**Ocean Science
Discussions**

Interactive comment on "An ocean modelling and assimilation guide to using GOCE geoid products" *by* K. Haines et al.

Anonymous Referee #1

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osd-7-1849-2010 review An Ocean Modelling and Assimilation guide to using GOCE geoid products K. Haines, J. Johannessen, P. Knudsen, D. Lea, and M-H. Rio

General comment

This paper is an overview (a guide, as stated by the title) of what should be done for using GOCE geoid products for assimilating satellite altimeter data into an ocean model. It presents a lot of details about these data (geoid and altimeter) and how they are processed and warn about many pitfalls that should be avoided. A lot of important remarks and details are given. This paper is would be very useful for anyone willing to assimilate such product. At the end of the paper (section 5) different aspects of MDT assimilation are presented with three different operational forecasting systems. The first one, with the Mercator-Ocean system shows a significant improvement on the

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quality of the analysis when using an observed MDT compared to the model one. The second one, with the Met Office (FOAM) system illustrates the importance of dealing with observation biases with such kind of data. Third example is done with TOPAZ and emphasise the fact that the impact of GOCE data might be regionally dependent. From this last example several questions arise. The experiment is done in a presence of a large offset that, as mentioned earlier in the paper (p1865 l6-9), could be removed prior to assimilation. Why is it not done here? Is there an equivalent offset in the other two example. How the other data (here SST and ice concentration, but in-situ for the FOAM case) influence the outcome of the experiment? In the presence of an unrealistic offset T and S data may prevent the system to do unrealistic adjustment.

Section 5.4 is less convincing than the rest of the paper. It seems difficult to draw conclusions from a model-data comparison when model and data are not relative to the same period of time. Are the results really significant? Compared to the interannual variability of such quantities? I am not quite sure this last section is of importance for the purpose of this paper anyway.

I found the figures generally a bit too small and of poor quality (i.e. resulting from the conversion from one format to another)

Once the few questions above answered and the matter of the figures resolved, I find this paper fit for publication and an interesting contribution to the field.

Minor issues:

- p 1855, l 27: Study (capital S) - p 1857, eq(2): i in superscript or subscript ? - p 1857, l 20-21: i instead of f in subscript. what does 'i' stand for ? 'f' is for 'filtered' I guess. - p1865, l 6-9: This may prove difficult. In some case it is difficult to estimate what is the 'zero' level on the considered domain. Please comment on that. - p1865, l 25: what is the procedure to calibrate the MDT to reduce the mismatch in SST ? I understand that there is a link, but it does not seem that trivial. - p 1871, l 8-18: This paragraph seems to be completely out of place It should be merged into section 6

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