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Dear Reviewer #1,

Thank you very much for posting short comment to RC C2162 to our manuscript. We have incorporated most of your recommendation in our revised version, and we think that your concerns have greatly helped to improve the manuscript.

In the following, we provide a numbered list of your suggestions followed by our replies. We have tried to make clear, where (and how) the recommendations were worked into the text by giving a page number, and, if possible, a label ("B + comment number"), which refers to a label within the text body. We hope this makes it easy for you to find your way through the modified manuscript.

We very much appreciate your careful reading of our work.

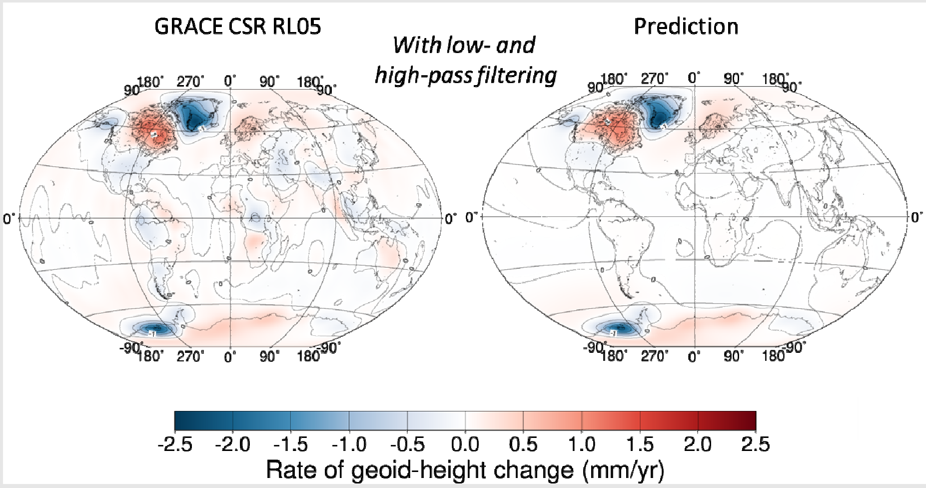
Yours sincerely
Ingo Sasgen

Response to reviewer comment RC C2162 of Anonymous Referee #1

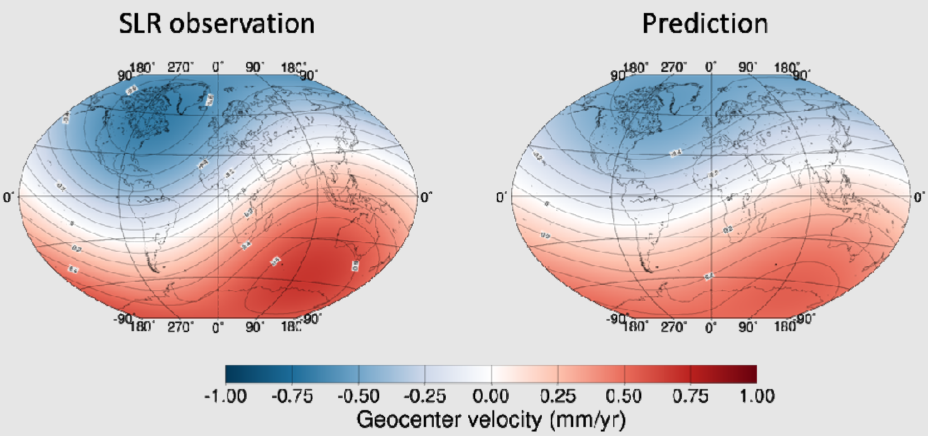
Major points

B1. The effect of the inversion for GIA in the Norther Hemisphere (p.3709) is not discussed enough. In the first paragraph of p.3710, the authors declare that far-field GIA causes a "nearly uniform" correction on the GPS rates of 0.6 +/- 0.2 mm/yr. Such a bias will almost entirely originate from a superimposition of geocenter motion and changes in the Earth oblateness, which on a decadal scale are caused by a number of processes: next to GIA, surely by changes in the ice masses, but also by land hydrology and ocean dynamics. The authors seem to be only inverting for GIA and for ice-mass changes in Greenland, Alaska and Antarctica, which in my opinion is not a satisfactory approach. In particular, constraining geocenter motion requires the definition of a global loading model (Blewitt, 2003). This is not necessarily a major problem, but the authors should discuss how any error in this initial bias correction will affect their inversion for Antarctic GIA. Form the first few lines on p.3715 it seems that a second inversion for far-field GIA is performed during the regional inversion for Antarctic GIA, which might allow to compensate for possible errors in the initial guess discussed above. However, the link between the two inversions is not clear and its implications not discussed.

