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Comment

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 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.

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

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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.

On pg 3513, Ins 20-21, we stated that the "primary consequence for using the LPZ in this way is that the GIA solutions created become regional to Antarctica. . .," so we think this already addresses the concern. Furthermore, in response to the comments of the second referee, we will add the following text in the same discussion to clarify the impact our choice of calibration zone may have: "Second, if any genuine GIA over the LPZ does exist, 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."

P3507L15ff: Have the authors tried to apply the Centroid-Gaussian correction to the

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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.

We have not applied the C-G correction to the ICESat data, as this is a relatively new development. That said, because we calibrate our biases to the LPZ, we suspect that the correction will have a very small effect on our results. As we understand it, the C-G corrections essentially manifest themselves as campaign dependent biases (Borsa et al, 2013, TCD; Tbl 1), so if we used the same approach to estimate the campaign biases with the new corrections, the values of our biases would likely change, but the final dh/dt values probably would not be affected. And it is these dh/dt values that are actually used in the combination.

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.

There are many different geocenter estimates available in the literature, and all are created using different methodologies. As a result, there is very little consistency between the datasets, with large variations in phase and amplitude. Early in the study, we experimented with using different sets of degree 1 coefficients, but soon realized that the range of values was too large to be of any practical use. This is one of the driving factors for calibrating the solutions in the LPZ. We admit that using the LPZ as a calibration area can only remove part of the effect of geocenter motion, but we still believe that this is better than relying on very uncertain independent estimates.

Taking the Cheng et al (2010) and Swenson et al (2008) coefficients as an example,

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Fig. 1 of this response plots the trend over Antarctica derived from the two solutions. Note the substantial difference in phase and amplitude. In addition, the two solutions differ in the way that GIA is treated. . Swenson et al. 2008 remove GIA for the estimation of the degree-1 coefficients, whereby SLR is observing "full-geocenter" motion without any GIA corrections applied. As a result, these two time series are not directly comparable.

When we use either of these deg1 solutions in the combination approach, and calibrate them over the LPZ (i.e., remove the LPZ bias), the end result changes very little, as shown in Fig. 2 of this response, even when considering the difference between EA and WA. In short, we think the LPZ bias correction does a good job of removing the mm/yr-level biases present in the deg1 coefficients, and we intend to include this analysis in the supplementary material.

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 / m³ 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.

Indeed, as suspected by the referee, the chosen rock density values were computed after running a few forward models of global GIA (ICE-5G, VM2 and IJ05+Northern Hemisphere contribution of ICE-5G). Density estimates were obtained from the ratio between apparent surface mass change (equivalent water heights derived from SH coefficients of the gravity field) and uplift rates. The physical reason for the lower density value in coastal areas (in particular under the largest ice shelves) is double: first of all, GIA also induces sea level changes (meaning that geoid changes over the ocean will

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be due to the movement of both rock and water masses), secondly, ocean loading will affect the evolution of GIA itself, where the latter has been extensively discussed by Simon et al. (2010, J. Geod., 84:305-317).

The 100 kg/m³ uncertainty was 1/3 of the difference between 4000 and the average value of 3700 proposed by Wahr et al. (2000, JRG 105, B7), which we thought was a reasonable measure of the uncertainty. Ultimately this results in a minor change to the final GIA estimates and the resulting uncertainties. It's physically more correct, which is why we included it, but in practice its influence was very small. For example, the usage of a constant rock density equal to 3700 kg/m³ instead of spatially varying density (3700-4000 kg) yields a difference in estimated mass change equal to -7 Gt/yr (for the CSR RL04 DDK3 case tested).

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.

Yes, the 400km filter is applied to all components in Eqns 1 and 3 to ensure that are all at the same spatial resolution. We will review the text and make sure this is clear.

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-stripping 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.

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

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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.

The de-stripping filter was not applied to the altimetry/FDM models, since the correlation effects the filter is intended to remove is particular only to the GRACE solutions.

