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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.

Anonymous Referee #1

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Review of the paper "Antarctic ice-mass balance 2002 to 2011: regional re-analysis of GRACE satellite gravimetry measurements with improved estimate of glacial-isostatic adjustment" by Sasgen et al., submitted for publication to The Cryosphere.

This paper presents a new estimate of Antarctic ice-mass balance based on GRACE data, where the major innovation with respect to previous studies is represented by a GIA estimate based on the simultaneous inversion of GPS and GRACE data. Their ice-mass balance estimate for the period 2002-2011 amounts to -103 +/- 23 Gt/yr, which in itself is not a surprising number and in line with other recent estimates (e.g., IMBIE results presented at recent meetings). However, the contribution of GIA is only 48 +/- 18 Gt/yr, which is indeed a low estimate. The only study that presents similar figures for the whole AIS is the recent paper by King et al. (2012), though with a significantly different partitioning of GIA between the WAIS and the EAIS that I will discuss later.

The paper is nicely written and the results are relevant to most scientists interested in the past, present and future evolution of the Antarctic Ice Sheet, therefore it certainly qualifies for publication in The Cryosphere. However, in spite of the clarity of the language and the apparent rigor in describing the methodology, I have found it difficult to judge the robustness of the results and their actual dependence on the input models and datasets. Therefore, I recommend a major revision, where the following major points should be clarified.

Major points

1) 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.

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2) 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.

3) 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).

4) 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. 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? 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. 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. 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).

Additional comments

5) 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.

6) 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.

7) 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 Norther Hemisphere.

8) 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?

9) 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.

10) 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.

11) 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).

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

Interactive comment on The Cryosphere Discuss., 6, 3703, 2012.