

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

### **Anonymous Referee #1**

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<http://www.ocean-sci-discuss.net/8/1535/2011/osd-8-1535-2011-discussion.html>

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 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.

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

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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).

As a consequence, I cannot recommend publication in the present form.

#### Details:

I cannot nor is it my job to edit this manuscript for language structure and grammar (I am a non-native English speaker, I understand the problem but it is up to the authors to fix). 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'. - '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.

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

Other issues: - p1537: Niiler et al 2003 used drifters and wind data only to construct

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their MDOT. Maximenko et al and Rio et al did combinations of geodetic and in situ.

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.

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

p1547 and Fig 12: the comparison to the Al bertella 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.

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.

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