Dr. Ingo Sasgen
German Centre for Geosciences GFZ
Earth System Modelling
Telegrafenberg A20
Tel.: +49 331 288 1145
Fax.: +49 331 288 1163
Email: sasgen@gfz-potsdam.de

May 3, 2013

Dear Matt King,

Thank you very much for posting short comment to SC C1881 to our manuscript. We have incorporated most of your recommendation in our revised version, and we think that this has 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 ("A + 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 short comment SC C1881 of Matt King

General comments:

A1. The histograms the authors provide in the supp material suggest that the distributions are not Gaussian. Are the authors sure that they can take and use a standard deviation from these distributions with any statistical meaning? I wonder if they should consider those effects which are random, and use them for the uncertainties, and those that are better characterised by being systematic and construct bounded estimates from the upper and lower values.

Whether the ensemble spread generated appears Gaussian or not depends on 1) the data adjusted to and 2) the region considered. But we observe that the viscosity variations lead to a more Guassian-like distribution of the apparent mass change (adjusting to GRACE or to GPS), while the ice models show systematic clustering at the basin-scale. For the Antarctic average and the large sectors, the a posteriori spread is similar to a Gaussian distribution. Cleary, systematic clustering would be reduced if GPS data existed allowing to constrain GIA at the basin-scale not sector-scale.

We have now moved the Figure with the basin-scale histograms from the supplement to the main text (Fig. 3 main text) and discussed this issue in a few lines [A1]. In addition, we now provide the standard deviation of our u and e GIA fields in the supplement (Fig. 3 supplement).

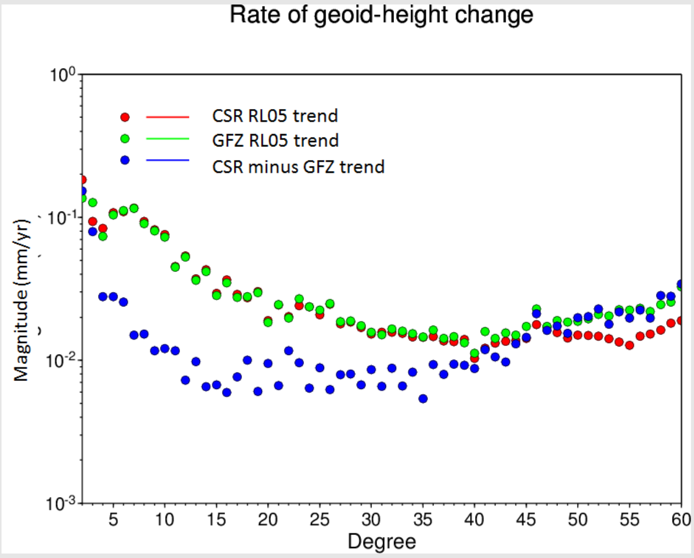
Please also see replies to comments replies A2, A4, A7, A8 and A9.

A2. There seems to be some missing information on what the analysts do with degree-1 and degree-2 in GRACE and destriping. Note for degree-1 the conventional approach of Swensen et al and the alternative of Rietbroek and the discussion in Barletta et alTCD. Did the authors note the any apparent mass jumps as identified elsewhere by Duan et al.?

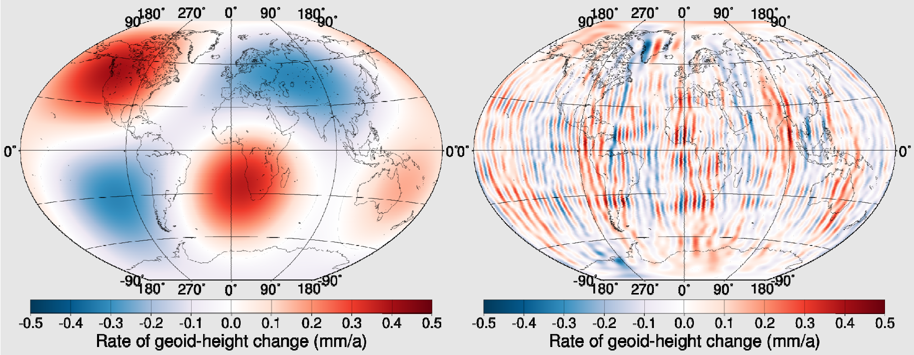
We have updated our sections on the GRACE data processing.

