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The Cryosphere Discussions

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

B. C. Gunter et al.

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

This manuscript by Gunter et al. describes a novel method of empirically deriving GIA-related uplift rates by combining a number of Antarctic data sets. The manuscript covers material that is clearly within the scope of The Cryosphere, it is well written in all aspects (abstract, methods etc.), the methods are generally robust, and the work has been carefully executed. The results clearly support the conclusions, however, in order to compare their results with uplift rates derived using forward GIA models, Gunter et al. apply a number of bias corrections to their empirically-derived results: I raise a

number of potential issues associated with these bias corrections below, and also list a number of more minor points. Overall, this is a good manuscript, and I recommend publication once the following points have been addressed.

Comments on bias corrections 1. GIA bias correction using LPZ, page 3513, line 8: The authors state that the 'rate of GIA' should be very small in the low precipitation zone (LPZ), and use this to estimate a 'GIA bias correction'. However, there is evidence (from the glaciological analysis of ice cores) of post-LGM ice thickening in this region, which would lead to regional subsidence at the present day – as predicted in both the IJ05 and W12 models, which are reproduced in the supplementary information, figure S3. If these models – which attempt to incorporate glaciological information relating to former ice thickness in this region – were correct, then the assumption that the rate of GIA-related uplift in this region should be zero would introduce a bias to all of the empirically-derived results. If the authors choose to retain their method, then I think this potential source of bias (and the underlying glaciological data) should be mentioned, and the implications for the final conclusions should be quantified in terms of Gt/yr (e.g. by comparing results that do and do not make use of the 'GIA bias correction').

It's important to emphasize that the LPZ bias represents a MEAN zero value over a large area. It does not necessarily mean that all parts of the LPZ will have zero GIA. This is why a large part of EA shows a subsidence trend in Fig 11, which is consistent (both in terms of magnitude and spatial distribution) with IJ05 and W12a, suggesting that our assumption is quite reasonable. It is correct that if the mean value of the GIA rates is not zero over the LPZ, then the results would be biased; however, as was mentioned in the text, we believe that the error introduced by our assumption is much smaller than the effect of other potential bias contributors (secular geocenter motion, C20 errors, reference frame differences, contribution from the Northern Hemisphere, ICESat campaign bias estimate errors, etc.), all of which are not precisely known and can each potentially introduce mm level errors (p 3513, lns 16-18). To make this clear in the text, we intend to add the following to the end of the discussion on the LPZ bias on p3513: "Second, if any genuine GIA over the LPZ does exists, then this would erroneously bias the empirically derived rates from the combination approach; however, as mentioned already, any error of this kind is believed to be much lower than that introduced by the various other (imprecisely known) bias contributors."

As a follow-on query: was this 'GIA bias correction' also applied to the uplift rates from the previously-published GIA models, e.g. ICE-5G, IJ05 and W12, or just the empirical results?

The LPZ bias was not applied to the GIA models, since these models are derived using various independent assumptions and parameters. For example, the ICE-5G model is not regional, and includes a contribution from the northern hemisphere. The IJ05 model is regional, but is solved for globally, so there will be a geocenter component involved. The only time a bias was estimated and removed from the GIA models was for the comparisons with the GPS site displacements. In this case, a bias between the GIA rates and the GPS rates was estimated and removed so that all GIA rates (modeled or empirical) would be reduced to a relatively independent regional network, making the comparisons more equivalent. We intend to clarify the application of the various biases in the discussion section of the revision.

2. Bias offset between GPS rates and GIA-related uplift rates: I understand the need to ensure that all the uplift rate estimates (from GPS, empirical methods, or forward GIA modelling) are in the same reference frame, but applying a uniform shift to the results of forward GIA models (e.g. ICE-5G, IJ05, W12) is unphysical. Consider the situation whereby a forward-modelled GIA solution fits the GPS data well in one region, say East Antarctica, but not in another, say West Antarctica, due to errors in the ice-loading model for West Antarctica. As I read it, the bias correction described in this manuscript would shift the entire field of GIA model-predicted uplift rates downwards in an attempt to better fit the GPS data in West Antarctica. This would lead to a worse fit in East Antarctica, where the model had originally been accurate. This is clearly an unsatisfactory situation if the purpose is to compare the accuracy of the various

forward and empirical models of GIA. Also, it is stated (lines 25-27, page 3516) that the choice of Earth model in a forward GIA model can lead to the type of bias offset that the above method attempts to remove, however, changing the Earth model in a forward GIA model will alter the amplitude and wavelength of the solid Earth response, not simply the absolute magnitude. Therefore, a bias correction consisting of a uniform shift, as described above, does not cover the case whereby the misfit to the GPS rates is due to the incorrect choice of Earth model in a forward GIA model. Combining this point with the observation that misfits to GPS rates are more likely to be associated with errors in the ice-loading model (which will be spatially variable but likely regionally correlated, and hence cannot be accounted for by a uniform shift), and the fact that the spatial distribution of GPS sites in Antarctica provide very poor sampling of the spatial pattern of GIA-related uplift in some regions, this suggests that a number of caveats need to be carefully explored and explained before this bias correction is applied.

