given? Is there is any documentation on the unperturbed, moist PUMA model?

4. Are 3 ensemble members enough to give statistically robust results from the atmospheric models? Presumably PUMA can be run in a much bigger ensemble?

Specific Comments:

1. Page 635, 24 lines from the bottom: I did not think that variations in the strength of the Aleutian low always went along with the variations of the AMO (in the same way that variations of the Aleutian and Icelandic lows do not always go together).

2. Page 635, further down the page: I think it is rather misleading to associate the onset of the latest cold phase of the AMO with the Great Salinity Anomaly (GSA). The GSA was most likely caused by the anomalous low winter NAO index at the end of the 1960’s and an associated enhanced flux of sea ice through Fram Strait (e.g. H{\"a}kkinen(1993, J.Geophys.Res.). Curiously, after that time, enhanced ice export through Fram Strait become strongly associated with the positive NAO index, as

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pointed out by Hilmer and Jung (2000, GRL), the opposite of the situation that led to the GSA.

3. Most of the data analysis is carried out using data that have been band pass filtered to pass only periods between 50 and 100 years. What happens if, for example, the SST data used to produce Figure 2 is not filtered in this way?

4. The dipolar pattern in Figure 3a looks like the NAO to me - also the principal component time series! How does the latter correlate with the winter NAO index? Also, please clarify what season the analysis applies to.

5. Bottom of page 640: How is the SST index referred to defined? Please clarify.

Conclusions: I think this is an interesting paper that has the potential to throw light on the recent Mann and Emanuel (2006, EOS) study, particular through use of the proxy data that enables the AMO to be studied several hundred years into the past, prior to the onset of the industrial anthropogenic forcing of the climate system. However, I think the analysis is a little confusing as to cause and effect, a confusion that probably arises because the AMO exhibits a decadal lag with the winter NAO index, a decadal lag that is missing from the analysis presented here because the analysis is concerned with the simultaneous atmospheric response to the AMO-related SST anomalies. That the NAO drives the AMO seems to be clear and deserves some attention from the authors.

Recommendation: This paper should make a nice contribution once the authors have addressed the above issues.

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Interactive comment on Clim. Past Discuss., 2, 633, 2006.