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Comment

Interactive comment on “Impact of combining GRACE and GOCE gravity data on ocean circulation estimates” by T. Janjić et al.

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We thank the reviewers for their comments and encouragement. We have performed a major revision of the manuscript. Specifically, we have fully re-written the introduction and conclusions. The body of the text has undergone careful editing. Several figures have been changed or removed. Comparisons with independent data have been clarified and strengthened. Here we will address the individual comments and answer the questions raised.

Reviewer 1

"A lot of good work has gone into this manuscript: a careful computation of altimetric profiles above two geoid models with several smoothing widths; assimilation of those

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data into a numerical ocean model using state of the art assimilation algorithms; comparison of the resulting model output with Argo data at 800m depth."

Thank you.

"Unfortunately one has to reread the manuscript several times, and scratch one's head to understand the contents. This is the consequence 1) English structure problems (the second author should have avoided many of these problems by editing the manuscript himself); 2) confused references (page 1537: Niiler et al 2003 is NOT an example of combining a geodetic and a traditional oceanographic estimates of a MDOT); 3) contradictions (page 1540 correctly states 'the geoid heights N must be computed from gravity field models estimated without any surface gravity ...' but page 1542 and figure 1 indicate that the pregeoid correction, the filtered difference between the EGM08 model that DID use in situ gravity and altimetry and one that did not is below a few millimeters; a minimal discussion of the contradiction is warranted); 4) unclear details of the data assimilation (the MOG2D correction has been applied; this removes barotropic signals from the altimetry but those signals are present in FEOM; how is the Kalman filter set up to avoid a conflict? is this not an issue?); 5) after going out of the way to compute altimeter PROFILES of the data, the assimilation is done using 10 day maps. Why? Why not daily assimilation along the profiles?; 6) suboptimal figures (figure 8 top and bottom look 99% the same; more informative would have been a bottom panel depicting the DIFFERENCE between forecast minus analysis; same with figures 14, differences from 13b would have been more informative)."

We apologize if we didn't do a very good job in presenting the material. We hope that the careful revision of the introduction and conclusion, changes to the figures, as well as the now extended discussion of the figures, clarify the details of our work. In particular we would like to emphasize that our goal was

a) to present the increase in information content in the data that is due to the higher resolution of the geoid model (this is only possible due to the new GOCE gravity satellite);

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b) to show how we are dealing with the increase in the data information content in our data assimilation algorithm; c) to illustrate the effect of an increase in data resolution on the results of data assimilation for the surface data; d) and finally, to show that by assimilating only absolute dynamical topography data using an ensemble Kalman filter, we are able to obtain improvements for temperature at 800 m depth in the Weddell Sea area. This area is not well observed and plays an important role due to the impact of circumpolar bottom water production on global deep sea circulation.

In our approach we use profile filtering to obtain absolute dynamical topography data for the same time period as the assimilation period. In this way we avoid at least two inconsistencies: a) the SLA and the mean DOT are not calculated or assimilated separately, b) all the simplifying assumptions that go into the construction of the mean DOT are avoided (for example Ekman corrections to incorporate drifter data). Although using along-track data would be straightforward and requires no major changes to the Kalman filtering, we decided to use maps of fully referenced dynamic topography instead. This is in line with our previous work and allows us to compare the results. Also, our work is focused on newly available data resolution. A problem with using along-track data would be also the temporal resolution, where even after filtering, more variability of the observations are captured than in the 'time centered' maps. Again, we considered only the differences due to the changes from GRACE to GOCE data as well as the ability of the data assimilation scheme to propagate information into data sparse areas and into the depth of 800 m.

We have included a short discussion on the fact that in Eq. (4), differences to the hybrid model EGM2008 are performed although with Eq.(1) it was emphasized that for the DOT estimate geoids from satellite-only gravity field should be used.

Mog2d corrections were applied to the data. This we believe is justified. The correction consists of the removal of scales shorter than 20 days, and of the inverted barometer effect correction. Our model does not take into account changes in atmospheric pressure and therefore we used the inverted barometer correction. Again, as we con-

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centrate on the spatial resolution of GRACE and GOCE geoids, the set-up of the model was chosen as slowly varying in time, e.g., interpolated monthly forcing fields were applied. The model deliberately does not reproduce short timescales of the dynamic topography. Primarily, filtered forcing as well as implicit scheme for calculation of SSH makes the use of the Mog2d for the altimetry data necessary.

"A few examples that actually confuse the meaning: filtered up to degree half width of 241km' (p 1543 and many others): replace 'degree' by 'a'. "

We apologize, corrected. Thank you.