Now we also updated to RL05 for which the apparent jumps caused by the AOD product are not an issue. [A2]

Comment on the GRACE filtering. In this paper we damp the low- and high-frequency GRACE coefficients, i.e. band-pass filter the data according to the function specified in Sasgen et al. 2012, supplement, in order to 1) reduce the far-field signal over Antarctica, and 2) suppress high-frequency noise resulting from the harmonic downward continuation of the gravity-field measurement. An indication that this procedure may be of advantage is provided when looking at the difference in the CSR RL05 and GFZ RL05 gravity trends. The degree-power spectrum reveals very good agreement in the mid-spectral range, with large deviation in the upper spectral part (as expected), but also with decreasing amplitude for the lower-degrees 2, 3 and 4

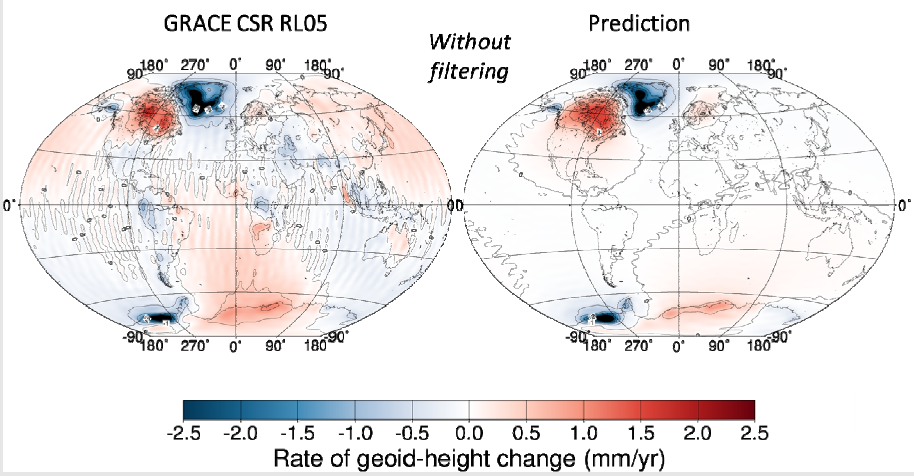


The difference in degrees 2 and 3 (left) is comparable in magnitude to the difference in the remaining spectrum of degree 4 to 60 (right). The pattern suggests the main difference in degree s21.

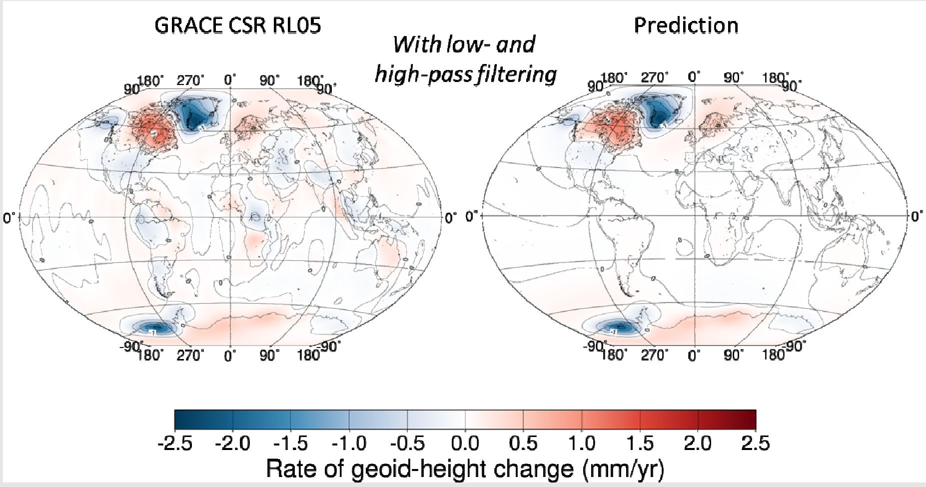


Although, GIA-induced rotational variations is accommodated in our forward model, and, for test also the rotational variation caused by present-day ice mass changes in Greenland, we cannot currently reproduce the trends in degrees 2 and 3. This either means that our model is incomplete in the sense that a process is missing (core motion?) and/or that the GRACE coefficients in CSR or GFZ contain errors/artifacts. In both cases, we consider it best to reduce the influence of these coefficients in the adjustment of the forward model, which is, however, complete up to degree and order 340 (present-day changes) and 170 (GIA).

