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Ocean Science Discussions

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.

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We would like to thank the reviewers for their thorough reading and very interesting suggestions. Many parts of the paper have been rewritten to improve it and satisfy the reviewers' remarks.

New material and main changes.

- we agree that mean SSH fields are of interest: new figure 2, and new section 3. These are compared with Niiler et al (2003)'s "observational" reference.

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- significance of temporal correlations have been computed: modification of fig 4 (now numbered 6).

- reshaping of this fig 6 to match the organization of std(SLA) panels in the now-labeled fig 4.

- the "old" figure 5 about near-coast changes in temporal correlations was not very clear nor statistically significant, and also quite restricted in scope. We now present and comment more generally on local changes in (significant) temporal correlations at global scale (new fig 5)

- Suppression of the confusing fig 6 and section 6 from the old manuscript. The new fig 7 is, we hope, much clearer and helps discussing the links beween increases in std(SLA) and changes in correlations (now discussed within the other sections); new material in the conclusion on this subject.

- We tried to clarify the discussion about resolution-induced decreases in interannual temporal correlations (in the conclusion)

Please find below our detailed answers to the reviewers' remarks. Note that, unless specified otherwise, we use the new figure and section indexes in our answers.

========== REVIEWER 1 ==========

*** Reviewer's preliminary remark: 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.

We agree with the reviewer (see line 26 page 1524, previous numbering) that decreasing resolution is already known to damp mesoscale activity, but the introduction (page 1517, lines 13-18, previous numbering) explains our main focus is an issue that, we

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think, hasn't been addressed yet and might be of interest: how does model resolution affect slower variability? Results at shorter timescales were also presented since mesoscale (fast) and interannual (slow) variabilities are often collocated and exhibit interesting links (e.g. comparable sensitivities to resolution, or discrepancies w.r.t. observations), thus clarifying the key importance of resolved eddies for the interannual variability.

*** 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.

We agree with the reviewer: we now evaluate rigorously the significance of each temporal correlation as described in equation 3. This method (von Storch and Zwiers 1999) does not assume that each pair of model and observed timeseries are white or have the same autocorrelations. This provides 16 maps (4 model-observation comparisons x 4 spectral bands, not shown) of significance levels associated to each temporal correlation map. Temporal correlation maps were then masked where local coefficients were not significant. The middle column in Fig 6 has been updated using these significance levels and now presents the average of *significant* (i.e. satisfying eq 4) temporal correlations in each latitude band. The new Fig 5 shows resolution-induced changes in significant temporal correlations. This more rigorous assessment method yields results that are largely consistent with our previous results, and some additional information. We have updated the text accordingly throughout the paper.

*** The use of the word "significant" is used throughout the paper and when used,

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needs to be associated with a quantitative meaning, rather than subjectively or in a qualitative sense. In other words, how is “significant” determined?

We agree. We have removed the word "significant" unless quantitatively grounded.

*** 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.

We agree. A new figure (labeled 2) and a new section labelled 3 have been added about mean sea surface heights (MSSH). The observational reference is taken from Niiler et al (2003). This latter field and simulated MSSHs are collocated onto the same AVISO grid. Latitude-dependant estimates of spatial correlations between simulated MSSHs and this reference MSSH are also shown in Fig. 2 and discussed in section 3. This MSSH assessment remains secondary compared to our major objective (assessing variabilities), but is now mentioned in the abstract, introduction, in sections 2 & 3, and in the conclusion.

*** 3. The paper has a tendency to reference figures before they have been formally 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.

We agree. Section 2.2 has been completely rewritten to (1) clarify and update the description of our processing technique, (2) take MSSHs into account, and (3) introduce figures progressively more precisely, with the reviewer’s specific remark in mind.

*** 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.

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This confusing figure has been removed. Temporal correlation changes near the coast are visible in he new figure 5, and are discussed in sections 4 & 5.

*** 5. The writing would be clearer if words instead of symbols were used for much of the text, rather than saying “sigmaA(i , j) and sigmaM (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.

This is true. We have corrected the whole manuscript to take this suggestion into account.

*** 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 1 degree local resolution.” ?

This was not clear indeed. Here is the new version (section 4) "At these latitudes, none of our model grids can adequately resolve the first Rossby radius (see Fig. 1) and thus the most unstable baroclinic waves (baroclinic instability occurs at larger scales). However, the contribution of barotropic, topographically-influenced fluctuations on SLA increases with latitude at these frequencies (Guinehut et al., 2006; Vinogradova et al., 2007). This resolution-induced increase in high-latitude mesoscale resolved variability might thus come from stronger (and/or less damped) barotropic motions, rather than enhanced baroclinic instability."

*** 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)

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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.

We agree that this figure was confusing. It has been removed. The connexion between resolution-induced changes in SLA variability changes and correlations is now discussed from new figure 7.

*** 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.

Section 6 has been removed. As explained in point 7 above, the new figure 7 clearly shown the meridional structure of changes and highlights the specificity of the Southern Ocean.

*** 9. Section 7 can be much improved with more quantative 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.

We agree on the remark. This section (still numbered 7), and the previous ones, now take advantage on the new figures 5 and 7, which present the changes on all our metrics when resolution is changed. This should satisfy the reviewer's request.

*** 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.

We agree with the reviewer that the study of climate variability is also based on spa-

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tiotemporal modes. EOF analyses have been recently applied on these 4 runs + AVISO (global and several regions, various time- and space-scales, projections of certain EOFs onto others, etc) and yield a complementary, consistent, but large ensemble of new results. We think this quite large set of new results are not necessary to the present study that is already dense, and would be more meaningful is presented together in a dedicated paper (in preparation).

*** 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.

We have rewritten many parts of the paper, in particular the conclusion, and added new material. Our new analyses, procedures and figures did not happen to modify our previous results substantially, though. Our conclusions have been reordered in, we hope, a more logical order.

*** 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.

We agree, and we are conscious of this fact. This does not imply, however, that resolution and numerics have a minor impact on simulation results. After a study of the (major) impacts of numerical changes at constant 1/4° resolution in global models (Penduff et al, 2007; Le Sommer et al 2009), we focused the present paper on the sole (but substantial) impacts of resolution (and resolution-dependant SGS parameter) changes with the same numerics, as clearly stated in the introduction. Studies like the present one are complementary to those evoked by the reviewer.

Technical comments: *** 1. Pg. 1522, line 10 – better wording “Each latitude band ... “ , rather than “Each stripe ? ...”.

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OK, fixed

*** Generally, a reliance on symbols in section 2.2.2, when words would be better (i.e. ".... finally computed for the lambda'th, latitude band ...").

OK, we have tried to remove symbols when possible.

*** 2. Pg. 1522, line 15, awkward use of phi, when the symbol is not used in the equation above it

We agree. We now write: Let $\overline{\phi}^{t}$ denote the time average of *any* variable ϕ

*** 3. Pg. 1523, line 17, should this say "the Brazil-Malvinas Confluence", rather than just the "Confluence"?

Yes indeed. Fixed.

*** And should the reference to Agulhas, be the Agulhas Retroflection?

Yes, we agree. Corrected.

*** 4. pg. 1524, line 24, reference to "this skill". Skill has not been defined up to this point. Please define.

Yes indeed. We replaced it by "in terms of SLA spatial correlations"

*** 5. pg. 1528, line 12 ... "unsensitive" should be "insensitive"

OK, fixed.

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

Thanks for mentioning. We have tried to correct all typos and grammatical errors.

Interactive comment on Ocean Sci. Discuss., 6, 1513, 2009.