Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2020-83-AC1, 2021

Earth System Dynamics Discussions

ESDD

Interactive comment

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.

Pablo Ortega et al.

pablo.ortega@bsc.es

Received and published: 8 February 2021

Response to Anonymous Referee 2

General Comment:

Variations of the Atlantic Meridional Overturning Circulation (AMOC) and the associated meridional heat and fresh water transports are an important driver of climate variability in the North Atlantic region and beyond. They are also thought to provide a source of predictability on decadal time scales. Previous studies have described mechanisms behind this variability often focusing on the source regions of North At-

Printer-friendly version

Discussion paper

lantic Deep Waters. The authors describe the AMOC characteristics of about 20 CMIP5 models, most of them at resolutions in the ocean models of 0.5 to 1 degree, and two coupled models using an eddy-permitting ocean grid. They relate AMOC variability in the sub-polar and sub-tropical Atlantic to density changes in the Labrador Sea (LS). Such a connection has been invoked in several earlier studies, but the merit of this is that they provide an investigation in a multi-model context. They concentrate on the buoyancy-driven part of the overturning circulation and use long preindustrial control simulations to provide statistically sound results. A striking result is that there is a high degree of coherence in the models (at least in this specific class of models) regarding the relation between the LS density structure and the sub-polar North Atlantic circulation. Even the corresponding relation to the lower latitudes is still quite robust, even though model differences lead to different strengths of the correlations. The study itself and the individual analyses are well thought-through and the text describes adequately the results presented in the figures. Apart for sharing the critique of the first reviewer on missing discussions of some important previous papers (I would also include, for example, Delworth and Zeng (2016) where the mechanisms are nicely described), I feel that the authors overstate a bit the robustness of the "observational constraints". For example, given the results of Jackson et al. (2019), I don't think that the numbers of AMOC strength from DePreSys (used in figure 8) are any better than the zoo of results provided in that paper. Other than that, I have only some more minor issues that I would like to see corrected or commented (see below). Therefore, I recommend publication after minor revisions.

Response: We thank the reviewer for the time devoted to reviewing the article, and for all the good points raised, which we have addressed carefully to improve the quality of the manuscript. In the new version, we have extended the discussion of recent literature, as requested by the two reviewers, and expanded the discussion on the limitations related to the observational datasets used in this study, where we recommend further work to assess the validity of the observation-based constraints (lines 585-591[1]).

The detailed answers to all the different comments, explaining how they have been addressed in the new manuscript, are included below. Also, a revised version of the article, highlighting the modifications with respect to its original version will be uploaded in the system after the editor's approval.

Specific comments:

Abstract, ln 27: Given all the uncertainties that the authors discuss themselves, I recommend not to end the abstract with this statement.

Response: We have removed the final sentence from the abstract.

Experiments: Ln 134 ff: I suggest to discuss already here why you have chosen the piControl runs and not the historical simulations (I assume to have better statistics in the long runs). This needs to be justified as several studies have pointed to the forced component of the AMOC and AMV in the 20th century.

Response: We now provide an explanation in the text (lines 160-163) of the reasons to use the piControl runs over the historical simulations, which includes the computation of more robust statistics thanks to the availability of longer experiments, and also an easier interpretation of correlations thanks to the absence of forced trends, which can lead to spurious significant linear relationships.

ln 140: which of the MPI models? Ln 141: there is no IPSL model in table 1

Response: This part has been rewritten. IPSL was the model with the coarsest effective resolution in Table S1 of Menary et al (2015), but it had not been included in this study because it lacked the necessary variables. We now mention three models in its place (GISS-E2-R/GISS-E2-R-CC/CanESM2), as the three have the coarsest resolution used in this study. Also, we have also noticed that the same table in Menary et al

[1]All line numbers refer to the version of the revised document with track changes

(2015) included GC2, which has a finer resolution than all the MPI models previously mentioned, and have changed it accordingly in the manuscript.

Menary, M. B., Hodson, D. L. R., Robson, J. I., Sutton, R. T., Wood, R. A. and Hunt, J. A.: Exploring the impact of CMIP5 model biases on the simulation of North Atlantic decadal variability, GRL, 42(14), 5926–5934, doi:10.1002/2015GL064360, 2015.

Ln 155 ff: EN4 should be introduced here and not in a half sentence when it is first used.

Response: This is a good suggestion. EN4 is now referenced here.

Labrador sea density: Ln 196 ff (Figure 1 b): it is interesting that some models show very cold temperatures near the bottom. Is this originating from the overflows? Are these models specifically good in representing the overflows or include a parameterisations (CESM)? Do these differences matter in the later analyses?

Response: The figure included in this response shows that the three models exhibiting very cold temperatures near the bottom (4000m and deeper) in Figure 1b are CanESM, GISS-E2-R-CC and GISS-E2-R. The four CESM model simulations (that as far as we know are the only model configurations with parameterised overflows; Danabasoglu et al., 2010; 2012) lie within the intermodel spread, which suggests that explicitly resolving the overflows (as opposed to parameterising them) does not necessarily affect temperature stratification in the Labrador Sea. It's also worth noting that the four CESM simulations show very high differences across them regarding the magnitude of the AMOC26 vs PC1-LSD cross-correlations. This can be inferred from Figure 8b, which points to other key contributions unrelated to the overflows to explain those large inter-model differences.

We cannot discard, however, that in some specific models, like GISS-E2-R-CC and GISS-E2-R, the overflows have a decisive impact in the deeper ocean levels, and through them in the LSD vs AMOC26 relationship, as both models are within the ones supporting a stronger correlation between both variables (e.g. see Figure 8b). However, these models are not necessarily more realistic, as the very cold temperatures they present near the bottom are not supported by the EN4.2.1 observations. A detailed examination of the role of the overflows would take considerable time and effort, and we have preferred not to do it. Instead, we have included a discussion (lines 473-477) on the possible reasons behind the differences in density stratification across models, mentioning the potential role of the overflows.

