

Title: Application of the Ensemble Optimum Interpolation in a North/Baltic Sea model: assimilating temperature and salinity profiles.
By Weiwei Fu and Jun She Shiyu Zhuang

The paper is of average overall quality; some improvements can be gained from refining the syntax but the major improvement will come from further enhancing the analysis of the results. With respect to the syntax, I encourage the authors to perform a critical review of the syntax and structure. As it stands I can not recommend its publication and I would suggest the paper is considered again after a major revision.

I do strongly agree with the general comment from the authors that very few studies have tackled the problem of data assimilation in coastal and shallow sea's models and that their contribution is a valuable one on this sparse research topic. As with all data assimilation methods, there are pros and cons and the authors do argue their choice of method in enough detail. There is however one aspect that I have missed in this paper; while the EnOI is a multivariate method, this capability has not been assessed by the authors. Indeed, they make no attempt in evaluating the effect of assimilating T and S on the U, V and surface elevation.

One major criticism I have of this paper is the fact that most of its conclusions are based on one "experiment", i.e. only one assimilation day is compared to sst images, only one estimate of the persistence time of assimilation, and in all occasions only the most basic statistics are used. There are already enough papers of this sort and I would welcome a more complex contribution to this topic and I believe the authors are capable of delivering it. Tools like PCA, EOFs, Multi-dimensional scaling, wavelet analyses etc should at least be considered and the most appropriate ones used. A description of these methods can be obtained in Stow et al. (2009); Allen and Sommerfield (2009); Doney et al. (2009); Jolliff et al. (2009).

Specific recommendations

L170-172 these lines are confusing, do they mean that the stationary ensemble covariance is different for every 3 month period of assimilation?
L177 long-range

3.3 Localisation

Do the radius chosen produce updates on all of the model domain?

4 Assimilation

What was the criteria used for setting the temperature and salinity errors to .3 C and .1 respectively? Did the authors perform any tests with different T/S errors?

4.1 Quantitative assessment

I am afraid the title of this section is misleading as no quantitative assessment is done on the effect of the assimilation. The comparison of only one assimilation step with a SST image qualitatively and a discussion on the improvements on surface salinity with respect to known hydrographical features of the region does not constitute a quantitative assessment. One suggestion is to use periods where the available observations are not enough for assimilation but can still be used for a quantitative evaluation of both salinity and temperature at a given model step.

L330-331 If using the EnKF rather than the EnOI, the flow dependent error covariance would also be present and so this feature is not unique of a stationary error covariance matrix.

L332- While mean increments can be illustrative, a more complex statistical method for summarising the results should have been used. Reductive methods like EOFs or PCA can provide more information in a similar number of graphs than mean time-evolving profiles of increments.

L348 I am glad the authors acknowledge the need for further investigating the effect of the assimilation on the currents. Unfortunately I do think this is something that should be included at some level in this paper.

4.2 Comparison with

This section can be streamlined further as there is some repetition (L350-355 and L357-358) and the syntax needs to be more carefully checked.

4.3 Effect of the initial state

I am afraid this section also has more text than it needs to, with repetitions of concepts and information on several occasions. It would be good if a second correction is attempted by the authors.

5 Conclusions

In general terms, this section includes a long summary of the paper which is redundant and should be deleted, leaving the paper without a conclusions section. This probably highlights the main problem of the paper: lack of in depth analysis of the results. As an example I could suggest the authors look at the spatial distribution/structure of the increments, provide an statistical analysis of the forecast system persistence time (by running multiple instances of the experiment described here), analyse potential changes to vertical heat fluxes due to the assimilation, look for scientific explanations of why the average RMS doesn't monotonically decrease, discuss if the average RMS does actually give a meaningful description of the overall system, are there instances where too few observations actually deteriorate the model solution (i.e. optimum number of obs to ensure a better model forecast), and what happens to the non-assimilated variables which are also updated? I am certain the authors can come up with many more of these.