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Discussions

Interactive comment on "A clustering-based approach to ocean model-data comparison around Antarctica" *by* Qiang Sun et al.

Anonymous Referee #2

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This manuscript proposes a method for model-data as well as inter-model comparisons. In regions with strong fronts, simple shifts in the geographical distribution of hydrographic properties can give rise to large data-model biases when compared on a point-by-point basis. Instead, the authors propose that a clustering algorithm can be used to group different hydrographic regimes, which provides a more insightful interpretation of potential model biases. The authors apply their technique to observations and model output for the West Antarctic continental shelf/slope region.

Overall, this is a potentially useful approach and is worthy of publishing as a methods paper. I have a few concerns with how the results are discussed and some enhanced explanation of how this method could help identify issues with the representation of physical process in numerical models should be included. I hope the authors find the

comments below helpful in revising this manuscript.

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Major comments:

My primary concern with the manuscript is the choice to apply the clustering technique to a vertical profile, rather than to individual water masses. I found the discussion of this confusing at various places throughout the text. Typically, we use tracers to define water masses with distinct properties, formed by a specific physical process in a specific place. These water masses are then used to trace circulation pathways as tracer properties become modified and ultimately destroyed during their transit to new formation regions (Groeskamp et al. 2019). Due to this lateral transport, throughout most of the ocean a vertical profile at a given latitude/longitude position samples a number of different water masses formed far from the profile itself. Consider an example: AAIW is formed on the norther side of the ACC and spreads into both the Atlantic and Pacific basins at intermediate depths. A clustering algorithm, similar to the one described here and performed on vertical profiles in these two regions, would produce two different hydrographic "groups" because the Atlantic profile would detect NADW whereas the Pacific profile would not. This would be true even if AAIW properties were the same. Thus the clustering method, defined this way at least, would not be helpful in identifying biases or differences in AAIW formation.

The authors need to more carefully state how this method can be used to address biases in water mass formation and circulation. Clustering on a vertical profile convolves these two processes, whereas clustering distinct groups of water properties would, I believe, provide a clearer assessment of the former. In particular, the authors refer to "core water masses" in Figure 8 âĂŤ but this is not an accurate description. In panels (b) and (c) of Figure 8, there are at least two water masses contributing to this clustering group, forms of CDW and WW. The formation of these two water masses happen through different processes and in quite different locations, and their changes need

to be considered independently when trying to understand why five or six groups are selected by the clustering algorithm or why there are biases in the model data.

Ultimately, the choice to cluster on the profiles still provides information about model-data biases âĂŤ I am not suggesting the authors need to revise their analysis. However, the motivation for this choice and the discussion of the manuscript's results could be improved. I provide a few more specific comments and suggestions below.

Minor comments:

- Line 46: "These errors may influence the future rate of regional warming," Be clear you mean warming in the model here.

- Line 56: "Strong gradients are evident . . . " Perhaps give a few examples?

- The paper should include a more detailed description of the WOA and CESM2 hydrography used in the study as well as the surface forcing for the latter. For instance, it would be helpful for the reader to know how meltwater fluxes are parameterized or applied in CESM2. I assume there is no representation of ice shelf cavities. Similarly, a brief description of the types of data that is included in the WOA would be helpful: Are Argo floats included in the 2000-2500 m depth ranges? Are seal data from the MEOP data base included (e.g. Pellichero et al. 2017)? In fact, using data that only goes up to 2004 is really not ideal considering how much effort there has been to improve observational coverage in West Antarctica over the last decade and a half.

- Line 116: "We wish to identify regions that exhibit a similar vertical structure". Here the authors should provide additional justification for this approach considering formation sites are spatially distinct. In fact, it would be equally interesting to perform the clustering analysis on individual density surfaces. Combining different clustering analyses may provide complimentary information.

- Line 126: "rearranging the data nearest them." I did not understand how (or why) this rearrangement was carried out.

- Section 3.3: It would be useful to know how much seasonality exists at the target depths that have been selected for the clustering analysis (either variability over a year in the model output or data availability for the WOA).

- Line 255-256: "These three groups (1, 4 and 5) represent the three "source" ABRS hydrographic regimes." Based on my major comments above, I do not like the use of the term here "source" because these regions are not necessarily isolating water mass formation processes. The authors should be clearer about what this grouping represents.

- Section 3.4: This manuscript would be improved if the authors went beyond simply stating the differences between the CESM2 and WOA clustering and provided some explanations for why the geographic distribution of the groups differ.

- Line 292: "The region identified as HSSW (group 5), in the southwestern Ross Sea, remains." Again, I would not identify a cluster group as a water mass. You might say, "The region identified as Group 5 in the SW Rosss Sea, which is associated with HSSW formation, remains."

- Line 358: "Our comparison suggests that mean-state biases of CESM2 on the ACSS result from both local and remote processes." The authors do a good job of explaining the changes in T/S properties that give rise the distinct clustering groups, but the discussion of physical processes either missing or misrepresented in the models receives less attention. This statement in the conclusion is quite broad and not well justified. As mentioned above, further discussion of how the model represents glacial melting, sea-ice formation, surface fluxes, interior mixing, etc., and how this might impact tracer properties, would strengthen the manuscript.

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