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Discussions

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Comment on os-2020-119

John M. Huthnance (Editor)

Editor comment on "High-resolution stochastic downscaling method for ocean forecasting models and its application to the Red Sea dynamics" by Georgy I. Shapiro et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-119-EC1, 2021

For whatever reason, lack of response from invited referees has unfortunately left me having to write this report in lieu of an additional independent referee, in order to maintain progress.

The manuscript describes an alternative approach to downscaling with the aim of providing finer resolution in a (sub-)region using a coarser-resolution wider-area model for boundary conditions.

A clear limitation is an assumption (lines 95-96) that the coarser-resolution wider-area model is accurate at all its grid points. Probably a finer-resolution model (impractical – the point of the manuscript) would be more accurate and give different results at the coarser-model grid points. However, the assumption leaves no scope for adjusting values at the coarser-model grid points. Thus the limited accuracy of the coarser model is "built in" and the method is strictly interpolation, albeit allowing for statistical properties of finer-scale fluctuations (anomalies). It seems to me that this is reflected in the validation (section 2.4) that the comparisons with OSTIA and ARGO data show very similar bias and RMS error for the coarser and finer models.

Another assumption is that the distribution of fluctuations (anomalies, at any one depth) is statistically uniform and isotropic horizontally (line 129). This is inherently a limitation on the area of the (sub-)region where interpolation for finer resolution is desired. It may imply avoidance of nearby coasts, other distinct topography or water-mass boundaries (for example), despite the optimisation of weighting coefficients allowing for coasts.

The manuscript is written logically in mostly good English. Some of the above issues are raised again in the following detailed comments on which clarification is sought.

Detailed comments.

Abstract. It is important that the abstract is clear and easily understood. Please clarify:

Line 14. What is the “double penalty” effect?

Lines 20-21. “areas smaller than the Rossby radius, where distributions of ocean variables are more coherent”. If the point about “more coherent” is necessary then what is more coherent with what? Maybe small structures have internal coherence but their occurrence and scales are more likely to be stochastic, not coherent.

Line 23. 1/24th degree from 1/12th degree is only a factor of 2 and begs the question of how much refinement the method works for.

Line 25. “. . cost function which represents the error between the model and true solution.” In practical use the true solution is not available.

Section 2.2

Lines 167-168. “The correlation matrix is calculated . . . for each grid node on the fine mesh.” This is possible where the true field is known (as here) but not in practical application unless there are data with resolution as good as on the fine mesh. Such data cannot come from the coarser model.

Line 170. Surely the “final stochastic downscaling is carried out using” Equation (1) with the now-known pi. Eq. (7) was used earlier to calculate the correlation matrix.

Section 2.3

Lines 245-247. Regarding the comment on lines 167-168, actual data for Eq (5) only exists at nodes of the coarser grid. Do the other 75% of points on the finer grid invoke the assumption that deviations □□′ are statistically uniform and isotropic in the horizontal plane? Please clarify.

Line 256. “previously considered” meaning nearest adjacent (node) already solved for?

Section 3

Lines 324-325. I think that one cannot argue from the accuracy of the idealised experiment in view of the question about data at nodes on the fine mesh (lines 167-168 comment). In the Red Sea example the finer-resolution model has accuracy very close to the coarser-resolution model and may well have more small-scale features (as figure 9 – yet to come – suggests) but it is not yet clear that “it also improves the accuracy of simulation.”

Line 373. What is the basis for “underestimates”?

Line 381. “vorticity” should be “enstrophy”?

Section 4.

Lines 416-417. Same comment as on lines 324-325.

Line 429. Repetition: “optimal . . optimised”

Line 430. “short range, comparable with the resolution of the parent model”; is this a limitation on the refinement from coarser to finer?

Lines 475-480. I think the origin of “greater granularity” in the finer model should be further discussed. Conceivably the statistics are related to those determining the correlation matrix etc. but is there any deterministic element in the small-scale (c.f. lines 521-522), or (more likely) in the seasonal variation of their statistics (line 479)?

Lines 489-495. The sign of vorticity can be biased (e.g. in coastal eddies) but does not show in enstrophy. Has SMORS a basis for showing such bias? How does any such bias in its output compare with the best available evidence? More enstrophy is likely in the finer-resolution model but does its increase take it significantly closer to the “truth” – is there evidence to test that? Certainly the finer resolution in figure 14 presents a more convincing picture but it appears to add little except interpolation; all the features are embryonic in the coarse-resolution figure.