

Interactive comment on “Sequential assimilation of multi-mission dynamical topography into a global finite-element ocean model” by S. Skachko et al.

Anonymous Referee #2

Received and published: 30 July 2008

In this paper the authors present an estimation of the global ocean circulation for the period January 2004 till January 2005 using finite-element ocean model. In order to have a more accurate estimation they applied an adiabatic pressure correction and a sequential assimilation technique for assimilating altimeter data. The altimetry information is propagated into the interior of the water column using the first baroclinic mode of the displacement which permits the corrections of temperature and salinity fields accordingly to the sea surface height update. Generally it seems to me an interesting paper without major revisions. Nevertheless some results have to be more deeply investigated. I will be more precise in the "minor comment" section.

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Minor comment

1)The authors show large improvement in correcting the bias via adiabatic pressure correction. In my opinion this correction is useful for short numerical experiment since, as the authors explain, such correction reduce quite a lot the variability associated to the model. Such correction reduce much of the inter-annual variability. I think this is an important point to be stressed since in a framework of a longer analysis experiment, for example, such correction may hide important climatological signals.

2)I think it is not well explained why the authors choose only the first baroclinic mode. They refers to the work of Fukumori et al. (1999) where it is written that "In the tropics....wind-driven baroclinic changes are dominant, with the first baroclinic mode contributing most of the variance. Variabilities associated with high-frequency wind-driven barotropic motions are the largest sea level signal....". I would suggest to evaluate which is the contribution of the barotropic and some of the first baroclinic modes to the sea surface height in order to have an idea of how many modes are really necessary.

3)Pag. 259 line 24-27. Pag. 267 line 6-12. Pag. 269 line 20-25. In these two sections the authors say that most of the sub-optimality associated to the corrections are due to the discrepancy between the real baroclinic modes and the way in which they are computed (without considering the vertical shear). My impression is that this conclusion is not well demonstrated. It seems to me more an hypotheses of the sub-optimality rather than conclusion.

4)It is not very clear why in figure 3 (evolution of the Root Mean Square Error of SSH for the world ocean) the V1 and V2 model set up show the same error at day 0 of the assimilation experiment (January 2004). From the paper I understood that V1 is run without any correction for the spin up period while V2 is run with the adiabatic correction also during the spin up period. So the improvement of the adiabatic correction should be visible also at the beginning of simulation period (January 2004). Looking at the

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figure 3 it seems that at day 0 V1 and V2 have the same RMS error. However in figure 2 it is shown that at the end of the spin up period the SLA error associated to V2 is much less than the V1. These two results seem to me inconsistent with each other.

5) In figure 3 I think that the large increase of RMS error in V1 at the day 10 should be commented in the paper.

6) Again in figure 3 the difference between the assimilation estimates and the forecast tends to increase. This is not a good result for an assimilation scheme since it could mean that the information inserted into the model is rejected by the model itself. That means that the system is not able to propagate the information carried on by the observation in the future. There could be other possible explanations for example that the error associated to the observation is too small or that the model is so much non-linear that even if it starts from a quite correct initial condition (analysis fields) the error growth rate is so big that after 10 days (next assimilation time step) it has forgot almost all the information.

Technical details

1) Eden and Greatbatch (2003) reference is wrong. I think it should be 2004 and there is one more coauthor: Boning.

2) Pag 266. line 12 "...error variance are generally 5 cm lower than..." Variance has quadratic dimension. Please correct variance with standard deviation.

3) The caption of figure 6 is wrong but it is correct the description of it in the paper

Interactive comment on Ocean Sci. Discuss., 5, 255, 2008.

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