Ocean Science Discussions

Ocean Sci. Discuss., author comment AC3
https://doi.org/10.5194/os-2020-119-AC3, 2021

Reply on EC1

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Author 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-AC3, 2021

Responses to reviewer 2 on the manuscript 'High resolution stochastic downscaling method for ocean forecasting models and its application to the Red Sea dynamics'

General.

Comment. A clear limitation is an assumption (lines 95-96) that the coarser-resolution wider-area model is accurate at all its 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.

Response. This limitation has been removed in the revised manuscript by adding a new sub-section 2.3 Effect of noise in the input data. The calculations when the parent model is noisy (i.e. not accurate at all of its grid points) show that SDD method gives much better approximation to the true field that 'strictly interpolating' methods such as bi-linear or bi-cubic interpolation of the coarse mesh data. For example in case of 10% noise in coarse grid, the RMSE error between the fine grid data generated by the SDD method and the true field is the same 10%, by bi-linear it is 25%, and by bi-cubic it is 19%. Moreover, we present an example where the coarse mesh is eddy permitting and the fine mesh is eddy resolving, with a resolution doubled in each spatial dimension. In this situation, mesoscale eddies are embryonic in the coarse mesh and can be restored into the fine grid.

Comment. 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.

Response. The assumption that a finer model would be more accurate is not always the case, at least when standard point-wise metrics are used like RMSE and bias. The following quote from (Crocker et al. 2020) explains the situation (emphasis added):

One of the issues faced when assessing high-resolution models against lower-resolution models over the same domain is that often the coarser model appears to perform at least equivalently or better when using typical verification metrics such as root mean squared error (RMSE) or mean error, which is a measure of the bias. **Whereas a higher-resolution model has the ability and requirement to forecast greater variation, detail and extremes, a coarser model cannot resolve the detail and will, by its nature, produce smoother features with less variation resulting in smaller errors. This can lead to the situation that despite the higher-resolution model looking more realistic it may verify worse (e.g. Mass et al., 2002; Tonani et al., 2019).**

This is particularly the case when assessing forecast models categorically. If the location of a feature in the model is incorrect, then two penalties will be accrued: one for not forecasting the feature where it should have been and one for forecasting the same feature where it did not occur (the double-penalty effect, e.g. Rossa et al., 2008). **This effect is more prevalent in higher-resolution models due to their ability to, at least, partially resolve smaller-scale features of interest.** If the lower-resolution model could not resolve the feature and therefore did not forecast it, that model would only be penalised once. Therefore, despite giving potentially better guidance, the higher-resolution model will verify worse.

The manuscript was amended to include the clarifying text and references (Lines 322-342)

Comment. 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.

Response. The assumption is actually that the distribution of fluctuations is statistically uniform and isotropic horizontally only locally, within the radius of computations given by Eq (8), not over the whole area. Clarification is added in Line 137. Such assumption is not unusual. Modern data assimilation schemes assume statistical uniformity/isotropicity in the horizontal. For example, "The NEMOVAR ocean data assimilation system as implemented in the ECMWF ocean analysis for System 4" section 4.6.2 "Length scales" reads: "The horizontal background-error correlations for X = T , S U and η U are assumed to be isotropic poleward of a given latitude φ_L , with an identical length-scale $L_\lambda = L_\varphi = L$ used for all variables and at all depths". (https://www.ecmwf.int/en/elibrary/11174-nemovar-ocean-data-assimilation-system-implemented-ecmwf-ocean-analysis-system-4) Some data assimilation schemes allow for non-homogeneity in the length scale, but *local* homogeneity is still required. This means that when computing the correlation matrix, homogeneity is assumed within the computation radius of every node.

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

Line 14. What is the "double penalty" effect?

Response. The double penalty effect is described in the literature as follows. If the location of a feature in the model is incorrect, then two penalties will be accrued: one for not forecasting the feature where it should have been and one for forecasting the same feature where it did not occur (the double-penalty effect, e.g. Rossa et al., 2008). Double penalty phenomenon is more evident in the high resolution models (Crocker, R., Maksymczuk, J., Mittermaier, M., Tonani, M., and Pequignet, C.: An approach to the verification of high-resolution ocean models using spatial methods, Ocean Sci., 16, 831–845, https://doi.org/10.5194/os-16-831-2020, 2020). The SDD method honours the data on the parent coarse grid and hence the spatial structure is anchored onto the coarse grid, therefore there is no additional spatial shift and no additional double penalty effect compared to the parent model. Clarification is given in lines 322-342 of the revised MS.

Comment 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.