The figures below show the unfiltered GRACE trends from CSR RL05 (Jan. 2003 to Sep. 2012), and the adjusted forward model (both cut-off degrees 2…60). The model prediction consists of present-day ice-mass changes in Greenland, Antarctica, Alaska and Ellesmere Island; the GIA models consist of Huybrechts, 2002 and NAWI (Zweck & Huybrechts 2005) for VD3. All model components are adjusted to the GRACE data. It is visible that the adjusted prediction only partially contains the degree 2 and 3 pattern.

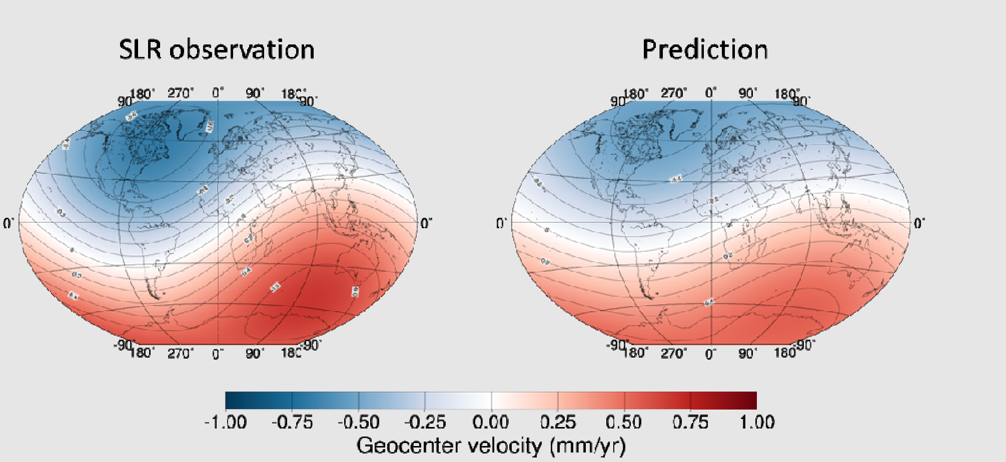


And the same like the Figure above, but after low- and high-frequency damping, used before model adjustment.



A3. Likewise, the uncertainties in degree-1 are not discussed unless I missed them. These become important in terms of how they propagate not just into the rate but the accelerations. They are substantially large to remove many apparently statistically significant accelerations.

We have added a paragraph on our treatment of degree 1. Similar to point A2, we neglect degree 1 in the adjustment due to its uncertainty, but now show that the modeled degree 1 lies within the uncertainty range of the SLR estimate.



The diagram above shows the geocenter motion with SLR tracking (left; Cheng et al. 2010) and the model prediction (right) for January 2003 to September 2012. It is visible that the model reproduces the degree-1 terms to a large extent in direction and magnitude. The prediction, however, shows a lower amplitude which may be related to a too weak lower-mantle viscosity. Nevertheless, observation and model agree within the uncertainties.

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

A comment was added. [A3]

A4. There appears to be something strange with the results along the Siple Coast. Kamb Ice Stream is thickening very strongly (in reality) and yet the solutions presented suggest it (basin 18) is not thickening any more than basin 19 to the north. That cannot be right. I assume the authors noticed this, so I wonder if it is to do with the methodology or something else? If methodology then does it suggest issues elsewhere? Leakage between basins?

For the new RL05 data, we now obtain +9+/-5 Gt/yr for the Siple Coast basin 18 and +6+/-4 Gt/yr for basins 19, i.e. strongest increase for the basin including Kamb Ice Stream. For GFZ RL05 values are +11+/-1 Gt/yr for and +5+/-1 Gt/yr, while for CSR RL05 8 Gt/yr (basin 18) and 7 Gt/yr (basin 19) (Jan. 2003 to Sep. 2012). This is an indication that the earlier unrealistic values were caused by remaining systematic noise in the data. Uncounted leakage, however, may also play a role as presented in the supplementary of your paper (King et al. 2012); our forward model assumes mass change in the fast-flow regimes mainly along the coast – Kamb Ice Stream, however, is thickening upstream, and so the estimate could even be more influenced by surrounding mass changes.

A5. The authors' approach, if I understood correctly, scales the GIA uniformly across each of 5 regions to fit GPS or GPS/GRACE. They interpret mass changes in many basins within each of those 5 regions. Does that not mean that the sub-region GIA shape is entirely governed by the shape of the GIA model? If so, this is, in turn, largely governed by the shape of the ice history I think. So does this not mean that the ice histories play a very large role in the estimated ice mass changes in these basins within each adjusted region?