As you noted, shifting the GIA rates (both modeled and empirical) to the GPS network was done in the interest of making the WRMS comparisons as equivalent as possible. Without this, we felt that the WRMS calculations would be dominated by other factors not related to the regional variations in the uplift rates, e.g., geocenter motion, GIA from the northern hemisphere, etc. We agree that using different Earth models will result in more than just a uniform bias correction, and may have some spatial variability; however, without more detailed knowledge of these errors, we felt that a uniform bias to remove the first order effects was the most appropriate step, and was better than not removing anything at all. With this in mind, we made sure that the WRMS results computed both with and without the bias corrections were presented in Tbl 4. And experiments in which the systematic bias for the GPS comparisons was estimated using only a subset of stations, e.g. those in EA or WA, showed that doing so can improve the WRMS of one region, but often at the expense of the other. For example, using only EA stations to estimate the bias resulted in a better fit to the EA stations, but often resulted in a worse fit to the stations in WA. We also found that the grouping of stations used in these experiments, i.e., which stations were included in the EA or WA subsets, can also influence the WRMS values of the two regions. As such, we prefer to estimate the bias using all stations, but agree that some discussion of this is needed in the text.

Minor points 1. There is frequent reference to 'the rate of GIA', however, GIA encompasses arrange of processes including solid Earth deformation and deformation of the shape of the geoid, and therefore it should be clarified that the authors are referring to the rate of solid Earth uplift associated with GIA. It is important that this shorthand does not creep into the literature to ensure that the range of processes associated with GIA is clearly understood. To this end, the text on lines 7-9, page 3499, should also be expanded to include a full description of GIA.

We have no objections to this, and will clarify this in the revision.

2. Page 3502, line 18: please include references for the Antarctic climate and firn densification models here.

Agreed.

3. Page 3502, lines 18- 23: The authors describe the different data and processes that are considered on the floating and grounded portions of the ice sheet, respectively, mentioning that surface processes will not contribute to mass change over the ice shelves. By my interpretation of equation 3, all of the mass change observed over the ice shelves will therefore be attributed to solid Earth uplift. However, there will be a small contribution to the mass signal in this region from sea-level change: is it necessary to account for this when interpreting the GRACE data? If not, then please justify this by roughly quantifying the magnitude of the signal due to ocean mass change.

The ocean mass change over the ice shelves was assumed small compared to the mass changes associated with the solid earth uplift, so these effects were not directly treated in this study. To verify this claim, we computed the ocean mass change over the entire 400km extended integration area, using the ocean mass change predictions

from Bamber & Riva (TC, 2010), which includes the effects of self-gravitation. The total mass change over the entire Antarctic coastal region (i.e., within the 400km zone) was only 4.5 Gt/yr, confirming the impact is small. This information will be added to the revision.

4. A follow-on comment: GRACE data up to 400 km offshore of Antarctica are used (page 3514, lines 3-4). Given that mass change will take place over the oceans, please clarify whether you need to account for this contribution.

See previous comment.

5. Page 3502, line 22: please clarify what is meant by"...the GRACE and solid earth densities were used..."; at the moment it reads as if you are referring to 'GRACE densities'.

The sentence was meant to say that the GRACE trend data, along with the solid earth densities, extend out into the ocean, whereas the FDM/SMB and ICESat data only extend to the grounding line. We will reword this in the revision.

6. Page 3503, line 8: please briefly explain the difference between 'unconstrained' and 'regularized' solutions when discussing the GRACE data.

Agreed.

7. Page 3503, lines 12-13: please provide a reference or more detailed explanation to accompany the comment that "...the secular trends that are removed from select zonal coefficients were restored, as these rates are believed to mostly represent the effects of GIA."

This is a technicality of the GRACE processing. In the publicly released gravity field models by the CSR and GFZ, they remove a secular trend from some low-degree coefficients, such as C20, C30, etc. (the exact coefficients depend on whether the fields use RL04 or RL05 processing). We restore this, because it's suspected the main cause of this trend is GIA, which is what we're interested in observing. A reference to the GRACE L2 processing handbook, as well as the Cheng & Tapley 2004 paper (linking the secular rate to GIA), will be added for readers wanting to know more about this.