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?

This was investigated earlier in Gunter et al (2009) using data sets that only extended to 2007, and that analysis suggested that there could be some sampling problems due to the distributed ICESat measurement campaigns. So, earlier in the study, we looked into whether this would have a significant impact. It turns out that the full ICESat time series has sufficient sampling to adequately recover the long-term surface height trend. The table shown in Fig. 3 of this response illustrates the difference between using the full GRACE/SMB/FDM time series versus a solution using only data available over the ICESat campaigns. As can be seen, the differences are small.

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

The difference in the Atmosphere and Ocean De-aliasing Level 1B (AOD1B) product between RL04 and RL05 is an improved de-aliasing of non-tidal ocean mass variability, whereas the atmospheric part is exactly the same in both releases. Therefore, by not removing the oceanic de-aliasing product from L1B data (or by adding it back to L2), we would not take any advantages of AOD1B RL05. And adding back the ocean

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component would re-introduce these non-tidal signals to the shelf areas, and further complicate interpretation of the results. So while there may be known issues with the AOD1B product, we feel it's beyond the scope of the current study to investigate these.

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.

\dot{m}_{firn} (called SMB in Fig. 8) was derived by calculating the SMB cumulative anomaly relative to the reference period of 1979-2010. For Fig.8, GRACE-SMB means $(\dot{m}_{GRACE} - \dot{m}_{firn})$, and represents the mass change in GIA and the ice layer, while $(\dot{h}_{ICESat} - \dot{h}_{FDM})$ represents the height change in GIA and the ice layer. Regarding the added explanation of RACMO2, the following expanded text will be added at p 3509, In 10:

"... , and firn compaction. To account for these, we use output of the RACMO2 regional atmospheric climate model, driven by ERA-Interim atmospheric reanalyses for the period 1979-2010 and run at a horizontal resolution of 27 km (Lenaerts et al., 2012). In conjunction with the time-varying estimates of SMB of RACMO2, which is the sum of mass gains (precipitation) and mass losses (surface runoff, sublimation and drifting snow erosion) at the ice sheet surface, we use a firn densification model (FDM, Ligtenberg et al. (2011)) forced at the surface with 6 hourly climate output of RACMO2. The FDM provides temporal surface height changes due to SMB variations, liquid water processes (snowmelt, percolation, refreezing and runoff) and firn compaction. Figure 7..."

B6. P3511L03: I understand that residual height changes exceeding the some combined ICESat & RACMO uncertainty threshold are attributed either to ice dynamics (917 kg/m³) or underestimated snowfall (except for ice-dynamic thickening of the Kamb

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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 $2 \times \sigma_h$?

The surface densities were not shown in the paper. . .the reader currently needs to look into the original Kaspers et al (2004) paper, or the Gunter et al (2009) paper to see a plot of these values. This has been commented on by the other reviewers as well, so we intend to include a plot of these densities in the SM of the revision. As far as the sensitivity of the solutions to the σ_h threshold is concerned, the impact is relatively small. The table shown in Fig. 4 of this response summarizes the impact on the GIA estimates when using different σ_h thresholds, using the case involving CSR RL04 DDK3:

The $2 \times \sigma_h$ threshold was chosen as a balance between the FDM and ICESat surface heights. A low σ_h threshold puts more emphasis on the ICESat-derived heights, while a high threshold puts more emphasis on the FDM.

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.