The GPS data represent a reasonable constraint on the GIA signal for the five regions. The sub-regional GIA signal is, as you mentioned, mostly governed by the spatial distribution of the ice load (and its temporal retreat history), Of course, we cannot exclude that the variety represented by our three load histories is insufficient (a way to get around this could be a statistical permutation for the sub-sector histories within some physical bounds). But this would require a tremendous amount of extra work not compatible with this paper. Also, looking at the GPS residuals with a mean bias close to zero suggests that currently no further information can be extracted from the uplift rates.

Anyway, we still tried to test the sensitivity of our apparent mass changes estimates to the glacial history by constructing a 'simplest case' synthetic load model with a uniform ice retreat in each of the five regions; for this we integrated the ice thickness of LH1, LH2 and LH3 and redistributed it over the area of grounded ice at the LGM. The temporal evolution is linear from LGM to present-day. The table below lists how our basin-scale apparent mass values change by excluding one of the load histories,

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.

A related comment is included in the paper [A5].

A6. The ice histories are quite old, and in all cases no longer supported by more recent field geology for many areas of Antarctica. I guess the comes to the question in 5 – does this matter? It seems to me it must. Some commentary to discuss the reasonableness of the ice histories used is required – ie, justification against up-to-date glacial geology.

We have chosen these histories, because they are based on three different approaches (modeling, geomorphologic evidence, RSL data). Because they are outdated, we have the possibility to modify the histories accounting for more recent data (GPS uplift rates), which is, for example not the case for IJ05_R2. Please see also reply to A5.

A7. The authors may wish to include now the GPS rates given in Groh et al., 2012 and some discussion of their results.

We have now discussed the impact of the GPS rates given in Groh et al., 2012 on our results [A7].

A8. Do the authors consider temporal autocorrelation in the series? This is not commonly done, but it is obvious that after modelling various trends and period signals, the residuals are temporally correlated (i.e., not independent); not taking this into account makes the uncertainties too optimistic. Horwath and Dietrich GJI include a one-linear noting a factor of 2 increase (I believe more fully explored in Horwath's PhD thesis). The authors should include a consideration of this if they have not already, as this will especially bear on what accelerations are statistically significant.

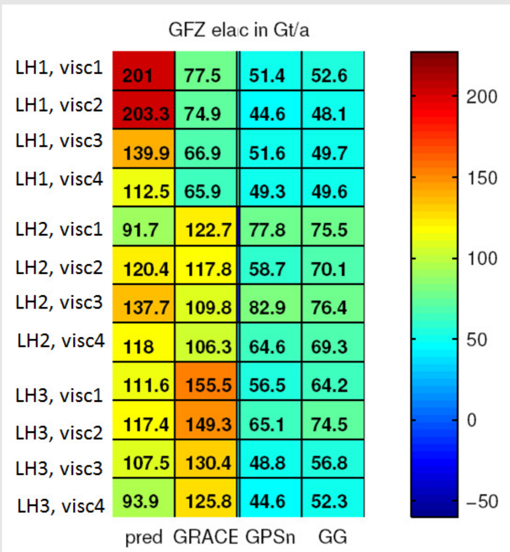
We recognize the temporal correlated noise in the GRACE time series. Between the GRACE releases, however, we find that this systematic noise manifests differently (e.g. different coefficients contain spurious trends) (see e.g. Sasgen et al. 2012, doi:10.1016/j.jog.2012.03.004). Calculating the average and uncertainty range computed from the two releases is a very heuristic approach toward reducing the problem, which will be updated in the future. Nevertheless, the overall uncertainty estimates derived are very similar to those of King et al. 2012.

A9. In this and earlier work by Sasgens et al, I have not been clear why the Ross Ice Shelf is not used as FRIS is – as a second "zero" mass change. Could the authors comment? By the way, I do not see the authors have considered that FRIS will experience non-steric sea level rise so is probably non-zero. It's a small bias (a few mm/yr water across WAIS) but it should be considered and at least commented on. That non-zero rise is probably entirely unknown in this region, but the author's own global GRACE estimates used for the far field elastic correction should give them an idea.

Our selection of the FRIS region in the earlier work was guided by the signal stand-out in the GRACE data. 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 refinement of the model. This can be seen in the table below:



Shown is 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.

A10. P3706L7: Add the significant uncertainty associated with input accumulation and some uncertainty in converting surface velocity to depth-averaged velocity.

Comment added [A10].

A11. L15: Did the authors leave out lithospheric thickness for a reason?

