Ocean Science

Discussions

EGU

Interactive comment on "Can the boundary profiles at 26N be used to extract buoyancy-forced AMOC signals?" *by* Irene Polo et al.

Anonymous Referee #1

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This paper addresses a significant question and presents interesting results which should be published. There are though some important details that need to be addressed in the presentation of the results before the work can be published.

The main result is that, in the model simulations, the variance of density variations on the boundary is, like the AMOC, dominated by the wind on inter annual timescales and only on decadal timescales does the buoyancy forced signal emerge from the "noise" of the wind forced variability. This is not too surprising and the paper could be strengthened by more emphasis on the structure of the variations associated with wind and buoyancy forcing, which has been less studied in previous work.

Below are some specific points to consider:

1. More often than not the axes in figures are not labelled and/or units are not given. This is a fundamental requirement.

2. In the introduction (l95) it is staled that section 7 "describes the limitations of the interpretations, as well as possible applications e.g. in data assimilation", but there is no mention of assimilation in section 7.

3. Table 1. Its stated that all correlations are significant if greater than 0.3, but doesn't the significance of correlation depend upon number of independent data and hence whether the time series ae filtered or not?

4. Figure 1b. I would like to see the time series of WIND shown too, without that it is not clear how much variability there is in wind on longer timescales.

5. Figure 1a,b Was the same standardisation applied to each time series? Probably better not to standardise then it would be obvious to the reader what is being compared.

6. Notation in the figures is sometimes inconsistent. E.g. "AMOCg" and "1PC" in Figure 1 , but "AMOC" and "PC1" elsewhere. Also abbreviations identifying filtered and unfiltered data vary.

7. Calculation of EOFs (l228) Figure 3c,d show EOFs calculated using data below 800m, so how are these extended up to the surface as seen in the Figures? Some more information is needed in the methods. And are these EOFs for annual or monthly data?

8. The EOFs presented in this paper are dimensional. This is useful, but they are not always calculated this way (sometimes they are normalised) so the text need to make clear what methodology is used here. And it is particularly important to show units on axes to avoid confusion! I presume the variance of the PCs is standardised?

9. The results of different truncations in Figure 4 suggest anomalies propagate at different speeds at different depths. This would imply that there is no direct correspondence between the EOFs and the modes of boundary waves. This should be discussed.

10. Discussion of Figure 8 should highlight how low the variability is in run BUOY over this period.

11. To what extent does the length of the model run affect the results in Figure 9? The run is only about 60 year long so I suspect that for the longer time periods the reduction in the inter-quartile range is a reflection of the length of the model run, rather than the uncertainty of the correlation.

Other minor comments:

l15 "a propagation speed leading to" is unnecessary

l16 Abstract - what does maxima refer to?

l67 Despite many model studies (insert 'model')

Fig 9 "extension of the " is not needed. Often the captions are not as well written as the text.

l261 More variance of what?

l459 What does "simultaneously" refer to?

l460 "It is worth notice that " Also rephrase to making meaning clear " . . . that, due to . . . , time filtering is . . ."

l470 "but it is not that obvious for the EB" not clear what this is referring to.

l476 Which PC1 NEMO (Control, WIND or BUOY? and evaluated over what time period?

l490 1st conclusion - expand what "characteristic signatures " means

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