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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 #3

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Dear Authors,

The manuscript "Empirical estimation of present-day Antarctic glacial isostatic adjustment and ice mass change" by Gunter et al. presents an estimate of glacial-isostatic adjustment (GIA) from GRACE gravity field trends and ICESat surface elevation trends, relying on a density estimate for present-day processes from an regional atmospheric climate model. The estimation approach is based on mass and volume conservation as presented in Riva et al. 2009, in which the concept is proven, and now improved by i) relying on a longer GRACE and ICESat time series and improved processing of the data, ii) involving an improved density estimate for surface-mass processes, and, iii) introducing assumptions on the GIA signal in central Antarctica. The paper lies timely in a series of updated GIA corrections for Antarctica, i.e. IJ05_R2 (Ivins et al.

2013), AGE1 (Sasgen et al. 2013) and W12 (Whitehouse et al. 2012), which follow different modelling-based and empirical approaches. The work represents a thorough and successful effort to derive a best GIA estimate and meaningful uncertainties from GRACE & ICESat. Therefore, I strongly recommend the paper for publication in the The Cryosphere. There are, however, two concerns related to assumptions made in the paper that I would like to see responded to in a revised version of the manuscript.

A1. Assumptions on negligible GIA and present-day ice-mass change signal in central Antarctica (use of LPZ)

A2. Effective density estimate for GIA

A1. This is my greatest concern regarding the study. The authors state the low-precipitation zone is used to remove biases of different origins, i.e. geocenter motion, reference frame, campaign biases in ICESat, etc. In this sense, the paper represents regional anomalies w.r.t. the chosen calibration area, both for the GIA and ice-mass balance fields derived, at least if their assumption of zero mass change and zero GIA uplift in the LPZ does not fully hold. This could be stated cleared in the paper.

P3507L15ff: Have the authors tried to apply the Centroid-Gaussian correction to the ICESat data presented in Borsa et al. 2013 (TCD). Even though calibrating the ICESat inter-campaign bias with the LPZ may be sufficiently accurate it would make a strong case for attributing an offset in to GRACE errors, Antarctic GIA or far-field mass variations and related geocenter motion.

P3513L17: Degree-1 does not only have a z-component over Antarctica. Therefore, a tilt between EA and WA will remain, causing a bias in the mass / GIA estimate between both regions. As different degree-1 trends show different tilts, I would encourage the authors to apply the estimate from Cheng et al. 2010 in addition to Swenson et al. 2008 (http://grace.jpl.nasa.gov/data/degree1/). This should be a fairly simple additional calculation.

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A2. Following Riva et al. 2009, the paper treats GIA as a surface mass process, relying on an effective rock density obtained from calculating the ratio between surface uplift and gravitational potential using an Earth model.

P3502L18ff: It is difficult to judge how accurate this relation is, considering the variety of different load and Earth structure models possible. How representative is the standard deviation of 100 kg / m^3 assigned to the rock density. Also, Riva et al. 2009 is a bit short on this; particularly, it needs clarification whether including the full sea-level equation also means including GIA caused by the water redistribution from the Northern Hemisphere. Please provide more details on this.

Some minor points.

B1. P3502L05: It is clear that the individual terms in the nominator of Eq. 1 are smoothed with a 400 km Gaussian filter. I assume this is also applies to the density fields in the denominator? Please clarify this.

B2. P3502L06: It is mentioned that a Gaussian smoothing of 400 km is applied. Later it is mentioned that a 200 km Gaussian filtered is applied to the unconstrained solution (P3504L28). Please clarify which filter is used in which context. And related to this: was the de-striping filter also applied to the altimetry / RACMO fields? This may be important since the filter slightly distorts the spatial pattern of the signal, which may create artefacts.

B3. P3503L14 A remark. To achieve more consistency between the ICESat and GRACE data sets, would it make sense to estimate the GRACE trends from the time epochs with ICESat data only? Could this change the results?

B4. P3504L03ff: AOD1B RL05 appears to have spurious trends, particularly over the shelf areas of Antarctica, http://www.gfz-potsdam.de/forschung/ueberblick/departments/department-1/erdsystem-modellierung/services/grace-de-aliasing-product-aod1b-rl05/known-issues-aod1b-

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rl05/ . It may be worthwhile to use RL05 without the ocean de-aliasing product removed.

