

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

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Received and published: 23 November 2013

We would first like to thank this referee for the thoughtful comments and suggestions provided, and we appreciate the time he/she spent in reviewing the manuscript. The points raised were very constructive, and will help strengthen the revision.

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

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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.

The earlier work by Riva et al used ICESat data spanning from March 2003 to March 2008, while the current study covers the timeframe March 2003 to October 2009. This extends the timeframe of the analysis by an additional 18 months (or a 30% increase). Even so, the authors agree that the original wording suggested a longer extension of the data sets. The wording will be changed to read "over a slightly longer period of time" to emphasize the extension is only a fractional change.

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.

While the total magnitude of the mass loss may be subject to debate, the literature of the past decade is clear that the general trend is negative, which is all we were alluding to here. This was also one of the main conclusions from the IMBIE study...that the overall trend is negative, but the uncertainties are large.

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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.

We agree this needs clarification. The actual sensitivity of ICESat is relatively constant, and is the same whether the instrument is observing bedrock or ice sheets. The intention was to highlight that surface height changes due to accumulation/ablation typically have much larger amplitudes than those due to GIA, making these signals easier to detect with ICESat (better signal-to-noise ratios). The text will be revised to remove the use of the word "sensitive."

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.

Yes, technically the height change is influenced by all surface and uplift processes, and this is accounted for in the derivation of Eqns 1 and 3; however, as was discussed in the previous comment, the signal-to-noise ratios for the surface processes are well above one for most regions, while the mm-level GIA uplift trends fall below the noise threshold of the ICESat measurements for many regions (compare Figs 10a, 7a, and 4b).

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)

We will consider this for the revision...the SM may indeed be a better place for Fig 1.

Page 3504 Line 3: "For the unconstrained..." – applying a Swenson and Wahr style de-stripping approach introduces new errors in the form of concentric circles (i.e. zonal

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patterns). These are caused 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 destripping approach is deemed as unsuitable and an EOF approach for instance would be advised instead.

We recognize that the use of Gaussian smoothing, along with de-stripping, is not an optimal approach and can lead to the unintentional attenuation of signal. That said, this technique is still widely used by non-experts when working with GRACE data, due to its relatively easy implementation. Furthermore, many prior GRACE-based mass change studies made use of this approach, so we thought it useful to include this approach in the analysis. The technique's deficiencies are also why we examined a range of other GRACE solutions that made use of more specialized filters. We tried to include those solutions that were publicly available so that the results could be reproducible. We're not aware of any publicly available EOF-filtered GRACE solutions, so they were not included; however, we would expect them to perform similarly to the filtered solutions that were examined.

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

Any reference to long or short term is subjective, so to clarify this in the revision, we intend to better quantify this in the text as "long-term (multi-year) surface height".

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?

These areas are known to be very dynamic, with high rates of accumulation (see Fig 7), and associated cloud cover. This, combined with some steep topography near the coastline, is likely the cause for the higher ICESat uncertainties in the ASE. We will note this in the revision.

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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/m³. This is the case in Terre Adelie for instance. However, Figure 8c seems to show densities lower than ~500kg/m³ 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.

Figure 8 was mainly intended to illustrate the reasoning behind our decision to assign surface density values to the height differences seen between ICESat and the FDM. The limits of 20 kg/m³ and 6 cm/yr for Fig 8 were chosen simply to highlight the primary areas where these differences are seen, and to verify that the derived densities (Fig 8c) fall within expectations, i.e., from 350-900 kg/m³ with a mean of 396 kg/m³ (close to that of snow). The Kamb Ice Stream is a special area where ice thickening occurs and, as a climate model, this thickening is not accounted for by RACMO2. That makes the derived densities for this particular area unreliable, and we are considering masking this area out for the final revision for this reason, or at least noting this in the text. To be clear, for ICESat-FDM height differences that are larger than $(2 \times \sigma_h)$, where σ_h is defined in Eqn 2, we use a static density profile derived from Kaspers et al (2004). This is the same density map used (and plotted) in Gunter et al (2009). In the revision, we will clarify this so that the reader understands that these are the densities used, and not 917 kg/m³. We will also include a plot of these densities in the SM.

