

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

Anonymous Referee #2

Received and published: 11 October 2011

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. 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

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have no idea whether the improvement is significant or not.

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. 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.

Specific Comments

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

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.

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).

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

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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.”

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.

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.

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 Subantarctic front now coincides with the estimates of location of this

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front by Orsi et al.” I suggest using difference plots instead (assimilated – forecast), or at least adding this, and showing differences are along fronts.

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.

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 60°S, but to my eye, the more significant changes are between 65 and 60S.

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.

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