We fully agree that the interpretation of ice-mass changes and GIA in the deformation and gravity fields requires a global inversion approach; particularly, if the signals of interest are small compared to those arising from the far-field, as it is the case for Antarctic GIA and average surface-mass density trends. However, constructing a complete global loading model (including hydrology and ocean, core motion for degree 2, etc.) is beyond the scope of this study and probably not necessary for the trends; GRACE indicates that our forward model consisting of ice-mass changes in Alaska, Greenland, Ellesmere Island, most of Antarctica, and GIA in Antarctic and Greenland captures the dominant signal, certainly in the high latitudes.



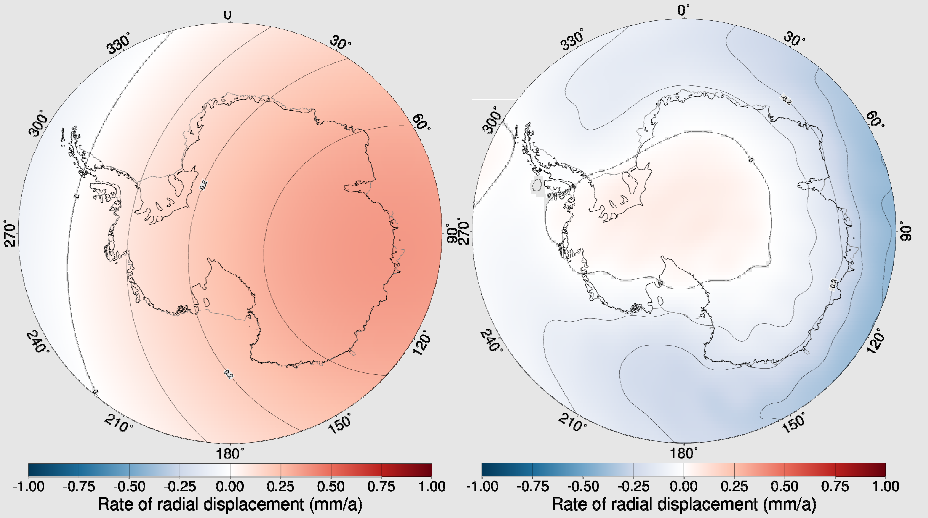
And it also reproduces degree 1 trends within the range of uncertainty of SLR tracking:



Geocenter velocity (mm/yr)				
Component	SLR	Prediction	SLR-2sigma	SLR+2sigma
x	-0.10	-0.06	-0.26	0.06
y	0.35	0.18	0.18	0.53
z	-0.58	-0.51	-0.81	-0.34

Considering the large uncertainty in degree 1 (Cheng et al. 2010), the large uncertainty in the GIA correction, which is the dominant signal (Klemann & Martinec 2009), as well as th large sensitivity of the Antarctic mass balance by a z-directed shift of the coordinate system (Barletta et al. 2012, TCD), we conclude that it is advantageous to filter out (set to zero) the degree-1 coefficients in the adjustment.

That the GIA-correction from the Northern Hemisphere is not only related to changes in degree 1 can be seen from the Figure below.



The Figure above shows the rate of radial displacement in Antarctica (centre of mass), caused by the GIA in the Northern Hemisphere separated into the spectral band of degree and order 1..2 (left) and 3...60 (right). Load model NAWI. Upper- and lower mantle viscosities, 2x10^20 Pas 5x10^21 Pas; lithosphere thickness 100 km.