8. Page 3504, lines 23-28: it is stated that additional Gaussian smoothing (approximately 200 km) was applied to the unconstrained GFZ solutions, however, on page 3502, line 6, it is stated that, in general, 400 km Gaussian smoothing is applied to the various components of equation 1. How does the additional smoothing of the unconstrained GFZ solution affect the magnitude of the GIA uplift rates that are deduced for this solution?

Originally, the initial 200km smoothing applied to the GFZ models was done to better visualize the trend maps from these solutions. We agree that applying an additional 400km smoothing to these solutions is not consistent with how the other solutions are treated, so in the revision, only a single 400km smoothing will be applied to the GFZ solutions. Doing so only changes the total mass change estimates by a few Gt/yr, and does not affect the interpretation of the results.

9. Page 3506, line 14: 'total sum' rather than 'total distance'?

Agreed.

10. Page 3506, line 27: I could not find the Urban et al. (2013) article, which seems to include important information regarding the analysis of the ICESat data. Please ensure that this article is available by the time this article is published, or provide the necessary information here.

Agreed. The intention was to have both papers submitted simultaneously, but this companion paper will likely not be available in time to include it as a reference here. The editing criteria used for this will be provided, as well as a separate reference to the EVUW calculations.

11. Page 3507, lines 1-2: Is this cut-off value reasonable, e.g. can you give an example

of the maximum rates of glacial thinning or ablation processes that have been observed in Antarctica? How many pairs had to be rejected according to this criterion, and is there any regional clustering to the rejected pairs?

Earlier studies of individual glaciers, e.g., Pritchard et al (Nature, 2009), suggested maximum thinning rates to be 9 m/yr in the ASE and AP, so the 12 m/yr cutoff we used seemed a reasonable balance between true possible elevation change and the desire to remove spurious data points. Most of the edited data points are in regions of steep topography (typical for altimetry), although they can occur in all locations for a number of reasons (clouds, processing errors, etc.). Of the original 162M total possible OFPs over Antarctica, the dh/dt cutoff criteria removed approximately 7M OFPs, or 4%.

12. Page 3508, line 9: The phrase "...one or both..." is a little confusing since it is not clear to me how an overlapping footprint could only overlap with one laser shot. Please clarify.

The ICESat laser shots can have different sizes and orientations depending on the campaign, so it's possible that the fooprint of one campaign would fit entirely within the footprint of another campaign. In this case, the first footprint would have 100% overlap, while the larger footprint would have, say, 75% overlap. The overlapping criteria for the bias estimates means that at least one of the two footprints that make up the OFP must be overlapped by at 80% of its area. Note that an OFP always involves two footprints.

13. Page 3509, line 25: please clarify the size of a 'grid cell'.

Agreed. All grids were created or resampled to 20 x 20 km grid cells.

14. Page 3510, lines 9-12: Please give some detail as to how the densities are assigned in the case that there is a positive height difference, i.e. how rho_surf is calculated?

This was also raised by the first referee. The surface densities are based on a static density profile developed by Kaspers et al (2004). We intend to include a plot of these densities in the SM for those interested.

15. Page 3510, equation 3: is there any difference between h_dot_GIA (left-hand side of equation 1) and h_dot_rock (left-hand side of equation 3)? If there is, then please define the difference.

No, there is no difference between the two. We will make this consistent in the revision.

16. Page 3510, equation 3: Not all of the mass change observed by GRACE that is then associated with GIA will be due to solid Earth uplift; there will be a small contribution from the deformation of the geoid, i.e. a change in sea surface height. Is this factor considered by the authors (perhaps folded into the value chosen for the density of the underlying rock?) or is this effect negligible?

Indeed, the change in sea surface height due to GIA is accounted for by the reduced effective rock density over oceanic areas.

17. Page 3510, equation 3: would it be possible to conceptually describe the terms included in this equation to aid understanding of your methods? E.g. I think the equation essentially says that the mass rates due to model-predicted SMB and firn compaction, and any mass change due to dynamic ice thinning or an underestimation of SMB, are subtracted from the observed rate of mass change, leaving the rate of mass change due to GIA, which is converted into an uplift rate by...

We can certainly add a few lines of text explaining the methodology of Eqn 3 in words. What you have described is essentially the core of the approach. The mass changes due to GIA are converted to rates by dividing by the densities.

18. Page 3513, line 5: Does the value of 50 Gt/yr refer to the potential bias if uplift rates were incorrect by 1mm/yr over the whole continent? The spatial extent of this statement is not clear.

Yes, it was meant to represent the mass change due to a hypothetical 1mm/yr bias

over the whole continent. We'll clarify this in the revision.

19. Page 3513, lines 22- 23: '...contributions' of what from the northern hemisphere?

The contributions mentioned refer to the long-wavelength GIA signal driven by growth and melt of the large ice sheets in the northern hemisphere (over North America and Scandinavia). These far-field processes would normally induce a near continent-wide bias on the rates for Antarctica, but since we are calibrating the empirical rates to the LPZ, their influence is largely removed.

