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Comment

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

Anonymous Referee #1

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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 first time in oceanography. I recommend the publication of the manuscript only after a revision that will take into account my comments.

Particular comments:

1. Line 20, page 1133. I do not agree that satellite observations have low quality in

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coastal regions. This statement is completely subjective.

2. Line 6, page 1134. Why is the complex specification 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.

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

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

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

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

7. 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?

8. 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.

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

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

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

12. Section 3.2. The EOFs are used for the description of vertical covariances by Dobricic and Pinardi (2008).

13. Line 5, page 1140. How the formulation with vertical correlation function based on

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Rz relates to the one by Weaver et al. (MWR, 2003)?

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

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

16. Line 8, page 1142. It should be “Dobricic and Pinardi” instead of “Dobricic”.

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

18. 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.

19. Line 8, page 1144. This simple formula is well known and is given in many textbooks. Maybe the reference can be given as “(e.g. Zhuang et al ...)”.

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

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

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

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

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