We have modeled the GIA signal with lithosphere thickness of 60 km for AP, 150 km for EA and 100 km for the rest of Antarctica. We fully agree that the thickness for AP is probably a factor of two too large, and also the viscosity values chosen are most likely too large. Moreover, there might be a ductile layer present in West Antarctica, which will further decrease the effective thickness of the elastic lithosphere.

Looking at the distribution of the GPS residuals, which are centred around zero with a variance comparable to the measurements suggests that most of their information is extracted. Concerning another free parameter in the GIA modeling (which we didn't for reasons of efficiency in deriving the permutation estimates), will probably only provide a slightly better fit to the GPS, but will increase the variance of the apparent GIA estimate as more variability will be created in regions that are not constrained by GPS. This is a matter of ongoing investigation.

A comment was added [A11].

A12. L17: there is an implicit expectation that GIA in Antarctica is uplift, whereas models for several years (IJ05) have suggested subsidence in EA. Worth making this possibility clear here to avoid propagating this misconception

Included additional sentence on GIA-induced subsidence. [A12]

A13. P3707L1: as for L15 on previous page L6-11 is not entirely factually correct and could be shortened. The inland sites were being deployed since _1995 not 2007. The IGS sites remain very important for the analysis since they are most precise and pretty much all there is in East Antarctica. And the Thomas et al. results do not suggest the other sites provide an advance over them. I suggest that the authors just say that GPS uplift rates are now available across much of Antarctica but the longest, and hence most precise, records remain along the coastal perimeter.

We have modified the text. [A13]

A14. L15: ICE-5G importantly has the largest bulge on the Siple Coast, so this statement does not always hold true

Reformulated [A14].

A15. L28: "those consistent" should be "consistent with" but this is not entirely true even. The Whitehouse et al. model was compared to GPS uplifts at the end of the process but only tuned (in the case of the W12a modification) to it in the southern peninsula. So it is not entirely true to say they were selected to be consistent with GPS. It would be more true to say it was selected to fit geologic and relative sea level constraints and, in the southern AP, GPS uplift rates.

Accepted. We now rewrite the sentence: "It also differs from the approach followed by Ivins and James (2005), Whitehouse et al. (2012b) and Ivins et al. (2013), which is based on selecting from a suite of GIA scenarios those that fit geologic and relative sea level constraints and – in the case of the W12a modification (Whitehouse et al., 2012b) in the southern AP, GPS uplift rates, without attempting to formally minimize the misfits to both space gravimetry and terrestrial GPS data." [A15]

A16. P3708L18: perhaps start as "A priori, this involves . . ."

Included [A16].

A17. P3709L12: is there a citation for the reference to limitations in this model?

Actually, no proper reference exists for this statement. And a validation with SLIs is still required. Sentence changed. [A17]

A18. L25: given this is just referring the post-breakup period the O'Higgins site is called OHI2. It probably cannot be said that SMRT is dominated by post-Larsen B breakup since our record ends in 2004 I think

Corrected, The sentence now reads: "The GPS stations of the northern Antarctic Peninsula (OHI2, ROTB and PALM) tend to exhibit a kink in the time series of the vertical component after the Larsen Ice Shelf breakup in 2001 \cite[]{thomas:et:al:2011}. Here, we include estimates of the vertical motions for these stations prior to the breakup event of 2002, though the crustal motion is likely to be a mixture of viscous and elastic responses that have memory of the losses prior to 2002 \cite[]{rignot:et:al:2005}. The complexity of the response is exacerbated by the quite low asthenospheric viscosity that occurs in mantle adjacent to the Bransfield Strait and a young mantle slab window \cite{ivins:et:al:2011,simms:et:al:2012,nield:et:al:2012}. Also, for SMRT, only GPS uplift rates prior to 2002 are included, despite the fact that the station record does not exhibit a significant change of the trend from 2002 until ceasing measurement in early 2005 \cite[]{thomas:et:al:2011}." [A18]

A19. P3710L6: this is an important correction. Note, the given term may over-estimate the effect for some of the earlier GPS data, given the acceleration of Greenland mass loss since the mid 1990s.

Reviewer #2 noted that the GPS data of Thomas et al. 2011 is provided in the centre of mass, not the centre of figure. This is actually a real mistake we made in the data interpretation, we now correct by re-calculating surface deformation in the centre of mass (and geoid change in the centre of figure for comparison with SLR). Interestingly, the GIA patterns now have to accommodate more uniform uplift rates, which leads to a different spatial distribution than in the previous version of the manscript.

