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Interactive comment on "Labrador Sea sub-surface density as a precursor of multi-decadal variability in the North Atlantic: a multi-model study" *by* Pablo Ortega et al.

Anonymous Referee #1

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1 General comments

Ortega et al. analyze the internal multidecadal variability of the CMIP5 piControl ensemble (∼1° horizonal resolution) and two additional model simulations with a higher resolution (1/3° and 1/4°). Cross-correlations show significant links between subsurface density variability in the western subpolar North Atlantic (represented by the principal component of density; PC1-LSD) and the AMOC at different latitudes when the Ekman component is removed. The correlation is not coherent over different latitudes and the driving factors of the relationship are derived.

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Discussion paper

The manuscript provides a new and certainly helpful index for density anomalies in high latitudes and the multi-model perspective offers robust insights into the internal variability of coupled climate models. The text is well written and good to follow. I am not very surprised by most of the findings as in the context of Atlantic Multidecadal Variability (AMV) analyses of various models showed similar results (see point 3 in the next section). The discussion of these is almost missing. Those and other issues are described in detail below. Once these are addressed, I would like to see the manuscript published as it improves our ability to "predict earth system change" (from the scope text of ESD).

2 Specific comments

The main rationale of this paper is the interpretation of cross-correlations of processes that exhibit a red frequency spectrum. Large significant correlations are found between (subsurface) density anomalies/variability in the western subpolar North Atlantic (spatial average over the Irminger Sea, Labrador Sea and the boundary current along Greenland; blue box in Fig. 1a) and the subpolar gyre circulation strength in the same area (SPGSI) and the AMOC at 45°N (AMOC45). This link decreases with distance as shown for the subtropical large-scale circulation, represented by the AMOC at 26°N (AMOC26).

I am not very surprised by these results: 1) The active tracers temperature and salinity are transported to the subpolar gyre (SPG) by the strong Gulf Stream and the North Atlantic Current. Both tracers change density and hence the circulation. The stronger the circulation, the stronger the tracer transport/input. In this very general sense, Fig. S1 e and f are nearly identical: here, it does not matter if one correlate the SPG strength with the deep Labrador Sea density itself (dLSD) or with its first principal component (PC1-LSD). Then, by definition, the rather red frequency spectrum yields smaller cor-

relations with increasing lag and/or space. Based on this I would expect lower correlations with the remote AMOC26 compared to AMOC45. Density anomalies propagate with the Western Boundary Current (WBC) – or the western boundary densities (WBD) as used here – but model differences in re-circulations, current strength and location or diffusion yield a larger model spread on the long way to the subtropics.

2) Since you remove the Ekman component from the AMOC indices, short-term variability is removed. I speculate that if the Ekman component would be included, the spectra would be closer to white noise (e.g. Figure 2 d of Ba et al. 2013) and the correlations would decrease more rapidly due to the more short-term wind-induced fluctuations (Wunsch and Heimbach 2012). If this is the case, does the main finding, the multidecadal variability in the Labrador Sea density as a precursor of the AMOC, applies also in the real world (or at least in the models) with Ekman contribution? If not, what do we learn instead? The role of the Ekman component should already be mentioned in the abstract.

3) Similar findings were described in the context of the Atlantic Multidecadal Variability (AMV). As you refer to Knight et al. 2005 in the introduction, I think the findings should be discussed with those studies that relate local (subsurface) hydrography and large-scale ocean circulation variability (e.g. Polyakov et al. 2010, Ba et al. 2013, or preferably with section 3 of the review of Zhang et al. 2019). This is of particular interest since the CMIP5 ensemble tends to underestimate internal variability (Cheung et al. 2017).

In addition, I miss several points in the discussion/conclusions:

- The box chosen for PC1-LSD is rather large. From my experience, the signal will be dominated by the WBC and is possibly quite different from the Irminger or Labrador Sea interiors. Due to this choice, it is difficult to address some of the uncertainties that you summarize in the introduction (L93f). Wouldn't this study be good opportunity to test different locations by calculating e.g. PC1-IS for the

Irminger Sea?

- Likewise, it would be great if you could redo the calculations with the historical CMIP5 ensemble in addition to the pure internal variability of the picontrol runs. I am aware that this would be beyond the scope of the paper as it is at the moment, however, if you and your colleagues discuss the work further, I would really like to see these runs included.
- I miss a final statement if and why PC1-LSD is better suited than e.g. dLSD or any other measure for deep water formation activity? What kind of index should be used by the community? Can you give a recommendation?

3 Technical corrections

- Please state the version of EN4 you used somewhere in the main text (not only in the url in "Data availability").
- Regarding all scatter plots: it is a bit difficult for me to distinguish the yellow/light green symbols representing the different models (I need to zoom in with the pdf viewer, don't know how it looks printed). If you want to stick to the colors, you could use capital letters instead of the symbols?
- The references are very annoying to check due to missing line breaks.

L134f & L139f: Can you give the number of model years of the picontrol and present day simulations?

L141: Rephrase "IPSL" so that one can find the model in Table 1.

L149f: Can you briefly describe the main differences between picontrol and present day experiments) one sentence enough)?

L168: EOS-80 is deprecated. TEOS-10 should be used in the future.

L179f: I would like to see the 10-year running mean time series in addition to the running trends in Fig. 2b.

L211f: Most models are too dense by > 0.1 kg m^{-3} at the surface and > 0.2 kg m^{-3} at the subsurface as shown in many previous papers. In my view, "good representation" is too optimistic.