B5. P3509L19: Please state whether you use a deriving \dot{m}_firn from RACMO2 requires the definition of a climatological reference period, likewise Fig. 8: what exactly is meant by GRACE-SMB. In general, it would be good to include a definition of SMB and add some details on the RACMO2 simulations.

B6. P3511L03: I understand that residual height changes exceeding the some combined ICESat & RACMO uncertainty threshold are attributed either to ice dynamics (917 kg/m^3) or underestimated snowfall (except for ice-dynamic thickening of the Kamb Ice Stream). Are the densities derived using the Kasper et al. (2004) approach shown somewhere? How sensitive are the results to the choice of the threshold 2*\sigma_h?

B7. P3511L20: This statement is certainly not correct. Firstly, Nield et al. 2013 present evidence for GIA-induced changes in the uplift rate along the AP (http://adsabs.harvard.edu/abs/2013EGUGA..15.9407N). Secondly, Rignot et al. 2008 mass budget estimates rely on discharge estimates for 2006 / 2000 subtracted from the long-term accumulation mean for 1980–2004. The associated elastic correction may simply be inaccurate for the specific GPS sites covering individual time periods, given the presence of interannual elastic and ice-dynamic effect. The authors could compute the elastic correction from the mass estimate for GPS stations with a temporal coverage of roughly 2003-2009 and compare it to the ones applied in Thomas et al. 2011 to get an estimate how important this is.

B8. P3513L13: Eq. (3) does not contain \dot{h}_GIA, but used \dot{h}_rock. Please make consistent.

B9. P3513L20: Please rephrase last sentence “The primary consequence. . .”. Specify what is meant by “global GIA effects, such as the contributions from the Northern Hemisphere.” The far-field sea-level influence? Geocenter motion? In my view calculating \dot{h_GIA}-<\dot{h}_GIA_over_LPZ> yields regional anomalies of GIA in Antarctica.

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Whether these are close to the full signal or not depends on the validity of the assumption <\dot{h}_GIA_over_LPZ> != 0.

B10. P3514L03 and Table 2: A 400 km buffer zone is used to account for the leakage due to smoothing when integrating the GIA mass estimate. I assume this is also applied to the equivalent mass change from GRACE. What about the omission errors in the GRACE ice-mass balance estimates, resulting from limiting the spectral range to spherical harmonic degree and order 2 to 60 and the smoothing operation? Has a calibration procedure been applied similar to Barletta et al. 2013?

B11. P3514L11: Again, GRACE ice-mass balance estimates represent anomalies w.r.t. to those that might occur in the LPZ. I think, reducing the bias in ICESat without the assumptions of LPZ (see A1) would make a much stronger case, also in the context of the IMBIE results, which significantly diverged in East Antarctica.

B12. P3516L25: Please provide additional details on the removal of the “bias offset” (maybe just call it “offset”) of the GIA predictions and the GPS network rates. Why were LPZ-bias term not removed from the GIA predictions (IJ05 etc.) prior to the removal of the “bias offset”? Was the GPS offset estimated based on uniform or non-uniform uncertainties of the uplift rates?

B13. P3520L18ff: Recently, a new Antarctica ice mass balance estimate was published by Sasgen et al. 2013, which yield -114 ± 23 Gt/yr (2003-2012), with a good agreement between also in EA 26 ± 13 Gt/yr. It relies on an independent estimate of GIA constrained by GPS uplift rates. As this supports your analysis and should be included in the paper.

B14. P3528, Table 2: Again, why isn’t a LPZ bias, or better offset, for the GIA estimates based on modelling (IJ05, W12, ICE-5G) presented. It would help to judge how strong the assumption of zero GIA over LPZ differs. For example, W12 does have subsidence in central Antarctica...

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B15. P3529, Table 3: Please indicate in the caption that the GPS-GIA rates misfit is after correcting for the LPZ bias.

B16. P3530, Table 4: Likewise, B13, please indicate that that the GIA estimates of the paper include a LPZ bias and offset correction, while the published ones (and the GPS data) only include an offset correction.

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