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Comment

***Interactive comment on* “Impact of model resolution on sea-level variability characteristics at various space and time scales: insights from four DRAKKAR global simulations and the AVISO altimeter data” by T. Penduff et al.**

Anonymous Referee #1

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Review of “Impact of model resolution on sea-level variability characteristics at various space and time scales: insights from four DRAKKAR global simulations and the AVISO altimeter data” by T. Penduff, M. Juza, L. Brodeau¹, G. C. Smith, B. Barnier, J.-M. Molines, and A.-M. Treguier

This manuscript describes the results of a statistical analysis of a set of models ranging in resolution from 2 degrees to $\frac{1}{4}$ of a degree (2° , 1° , 0.5° , and 0.25°). The models are from the same family, the NEMO code, but have differences in the details. Most significantly, the differences in model structure are between the two lower resolution

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models and the two higher resolutions. The forcing of the 4 simulations is consistent. This paper analyzes the sea surface height anomaly signal as compared to altimetric measured observational data of the sea level heights. While the paper does a reasonable job at describing and quantifying the variances in sea level anomalies (SLA) and correlations between the observed and simulated signals, there are some important aspects (such as noting what the significance level is for correlations) in the analysis that have been left unreported and which may or may not change the conclusions (refer to the specific comments below). The paper's conclusions are not new which, in general, relate to finding that mesoscale-emitting models result in a more realistic simulation of the SSH variance over low-resolution models. However, the analysis does contain some valuable insight into which temporal scales improved for this particular model using the described analysis techniques. The paper could easily be extended to make it more complete. Specific Comments:

1. There is no discussion anywhere in the text about the significance level of the correlations (and considering degrees of freedom of the filtered data). The level should be different for each filtered band. Instead of finding the average correlations over a latitude band, it would be more illustrative, in terms of the quality of the simulation, if we knew how many grid points had a significant correlation for a given latitude. For example, looking at Figure 4 and the middle column, there does not seem to be any significant difference between any of the simulations. In figure 4, column 1, except at the high southern latitudes, the variances are quite similar, especially when compared to the observed variances. The use of the word “significant” is used throughout the paper and when used, needs to be associated with a quantitative meaning, rather than subjectively or in a qualitative sense. In other words, how is “significant” determined?
2. Pg. 1520, section 2.2.1, It would be interesting to know if there was a spatial pattern associated with the grid point mean that was removed from each simulation. The differences in the pattern/biases between the simulations may be informative.
3. The paper has a tendency to reference figures before they have been formally

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introduced (e.g. Section 2.2.2 & figure 3 &4). This makes the reading of the paper somewhat difficult. One way to solve this problem is to add two paragraphs to section 2 to fully describe the figures in general. Fig. 5 is first introduced on pg 1525, without any description of it and it is left to the reader to attempt to figure out the connection.

4. Figure 5 is not adequately described in the text and it is not clear what the figure is trying to show and how it is related to the text (pg. 1525 and pg. 1527). I don't know if any of the lines are significantly different than any other for each resolution.

5. The writing would be clearer if words instead of symbols were used for much of the text, rather than saying " $\sigma A(i, j)$ and $\sigma m(i, j)$ ", why not just say "The altimeter and model variances . . ." (section 2.2.2, pg. 1523, line 1). This is just an example. The complete manuscript should be carefully considered to make the reading easier.

6. Pg. 1525, line 9, what point is being made by this phrase " these eddy scales follow the internal Rossby radii that decrease polewards much faster than the $\frac{1}{4}$ degree local resolution."?"

7. Section 6, Fig. 6, Figure 6 is not adequately described within the body of the text. It is unclear what is being shown. I'm assuming that the shows a normalized variance quantity (model variances normalized by the altimeter variance), but I'm not sure what the y axis is, is it just the difference between the average correlation of the full grid between the two resolutions or for a latitude band? I am not sure the regression lines add any actual value because the scatter is so large in many of the cases (though not all). My interpretation of Fig 6a, is that if the dot falls in the first quadrant (x and $y > 0$) then it may indicate an improvement (one still needs to determine quantitatively what is significant). Such improvements are indicated in the top right subplot, and in the right column. Figure 6b over interprets the results from Fig 6a, I believe. The authors should specifically point out the reduction in the correlations going from low resolution to high in first column while increasing variance in Fig 6a. This is a curious result and does not support the overall conclusions of the paper.

8. Section 6: The authors write specifically about the Southern Ocean in this section, but no evidence is presented (or it is not clear to me) in Figure 6 where one can separate out ocean basins.

9. Section 7 can be much improved with more quantitative analyses, rather than just a description of the changes with estimates of percentages of changes. First, maps of ratios of the subplots in Figure 3 could be shown to support the discussion. Second, since the temporal evolution of the patterns, rather than just variance is important in climate studies and ocean/atmosphere interactions, simple EOF or principal component analysis can help to elucidate the differences that relate to large-scale interannual variability.

10. Section 8: If/when the authors address the comments above, the conclusions may need to be revised, including improving their justification for some of the conclusions. The authors need also to realize that there are other aspects of modeling that may improve the realism of simulations other than grid resolution, such as improved understanding of mixing processes and improved numerical schemes for various processes that have been incorporated into higher resolution models.

Technical comments:

1. Pg. 1522, line 10 – better wording “Each latitude band . . . “ , rather than “Each stripe λ ...”. Generally, a reliance on symbols in section 2.2.2, when words would be better (i.e. “. . . finally computed for the λ th, latitude band . . .”).
2. Pg. 1522, line 15, awkward use of φ , when the symbol is not used in the equation above it
3. Pg. 1523, line 17, should this say “the Brazil-Malvinas Confluence”, rather than just the “Confluence”? And should the reference to Agulhas, be the Agulhas Retroflexion?
4. pg. 1524, line 24, reference to “this skill”. Skill has not been defined up to this point. Please define.

5. pg. 1528, line 12 . . . “unsensitive” should be “insensitive”

6. There are a variety of corrections that should be made concerning grammar, for example, sometimes “increase” is used instead of the “increases” (pg. 1530, line 6)

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