B2. The impact of the addition of GRACE data to the GPS-only inversion is not clear at all, since Figure 4 only shows averaged values. After trying to gauge the impact of the use of GRACE data in each individual sector from Figure 3, I resorted to making use of the SH coefficients provided in the Supplemental to generate (unsmoothed) plots of GIA-induced surface mass changes for the GRACE-GPS and the GPS-only models. Such two plots should definitely be in the paper, because I believe that, at least for the case of Antarctic GIA, spatial patterns are more relevant than their actual amplitude. Moreover, they serve the double purpose of allowing to see the effect of the GRACE constraint and of facilitating the comparison against other available models. I recommend showing equivalent mass changes (not geoid changes), because they scale almost linearly with uplift rates. As a result of this effort I have verified the validity of the author's generic statement at lines 8-11 of p.3713 that the fit of each parameter Sr is influenced by the contribution of all sectors. Though the statement is somehow obvious to anybody with basic understanding of the physics of GIA (but I guess not to any reader of TC), in its current form it lacks a quantitative measure of this influence.

We have included an additional statement on the contribution of all sectors to the parameter estimate [B2a]. The effect is also discussed in slightly greater detail for the case of including a new GPS uplift estimate from Groh et al. 2012 for the Amundsen Sea Sector [B2b],

We also now provide a plot of the radial displacement field for the GPS and GRACE+GPS constrained GIA model. We decided to show radial displacement rates not equivalent water height changes, because it allows us to compare the fields with the GPS uplift rates, which are now also plotted. Equivalent water height is also a bit of an awkward quantity for GIA, which also shows mass change in the Earth interior [B2c]

B3. About the use of GRACE data, I wonder why the authors have limited it to the FRIS, and not also to the Ross Ice Shelf, where the same arguments hold, and to the central part of the EAIS, where accumulation is almost null. Those two areas would be important because the Ross Ice Shelf is a region of large GIA differences between various existing models, while an additional constraint over the EAIS might help to resolve potential issues arising from the far-field effects discussed in (1).

Our selection of the FRIS region in the earlier work was guided by the signal stand-out in the GRACE data and by most models showing the dominant anomaly there. Although subsequent papers have shown that part of this signal may have been related to tidal aliasing.

We don't want to argue too strongly for GRACE as a GIA constraint for Antarctica, now that the magnitude of predictions reconciling with GRACE have considerably dropped, increasing the problem of leakage, real and mismodelled (GAD) trends in the ocean, etc.

The main reason for using only the FRIS to derive a single scaling factor is that the initial load distribution of the LH1, LH2 and LH3 within each sector is intended to remain unchanged, when combining with the GPS data and fix the ambiguity w.r.t the viscosity distribution. As a consequence, the scaling factor changes the magnitude of the GIA prediction, not its spatial pattern, placing some confidence in the prediction of LH1, LH2 and LH3. In this sense, the GPS data are used for the regional refined of the model (see reply to point B5).

Conncerning the EAIS; in the course of the study, we also estimated the GIA signal over central EA from GRACE, determined the associated surface-displacement (assuming no present-day ice-mass changes) and constructed a pseudo GPS observation at the South Pole, which entered the adjustment to the GPS data. Results mainly changed for ICE-5G due to a strong signal in central EA related to a major disc-shaped de-loading centred over the pole. This 'disc' appeared to be spurious and we therefore modified the maximum ice height of the central loading disc from 765m to 444 m, to obtain a smoother transition to neighboring regions, which removed the effect in the GPS adjustment.

Moreover, considering that the largest deviations in EA mass balance from radar altimetry, mass budget and GRACE lies in EA, we think it is important to provide a GIA estimate that is largely independent from assumptions on changes in EA. We included a more detailed discussion on EA [B3]

B4. The authors claim that their results are largely independent on the input GIA models because of the large spectrum of ice and earth models used in this study (l.21-24, p.3710). Personally, I find that using permutations of 3 ice histories and 4 viscosity models over 5 areas does not necessarily proves this statement to be true, considering that model results are highly correlated.

Agreed. We have weakened the statement: "Due to a broader sampling of the parameter space compared to \cite{wu:et:al:2010}, AGE1 is more independent from assumptions on the viscosity distribution or glacial reconstruction taken there. However, it still relies on three roughly similar glacial reconstructions (not including all geomorphological data available today) and a limited range of mantle viscosity distributions; including regional advance and retreat scenarios, which are not captured by the glacial histories, or a more complex rheological structure underneath Antarctica, such as a ductile crustal layer \cite[e.g.][]{schotman:et:al:2005}, may influence the resulting AGE1 GIA estimate and its uncertainty range. Nevertheless, AGE1 represents an GIA estimate, alternative to the predictions of \cite{ivins:james:2005} or \cite{whitehouse:et:al:2012}, for correcting GPS, GRACE and altimetry trends in Antarctica." [B4]

B5. In particular, I am not sure I understand the implications of what the authors write about the constrained least-squares approach (l.21-25, p.3712), when they state that the parameter estimate must be close to an a priori value: what would it happen if, for example, all three ice histories missed an area of large GIA?