L215f: Please give a reference on how you calculated the EOF (there is also no ref in Ortega et al. 2017).

L218f: It's not clear what it means when the "vertical profile in Figure 1 is weakened or strengthened". Can you clarify this related to stratification? Does this apply to the temp/salt profiles as well?

L223: In what way is the multidecadal variability seen in Fig. 2b "important"?

L226: This sentence confuses since the deep LSD was not mentioned before. Why not something like "In addition to the PC1-LSD index we define the deep LSD (dLSD) as . . ."?

L237f: Add to the caption that σ_2 density is used at depth. Is the sigma symbol missing in "(2)"? Can you add axes to Fig. 1a?

L245f: Is it possible to add calendar years e.g. as an upper x-axis to Fig. 2b since one of the runs represent present day? Is there a reason why you don't show a PC1-LSD time series of one (or more) of the CMIP5 models in Fig. 2b?

L266: Please state the depth d that you use as the Ekman depth since Baehr et al. 2004 give a range (50-100 m).

L271f: By chance, could you name the main differences of the Fourier spectra if the Ekman is included (one sentence enough)?

L287: AMOC45 and AMOC26 and not AMOC45N and AMOC26N.

L280f: Please also write at least one sentence to Fig. 4d (or remove Fig. 4d since Fig. 4d = Fig. 6a).

L292: You write that the cross-correlation lags in Fig. 5 are up to 10 years. In the caption of Fig. 5 you write that the lags can be any (for me, this indicates that lags > 10 years are possible). Can you please clarify that in the text and/or caption?

L296f: Can you speculate why the model spread increases when dLSD is used compared to PC1-LSD? Also, in my view, the model spread increases only for the AMOC indices. The cross-correlations with the SPGSI and ESPNA-T700 indices are more or less independent of the choice of LS density index (Fig. S1).

L312: "... 2005 to 2015 (Robson et al. 2016; Ortega et al. 2017)" → "... 2005 to 2014 (Robson et al. 2016)"

L314f: Put the ESPNA-T700 definition to section 3.2, where you describe Fig. 4 (Fig. 4d = Fig. 6a).

L314: "temperature" → "potential temperature"

L326f: The ocean plays a role in what? This sentence is a little strange to me (of course, the ocean plays a role?).

L336: Correct "of the first on the".

L377f: Do you take the maximum cross-correlations of PC1-LSD and AMOC26 or the maximum of the absolute values of the cross-correlations? It is difficult to judge from Fig. 4b but maybe you can clarify that all the maximum cross-correlations occur when PC1-LSD leads AMOC26 (if this is the case).

L378: AMOC45 and AMOC26 and not AMOC45N and AMOC26N.

L389f: The sentences "Models with ..." and "Models that have ..." have the same

content. Remove one of them.

L393(1): In Fig. 9, depth averages from 0-100 vs 500-1000 m were used. In the header of Fig. S2 b as well as in the caption of Fig. S2 is written that depth averages from 0-100 vs 500-1000 m were used. I am confused since these are the same depth ranges but for you derive different results in Fig. 9 c compared to Fig. S2 b.

L393(2): I am not totally convinced by the stratification definition used in Fig. 9 and Fig. S2. From Fig. 9 a and b you infer that "no link has been found between the PC1-LSD vs AMOC26 relationship and temperature and salinity stratification in the Labrador Sea" (L390). Assuming that you indeed find significant and non-significant correlations for the same variable depending on the depth range, I would rephrase the sentence from "no T/S link" to something like "weaker T/S link than density ... depending on depth range".

L403: green → blue; EN4 → (EN4)

L405: Add to the caption that σ_2 density is used at depth.

L407: LSD → PC1-LSD; Add " (WBD)" at the end of the sentence.

L412: "It represents the correlations" → "It represents the in-phase correlations" (or lag-0 or . . .)

L413f: Please add the section lines to Fig. 1a and state that σ_2 density is used.

L421: "Figure 10f" → "Figure 10g"

L426: "HiGEM 57" → "HiGEM 57N" in header of Fig. 10e.

L434: I find the formulation "deeper WBDs" a little misleading since its the correlation between PC1-LSD and the zonal density field which is deeper in those models, not the WBD itself (in the sense of a boundary current).

L435: The sentence "We also checked . . ." can only be understood together with the

caption of Fig. 11. Please clarify.

L440 & L444: “AMOC26N” → “AMOC26”

L444: “57N” → “57°N”

L446: “run .” → “run.” and “WDB” → “WBD”

L462f: The second and third findings can be merged.

L481: “wstern”

L505: Given the high latitudes I would not say that two 1/3° and 1/4° horizontal resolution ocean models with a similar climatology as the 1° CMIP5 ensemble imply that the representation of the mesoscale might not be as important as the climatology itself. The first baroclinic Rossby radius is ∼10 km or lower and as you give with the 1/12° reference, resolution does matter.

L510: Is it really “including in” or just “including”?

Caption of Fig. S1: “. .” → “. “

4 References

Ba et al. 2013: https://link.springer.com/article/10.1007%2Fs00382-012-1633-4

Baehr et al. 2004: http://hdl.handle.net/11858/00-001M-0000-0012-006A-A

Cheung et al. 2017: https://journals.ametsoc.org/view/journals/clim/30/12/jcli-d-16-0712.1.xml?tab_body=fulltext-display

Polyakov et al. 2010: https://link.springer.com/article/10.1007%2Fs00382-008-0522-3

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