Danabasoglu, G., W. G. Large, and B. P. Briegleb, 2010: Climate impacts of parameterized Nordic sea overflows. J. Geophys. Res., 115, C11005, doi:10.1029/2010JC006243.

Danabasoglu, G., S. Bates, , B. P. Briegleb, , S. R. Jayne, , M. Jochum, , W. G. Large, , S. Peacock, , and S. G.Yeager, 2012: The CCSM4 ocean component. J. Climate, 25, 1361–1389.

Figure 1 a: I suggest to use a more intuitive and color-blind friendlier color map.

Response: The color palette has been changed to viridis (https://www.thinkingondata.com/something-about-viridis-library/) that is color-blind friendly. Also, in the new version of Figure 1a darker colors represent higher differences in density, to make it more intuitive.

Figure 2b: it is not immediately clear that the orange and red curves belong together (one might think they are from MRI as in the previous figure), the thick green looks more like black in my print-out.

Response: The colors in the timeseries of the decadal running trends are now only slightly darker than for their respective raw time series, to make it more clear that they represent the same simulation.

Figure 3: why is the 10 year time scale highlighted?

Response: It is highlighted to separate the interannual from the decadal/multi-decadal

Printer-friendly version

Discussion paper

timescales. This is now indicated in the figure caption.

Figure 4d: what is the difference between this and figure 6 a?

Response: They are the same. This panel had been included in both figures by mistake. Figure 4d has been removed.

Ln 340ff: if you want to explain cooling or warming, shouldn't one look at heat transport divergence/convergence?

Response: Our goal with Figure 7 is not so much to explain the ESPNA T700 changes in terms of heat transport convergence/divergence, as a sort of heat budget analysis, but to link it to changes in the heat transport that can be directly attributed to AMOC variability.

Ln 351: could it also be related to a dynamical spin-up or down due to variations in the horizontal density structure (e.g. Born et al, 2013).

Response: This dynamical spin-up could explain a delay in the response of the SPG, but this is not supported by Figure 4c, showing that PC1-LSD and the SPG are in phase. We don't see how the changes in the horizontal density structure (which would mostly affect the SPG strength) could explain a delay in the OHT-gyre component. For us the most likely explanation is the delayed advection of temperature anomalies, already mentioned in the manuscript.

Figure 8: If the DePreSys AMOC is so far off from RAPID, why should it give a good observational estimate at 45N?

Response: This is a very good point. We now acknowledge this difference between DePreSys and RAPID in the text (lines 449-452), and recommend caution when using DePreSys as an observational reference for the AMOC45.

The symbols here and in fig 9 are very hard to decipher, e.g. I can't see GC2 in 8b

Response: We have changed the size of the symbols, and the color palette to improve

Printer-friendly version

Discussion paper

the visibility.

Figure 9: caption: it is hard to see any green line. Is there an "and" missing between "assimilation run" and "EN4"?

Response: The blue line was mistakenly tagged as green, and EN4 should have been included between brackets. Both errors have been corrected.

Ln 410ff: could the DePreSys run help to define which depth level of the DWBC is most consistent with observations?

Response: It can. Indeed, the depths at which the largest correlations between PC1-LSD and the WBDs are attained in DePreSys3 are provided as vertical dashed lines in Figure 11. We were not previously discussing these DePreSys values in the article, but we do so in the new version of the manuscript (lines 521-523)

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-83, 2020.

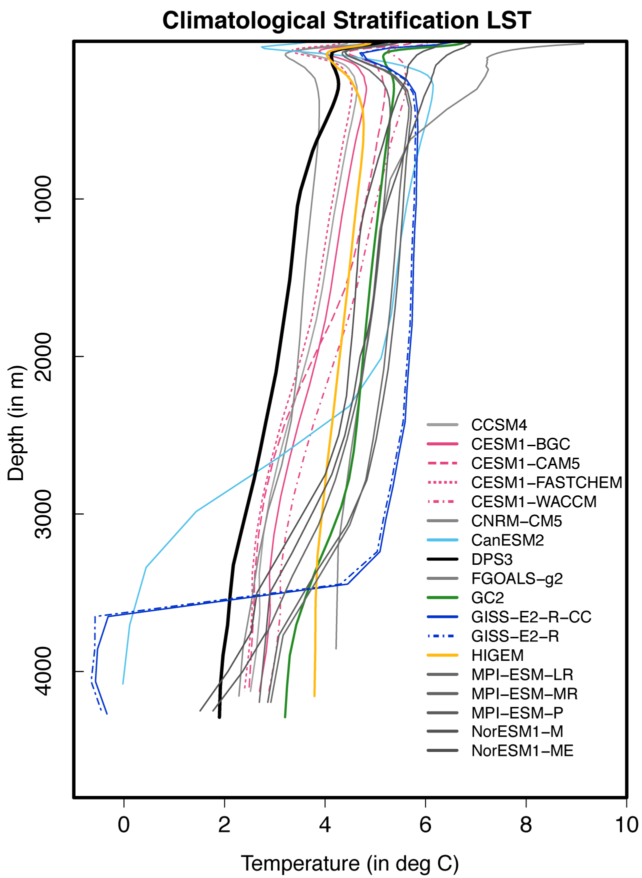

Fig. 1. Climatological mean of the spatially averaged Labrador Sea (blue box in panel a) temperature as a function of depth in the simulation ensemble, the DePreSys3 assimilation run and EN4.

Printer-friendly version

Discussion paper