We certainly agree that elastic effects are an important consideration when utilizing GPS displacements for GIA studies, and that having more accurate elastic models would improve the interpretation of the GPS comparisons. The statement in P3511L20

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is regarding GIA uplift rates, which generally behave secularly over decadal timescales. We chose to use the mass flux information from Rignot et al (2008) because: 1) this would provide a first-order approximation of the elastic rates, 2) the data set was derived independently, and 3) the same corrections were applied to the rates shown in Thomas et al (2011) and Whitehouse et al (2011), allowing for more direct comparisons. In order to improve upon the corrections for the elastic effects, the mass load from an independent source (i.e., not ICESat or GRACE, as these are used in the combination) should be well known, which is certainly not the case for most of Antarctica, particularly for the northern Antarctic Peninsula. This is why we provide WRMS values for the comparison between estimated/modeled GIA and GPS rates for 35 GPS stations, being consistent with Thomas et al. (2011) and Whitehouse et al. (2011); and for 29 GPS stations, among others excluding GPS stations in Graham Land.

To clarify the application of the elastic corrections, we intend to modify the text starting on p 3511, ln 14, with something similar to the following:

"The processing of the GPS data followed the approach of Thomas et al. (2011), and includes data from both campaign and permanent stations. Elastic deformation effects were accounted for using the model of Thomas et al (2011) based on ice mass flux observations (Rignot et al 2008) with the exception of sites in the northern Antarctic Peninsula where the elastic model does not accurately reproduce near-field displacements (Thomas et al. 2011). In this region, we therefore follow Thomas et al (2011) in adopting velocities for the period before 2002 as upper bounds on millennial-scale GIA. At the remaining sites the elastic corrections are generally small (typically <0.3mm/yr) due to their location in the far field of the dominant ice mass changes within the Amundsen Sea Coast and the northern Antarctic Peninsula. Ice mass loading varied non-linearly over the GPS data period and this is not reflected in the elastic model, but for sites outside the northern Antarctic Peninsula this is largely due to accumulation fluctuations, and they generally induce small and largely site-specific biases in the elastic model."

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

Agreed. This was also noted by one of the other reviewers.

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. Whether these are close to the full signal or not depends on the validity of the assumption $(\dot{h}_{GIA_over_LPZ}) \neq 0$.

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.

As mentioned earlier, we recognize in the text that the calibration of the solutions to the LPZ does indeed make the results regional to Antarctica, but that we think this approach is justified given the uncertainties associated with other unknown biases (see also response to comment 1 of referee #2).

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?

All components used in the combination are smoothed at the same 400km level (P3502L6), and integrated using the extended boundaries. This is done to recover the mass signal that gets distributed over the ocean during the smoothing operation. We use the maximum resolution of each GRACE solution, and so do not contribute any additional omission error in our processing. If we were to use only the 60x60 coefficients

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of a native 120x120 solution, then we would introduce omission error. But smoothing a 120x120 solution only redistributes the mass across a different spectrum, and does not remove any mass signal. As such, our extended integration zone eliminates the need to scale the resulting GRACE solutions.

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.

The cm-level campaign biases in ICESat create dominant errors if ignored, so some methodology is needed to estimate their magnitude. Other techniques in the literature exist, but we feel the LPZ offers an ideal calibration zone for this because: 1) the LPZ is one of the driest places on Earth, and gets very little precipitation and, hence, surface height change, 2) it is a large area, which helps improve the reliability of the results, and 3) it is local to Antarctica, whereas many other techniques are based on regions in other parts of the world, and 4) the density of ICESat groundtracks is highest in the polar region, meaning many more observations can go into the estimation of the biases.

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?

The explanation and application of the biases is something all three reviewers have commented on, and we intend to add a separate discussion on these in the paper to make sure this is clear. In short, the LPZ-bias is not applied to the models because these models have various assumptions and parameters that go into their creation that may be unintentionally removed with an LPZ-bias correction. The LPZ-bias was

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needed for the empirical GIA rates to remove the influence of various systematic errors in the data sets used. The systematic bias removed when comparing to the GPS rates was done to make sure the comparisons were done in the same relative network, so that errors such as reference frame differences didn't dominate the WRMS calculations. The offsets with the GPS networks were determined using non-uniform weights (this is not stated in the original text, but will be added in the revision).

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.

[A good suggestion.](#)

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.

