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Ocean Science Discussions

Interactive comment on "Upscaling of regional models into basin-wide models" *by* Luc Vandenbulcke and Alexander Barth

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We thank the reviewer for his review, the comments and interesting suggestions. Below we reply to the specific comments.

1. The reviewer is entirely right that the configuration used to test the upscaling method is based on a nested grid setup using the same model code (Nemo) for both the parent and child grid; and furthermore the vertical grid is also identical (only the horizontal grid is different). This may influence the conclusion compared to a configuration with 2 different model codes. However we think that it is not a fundamental limit of the method
a) Concerning the vertical grid, in the "normal" case of assimilating real observations,

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the latter are on a different grid than the model. Similarly if the child model was on a different vertical grid than the parent model, it would still contain useful information, and be worth to be assimilated in the parent model. What may happen however, is that some observations could be lost (e.g. the lowest model of the child model could be out-of-grid in the parent model)
b) if different model codes are used, the models could represent different processes. Hence, this should be taken into account by modifying the (representativity part of the) observation error covariance matrix. Examples of contributions to the representativity error could be
- different vertical coordinates
- different representations of the surface: rigid lid, free surface (with a linear or non-linear representation e.g. in Nemo)
- hydrostatic model, or not
- different atmospheric forcing fields
- different turbulent closure schemes
- different numerical schemes for advection, horizontal diffusion etc.
It is our opinion however, that between the parent and child models, the most striking difference is the horizontal resolution, and that therefor, the general conclusions of the paper are valid, and upscaling should not be limited to the case of parent and child models being identical.
This is now better explained in the paper

2. If one considers that the child model is better in its domain than the parent model (e.g. by comparison with real observations), then it would be desirable to upscale it into the parent model. This would be the case is some processes are dependent on resolution, in straits, etc; and is closely linked to the first specific comment in the review.
We provide now a table in the annex of the paper giving details about the setup of both models; but upscaling should not be limited to identical parent and child models (see

answer to comment 1)

3. The reviewer is correct, that the assumption of spatially independant pseudo-observations is very strong. We are actually working on a non-diagonal observation error covariance matrix, but this is a large work that would not fit into the current paper. However, the assumption is partly alleviated by increasing the (diagonal) part of the matrix, in order to compensate for the (missing) non-diagonal elements. Increasing the diagonal elements in the matrix by a factor 3, for example, is similar to thinning observations with a factor 3. This is now stated in the paper.

4. We added an annex to the paper with the details of the data assimilation filter used in the study.

5. In the state vector of the parallelized parent model, we include the tiles covered by the nested model, but also the tiles immediately around that area. Therefore, as the reviewer correctly assumes, the correction is not cut off at the margins of the area covered by the nested model, but propagates outside. The extent of the correction outside the area, depends on the radius used in the localization method. This is now better explained in the paper.

6. We moved the remark to the conclusions

7. In the study (section 3.4), L3 satellite images are used. Only in figure 5 is the L4 image used for visual comparison of model and satellite image. This is now clarified in the article.

8. added units

9. The reviewer is right that the list of advantages should be accompagnied

with a list of possible limitations (or disadvantages). This is now included in the article, and copied here:
a) the child model should be "better" than the parent model
b) exactly as when assimilating real observations, the data assimilation procedure itself uses approximations, and this could degrade the analysis
c) if the parent and child models are very different, the parent model could not manage to ingest the pseudo-observations
d) the coupling with upscaling is not as strong as with real two-way nesting

Potential remedies for limitations b and c would be
b) see all the research about this problem (in the context of assimilation of real observations), such as anamorphosis techniques (when a non-linear relation exists between model variables and observations), particle filters (when the error distribution cannot be considered Gaussian), etc
c) the observation error covariance matrix should be specified carefully to filter out the processes of the child model, that cannot be represented in the parent model

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