Ocean Science Discussions

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Reply on RC1

Georgy I. Shapiro et al.

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-AC2, 2021

Responses to reviewer 1 comments.

The authors are thankful to the reviewer for thorough and helpful comments. Our responses are given below. References to line numbers are for the amended MS.

Comment. Figure 2 uses an example in which the grid barely resolves the fields in the x-direction. It may be that it is only in such cases that OI gives significantly better results. Are the authors able to explore that in a little more detail?

Response. It is correct that the maximum enhancement produced by SDD compared to simple interpolation is expected when the parent model barely resolves the field. If the ocean feature is well resolved by the parent model, there is no need for further refinement. For example, if the zonal size of the eddy is increased to 40 km instead of 13 km, and hence it is reasonably well resolved by the parent model with Δx=10km, then the RMSE produced by SDD is similar to that of bi-cubical interpolation. On the other hand, if the parent model misses the features completely, e.g. no eddy permitting, then the SDD method does not have enough information to re-create the smaller scale features. The clarification is given in lines 215-220.

Comment. Second the model field that is being interpolated should not usually be regarded
as being precisely correct. More specifically an ocean (or atmosphere) model is well known to be unreliable near the grid-scale. Fields are typically either noisy at the grid-scale or overly damped. It is also a moot point whether the fields represent point values or grid cells means. To what extent it is possible to extract more information near the grid-scale from model fields by using OI needs more investigation. The fact that OI is well suited to interpolation of noisy (stochastic) data makes such an investigation more attractive than it would otherwise be. The cost of increased resolution is so high that such an investigation is worthwhile, though the value of increased resolution is not necessarily in the increased detail in the simulations.

Response. This is correct, the SDD method works better than simple interpolation in a noisy situation. We have added a small subsection 'Effect of noise in the input data' and a table showing the errors produced by different methods when the parent model outputs are noisy. This also shows that the coarse field does not need to be regarded as precisely correct. We are thankful to the reviewer for this comment and suggestion to discuss the treatment of noisy data in the MS.

Comment. Abstract: The abstract describes the sort of problem that is being addressed and does introduce the principal idea in lines 20-22. The important point about the lack of a double penalty is clearly made on lines 14 and 27. So the abstract is a reasonably informative summary of the paper.

Response. Thank you.

Comment. Lines 20-21: I found the sentence starting on line 20 somewhat difficult to follow.

Response. The sentence is now re-worded.(Line 20)

Comment. Lines 54 and 55: it might be worth mentioning that the commonly used, more "modern", variational methods are also closely related to OI (Lorenc 1986, QJRMS).

Response. The reviewer's suggestion is implemented in lines 56-57.

Comment. The literature on methods for post-processing of model outputs using Kalman filters should probably be discussed in the introduction. I think the main idea being pursued in this paper is somewhat different from the main ideas in that literature but the techniques are clearly related.

Response. The reference to Kalman filtering and other methods are briefly given in lines 47-48 and 56-57. The reference to work by Lorenc (1986) is given where it is shown that the Kalman filter and more modern variational methods are closely linked to the original OI and they can be described using a common Bayes analysis framework.

Comment. One might ask whether the method proposed is a post-processing of model output or a statistical model in its own right. It is described both as a Statistical Model (in SMORS) and a Stochastic Deterministic Downscaling (SDD) method. Personally I would view it as a post-processing method but do not feel strongly about this semantic issue.

Response. We prefer to term the SDD method as part of the model based on how it is implemented in the code. This is shown in the flowchart in Fig.4.

Comment. The introduction does introduce relevant material but at the end of it one does not have much more insight into how the proposed technique works. The structure of the paper is not described.

Response. In the amended MS the structure of the paper is now explained in lines 82-86. The insight into how the proposed technique (SDD method) works is given in lines 66-73.

Comment. Lines 107-109: The primed quantities (that are interpolated) are deviations from monthly means for the Red Sea. These values would not normally be available in real-time. I imagine that deviations from climatology would be a satisfactory alternative.