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.

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Yes, the GIA values referenced are indeed the empirical GIA estimates. We tried to label references to the GIA rates derived from the combination with the prefix "empirical," but we will go through the text again and make sure this is clear in all cases.

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

Agreed.

Page 3515 Line 6: "For the rock..." – the value of 100kg/m³ seems arbitrary. Comment.

The 100 kg/m³ value represents a 33% uncertainty in the difference between the two rock densities used, i.e., $0.33 \times (4000-3700) = 100$. The authors are unaware of any precedent for such uncertainties, so this value was deemed reasonable. This is not explained in the text, however, so we will add this so that at least it's clear how the value was determined.

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

Elastic effects are removed using modeled values based on ice mass flux estimates taken from Rignot et al (2008). This is described in the GPS processing section 3.4 (page 3511, line 16).

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.

There are several biases computed as part of our analysis, and the comments of the other reviewers also suggest that we need to make the description of these biases more clear. This will be addressed in the revision with an additional paragraph(s) in the

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discussion section. To answer the question, the "systematic bias" of Tbl 4 was derived by taking the weighted mean difference between the GPS and modeled/empirical GIA rates at the station locations. This systematic bias was then removed from the GIA rates in an effort to make the comparisons between the empirical/modeled rates with the GPS observations more consistent. The empirical GIA rates used in these comparisons have already been corrected for the LPZ-bias, so the systematic bias estimated is in addition to this.

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)?

Height changes over the ice shelves were indeed ignored since the hydrostatic equilibrium of the floating ice would not result in changes of the total column mass. Therefore, the empirical rates for these regions were estimated almost entirely from the GRACE data, under the assumption that ocean mass changes in the region were negligible.

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

Yes. We will clarify this in the revision.

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)

A good suggestion. Furthermore, the mean mass balance for the RL04-based solutions is -9.6 ± 7 Gt/yr, and for RL05 is -39.8 ± 5 Gt/yr, highlighting the differences between the two data releases.

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

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extent to Figure 11.

The concern about making these difference plots is that it would highlight changes only with respect to the chosen reference field. This might actually complicate interpretation of the results, especially for Figs 9 and 11. The point of Fig 2 was to highlight that the various GRACE solutions are quite similar in appearance, regardless of whether the solution was RL04, RL05, or with/without specialized filters. The subtle differences seen in Fig 2 manifest themselves in Figs 9 & 11, so we feel its useful to keep the figures as they are to make such connections visible.

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

Yes, this gets back to this reviewer's earlier comment on labeling all derived GIA rates as "empirical" to clearly separate them from the modeled rates. The caption will also be updated as suggested.

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 $\text{kg/m}^2/\text{yr}$ 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?

The left panels represent the empirical GIA estimates (10a shows the rates, 10c are the corresponding uncertainties), while the right panels represent the ice mass change (10b shows the ice mass change, 10d are the corresponding uncertainties) in units of equivalent water height. We chose EWH as the unit for the mass rates, since this is commonly seen in the literature. Labeling the color bars as "mm EWH/yr" would make this more clear. The right panels do not represent the mass change of the GIA signal, only the ice mass change, explaining why the two patterns are different. We will

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highlight this difference in the text to avoid any confusion.

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.

Looking through the various cases of Fig 11, some stations match better with the empirical/modeled rates, while others do not. The agreement between the GPS rates and uplift rates are furthered summarized in Tbl 4, where the WRMS of the rate differences are computed, with and without a systematic bias removed, as well as over different subsets of stations (29 and 35). Note also that the scale used was intentionally reduced to +/- 6 mm/yr to highlight differences.

Technical corrections

Page 3501 Line 15: "will assess" – use present tense Line 21: "lower" – change to "less negative"

Agreed.

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)

Agreed.

Page 3519 Line 21: "show" rather than "shows" Acknowledgments: "Himanshu Save" rather than "Himansu Save"

Agreed.

Interactive comment on The Cryosphere Discuss., 7, 3497, 2013.

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