""Subantartic, Polar and ACC fronts' (p 1546, Figure 12 caption and others): you probably mean Subantartic, Polar and SOUTHERN ACC front, there is no 'ACC front' In Fig 14... 'for four SIMULATIONS': SIMULATION usually means without assimilation, but the figure (correctly) shows one simulation and three assimilation results. "

"Simulations" were replaced by "experiments" in order to avoid confusion and southern ACC is corrected.

"A few examples that simply distract: -'on the other side' -> 'on the other hand' (p 1537) 'averting' -> 'preventing' (p 1538)"

Corrected.

"Other issues: -p1537: Niiler et al 2003 used drifters and wind data only to construct their MDOT. Maximenko et al and Rio et al did combinations of geodetic and in situ. "

Done.

"p 1542: explain why you earlier emphasized that the geoid should not contain in situ gravity derived from altimetry over the oceans (correct statement) but the EGM08 'geoid' contains just that, in situ data, and the 'pre-geoid correction' is negligible over the oceans. This requires some discussion."

We did not state that the pre-geoid has been neglected. It is small for a filter length of

241 km but becomes larger with a shorter low pass filter (Fig. 2). However, the pre-geoid correction is essential as it compensates the effect of subtracting a hybrid geoid in the 1D filter operation of Eq. (4). As already mentioned above, we have included a short discussion on this.

"p1543: indicate in the caption to Fig 3 which 10 day is used, rather than in the body of the text."

Done.

"p1547 and Fig 12: the comparison to the Albertella et al surface geostrophic velocities is confusing. Who should we believe? The ALbertella velocities seem sharper, why does this complicated process of data assimilation not produce results as good as ALbertella's? This requires some interpretation, which is lacking."

Figure deleted.

"Fig 4: I would recommend one MDOT and the other 3 the difference from the first one. Fig 8L: I would recommend one MDOT and one difference from the first one. Fig 14: same, one T and V and the others differences from the first one."

We hope that the current choice of figures makes things clearer.

Reviewer 2

"This paper examines assimilating finer resolution dynamic ocean topography (DOT) maps from a combination of altimetry and a GRACE/GOCE geoid into an ocean model and evaluating whether there is an improvement. The discussion of the mapping of the DOT is fine, although I believe there are more plots than are really necessary to get the point across. My major criticism is that the analysis of whether there is an actual improvement in the model is very weak and not very quantitative. Most of the discussion is comparing figures and stating there are differences, when frankly, I cannot see any based on the way the data are plotted. "

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We modified the figures as suggested by the reviewers and added additional explanations to them. We hope that the careful revision of the introduction and conclusion, changes to the figures, as well as extended discussion of the figures, clarifies our work.

"The only quantitative measures that there is an improvement is that the assimilated runs agree closer with the data that was assimilated (which is not a convincing argument) and that statistics compared to independent profile data/velocities are closer by a tiny amount (0.1 .C, 0.1 cm/sec); since there is no effort made to quantify the uncertainty on the profile temperature/velocity maps, I Based on this, I cannot recommend the paper for publication as it is written, and suggest a major revision. I have made specific comments on suggested changes below. "

The goal of this work was to show the differences in the data assimilation results due to the increase in resolution of the data. The impact on the surface variables seems to follow well the increase in the resolution of the data. The impact on subsurface variables is illustrated with temperature at 800 depth in an area that is not observed well. The impact on the temperature at 800 m is still there (maybe better seen now with the different choice of figures). In particular the RMS error north of 60oS is reduced by 50 percent. We also added the comparison with the ARGO data on different depths and additional discussion on comparison with the front locations obtained from hydrographic data.

"I would recommend that the number of figures in the first part (Figures 1 – 9) be reduced, keeping only the essential ones to describing the filter and changes due to smoothing, and more plots added to the analysis section. Also, more significant, statistical comparisons need to be made, as I have suggested in the comments section."

We modified the figures.

"Need to discuss other earlier attempts to assimilate GRACE/altimetry mean DOT into ocean circulation models, such as

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Stammer, Detlef, Armin Köhl, Carl Wunsch, 2007: Impact of Accurate Geoid Fields on Estimates of the Ocean Circulation. J. Atmos. Oceanic Technol., 24, 1464–1478.

Although it is referred to later, it is not clear it is in the context of assimilating DOT into a model. As the introduction is written right now, a reader might think this was the first study to attempt this. "

We have rewritten the introduction.

"2. Also in the introduction, should list some references for examples of the geodetic DOT as well as the hydrographic method, in addition to the reference from a combination that is already given (Niiler et al., 2003)."

We have rewritten the introduction.

