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Ocean Science Discussions

Interactive comment on "Implementation of position assimilation for ARGO floats in a realistic Mediterranean Sea OPA model and twin experiment testing" *by* V. Taillandier and A. Griffa

Anonymous Referee #2

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Review of manuscript: "Implementation of position assimilation for ARGO floats in a realistic Mediterranean Sea OPA model and twin experiment testing" by V. Taillandier and A. Griffa

The article focuses on development and verification of methodology for assimilation of data from ARGOS position observations. The article (i) describes an efficient methodology for assimilation of Lagrangian trajectories in 3-D OGCM and (ii) presence results from estimations of the skills of the assimilation scheme and discuss possible ways of its improvement. Even though I think the paper requires some modifications and revisions I recommend it for publications.

My comments and suggestions for revision are mainly related to the description of the methodology of assimilation. I think the authors should discuss in more details the following aspects of their methodology:

General Comments.

Comment 1: Equations (1)-(3) define the method of choice of the function R(z) of vertical distribution of the velocity. The normalization approach is based on the condition for volume conservation. Another possible approach would be just shifting the velocity profile by the difference between the non-divergent and initially divergent barotropic velocities. The latter procedure (shift of the velocity profile) in opposite to relation (3) would not create additional shear in the corrected velocity. This additional shear baroclinic velocity in (3) is imposed only by considerations for corrections needed for barotropic velocity. This additional shear will be then projected on T and S changes by (6). I think the authors need to motivate this choice of R(z) normalization.

Comment 2: I think equation (6) for the cost function requires additional discussions. As formulated it means that all elements of the vector of the misfit between the velocity correction and its geostrophic component should have the same weight for all depths. Are there any specific reasons for this choice? Usually realistic ocean models have irregular vertical levels and the contributions to the misfit from each level may not be equivalent. Another question is if there is always a unique solution for the corrections of T and S from (6)? Is M a linear operator or it is a locally linearized operator at points T0(z), S0(z), where T0(z) and S0(z) are the vertical profiles of T and S. How the matrix of M is computed in this work?

Comment 3: Fig 5 shows the position of hydrographic sections used to estimate the error in the T and S corrections. Though the method of correction of T and S is not in any means a simple one, I would say that the correction along these sections would be “easy” for the method to capture, because there is a strong mesoscale flow variability along them. Did the authors try different sections in their skill scores studies, for

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instance like the meridional section along 7.5E.

I would expect that the authors discussed these questions in one way or another in the publications they have already published. However I think short and rather general answers of the above comments should be added in the paper. Therefore I recommend the paper for publications after a minor revision.

Interactive comment on Ocean Sci. Discuss., 3, 255, 2006.