The scaling factor derived from the FRIS is mainly intended to compensate for the trade-off between magnitude of the load and mantle viscosity. It should give a first-order estimate consistent with GRACE, while retaining the spatial pattern of the GIA prediction. In a further step, the GPS data is used to estimate the sector-wise GIA prediction, and, in the constrained estimate, a sector-wise modification of the GIA prediction that is close to the initial spatial pattern. The implications are, that, depending on the GPS error, the combination with GRACE will force the prediction to be closer to the initial GIA prediction. If, for example, none of the models predict subsidence in the Amery Shelf as shown in the GPS data, the combined estimate will still retain this feature to some extent – how much depends on the uncertainty of the GRACE (32 %) and GPS data (sector-wise dependent). We have added a clarifying sentence [B5a].

Concerning a missed area of GIA; recently, Groh et al. 2012 determined a GIA signal of about 34+/-12 Gt/yr for the Amundsen Sea Sector by the combination of GRACE and ICESat data. This can be considered as an extreme case for our investigations, because 1) the signal is huge and 2) it is not captured by LH1, LH2, and LH3. We have included the GPS uplift rates of Groh et al. 2012 in our adjustment and discussed the results showing only a minor effect on the mean of the apparent mass change. The stations increase the sector field for FRIS to which this region is connected. In parallel, the GIA signal over the RIS is reduced, because the 3 stations lie in the region of the peripheral forebulge exhibiting subsidence after RIS deglaciation. In total, both effects nearly cancel in our GIA estimate (47+/-18 Gt/yr including Groh et al. 2012; vs. 49.59+/-13.35 Gt/yr) [B5b].

This situation, however, will be completely different if we allowed for an additional GIA sector 'Amundsen Sea' to be adjusted. Then, it is expected that an additional GIA signal similar to the estimate of Groh et al. 2012 (34+/-12 Gt/yr) will be obtained, substantially increasing our apparent mass change estimate. Although we consider the GPS uplift estimate some sort of an outlier (compared to the results of Thomas et al. 2011), the contraction of our GIA estimate on the Groh uplift rates is now mentioned in the conclusion. This is certainly an area where further research is needed [B5c].

I think that the authors should provide some, at least rudimentary, sensitivity test to actually show that the output of their inversion is not largely affected by the input models.

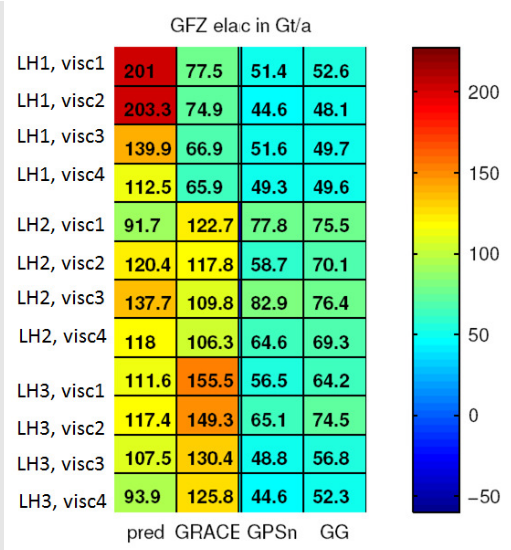
We have tested the sensitivity of our results to the input models by constructing a simple synthetic load history with a uniform ice retreat; for this we determine the average mass at the LGM in LH1, LH2 and LH3, spread it uniformly across each of the five sectors, and adopt a linear retreat scenario. The results are shown in the table below, showing the GIA induced apparent mass change based on LH1, 2, and 3 along with the uncertainty, and for the three cases when replacing one LH with the uniform history.