"3. Page 3. "The separation of the mean DOT and its temporal variation was introduced by Wenzel and Schroeter (1995) as an answer to the highly accurate repeat altimetry and the low accuracy of the geoid at that time." Again, this suggests Wenzel and Schroeter were the first to do this in the data before 1995, but it was a common practice for many years before that, although not in a model assimilation. In fact, early TOPEX GDRs had a model of the mean DOT on them so that this could be removed. I suggest revising the sentence to read:

'The separation of the mean DOT and its temporal variation was introduced by Wenzel and Schroeter (1995) in assimilating DOT into a model as an answer to using the highly accurate repeat altimetry and the low accuracy of the geoid at that time.'

The sentence is revised accordingly.

"4. Page 4. Problems with altimetry in the Southern Ocean. Probably a bigger problem with altimetry in the Southern Ocean is the sea state bias, which is not mentioned. Stammer et al. (2007) mention this in passing (referencing Chelton et al., 2001) as errors related to wind and wave observations), but the problem is not due to errors in the observations, but the size of the waves and winds, which can bias the SSH away

from the real value by several cm. This will affect the mean DOT and the variable portion differently. At least a few sentences should be added here to discuss this problem."

The sea state bias is indeed a major remaining problem in the calculation of a mean altimeter surface. High seas in the Southern Ocean are a large scale phenomenon and we believe unsatisfactory correction of the sea state bias will not produce strong shifts in the gradients of the dynamic topography. A caveat is the potential aliasing of strong wind events into the 10-day maps which may change temporal shifts in frontal positions. However, across-frontal differences in dynamic topography usually amount to somewhat higher values than what we expect from uncertainties in the sea state bias corrections. In conclusion, we think frontal locations may be displaced temporarily (only slightly in the mean) but they will not be fully relocated. We added the following discussion in the text:

'In addition, radar altimetry suffers from systematic errors in estimating the sea state bias, an effect causing elongated radar distances in the presence of waves (Stammer et al. (2007) ,Chelton et al., 2001). This is particularly critical for the Southern Oceans with rather strong winds and large waves.'

"5. Page 12, figure 9. There are also significant differences in the tropical oceans and the Mediterranean Sea, where the assimilated analysis is closer to the observations than the forecast model. Overall, this is really a misleading figure, since assimilation should cause the analysis run to be closer to the data. I think it would be more interesting to present this as a difference between the forecast and the assimilated run; i.e., show where the assimilation makes the largest difference. It would be interesting to do this in a mean sense and in the variable (RMS sense). So, I would suggest current Figures 8 and 9 are unnecessary, and could be replaced by a single figure showing the mean difference between the runs and the RMS difference. Later on, you will show using the Argo data whether the assimilated run is an improvement or not."

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We followed the reviewer's suggestions here and removed Figure 9. Figure 8 is modified as suggested.

"6. Figures 11 and 12. I don't see the need to keep adding the 97 km filtering plots in addition to the 241 and 121 km. You have already demonstrated multiple times, most clearly in Figure 10, that there is not a significant difference between the 121 km and 97 km runs. Adding the extra case clutters the figures and analysis and makes all harder to read. Also, frankly, I cannot tell the difference between the plots with the color bars used. I definitely don't see: "Oceanic front lines are much better seen when modifying the half width of 241 to 121 km. This holds especially in South Atlantic where the turning of the Subantartic front now coincides with the estimates of location of this"

Figure 12 is removed and we modified figures in this section in order to avoid cluttering of the figures.

"7. Figure 12. Why compare a surface geostrophic velocity from altimetry/GOCE with 50 m depth current from the model? These would not be expected to be the same in the first place. Also, again, I really do not see any significant difference between the 241 km and 121 km case. A difference plot would better show the difference."

Figure removed.

"8. Figures 13 and 14. These are the best validation of the assimilation and length-scale of 121 km, but the analysis is weak. Table 2 presents an RMS of the differences for the global maps, but the differences are small (~ 0.1 °C or 0.1 cm/sec). Are these differences really significant? A map of the differences between the data plotted in Figure 13 and 14 might show more significant differences, especially the low SST at 10E, 60S that only appears when the data are assimilated. It is still not clear to me, though that the 121 km assimilation is any better. In Table 2, you list the differences north of 60S, but to my eye, the more significant changes are between 65 and 60S."

With these figures we wanted to illustrate the impact on subsurface variables due to the

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changes in the resolution of the DOT surface data. The impact on subsurface variables is illustrated by comparing the ARGO temperature at 800 depth with our results in Weddell Sea. The impact on temperature at 800 m is still there (maybe better seen now with the different choice of figures). In particular the RMS error north of 60oS was reduced by 50 percent.

"9. Conclusions are weak. Tell me exactly what you found and why this is significant. Based on my reading of the paper as is, the assimilation at 121 changed the forecast model slightly, but based on the statistics and analysis presented, I am not convinced the change is a significant improvement."

Conclusions are revised.

Interactive comment on Ocean Sci. Discuss., 8, 1535, 2011.

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