Ocean Sci. Discuss., 5, S151–S164, 2008
www.ocean-sci-discuss.net/5/S151/2008/

Ocean Science Discussions

OSD

5, S151–S164, 2008

Interactive Comment

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

S. Skachko et al.

Received and published: 11 September 2008

Answers to the Referee comments

We would like to thank the referees for their relevant and constructive remarks. They were carefully considered in our revision. Please find below some details on how the specific points raised by the reviewers have been considered. Referee comments are in bold type, answers are in italic type.

Answers to the Anonymous Referee 1

I find this article interesting, because the authors present a sequential assimilation method that takes into account the double problematic of the correction of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the variability of the dynamic topography and of the mean sea surface state produced by the model. However, I have several comments which I think should be considered before the acceptation of the manuscript for the publication in Ocean Sciences. I so propose the manuscript for publication after a revision. Specific comments: 1: I think the authors should better precise the meaning of SSH as used in the text. Actually this term is used by different scientist communities to design different quantities. When referring to altimeter measurement, SSH means the sea surface height referred to the reference ellipsoid. For modelers, SSH is used to refer to the dynamical topography given by the model. I think it is the last definition the authors gave to SSH. However as they started the article using "Dynamic Topography" and "MDT" terms, this can lead to confusion.

We agree with the reviewer comment. What we mean by SSH is the time dependent dynamic topography, now defined in the text.

2: In part 3: a brief description of altimeter data treatments should be added.

Done (Section 'Comparison of model results with observations', first two paragraphs).

3: In part 3: the geoid used need to be precised.

Done (Section 'Comparison of model results with observations', first paragraph).

Moreover, the authors should discuss the method that consists in combining altimeter SSH measurement with a geoid to obtain dynamical topography. Actually, short-length precision of geoids is often too low to accurately combine it with altimeter measurement. As an example, Rio and al. ("From the altimetric sea level measurement to the ocean absolute dynamic to- pography: Mean Sea Surface, Geoid, Mean Dynamic Topography, a three-component challenge",15 years of progress in Radar Altimetry Symposium, Venice 2006) showed that a MDT can be estimated with sufficient accuracy (5 to10 cm rms) at spatial scales down to 300-400 km. As a consequence a more important error is suspected to

be in- troduced in dynamical topography computed for this study with 1.59702; resolution. Geoid precision is still a limitation for high resolution dynamic topography construction from altimeter measurement. This should be addressed in the article.

Done (Section 'Comparison of model results with observations', first two paragraphs).

4: In some parts of the ocean, the authors used MDT from Rio et al. This MDT refers to a 7-year period [1993,1999]. This should be precised since some inconsistencies with the 1-year [2004] MDT computed by the authors might be introduced.

Done (Section 'Comparison of model results with observations', 4th paragraph).

5: Climatologic field used in the adiabatic correction need to be precised.

Done (Section 'Ocean model', the last paragraph).

Technical correction: Fig 6 : The legend does not correspond to the figure.

Corrected.

Answers to the 'minor and technical comment' , Anonymous Referee 2

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 informa- tion 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 interest- ing paper without major revisions. Nevertheless some results have to be more deeply investigated. I will be more precise in the "minor comment" section. Minor comment 1)The authors show

OSD

5, S151–S164, 2008

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.

We agree with the reviewer comment. To prevent the reduction of inter-annual variability it is also of interest to try more sophisticated correction methods such as for example the already mentioned approach by M.J. Bell, M.J. Martin, and N.K. Nichols (2004). This is now stated in the text.

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 evalu- ate 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 neces- sary.

We agree with the referee that it is of interest to evaluate how many modes are necessary to provide the best analysis quality. This was carried out for our experiments. We found that the first mode gives better results then superposition of the first two, five and all the possible modes. We added this information to the text (Conclusions, second paragraph).

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 then conclusion.

The suboptimality of the approach used by us readily follows from the fact that the vertical modes of ocean variability are obtained in the approximation which neglects the horizontal stratification (vertical velocity shear). The easiest way to see it is to recall that in rigid-lid approximation and with quasi-geostrophic scaling, the temperature (or salinity) perturbation in each geostrophic mode goes to zero at the upper surface, so that surface temperature is not disturbed. Thus in this limit perturbations in the surface pressure are not linked with perturbations of the surface temperature which obviously contradicts observations. If displacements of the free surface are taken into account, the situation changes and non-zero perturbations in surface temperature accompany the perturbations of surface elevation. Yet still modes are missing an essential part of the temperature variability linked to the presence of horizontal stratification (vertical velocity shear). Indeed, in the presence of the horizontal stratification any vertical displacement of an isopycnal causes surface temperature perturbations.