Mass change (Gt/yr)					
Basin #	LH1, LH2, LH3	2 sigma	LH1,LH2, UNI	LH1, LH3, UNI	LH2, LH3, UNI
1	4.6	1.0	4.7	4.5	4.3
2	3.7	1.6	4.9	3.3	3.6
3	5.1	2.6	6.1	4.5	5.5
4	1.1	0.8	1.1	1.4	1.3
5	1.3	0.6	1.1	1.2	1.2
6	0.8	0.7	1.0	0.8	1.2
7	1.4	0.5	0.8	1.2	1.5
8	0.1	0.5	-0.2	0.1	0.2
9	1.3	2.8	1.9	0.3	2.2
10	-1.1	1.2	-1.3	-0.6	-1.1
11	1.9	2.6	2.0	1.0	2.8
12	3.4	1.5	3.3	2.6	3.0
13	2.2	1.0	2.0	2.3	1.8
14	-0.1	0.7	0.4	0.4	0.3
15	1.3	0.9	1.8	2.0	1.4
16	2.3	2.5	3.3	1.3	2.5
17	3.2	1.6	3.6	2.8	3.6
18	4.0	1.5	4.7	4.1	4.0
19	4.9	1.2	4.3	4.4	4.8
20	0.4	1.6	1.0	1.4	1.4
21	1.0	0.9	1.8	1.4	1.6
22	1.4	1.0	1.3	1.7	1.7
23	-0.8	0.7	-0.7	-0.5	-0.6
24	3.5	1.0	3.0	3.5	3.6
25	0.4	1.2	0.4	0.5	-0.4
AntIS	47.1	16.9	52.2	45.5	51.4

The table shows only a minor deviation if one load history is replaced; typically < 1 Gt/yr per basin and < 5 Gt/yr for the entire AntIS. For the AntIS and all basins, except basin 7 (combination LH2, LH3, UNI), changes lie within the error bars of column 3.

We hope on your understanding that a more elaborate construction of alternative load scenarios, e.g. based on statistical variation of the load histories, or numerical modeling simulations beyond the scope of our paper.

On a related note, I find the range of chosen viscosity values for the upper mantle (Table 1) to be quite limited: this might have to do with the chosen ice histories (in the sense that the combination of the different ice histories and earth models might eventually provide a wide-enough spectrum of GIA predictions), but it certainly deserves a clearer explanation.



The above diagram shows the appararent mass change of LH1, LH2 and LH3 for the viscosity distributions VD1, VD2, VD3 and VD4, for the initial prediction (pred), the GRACE-constrained model (GRACE), the GPS constrained model (GPSn) and the GRACE/GPS constrained model (GG). It becomes visible that applying the GRACE constraint (only one scaling factor for all secors!) homogenizes the apparent mass change for different viscosities. Including the GPS data (sectorial subdivision!) homogenizes the the apparent mass change for different load histories. For example, ICE5G (LH3), which has a large GIA signal over the RIS compared to FRIS, retains a large mass change if adjusted to GRACE. This is not the case, for Huybrechts, 2002 (LH1), which has the dominant load over FRIS, Applying GRACE/GPS data increases the apparent mass change, but has, due to the comparably large errors of the GRACE scaling factor with respect to the GPS data, only a minor effect.

Finally, considering the existing trade-offs between ice histories and earth models, I do not see the need to model lateral variations in lithospheric thickness (l.25-28, p.3711), which also largely limits the possibility for other researchers to reproduce the results presented in this paper (since the access to 3D GIA models is very limited).

This is a misunderstanding. Due to the computational effort in 3D GIA simulations, we model each sector LH and VD combination individually with the 1D version of the viscoelastic code. An additional sentence was added [B5b].

Additional comments

B6. The authors seem to produce GIA estimates of radial displacement in the center of figure (CF) and GIA estimates of geoid height change in the center of mass (presumably of the whole earth, CM). I wonder why this choice, considering that the GPS data of Thomas et al. (2012) are expressed in the CM.

We are grateful for this comment, as it is in fact an error in our interpretation of the GPS data, which does considerably affect the attribution of mass change due to GIA to the Northern Hemisphere and Antarctica (related to your point B.1). We have now corrected this flaw, resulting in a higher Antarctic GIA apparent mass change from GPS, which are more consistent to the GRACE-based estimate. As a consequence, GRACE poses only a minor contribution to the combined estimate (due to its errors).