[Please see earlier response to B12 and A1.](#)

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

[The calibration to the LPZ is considered part of the processing that goes into all of the empirical solutions, so its inclusion is implied. Adding this to the table caption might make it too long, but at a minimum we can restate this in the text.](#)

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.

[See previous response.](#)

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

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7, C2495–C2511, 2013

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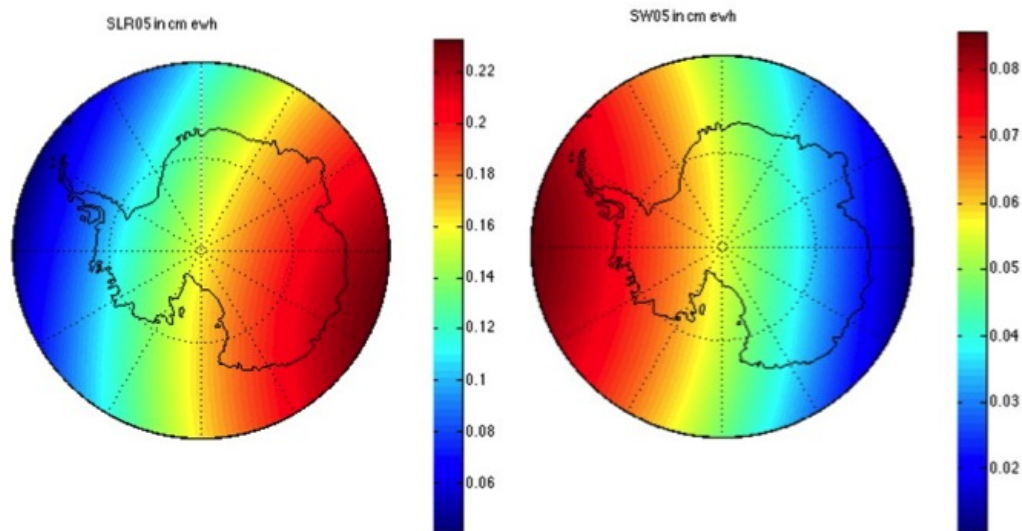


Fig. 1. Geographical comparison of degree 1 mass change trends, in units of equivalent water height, for the coefficients by Swenson et al (2008), right, and Cheng et al (2010), left.

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Solution	LPZ bias		Mass Change from			Derived GIA (Gt/yr)			GRACE - GIA		
	GIA	GRACE	GRACE (Gt/yr)			(Gt/yr)			(Gt/yr)		
	mm/yr	mm/yr EWH	EA	WA	AIS	EA	WA	AIS	EA	WA	AIS
CSR RL05 DDK5, deg1SW05	1.9	1.7	42	-78	-36	37	27	64	5	-105	-100
CSR RL05 DDK5, deg1SLR05	2.3	3.0	40	-83	-43	35	22	58	5	-105	-100

Fig. 2. Impact of removing alternate degree-1 coefficients on the final combined solutions.

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Solution	LPZ bias		Mass Change from GRACE (Gt/yr)			Derived GIA (Gt/yr)			GRACE - GIA (Gt/yr)		
	GIA	GRACE	EA	WA	AIS	EA	WA	AIS	EA	WA	AIS
CSR RL04 DDK3	1.7	1	53	-66	-13	48	40	87	5	-105	-100
at_ICESat	1.6	0.3	50	-64	-14	46	42	88	4	-106	-102

Fig. 3. Comparison of results when using the full GRACE/SMB/FDM time series versus a solution using only data available over the ICESat campaigns

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	GIA in Gt/yr			LPZgia
	EA	WA	AIS	
1-sigma	47	42	90	1.8
2-sigma	48	40	87	1.7
3-sigma	48	38	86	1.6
4-sigma	50	37	88	1.5

Fig. 4. Summary of the impact on the GIA estimates when using different sigma_h thresholds, using the case involving CSR RL04 DDK3

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