

DOI: 10.5194/os-2020-85
Version: Second Revision
Title: Effects of strongly eddying oceans on multidecadal climate variability
in the Community Earth System Model
Authors: André Jüling, Anna S. von der Heydt, Henk A. Dijkstra

Point by point reply to reviewer #1

May 26, 2021

We thank the reviewer for their careful reading and for their very useful comments on the manuscript.

1 Reviewer Summary:

I believed the author has addressed most of the comments raised in the first round of review. Overall, I think the conclusions are clearer and more convincing. I have some additional suggestions/comments that I think would help improve the manuscript.

2 Minor Comments:

- I suggest adding uncertainties in the mean spectral power and spectral slope estimates and/or carrying out hypothesis testing to determine if the difference seen in these mean spectral powers (and spectral slope) are statistically different.*
(1.201) We slightly changed the mean and slope estimation method to employ $\log(f)$ -weighting instead of using log-equidistant bins. We use the jackknife estimator for the uncertainty at each frequency and, assuming independence, we arrive at an estimate of the standard deviation of the spectral mean. The resulting standard deviations are visualized as error bars. For the slope estimate, we report the standard error.
- Line 5: remove ‘eventually’ from the phrase ‘eventually the global mean surface temperature’.*
(1.3) Edited as suggested.
- Line 9: I appreciate that you tried to be more explicit about the implications of the study in the abstract and discussion. But I’m not super satisfied with the abstract part. I guess what I’m thinking is it would be great to highlight the implications of this study (mesoscale variability is important to multidecadal variability) on studies that use CMIP class models in the abstract.*
(1.11) We added a sentence to the abstract which now makes the point that the current model generation may systematically underestimate multidecadal variability.
- Lines 46-57: I appreciate the addition of this paragraph to clarify the relationship between these climate modes, SHF, OHC, and GMST. However, how this paragraph is currently written is a bit awkward. Since the majority of the introduction section is about the SST modes, I would reorganize the paragraph a bit, such that it focuses on how representation of SST patterns affects our quantification of SHF, OHC, and GMST, instead of how it is currently written – how the latter*

three metrics are related to SST.

(1.47) Suggestion followed, we rewrote this paragraph.

5. *Lines 92-93: I would change ‘In idealized non-eddy ocean models, modes of multidecadal variability exist that depend critically on the prescribed eddy diffusivity’ to ‘In idealized non-eddy ocean models, the existence of modes of multidecadal variability depends critically on the prescribed eddy diffusivity’.*

(1.95) We edited the sentence as suggested.

6. *At some point in the intro, it might be worth mentioning what is the spatial scale of mesoscale just to highlight why 1 deg models do not resolve mesoscale.*

(1.84) We included a sentence to that effect.

7. *Line 121-123: I would also add Chang et al. (2020), where they did a longer simulation and supplemented it with extremely detailed analyses.*

(1.124) We added this excellent reference which had not been available at the time of the original writing.

8. *Line 141: For the sake of completeness, maybe it’s worth to test the sensitivity of your results with other SST products also (e.g., ERSST, COBE)?*

(1.??) We now include COBE and ERSST in an appendix figure. We describe the datasets in the methods and refer to differences where appropriate in the results section.

9. *Line 208: Out of curiosity, why is the HR-CESM is interpolated to specifically 0.4deg rectangular grid?*

This is a standard interpolation routine provided by NCAR for the high resolution POP.

10. *Line 222: ‘(Fig. (Fig. 2d)’ should be ‘Fig. 2d’.*

(1.229) changed as suggested

11. *Line 222-223: This statement is a bit confusing – it is implying that there is a causal relationship between the previous sentence and this sentence, but in reality, the larger amplitude in observed low-pass filtered PDO compared to models *does not* imply there is a larger proportion of spectral power >13 years. Please clarify.*

(1.229) Indeed there is a discrepancy between that statement and the mean MV power estimates of Fig. 4 so we removed the sentence.

12. *Line 268: The correct reference for MacDonald 2005 should be MacDonald and Case 2005. Apologies for providing the incorrect reference in the previous review.*

(1.279) We corrected the bibliography entry. It turns out this was a result of a faulty DOI database entry which Mendeley automatically retrieves.