In practice, computing true modes is difficult as problem is non-separable and vertical structure of the modes becomes a function of horizontal wave numbers. This is the reason why they are not employed here. The difference between the true modes and the modes used by us is believed to be the reason of suboptimality.

Surely, one may pursue a statistical approach deriving covariances from the model output. However, the statistics of the coarse model miss an essential part of variability present in observational fields. The major issue is the dominance of the seasonal cycle in the model variability, while in the real world there is strong signal on shorter time scales due to transient features such as eddies.

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

OSD

5, S151–S164, 2008

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 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.

We started all the assimilation experiments from the initial state corresponding to the spin up with the V_2 *model:* V_1 *model which was spun up was re-initialized at the initial time of our experiments with the initial condition of* V_2*. The initial state obtained via 10-year spin-up of the* V_1 *model does not have the same error at day zero, this error is almost two times larger than the* V_2 *initial state error. As we can see from figure 3, the* V_1 *moves rapidly from the initial state approaching its own model trajectory. We updated the text (Hindcast experiment section, 3rd sentence, and the corresponding figure caption).*

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.

As we stated above, the V_1 *model starts from the state corresponding to the* V_2*. The increase of error can be explained by the fact that the* V_1 *without any correction tends to its own model trajectory.*

6)Again in figure 3 the difference between the assimilation estimates and the forecast tends to increase. This is not a good results 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 ob- servation 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.

The referee is right about the difference between the analysis and model forecast states. However, this increase is mainly inspired by the adaptivity of the filter. As we can see from the figure, the analysis error is significantly reduced during the experiment period. The model does not respond to these improvements in an optimal way because of the reason mentioned in the article. Namely, the suboptimal update of temperature and salinity fields leads to the fact that the model state is not dynamically balanced. The model then diverges from the observed state to recover a new dynamical balance. That is why we stated in the article that we managed to get only a partial success in such a complex problem of assimilating the entire dynamical topography signal.

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

Corrected.

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

Corrected.

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

Corrected.

Answers to the 'Review of Skachko et al.' by Srdjan Dobricic

We are happy to see these comments here because they indeed transform the reviewing process in a discussion, something, as we believe, very valuable for scientific

OSD

5, S151–S164, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

papers. We are indepted very much for the comments. We present answers to these comments and also description of changes made in the revised version of the paper.

In this Ocean Science Discussion paper the authors describe the assimilation of SLA observations into a finite-element global model.

Please note that we do not assimilate SLA (sea level anomalies) in this work. We deal with a more complex problem, i.e., assimilation of the absolute signal, dynamical topography.

I think that this paper is interesting for the publication in Ocean Science. However, there are several comments that should be addressed by the authors before its acceptance. I also think that it is necessary to make another numerical experiment (Comment 16).

Comments: 1. Page 259, line 10. If I understand correctly, the method consists in nudging the velocity field towards the velocity which is in balance with climatology of temperature and salinity. Although this method may reduce the model drift from the climatology, it could introduce other types of systematic errors present in the objective analysis of climatological temperature and salinity. In addition, this method constrains the surface elevation gradients towards the objective analyses of the climatology.

The adiabatic pressure correction method does not do nudging of the velocity field in a strict sense. It replaces consistent pressure in the momentum equation with a linear combination of consistent pressure and pressure derived from the climatology. In this way the velocity field is indeed enforced toward that based on climatology, but not fully, as there is always contribution from the consistent pressure. Since the elevation field is driven by the divergence of the horizontal velocity field, it also contains a part 'based on climatology'. We have found that this technique prevents the model state from drift and thus facilitates using the sequential filter. It would not be needed if the drift were less significant. The reviewer is right in arguing that the use of the adiabatic

OSD

5, S151–S164, 2008

method entails other errors, as it modifies the wave relaxation processes (details could be found in the original paper). We mention in the paper that the use of this method is one of possible reasons for the suboptimality of the entire approach employed by us. Indeed, the link between fluctuations of elevation and tracers is affected by applying the adiabatic correction method.