Response. Ocean fields are more coherent within 1-2 Rossby radii than between more distant points, so that mesoscale eddies of that size are sometimes called oceanic coherent structures, see e.g.(G.I. Barenblatt et al (eds), 1992. Coherent structures and self-organisation of ocean currents. M.Nauka, 198pp. In Russian: Г.И.Баренблатт и др. (ред). 1992. Когерентные структуры и самоорганизация океанических движений : М. : Наука, 198 с. ISBN 5020008079; F. J. Beron-Vera, M. J. Olascoaga, and G. J. Goni,2008. Oceanic mesoscale eddies as revealed by Lagrangian coherent structures. Geophysical Research Letters, Vol. 35, L12603, doi:10.1029/2008GL033957;

P.F.J. Lermusiaux and F. Lekien, 2020. Dynamics and Lagrangian Coherent Structures in the Ocean and their Uncertainties, http://web.mit.edu/pierrel/www/talk/pfjl_lekien_final_oberwolfach05.pdf) and the references in the MS . Clarification is added to the text (LINE 21, 490).

Comment. 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.

Response. An increase of resolution by a factor of 2 (in each horizontal direction) increases the computational cost be a factor of 10 or more. The number of nodes is quadrupled (2x2) and the time step should be made 2 times smaller to comply with the Courant–Friedrichs–Lewy stability condition, which give the increase of number of computations by 2x2x2=8. The computation would require a larger number of computing cores and the overhead adds 20%-30% or more due to non-linearity of scaling. The cost of a relatively small HPC cluster is about £100K, so the purchase of 10 times larger computer can be a game-stopper. The SDD method adds a small number of calculations which can be performed even on a laptop computer. The efficiency of the SDD method is discussed in Section 2.2 of the revised MS.

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

Response. "True solution" or "true state" is standard parlance in data assimilation when calculating a cost function. In many formulations, variables for the true solution are included, even if that true solution is never known (see for example R. N. Bannister, A review of forecast error covariance statistics in atmospheric variational data assimilation. I: Characteristics and measurements of forecast error covariances, 2008. Quarterly Journal of the Royal Meteorological Society, https://doi.org/10.1002/qj.339) . Clarification is given in Line 180.

Comment. 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.

Response. The correlation matrix can be computed in practice using one of several methods (e.g. Hollingsworth and Lonnberg, 1986) which do not require knowing the true field. For instance, in H-L method, the true state is removed and only the errors remain by subtracting the background values from the observations. In this case, the only requirement is that the data is unbiased, the true state is not needed. In our paper eq. (7) is presented as a parametrised approximation by Fu et al. The text is additionally clarified below Figure 1.

Comment. 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.

Response. The text is corrected as advised (reference to eq(7) is replaced with Eq(1)).

Comment. 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.

Response. This is correct. In common with the theory of 2D turbulence (eg Rhines, P.B. 1975. Waves and turbulence on a beta-plane. J. Fluid Mech. 69, 417–443), the SDD model requires that all deviations are locally (within the search radius) isotropic and homogeneous. Additional clarification is added in Lines 136-137

Comment. Line 256. "previously considered" meaning nearest adjacent (node) already solved for?

Response. This is correct. We have amended the Ms to incorporate this clarification (line 305).

Comment. 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."

Response. We meant that the SDD model has the ability to forecast greater granularity, variation, and extremes with respect to simpler interpolation schemes shown (bilinear, bicubic and spline). We have amended the Ms to clarify this point. (Line 250-253).

Comment. Line 373. What is the basis for "underestimates"?

Response. We meant that the coarse model shows lower values of gradients. The text is amended as requested.(LINES 431-432)

Comment. Line 381. "vorticity" should be "enstrophy"?

Response. Yes. The text has been amended (LINE 440).

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

Response. Please see our response to comment for lines 324-325.

Comment. Line 429. Repetition: "optimal . . optimised"

Response. Thanks. The text has been amended to avoid repetition (Line 489).

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

Response. SDD has been designed to improve on the results from Eddy permitting models. From this point of view, it is not a limitation of the model but rather its desired area of applicability.

Comment. 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)?

Response. Yes, there is seasonally (monthly) variation of the statistics in the norm that is used to compute the innovations. The text is amended to emphasize this (Lines 541-543)

Comment. 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.

Response. We did not notice any bias in the sign of enstrophy in our calculations. It is correct that more enstrophy is likely in the finer-resolution model. More enstrophy is closer to the truth where the true filed is known as it was shown in the idealised experiment in Sections 2.2 and 2.3. In section 2.2 and 2.3 it is shown that the SDD method is significantly more efficient in recreating smaller scale features that common interpolation methods such as bi-linear or bi-cubic. It is correct that the SDD method is designed mainly to improve the eddy-permitting models where the smaller scale structures such as mesoscale eddies are only embryonic.

We thank Reviewer 2 for helpful comments.