Second Review of “High resolution stochastic downscaling method for ocean forecasting models and its application to the Red Sea dynamics” by Georgy I. Shapiro, Jose M. Gonzalez-Ondina, Vladimir N. Belokopytov

Overall comments

The authors have given good responses to most of my previous comments. This review repeats the previous comments which I do not think have been adequately addressed and explains why I am concerned about them.

I have made three additional comments to those from my earlier review. I apologise that I did not make these comments in the first round but these points have only crystallised in my mind following more study of the paper and authors’ responses.

I still think this paper is interesting and should be suitable for publication after significant revision provided the authors are willing to re-consider some of their claims.

Previous comments

1. As I read it, the main idea in this paper is that optimal interpolation (OI) is the best method for interpolating a field f of values specified on a set of regular grid points x, to another set of points. This may seem obvious from the name “optimal interpolation”, but OI is not usually thought of in this context. It’s usually thought of as a method for combining a number of irregularly spaced noisy observations with a climatology or model background to produce an optimal analysis. Figure 2 of the paper shows that, for some regularly gridded fields, OI provides much better interpolation results than standard methods like bi-linear or bi-cubic interpolation. If this is correct, which I think it is, it could be important for several reasons. First, in the data assimilation context, interpolating the model to the observations accurately is widely acknowledged to be a key step. So that doing that more accurately should improve the results. Second, as shown in figure 14, maps of fields with a lot of fine scale structure, such as the vorticity, may be rendered with greater fidelity using OI for interpolation rather than other interpolation methods.

   The authors did not respond to this (first) paragraph of my review which contained quite a number of relevant assertions and suggestions. My main point was that the proposed technique is simply an improved method for interpolation of fields based on the theory of optimal analysis. As the authors illustrate, this method can be successful even when the statistics of the correlation functions used are not very accurate (the statistics were very anisotropic and assumed to be isotropic). Optimal interpolation used in such a context is usually termed objective analysis. The method proposed is clearly not a dynamical downscaling methodology. Although, as the authors explain, optimal analysis can be considered to be founded on concepts derived from homogeneous turbulence and related to stochastic methods, describing the method as a stochastic downscaling will give most readers the impression that the method is based on multiple realisations with higher resolution, which is not really the case.

2. Title: I wonder whether the words Optimal interpolation (or objective analysis?) should be in the title. For example: “Extraction of near grid-scale dynamical information from model fields using optimal interpolation.” I’m a bit concerned that the main point of the paper is not evident from the title.

The authors did not respond to this comment either. My earlier comment implies that "objective analysis methods" would be a better wording than "optimal interpolation"

3. 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.

   My point here is that there is a literature on post-processing that is different from that on data assimilation. The authors could explore that by simply googling "Kalman filter post processing".

4. 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.

   Authors' 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.

   From my previous comments it should be clear that I feel more strongly now that the method should be viewed as a post-processing step.

5. Figure 6: The lack of a double penalty …

   Your response to this point was very helpful. Thank you.

6. 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).

   I'm quite concerned about this point. For many observations the nearest neighbour is further away on the coarse resolution grid. So the coarse grid value can be expected to be less accurate than the fine grid value. I think this gives the fine resolution grid model a significant advantage over the coarse grid one and could well be disguising a resolution penalty.

New comments

7. Lines 18-19 of the abstract state: "Then the method is applied to create an operational eddy-resolving Stochastic Model of the Red Sea (SMORS) with the parent model being the eddy-permitting Mercator Global Ocean Analysis and Forecast System."

   This claim that an eddy-permitting model is transformed into an eddy-resolving model is a significant exaggeration in my view.

8. I have tried to work out whether the proposed method truly provides some down-scaling. The following argument suggests that it does and that one would expect some form of down-scaling penalty to attach to it. It seems to me that the Fourier spectrum for the fine grid fields will be very close to that of the coarse grid fields down to the Nyquist wavenumber of the coarse grid. The fine grid will then have (probably small) non-zero amplitudes in the Fourier spectrum down to its Nyquist wavenumber. These intermediate Fourier amplitudes will be guided by the form of the correlation function. This additional power seems to me to be a modest form of down-scaling that could be based on the estimates of the statistics of the ocean fields.

9. The enstrophy field clearly has larger values on the fine than the coarse grid. I am not sure whether this relates to the additional power in the Fourier spectrum or the fact that the derivatives for the fine grid are calculated using smaller grid spacing than on the coarse grid.