B7. There seems to be some inconsistency in notation between the text on p.3712 (l.13-14), where it is stated that a single scalar parameter SˆGRACE is derived, and the explanation of the symbols of eq.2, where SˆGRACE is a vector.

Each position of the vector contains the value for the FRIS. Two half-sentences were added [B7a] and [B7b].

B8. It would be nice to also see a spatial plot of the result of the GRACE-only inversion, since a mean bias of -1 mm/yr with respect to the GPS results does not sound too bad. Moreover, this bias might have to do with the GRACE-only estimate of the contribution of the Northern Hemisphere.

We have now included a spatial plot of radial displacement rates, for GPS and GRACE/GPS in the main text [B8].

B9. It seems that the errors associated with the GRACE scaling factor are heavily affecting the solution (a 10% change in those errors changes the total GIA estimate by 10-20%). Is it possible that the given GIA uncertainty is too optimistic?

As mentioned in point B6, GRACE and GPS results on total GIA are now more consistent, and the effect of applying the GRACE constraint is minor. The greatest uncertainty in the GRACE estimate is leakage of present-day signals, and un- or mismodelled trends in the GAD product under the FRIS. An uncertainty of 32 % maybe too optimistic, but our tests show that most of the estimates lie within this range. The question also is: is the GPS uncertainty too optimistic (w.r.t . the GRACE estimate)? Considering that only 16 of the 46 GPS uplift rates are based on more than 5 years of data with measurement periods of > 50 days, it is questionable whether, e.g. interannual variations and long-term changes in snow accumulation average out.

B10.While comparing their ice sheet mass balance results to previous studies (beginning of the discussion section, p. 3717), the authors should also cite at least Horwath&Dietrich (2009, GJI 177), who provided a very similar estimate (-109 +/- 48 Gt/yr), though on a shorter time-span.

We now cite Horwath & Dietrich (2009) [B10].

B11.It is now inevitable to discuss the results recently published by King et al. (2012, Nature), who have a considerably lower estimate of ice mass change for the whole AIS (-69 +/- 18 Gt/yr), but an almost identical estimate of the total GIA contribution (46 +/- 18 Gt/yr, obtained from the differentiation of columns 2 and 4 in their Table S1). In particular, it is interesting how King et al. (2012) have a much larger difference between the WAIS and the EAIS, both in the GRACE trend (without GIA correction) and in the GIA solution. It might help that the GIA model by Whitehouse et al. (2012) has just been released (http://www.dur.ac.uk/pippa.whitehouse/). I realize that it is not completely fair to ask the authors to provide an explanation of this difference, but it worries me to see that both the GRACE trend and the GIA solution are substantially different (when taken separately over the WAIS and the EAIS) and, most unfortunately, with non-overlapping error bars.

We now discuss the work of King et al. 2012 in more detail. Actually, most difference resides in East Antarctica, where we predict +30 Gt/yr apparent GIA-induced mass change and King et al. 2012 +3 Gt/yr (without GIA correction our estimate: 55 +/-7 Gt/yr and King et al: 2012: 65 Gt/yr. Another reason is stronger mass loss in the Amundsen Sea Sector, (-125 Gt/yr our estimate compared to -96 Gt/yr), with differences mainly for basin 20 and basin 23. This difference we will address this in the future.

We apologize, but we think a direct comparison of both models is beyond the scope of the paper. We would like to emphasize that Antarctic GIA is still a very open issue. Our geodetic estimate and the elaborate work of the W12a are two very different approaches to address it. Ideally, both approaches should give the same answer or should be combined. We think it is already a very good advance that the total GIA seems to be robustly constrained.

B12. I am not sure that 9 years of data are enough to conclude that there exist a "persistent" imbalance caused by an altered ice-dynamic behaviour (l.20, p.3719).

No, you are right. But we think it provides a support to the findings from altimetry dating back more than 10 years longer. Statement changed and Rignot et al. 2011 as a reference included. [B12]

B13. It would be nice to show the FRIS area used to derive the scaling factor based on GRACE data, possibly in Figure 1.

The adjustment region is shown in Sasgen et al. 2007. We have subdivided it into 4 sub-regions to test the sensitivity on its placement.