20. Please make sure that you differentiate between the GIA solutions derived using the methods outlined in this paper, and previously published GIA models; both are referred to as 'GIA models' at various points in the manuscript.

Agreed. This was also mentioned by the other referees. We will go through the text and make sure any reference to the GIA rates derived from the data combinations are referred to as "empirical GIA rates."

21. Page 3514, line 4: please explain how '...the GIA mass change rates were obtained'.

The GIA mass change rates are obtained by multiplying the uplift rates derived from Eqn 3 by the density of rock.

22. Page 3515, lines 20-21: why are higher uncertainties 'most expected' in the Amundsen Sea Sector and Wilkes/Adelie Land?

These are the regions where the uncertainties in the SMB/FDM/ICESat data sets are the highest (Figs 4 and 7), which would translate into higher uncertainties in the GIA rates as part of the error propagation.

23. Page 3515, lines 25-27: The English is a little odd in the second-to-last sentence of section 5.1.

We will look into this and see if an alternate wording can be found.

24. Did the authors consider using the updated version of the IJ05 model, as described in Ivins et al. (JGR, 2013)?

We are aware of IJ05_R2, but this model was only recently published and available. Furthermore, the new model includes GPS displacement information, which we thought might complicate the comparisons to the GPS ground stations. Future work on this will certainly look at this updated IJ05 model, as well as other recently developed models.

25. Page 3518, line 14: when you say '...not typically predicted...' please specify whether the empirical estimates are larger or smaller than previous estimates.

We meant that the uplift in the ASE is much larger than predicted by most models. We will clarify this in the revision.

26. Page 3519, paragraph starting on line 21: it should probably be noted that some of the disagreement between forward-modelled and empirically-derived uplift rates may be due to the fact the forward GIA models do not typically consider ice-load changes during the last 1000 years. The response to any ice-load changes during this period would be classified as a GIA response, and may dominate the present-day signal.

We can note this in the text. It also serves to highlight the contribution this study might have in better understanding these unmodeled ice-load changes.

27. Table 2: Please clarify whether the LPZ bias rates listed in columns 3 and 4 are added onto, or subtracted from, the raw uplift rates. (ditto for Table 4)

Agreed. The LPZ biases are substracted.

28. Table 2: I find it very surprising that the total mass change rates derived from the GRACE data are so varied (columns 5-7) yet the ice-mass rates lie within 2 Gt/yr for all the GRACE solutions (columns 11-13). All of the uncertainty seems to be pushed into the empirically-estimated GIA rates. There is a brief comment about this in the discussion, but I think it warrants a closer analysis of your method, which does not

seem to permit any flexibility in the ice-mass rates that are derived.

The reason the ice-mass rates are all relatively similar has to do with the fact that we are only using a single altimetry/climate model combination. As a result, the ICESat/FDM surface height and SMB input is static, unlike the GRACE solutions, which include ten different processing techniques. It would be better to test a range of different altimetry and climate data sets, but this is currently beyond the scope of the current paper.

29. Please clarify in the caption of Tables 3 and 4 that the LPZ bias has been applied to all of the results listed in these tables.

This might make the captions excessively long, but we will consider it. At a minimum, this will be clearly stated in the text.

30. Figure 5: Please clarify in the caption that the LPZ is outlined by the thick black line. The edge of the Ross and Filchner-Ronne Ice Shelves should be outlined. The caption for Wilkes/Adelie Land (WA) is incorrect on the figure (AW).

Agreed.

31. Figure 6: The cyan and blue lines are labelled differently in the caption and in the key on the figure.

Agreed. Thanks for catching this.

32. If there is space, it would be useful to have 'Firn Densification Model' written out in full in the captions of figures 6 and 7 to save searching for the meaning of FDM back in the text.

Agreed.

33. Figure 9: Has the LPZ GIA bias correction already been applied to the rates plotted in this figure?

Yes, it should be assumed that all empirical rates from the combination will have had the LPZ bias correction applied. This gets back to the earlier comment, so we will make the point clear in the text.

34. Does the magnitude of the empirically-derived GIA-associated uplift rates depend on the wavelength used to smooth the GRACE data?

The smoothing of the GRACE data helps reduce the noisiness of the solutions, but undoubtedly attenuates the signal magnitude since the smoothing operation redistributes the signal over the 400km radius. That said, the geolocation of the peak signals should not be significantly affected, and the total mass change should remain the same after smoothing (the Gaussian filter simply redistributes the signal, so while the spatial resolution of the smoothed signal has changed, the total mass has not).

35. Figure 11: Please state what is actually plotted here in the caption (presumably uplift rates).

Agreed.

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