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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 #2

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The main and most important goal of this work is the presentation of a new empirical Glacial Isostatic Adjustment (GIA) model obtained using some of the most recent GPS time series and GRACE data. Empirical GIA models for Antarctica are very important especially as comparison with existing GIA models and as constraints for future full inversion of viscosity and ice histories for GIA prediction. The authors present an alternative and very interesting way to use GPS data to constrain their GIA pattern regionally.

However I have some concerns about the data treatment and the inversion procedure.

1) Do the authors consider the effect of the undetected geocenter motion in GRACE data? The geocenter motion is instead detected by GPS. In Sasgen et al. (2010) method (used in this work) they minimize the difference between the forward model and the observed gravity field where a min cutoff degree j=7 is used in order to reduce the influence of the far-field signal. However this means that in their forward model the low degrees are not constrained and they solely depend on the choice of the a priori information. Since the average mass variation at basin scale, especially for the large basins, largely depends on low harmonic degree (as a low pass filter effect), the authors' forward model in some cases could not be able to reproduce the correct average mass variation for large regions.

For the whole Antarctica the degree one in particular plays an important role (Wu et al. 2011), so the authors should show that their forward model contains a degree 1 (and other lower degrees) compatible with the observed ones. At least they should discuss how much their results are sensitive to plausible variations in the low degrees of their forward model.

2) Recently it has been shown that the dealiasing product for GRACE data had a problem between 2009 and 2010 (Duan et al. 2012) which can affect the trends based on GRACE data for some basins. Are the authors considering this issue? How much can their final results be affected by such a systematic bias in the GRACE data?

3) The inversion procedure needs a clearer explanation. I understand that in the inversion of section 3.2 the authors are using a sort of GRACE residual associated with GIA obtained after removing other signals (section 2.1). Is that correct? Why extracting this GRACE residual is necessary for the inversion? Why is it not possible to perform a simultaneous inversion for GIA and most the signal associated with present-day mass changes in Antarctica? It is not clear if the GPS are coherently corrected for the same elastic signal used for GRACE.

The authors present the statistical distribution of differences between the GIA obtained

with their many combinations, and this is interesting to understand if there are systematic biases of some kind (see M. King comment on this work); however I think that it would be also very useful to show the spatial pattern of differences between the authors' best model (AGE1) and the GRACE residual that they used for the inversion, and possibly superimposed also the difference in GPS observations and predictions. Did the authors calculate some sort of errors for the AGE1 empirical model? Table 2, shows some errors at basin level related to the GIA correction. Are those the AGE1 errors? Is it possible to see the spatial distribution of those errors?

The figures 2b, 2c, 2d do not contain any remarkable info with respect to others previously published, and they are not very useful for the manuscript discussion either. Instead a picture with the resulting GIA pattern in terms of mass equivalent (on a proper scale) together with the GRACE residual used for the inversion and the above mentioned differences would be much more interesting. Moreover the geoid representation is not the most representative in works where mass distributions are involved and where low degrees are probably neglected. A mass equivalent (w.e.) should be used instead. The authors should describe the inversion procedure using also a clearer mathematical representation. The expression (1) is not sufficient and it doesn't represent clearly the possible combinations involved in this process, i.e. it doesn't contain all the necessary indexes.

It is not clear in which way the parameter space is connected to the region index. In general each loading ice history + viscosity combination produces a field of u_p(Omega) and e_p(Omega), where p stands for a specific combination of the input parameters. Those fields are not dependent on local parameters, so how do the authors obtain a locally dependent GIA output? Are the authors varying the loading history locally , i.e. for each region r=1,..,5 with the others fixed? Does this local variations produce the u_r(Omega) and e_r(Omega) which are dependent on the r macro-regions loading histories? 3a) Why do the authors use only one GRACE value as constraint in the inversion? There is no explanation for that and this makes the procedure more

difficult to understand.

4) Why are the authors computing the GIA at degree 170 when GRACE resolution is at degree 60? And the GPS data are few and sparse so the spatial resolution that can be really constrained is very low. This could make the inversion problem ill posed, unless the authors are using other information to constrain the problem.

5) Have the authors tested the sensitivity of the GIA inversion with respect to a change of time frame for the trend extracted from GRACE (and the GPS)? How does this impact on the final result?

Please, see the other comments and issues in the annotated manuscript.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2235/2012/tcd-6-C2235-2012-supplement.pdf

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