The correction is now much smaller, yielding an increase of the apparent mass change in addition to the Antarctic GIA signal by about 3 Gt/yr [A19].

A20. L20: empirical estimate

'geodetic' replaced by empirical estimate [A20]

A21. P3711: delete "the" from section 3.1 title L26: the 80km lithosphere in the AP should be noted as being way too high for the northern Peninsula (e.g., Yegorova et al., 2011) and that the results could be affected by that in the northern-most basin.

For AP, we have adopted a lithosphere thickness of 60 km – not 80 km; this was an error in the manuscript text, which we have now corrected [A21a].

But we still agree that the lithosphere thickness is probably too high. Although much of the signal magnitude will be readjusted with the fit to the GPS data, the increased small-scale

GIA signal associated with a lithosphere of lower thickness will modify the spatial pattern, particularly in the area of peripheral forebulge and may affect the fit. Clearly more modeling simulations are needed in future with more complex Earth structures, e.g. also considering a crustal ductile layer

A comment was included [A21b]. And a reference to Yegorava et al. 2011 was given [A21c].

A22. P3712: it's worth noting that the method effectively results in non-physical behaviour at the boundaries of the region; that is jumps (although this is not a big issue in my mind)

Comment included [A22].

A23. L20: the GPS sites that are co-located but have 2 different velocities (like the WAGN sites) will be heavily correlated because they are co-temporal and hence rely on the same GPS satellite orbits – do the authors consider this correlation?

No. All stations are considered to be uncorrelated. It is difficult for us to judge this correlation, and we rely on the error estimates provided in Thomas et al. 2012. However, it was tested by assuming only uniform errors for all station velocities, which considerably changed the results. Therefore, we conclude that a realistic error assessment is important, even though changing the weighting (or excluding) of a single station velocity does not have a large effect.

A comment was added [A23].

A24. P3713: eq 2: does the first FtC^-1F need to be (FtC^-1F)^-1? F is the design matrix, but it would be helpful if the authors explained exactly its form L9: "long-wavelength GIA signal covers entire Antarctica" – this is somewhat imprecise since it's only the very low degrees that do this – say up to degree 6-10.

Equation corrected [A24a].

F matrix explained more clearly [A24b].

Statement on the long-wavelength GIA signal clarified [A24c].

A25. L17: "four sectors" – do you mean instead of 5? This needs a little more explanation L20: state the aliasing periods used since these have varied in the literature. Moore and King (2008) also give the K2 sideband as potentially important – did you examine this? What about S2 alias to 161 days – won't bias velocities but could reduce trend uncertainties? Were these computed per site or on some grid (the tidal aliasing will be regionally coherent)?

Clarified which regions are included in the leakage assessment. [A25a]

We updated our calculation including S2 and K2 tides. Frequencies specified [A25b]. There is indication that ocean tides are better removed in GFZ RL05 than CSR RL05.

The tidal frequencies were removed from the GRACE coefficient time series.

A26. P3714L7: "permuting" may be better as "iterating"

We now use 'permuting' [A26].

A27. P3716L14: how do you treat the correlation between the GRACE-derived GIA model and the GRACE data itself? Is this correlation sufficiently small to treat them as independent?

The correlation, if we understand your comment right, is reflected in the uncertainty of the scaling factor associated with leakage, which is the largest source of uncertainty.

A28. L16: are the errors 1 sigma or 2 sigma? What is the reference epoch (t=0) for the velocities? This is critical to specify when estimating accelerations as well.

2-sigma. Now stated in the beginning of the manuscript. Fig. 5 is an exception to this, because 2-sigma errors for the accelerations would cover too much information [A28a]

Reference period for velocities added [A28b].

Reference period for accelerations added [A28c].

A29. L19: the basins are switched here accidently – basin 22 is Pine Is and 21 is Thwaites. It is Thwaites losing the greater mass!

Corrected. [A29].

A30. P3717L8: is it Maude or Maud?

'Maud' is correct. Changed [A30].

A31. P3718L17: state reference epoch again

Reference epoch included once more [A31].

A32. L29: cf Groh et al who have strong Amundsen Sea GIA

Discrepancy to Groh mentioned [A32].

A33. P3719L11: worth noting that it is not as negative as the -200Gt/yr (for 2006) of Rignot et al 2011 either.

We now focus on the discussion of the results for EA and refere to Shepherd et al. 2012. [A33]