All this said, in practical terms using the adiabatic correction method proves to be helpful despite its all obvious limitations.

2. Page 260, line 24. I think that version V2 is already a suboptimal data assimilation system. It assimilates information from objective analyses of temperature and salinity using an improvised algorithm.

We are not inclined to consider version V_2 as a data assimilation system because both our runs V_1 and V_2 are started from the climatology as an initial condition. The model V_1 noticeably drifts from the climatology (which can be explained by a variety of reasons such as forcing and model topography). Applying the adiabatic pressure correction strongly suppressed the drift, but it does not serve data assimilation directly.

3. Page 261, Line 20. It is not explained how the MDT substituted the geoid.

Added to the manuscript. We mention that it is MDT of DGFI which is replaced by MDT of Rio05, not the geoid.

In order to prevent possible discontinuity appearing at the boundary of the two areas we applied a filter which smoothly extrapolates our measured dynamic topography towards that of RIO05 in the transition zone.

4. Pages 260-261. Is the model uncompressible? How is the steric effect present in the data set treated?

The ocean circulation model used by us is based on the Boussinesq approximation and in this way conserves the volume instead of mass. Thus there is always discrepancy at conceptual level between the model surface elevation and sea surface height

OSD

5, S151–S164, 2008

provided in data. However, as explained in a paper by Greatbatch (JGR, 99, 12767–12771,1994), the effect is basin-scale (if appropriate time scales are considered) and much smaller than the effect from an uncertain freshwater flux. Accordingly, we believe that omission of mass conservation is not the major issue in our case. The mass conservation can be partly recovered by adding density tendency term to the right-hand side of the vertically integrated Boussinesq continuity equation

$$\partial_t \eta + \nabla \int_{-H}^{\eta} u dz = - \int_{-H}^{\eta} \frac{d\rho}{dt} \frac{dz}{\rho}. \tag{1}$$

This, together with employing more realistic forcing is planned for future work.

5. Page 262, lines 10. I do not agree that only or even mostly the errors in bottom topography cause the model bias.

The reviewer is perhaps right, and along with topography many other issues can be responsible for the observed behavior. An apparent issue is the lack of realistic forcing and the absence of coupling with an ice model. In this way we cannot exclude that much finer tuning of the model is required to minimize the discrepancy between the model and data mean SSH fields, and that bottom topography is an important, but only one of many key features.

We changed the text of manuscript in agreement with this statement (6th paragraph in the section 'Comparison of model results with observations').

6. Page 262, line 13. I also do not agree that a long term mean difference prohibits the application of the sequential data assimilation. If observations are available frequently short model simulations will not have large biases.

We only meant to say that the amplitude of the difference between the mean elevation in the model and the observations is so strong that it cannot be repared by simply local adjustments of temperature and salinity with reasonable amplitude (via steric height change). We agree that in the general case more frequent assimilation means smaller

OSD

5, S151–S164, 2008

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

analysis increments. However, there are several problems in this direction. First, there is a lack of good quality observations with global coverage for more frequent assimilation. Second, large-scale ocean dynamics are slow, and sufficiently low intervals between subsequent data assimilation steps are needed to allow the model to approach its 'slow manifold' after every analysis. Third, temperature and salinity data in the volume of the oean are needed to faithfully correct the model state. This is still far beyond our abilities.

7. Page 262, line 25; Page 263, line 10. Again, I think that this is a kind of data assimilation which obviously reduces the model drift.

Please see response to comment 2. above.

8. Page 263, line 15. I think that the main reason for the reduction of the variability is the nudging of the velocity field towards the climatology.

This has been shown in Fig. 1. As we stated in the text, the price paid for the reduction of the systematical error is that it simultaneously reduces the variability of V_2 model. The physical reason of this effect is also clear — by correcting pressure one affects (reduces) the amplitude and phase speed of baroclinic Rossby waves and in this way a certain part of variability.

9. Page 263, line 27. What are the benefits of the V1 model?

There are no benefits. It serves mostly as an illustration to the fact that using adiabatic pressure correction is helpful.

10. Page 264, line 6 and line 20. Is there an ensemble of model forecasts or these are general statements about the possible application of the filter? If there is an ensemble it should be described with more details.

