

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

### **Anonymous Referee #2**

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The overall aim of this study is to identify the AMO signature in different data archives and to connect this to physical interpretations based on model studies. Although I found the paper potentially interesting, the authors do not achieve these goals. I was confused by its organisation and ad-hoc presentation of results. The authors seemingly make a random selection of data and present a very superficial analysis of model simulations for the instrumental period. I recommend major revisions, based on the points raised below, before the paper can be considered for publication.

Main points of criticism:

### 1) Data selection

The use of SLP (Luterbacher et al), starting 1659, is an obvious choice. Why then only use the annual SAT reconstruction (Mann et al) from 1730 onwards?

Why include just two proxy records (with again different lengths), although Jones and Mann (2004) and others describe many more high-resolution, well dated records for the last few hundred years.

Is the data selection based on a) the presence of a multi-decadal signal or b) other criteria. Please explain, and expand the dataset used.

How reliable are the Mann et al data over the ocean?

### 2) Model simulations

The study uses (new?) PUMA and (existing?) ECHAM4 AGCM runs, forced by the GISST data (1865-2000 period?), to complement the analysis of the instrumental data. It is disappointing that the PUMA runs at least are not extended to cover also the pre-instrumental period.

What are the 'former' and 'recent' versions of PUMA? Is there a better reference for this model than a technical report?

Some remarks about the results in section 3.3:

How surprising is it to simulate SAT patterns over the ocean that are similar to observations, when the model is forced by SST?

What about the simulated SAT anomalies over land, are they consistent with observed SAT? (Small point: a band of low-amplitude positive anomalies (obs) is definitely not similar to a band of negative anomalies (ECHAM).)

One could as well argue that simulated SLP patterns are not consistent with observations; the inter-model differences are huge.

Why compare observations with an ensemble-mean? the observed climate is only one realisation of the system, not an ensemble-mean.

In short, I find the model analysis unsatisfactory. One would expect an analysis of mechanisms here, not just a description of results. What is 'the understanding of multi-decadal climate modes' (abstract) derived from these model results?

### 3) Analysis of proxy data

Section 3 is an odd combination of results published earlier in the literature (eg the spectra for the two proxy records) and some new results. I find the ordering confusing (first the long records, then the instrumental period, then back to the long records).

### 4) Statistical significance

Good that the authors apply some sort of significance test, but I don't think that for low-pass filtered records the number of degrees of freedom is simply given by the record length divided by the filter period. At least the autocorrelation of each record should be taken into account. Anyway, it should be noted that the record length of 150/a few hundred yrs allows for 2/5 cycles of the AMO to be resolved - how significant can this be, even if the t-test looks good?

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

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