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Interactive comment on "Empirical estimation of present-day Antarctic glacial isostatic adjustment and ice mass change" *by* B. C. Gunter et al.

Anonymous Referee #1

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General comments

I strongly recommend this article for publication. Since Wahr and Wingham proposed the combination of altimetry and gravimetry to derive Antarctic Glacial Isostatic Adjustment rates in 2000, there has been one publication only (by Riva, the main author and others in 2009) actually implementing this approach. In the manuscript submitted, the authors have extensively revised the methods and expanded the data sources used in 2009 and are thus able to provide a valuable contribution to our current understanding of GIA rates in Antarctica. As pointed out by Shepherd et al. (2012), GIA introduces considerable uncertainty in estimating ice sheet mass balance. Models of Antarctic GIA display large variation. It is therefore crucial to estimate Antarctic GIA empirically if possible in order to constrain GIA rates and improve estimates of Antarctic Ice Sheet

mass balance.

Specific comments

Page 3498 Line 3: "The results improve. . ." – the wording "over a longer period of time" is misleading as the authors are still constrained by the ICESat operational period of 2003 – 2009. Whilst it is correct that data from four additional ICESat campaigns has been incorporated, this only extends the time period by approximately three months. So the statement that the time period is "longer" is slightly misleading.

Line 22: "Over the past decade. . ." – there has in fact not been general consensus about the amount of mass loss the AIS has been experiencing, which is why ESA and NASA initiated the IMBIE project. For the same reason, the manuscript is a valuable addition to the knowledge of the glaciological and geodesy communities.

Page 3499 Line 29: "This is possible. . ." – the altimetry products are not more sensitive to the volume changes associated with ice mass changes. They are almost equally sensitive to both (the difference between height changes caused by ice and by GIA is not that large) whereas it is GRACE which has a different sensitivity to ice and mantle rock material.

Page 3501 Line 25: "In short,. . ." – as mentioned above, the altimeter does not primarily observe surface processes. It observes a combination of surface and uplift processes.

Page 3503 Line 19: "Earlier studies. . ." – this K2 discussion is interesting but Figure 1 may be better suited for the supplementary material as this is not a GRACE technical publication (also, though existence of the K2 tide is verified, it does not affect the results)

Page 3504 Line 3: "For the unconstrained. . ." – applying a Swenson and Wahr style de-striping approach introduces new errors in the form of concentric circles (i.e. zonal patterns). These are cause by truncation of spherical harmonics (Gibbs phenomenon) and are visible in Figure 1 c) for instance, to the north of the Amundsen Sea Sector.

The polynomial destriping approach is deemed as unsuitable and an EOF approach for instance would be advised instead.

Page 3505 Line 10: “long-term surface height” – five to seven years of data cannot justifiably called long-term

Page 3507 Line 11: “Most of the uncertainty. . .” – Figure 4b also shows a large uncertainty in the Amundsen Sea Sector where there are no steep slopes and where sampling shouldn’t be poor. Comment.

Same line: what about the issue of clouds over the Antarctic Peninsula?

Page 3510: The assignment of different densities is not very clear and needs to be explained better. Figure 8 is difficult to understand. In Figure 8c it would appear that the Kamb Ice Stream is assigned a very low density (blue) when it the text it says that the density there should be that of ice (which is correct). Furthermore, if I understand correctly, areas where ICESat – FDM exceeds 6cm/yr, the density should be 917kg/mˆ3. This is the case in Terre Adelie for instance. However, Figure 8c seems to show densities lower than $\sim$500kg/mˆ3 in Terre Adelie. The colour scale used in Figure 8 is confusing and should be improved. Also provide information on surface densities used in the rest of Antarctica. In the ASE for instance, surface densities should be that of ice.

Page 3513 Line 13: “This mean value. . .” – by “all GIA values” (line 14) do the authors mean the empirical GIA values, i.e. those obtained from the combination of gravimetry and altimetry? It needs to be made clearer throughout the paper when the empirical GIA rates are referred to, for instance through the use of “GIA_emp” or similar.

Page 3514 Line 4: “Once the GIA. . .” – again, make it clear whether “GIA mass rates” are those obtained empirically

Page 3515 Line 6: “For the rock. . .” – the value of 100kg/mˆ3 seems arbitrary. Comment.

Page 3516 Line 13: “Examples include. . .” – how is the problem of elastic uplift dealt with in the GPS rates?

Page 3516 and 3517: The fact that different biases are computed for different data sets and methods makes it difficult for the reader to follow. How was a bias between GPS rates and modelled GIA estimated? And how was it computed for GPS and empirically derived GIA rates? It would be helpful for understanding if a clearer presentation of all the different biases (e.g. inter-campaign bias, biases based on the LPZ, biases between different data sets) was provided.

Page 3518 Line 9: “The same can be said. . .” – altimetry was not included over the ice shelves (page 3502, line 20). How were the empirical rates over the ice shelves derived (which are discussed in the comparison)?

Page 3521 Line 19: “The total GIA. . .” – do the authors refer to the total empirically derived GIA mass change?

Line 19 – 23: comment on the fact that the value for Antarctic Ice Sheet mass balance for the time frame 2003 – 2009 varies between 0Gt/yr (-100Gt/yr ice minus +100Gt/yr GIA) and -47Gt/yr (-100Gt/yr ice minus 53Gt/yr GIA)

Figure 2: this is essentially a figure of six almost identical plots. It might be more clear to plot 2a for instance and then plot five other plots which show the difference between 2a and the other five solutions shown. The same applies to Figure 9 and to a lesser extent to Figure 11.

Figure 9: “Estimated GIA rates” – do the authors mean empirically-derived GIA rates? If so, it should read “Estimated GIA vertical rates computed from the combination of altimetry and the following GRACE solutions:” as GRACE alone does not provide GIA rates

Figure 10: is the difference between Figure 10a and Figure 10b that one is given in units of uplift rate and the other is given in units of mass? If so, it would make it clearer

to use units of kg/mˆ2/yr for the mass rates rather than mm/yr (as it says next to the colour bar). Why are the patterns different for mass rates and uplift rates? Because of different densities applied?

Figure 11: this plot (though similar to the one used by Thomas et al. 2011) is very difficult to interpret. Visual examination seems to suggest that the different GPS locations do not agree very well with any of the GIA rates presented. Maybe there is a way in which the authors can plot the significance of the agreement between GPS rates and uplift rates? Figures 11a to 11c presumably refer to GIA rates derived from the different GRACE solutions and altimetry. Make this clearer.

Technical corrections

Page 3501 Line 15: “will assess” – use present tense

Line 21: “lower” – change to “less negative”

Page 3504 Line 23: Table 2 and table 1 need to be swapped (as table 2 is discussed first in the text, before table 1 which deals with ICESat – this comes later on in the text)

Page 3519 Line 21: “show” rather than “shows”

Acknowledgments: “Himanshu Save” rather than “Himansu Save”

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