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Ocean Science Discussions

Interactive comment on "A pre-operational 3-D variational data assimilation system in the North/Baltic Sea" *by* S. Y. Zhuang et al.

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In this manuscript the authors describe a 3DVAR data assimilation scheme for the operational use in the Baltic Sea. They validate the scheme by calculating the RMSE of temperature and salinity misfits with and without the assimilation of in situ data. I think that generally the technical details of the scheme and the validation are quite well presented. However, the manuscript completely lacks a proper reference to the previous development in the data assimilation in oceanography. Instead it almost exclusively references the meteorological literature, and it may seem that most of presented solutions are applied for the ïňĄrst time in oceanography. I recommend the publication of the manuscript only after a revision that will take into account my comments.

Reply: Thanks for the comments. This paper presents DMI's research in the framework

of ECOOP toward an assimilation system in the North/Baltic Sea. Detail description of the assimilation scheme is presented and some results are validated with observations. The manuscript is revised based on the reviewer's suggestions. For example, some references are added to reference more oceanography literature. Questions and other comments of the reviewer are also addressed in the manuscript.

Particular comments: Q1. Line 20, page 1133. I do not agree that satellite observations have low quality in coastal regions. This statement is completely subjective.

Rely: Thanks. The sentence is rephrased. The quality of satellite data depends on a couple of factors. In the Baltic region, it is more influenced by the cloud cover as compared to the open seas.

Q2. Line 6, page 1134. Why is the complex speciïňĄcation of covariances the key issue in the coastal area? This is also a completely subjective statement. For example, I think that the lack of observations is the key issue.

Reply: Thanks. We agree that lack of observations is a key issue in coastal assimilation. NeverthelessïijŇSpecification of the covariance is also very important because how to propagate the limited information (due to lack of observations) spatially play a crucial role in the coastal/shelf regions. The background covariances would have direct effect on the assimilation results.

Q3. Line 17, page 1134. It should be Purser instead of Purse.

Thanks. It's corrected.

Q4. Line 26, page 1134. Tides and barotropic response to atmospheric forcing do not have scales of 100m.

Reply: Thanks. The response scales are difference depending on regions. It usually has a scale of the order of 100m or less.

Q5. Line 15, page 1135. There is no explanation (reference) for this formula.

A sentence is added to further explain this formula.

Q6. Line 4, page 1136. It should be Courtier instead of Courtie.

It's corrected.

Q7. Section 2.2. How is the L-BFGS algorithm applied? Have the authors developed it or an already available scheme is adapted for the 3dvar?

Reply: Thanks. L-BFGS is an available scheme and adapted in this study to suit our implementation.

Q8. Section 3. The use of the control variable in the way presented in this section, and the presentation of U as a product of linear operators is well known in oceanography (e.g. Weaver et al., MWR, 2003). This should be mentioned and referenced.

Thanks. This is referenced as suggested by the reviewer.

Q9. Line 13, page 1138. The diffusion operator has been used in many oceanographic implementations (e.g. Weaver and Courtier, QJRMS, 2001).

Thanks. It is referenced.

Q10. Line 15, page 1138. Recursive Filter has been applied in oceanography by Dobricic and Pinardi (2008).

The reference is added in the manuscript.

Q11. Section 3.1. I do not see why one should describe the recursive ïňĄlter equations with so much detail. These formulae are available in several engineering textbooks and in previously mentioned references.

Thanks. We agree that recursive filter is described in detail by some technical textbooks. The description here is to show some basics and make the manuscript more self-contained..

Q12. Section 3.2. The EOFs are used for the description of vertical covariances by

Dobricic and Pinardi (2008).

A reference is added to the text.

Q13. Line 5, page 1140. How the formulation with vertical correlation function based on Rz relates to the one by Weaver et al. (MWR, 2003)?

The Rz is not based on Weaver et al (2003) but on our experience at this stage. We plan to change the vertical function as suggested by Weaver et al (2003) in the next step.

Q14. Section 3.2. I ïňĄnd this section very confusing. Do the authors use EOFs or Rz?

Thanks for the comment. The paragraph is revised to avoid some confusion. In fact, we use both Rz and EOFs. The vertical correlation is approximated by the empirical function. EoFs are used to reduce the computational cost.

Q15. Line 9, page 1140. I do not understand this sentence. Is the control vector deïňĄned by EOFs or by something else?

In the vertical, the correlations between different levels are calculated by the empirical function. Due to the huge size of the control vector, EoF is used to reduce the computation. For our model, there are 50 vertical levels, but the number of dominant EOFs (about 10) is much less than that. The sentence is rephrased.

Q16. Line 8, page 1142. It should be "Dobricic and Pinardi" instead of "Dobricic".

It's corrected.

Q17. Section 4.2. A new solution of the coastal boundary problem is given by Mirouze and Weaver (QJRMS, 2010). This paper is referenced.

Q18. Line 14, page 1142. Is this solution correct when the covariances extend over the land (for example over a thin peninsula)? I do not agree that this ad hoc solution is

a valid substitute for the numerically correct boundary condition.

Thanks for the comment. During performing the RF, the covariances are extended over grid points on land. Strictly speaking, this could cause it less accurate in some extreme cases like a thin peninsula. We agree with the reviewer that the numerical boundary condition can be addressed in other ways. Technique like 'imaginary sea points' similar as in Srdjan and Pinardi (2008) may be better and will be taken into account.

Q19. Line 8, page 1144. This simple formula is well known and is given in many text books. Maybe the reference can be given as "(e.g. Zhuang et al : : :".

Thanks for the comment. We agree with the reviewer that this formula well known and can be found in many textbook. It's revised as the reviewer suggested.

Q20. Line 17, page 1144. What does "quasi-Gaussian" mean? I thought that error covariances were modelled as Gaussian.

This sentence is rephrased. The error covariance is modeled as Gaussian.

Q21. Line 19, page 1148. Once again the similarity to previously developed oceanographic schemes should be mentioned

Thanks. Some references are added.

Q22. Fig.4, top. Horizontal covariances do not seem to be isotropic as stated in the text.

The figure is plotted where the distance between grid points are defined by degree instead of kilometer, which makes it appear not exactly isotropic. In the program/code, it is isotropic.

Interactive comment on Ocean Sci. Discuss., 8, 1131, 2011.