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The Cryosphere Discussions

Interactive comment on "Antarctic ice-mass balance 2002 to 2011: regional re-analysis of GRACE satellite gravimetry measurements with improved estimate of glacial-isostatic adjustment" *by* I. Sasgen et al.

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The paper by Sasgen et al. addresses an important issue – the present-day glacial isostatic adjustment of Antarctica and its effect on GRACE estimates of ice mass balance. New GIA forward models have been, and are being produced, but these are not perfect and so empirical (inverse) estimates of present-day uplift and geoid rate are useful. This new paper makes a step forward in empirical approaches by constraining the solution using new GPS uplift data as well as GRACE data and a suite of forward GIA models based on a suite of Earth models and three (old) ice histories.

I have some comments that I hope the authors will consider as I think they will help improve the work, as well as some editorial notes I picked up as I read the manuscript.

General comments:

1. 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.

The treatment of potential systematic error as random, notably the internal-leakage (from one basin to another, due to spatial resolution of GRACE not allowing it to capture large changes with small spatial scale), would benefit from some more explaining in that context. Do reasonable sub-basin distributions of mass change really exhibit as leakage into other basins which is random around zero for each basin? I.e., if you consider fast flowing areas or areas of greatest accumulation as being those most likely to be changing does this leakage into other basins really become normally distributed around zero in the other basin solutions? There are other scenarios (like even distribution of mass changes across each basin) which are not physically reasonable, of course.

2. 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 al TCD. Did the authors note the any apparent mass jumps as identified elsewhere by Duan et al.?

3. 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.

4. 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?

5. 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?

6. 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.

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

8. 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.

9. 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. The relative sea level change near the Ross Ice Shelf is constrained over the GRACE period by the tide gauge at Cape Roberts.

Editorial notes: P3705L7: in noting it is an improved GIA estimate I think the authors should explicitly state "empirical" here and throughout the paper. Non-specialists could be confused between this, which uses conventional GIA model code, and those that are conventional GIA forward models. L11: needs a reference epoch stated for the trends L13 should be "largest *ice* mass"

P3706L7: Add the significant uncertainty associated with input accumulation and some uncertainty in converting surface velocity to depth-averaged velocity. L15: Did the authors leave out lithospheric thickness for a reason? 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

P3707L1: as forL15 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. L15: ICE-5G importantly has the largest bulge on the Siple Coast, so this statement does not always hold true 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.

P3708L18: perhaps start as “A priori, this involves . . .”

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

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. L20: empirical estimate

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.

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) 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?

P3713: eq 2: does the first FtCˆ-1F need to be (FtCˆ-1F)ˆ-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. 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)?

P3714L7: “permuting” may be better as “iterating”

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

P3717L8: is it Maude or Maud?

P3718L17: state reference epoch again L29: cf Groh et al who have strong Amundsen Sea GIA P3719L11: worth noting that it is not as negative as the -200Gt/yr (for 2006) of Rignot et al 2011 either.

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