The SEIK filter is based on ensemble of model states. In our simulations we used an ensemble of 8 EOFs. The eight EOFs represent more than 90 percent of the variability. This covariance matrix is a consistent estimator of the 10-day model error covariance,

OSD
5, S151–S164, 2008

and is adequate for parameterizing the filter background error covariance. Please note that the SEIK filter updates the backgroud error covariance as it calculates the (low rank approximation of the) forecasted model error covariance matrix. We updated the text accordingly (Assimilation scheme section, the last paragraph, the third sentence from the end is added).

11. Page 264, line 18. Considering that observations are interpolated onto a grid before the assimilation, what justifies the diagonal form of the matrix R? How is the value 5cm chosen? Is it the square root of diagonal values?

Considering the model performance the largest contribution to matrix R is associated with model (representativeness) and probably not data error in satelite altimetry or the geoid used. The diagonal nature of R is by convenience only. Note however, that the assimilation increment (change in model state) is correlated due to the presence of the forecasted error covariance matrix. R has value of $25cm^2$ *along the diagonal. We updated the text (Assimilation scheme section. first paragraph).*

12. Page 264, line 25. On a grid with the horizontal resolution of 1.5 degrees such a small horizontal radius of influence practically decorelates the correction on each model point from all surrounding points!

This value was chosen due to preliminary experiments with different subdomains. The experiment with the radius value of 200km showed better results. In practice, except for high latitudes, it indeed corresponds to taking into account only all the nearest neighbours. We added text to the manuscipt to explain the choice of 200 km (Assimilation scheme section. first paragraph).

13. Page 265, last paragraph. Again I do not understand whether the background error covariance matrix is constant in time or it is evolved by an ensemble of model forecasts starting from the perturbed analyses.

The background error covarience matrix is evolved in time. In the SEIK algorithm,

OSD

5, S151–S164, 2008

Interactive Comment

the forecast field is computed as an average over the ensemble members, and the forecast error covariance matrix is obtained as the corresponding covariance matrix from the ensemble.

14. Page 267 and Fig. 3. It is very strange that initially the discrepancy between analyses and observations grows (red dots). What is the reason for this?

15 Page 267 and Fig. 3. Corrections (red dotted lines) grow during the assimilation. How is it possible when at the same time background states (blue dots) become more accurate?

Answer to 14 and 15. The initial background covariance matrix is imperfect. However, its evolution in time leads to more appropriate error representation which is evidenced by monotonic decrease of the analysis error as the time goes on. On the other hand, the improved initial states (red dots) of the V_3 analyses lead to improved V_3 forecasts. Their quality is then always better than the quality of the V_2 forecasts. At the first analysis (day 10) the fit to observations is better than that of the following analysis steps. The price for the good fit is that innovation is propagated via the imperfect error covariance to the full model state which reacts accordingly by deviating from a balanced solution. Later the covariance becomes more 'educated' and leads to increments that are better sustained. We introduced this into the Hindcast experiment section, 4th paragraph.

16. Section 5. I think that an experiment with the assimilation of SLA data and without the nudging to the climatology is necessary to fully understand the impact of SLA observations (V1 + SLA observations).

This would be a much simpler (and traditionally performed) experiment. Yet it will miss the geoid contribution which is of interest to our future studies. That is why we continue to work with assimilating the absolute signal, i.e. the anomalies combined with the Earth geoid.

OSD

5, S151–S164, 2008

Interactive Comment

17. Fig. 5. I guess that "with respect to observations" should be removed.

Done.

18. Page 269, Line 20. I am sure that small errors in the calculation of the first baroclinic mode cannot explain the problems in the analyses. There are many better ways (both statistical and variational) to estimate the vertical correlation of background errors than the use of the first baroclinic mode. Some of them are even mentioned in the introduction.

In practice, the covariances between model elevation and tracer fields are not without flaws, as coarse-resolution models miss an essential part of variability occuring in the real world. Most of their variability is due to seasonal cycle and does not reflect properly the presence of eddies or fluctuations of fronts linked to strong jet currents. That is why there is need for simpler, but more dynamically grounded algorithms. There is still no agreement on which one is most appropriate. We agree that variational techniques may be useful in solving such problems, and this is a possible direction of our future work. In this paper we explored possibilities for designing a computationally efficient method for assimilating dynamical topography. To this end we applied sequential filter.

We hope that we have given sufficient answers above and that the corresponding modifications of the paper make it now suitable for publication in 'Ocean Science'.

OSD

5, S151–S164, 2008

Interactive Comment