Response. This is correct. Clarification is given in lines 115-116.

Comment. Line 133: The use of Gaussian and SOAR functions for the autocorrelation function dates back well before Fu et al (2004). In data assimilation the difficulties / uncertainties in the calculation of this function are usually emphasised quite strongly. The textbook by R. Daley (Atmospheric Data Analysis 1991) is a good source of information on the techniques discussed in this paper. Lorenc 1981 QJRMS describes a fairly sophisticated method for retaining consistency of solutions between points.

Response. We agree. The text is amended as requested and an additional reference to Daley (1991) is added in lines 145-146.

Comment. The idealised case has a=4.1km and the parent grid has Δ☐☐ = 10 km. So the sinusoidally varying field in one direction is really close to the 2-grid point wavelength. It is very impressive how well the OI solution handles this problem (Figure 3 of the paper and line 201 -204). A brief summary of results for some less extreme interpolation cases would probably be informative.

Response. The efficiency of SDD in comparison to different interpolation method is added as a new sub-section in lines 225-252.

Comment. Figure 6: The lack of a double penalty is certainly an interesting result and the comparison with OSTIA data seems sound to me (though I'm not an expert in this issue). It's not clear to me what explains the lack of a double penalty. Is the model SST a relatively smooth field in which case OI and linear interpolation might give relatively small differences? I think there needs to be some further quantification of the double penalty. For example, one could calculate the rms of f' at all points on the high resolution grid that do not coincide with low resolution grid points for the OI, bi-linear and bi-cubic fields. How do these rms values compare with those in figure 6? One might also ask how these rms values compare with the rms of the values at the original (lower resolution) gridpoints. This calculations would help to shed some light on the lack of a double penalty.

Response. Double penalty phenomenon is more evident in the high resolution models (Gilleland, E., Ahijevych, D., Brown, B. G., Casati, B. and Ebert, E. E.: Intercomparison of spatial forecast verification methods. ☐Weather Forecast, 24(5), 1416-1430, 2009). 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 337-341 of the revised MS.

Comment. Lines 296-298: It seems strange to use nearest neighbour values in the ARGO inter-comparison. With the OI method one can do much better interpolations! Some readers may be concerned that the nearest neighbour method could somehow account for the lack of a double penalty (see previous paragraph).

Response. We use the nearest neighbour method for compatibility reasons, as it is used for validation of MyOcean / Copernicus Marine Environment Monitoring Service products, see e.g. Delrosso, D., Clementi,E., Grandi,A., Tonani,M., Oddo, P., Feruzza, G., Pinardi,N. 2016. Towards the Mediterranean forecasting system MyOcean v5: numerical experiments results and validation, 2016. INGV technical report, No 345, ISSN 2039-7941. Clarification and additional reference are given in line 358.

Comment. Lines 311-318: This point that the interpolation will reproduce the field exactly at the parent grid points (to within truncation errors) is an important one. Many readers would find it helpful to mention this earlier in section 2.1.

Response. We agree. Clarification is added in lines 160-163.

Comment. Some results have already been presented in section 2. So the section title seems strange. Results for vorticity or Vorticity diagnostics would be a better title.

Response. The results presented in section 2 have now been moved to section 3, and new sub-sections are created: 'Eddy and mean kinetic energy' and 'Analysis of vorticity and enstrophy'.

Comment. Line 353: describing the two models as eddy-permitting and eddy-resolving seems contentious at this point.

Response. Analysis of the efficiency of the SDD method for eddy-permitting and eddy-resolving models is now added throughout the text.

Comment. Line 360: Could you confirm that Figure 9 shows area mean values of vorticity? Lines 363-364 suggest it does. The phrase "Absolute values of vorticity" in line 360 gives the reader some cause for uncertainty on this point. I suggest you remove "Absolute" from that phrase.

Response. The text amended as advised (lines 419-420)

Comment. Figure 14 is a nice illustration of the potential of this method. I wonder whether there are any plotting packages which use this type of approach.

Response. We are not aware of any plotting packages which use this type of approach.