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

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_i, 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 paper suggests (line 413) that the SMORS method should be treated as experimental at this stage. It seems to me that this is correct for a couple of reasons. First the types of fields for which OI provides significantly better results than cubic bi-linear interpolation needs further clarification. 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? 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.

I think this paper is interesting and is likely to be suitable for publication after significant revision.

Specific comments

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.

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.

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

Introduction:

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).

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.

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.

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.

Section 2.1

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.

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.

Section 2.2

The idealised case has a=4.1km and the parent grid has $\Delta x = 10$ 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.

Section 2.4

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.

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).

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.

Section 3: Results

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.

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

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.

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.