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Ocean Science Discussions

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

Anonymous Referee #1

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This manuscript presents a gridded model-data comparison in the Western Antarctica continental shelf area based on an unsupervised statistical method, the K-means method. The authors claim that this method helps distinguish more robustly the distribution of T-S properties in the region, and brings a clearer picture on how the model and data compare with each other.

One major issue I have with this study is that the authors chose to focus on one of the most difficult regions in the World Ocean to test the method. The Antarctic continental shelf suffers from one of the poorest data sampling, making any climatology at best questionable. It is also a region with very complex bathymetry and complex dynamics making its representation in current ocean models utterly difficult. One may wonder why the authors should test their statistical method on such a complex region. This

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Discussion paper

raises the question of what is the main goal of the study: validate the statistical method or discuss the representation of hydrographic properties in a climate model?

If the main focus is on the statistical method, I suggest the authors present a similar study in a more favourable region. At least they should use better products than WOA as the reference, such as SOSE or MIMOC for the data product and present a comparison between more products to give confidence that results have any degree of generality. The authors should also explain on which basis they have decided to compare each of the five WOA and CESM groups one by one. Comparisons of geographical distributions in Fig 3 and of properties in the metric space in Fig. 4 shows very little resemblance, so it is not clear at all that the two fields can be usefully compared at all.

If the main focus is in discussing the degree of realism of CESM, important information should be added about this run. Technical details are missing, such as the vertical resolution of the ocean component, the type of atmosphere model, the representation of sea-ice in the model, and the parameterization of mixing processes. Also, the equivalent of Figs. 1, 2 and 5 using CESM data is missing. I have the impression that most conclusions drawn in the study regarding CESM could be obtained by simple visual inspection of such added figures.

Because most results in this study are at best preliminary, I recommend rejection of the manuscript and I suggest the authors undergo a thorough revision of their manuscript before resubmitting, making more apparent what is the main goal of the study and how their results can be considered original.

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