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Discussions

Interactive comment on "Simultaneous solution for mass trends on the West Antarctic Ice Sheet" *by* N. Schön et al.

Anonymous Referee #2

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The manuscript by Schon et al entitled "Simultaneous solution for mass trends on the West Antarctic Ice Sheet" explores a technique in which multiple satellite (gravimetry, altimetry) and in situ (GPS) data sets are combined in an effort to generate simultaneous estimates of ice mass change, GIA, and SMB.

In general, the author's efforts to derive a statistically rigorous combination of potentially disparate data sets is a worthwhile and challenging goal. Much of the methodology was already developed in an earlier work by Zammit-Mangion et al (2013), with the present work devoted mostly towards comparison and validation of the results with other studies in the literature. The results of the paper differ – sometimes significantly – from the other comparison studies, particularly for the GIA and SMB results. The authors make it clear that the methodology is a work-in-progress, so it's difficult to assess

whether the differences seen in these comparisons are due to genuine improvement or due to lack of refinement in the technique. Below is a list of items that I found while going through the paper.

Major concerns include:

1) Very little discussion on error analysis is provided. I think this should be addressed, particularly since the approach is statistically driven, so I would think error estimates for all of components would be available. The reader doesn't get a feeling for the errors for most of the input data sets and, other than the final ice mass loss values, none of the estimated components. Sigmas for the altimetry trends are given in Fig 2, but what are the uncertainties on the input GRACE mascons, the final SMB estimates, the final GIA rates? What is their spatial variation? Without this, it makes interpretation of the results and assessment of the comparisons more difficult (e.g., p3008 and p3009).

2) Related to the first item, I would have liked to have seen more discussion on the influence of the various constraints that are employed (dh/dt error cutoff, static surface density, length scales, ice velocity constraint on elevation rates, etc.). I suspect that these have a significant influence on where the mass change is allocated within the framework, particularly the ice velocities constraint outlined on p.3004. What if a different constraint is used? What if no constraint it used?

3) The resulting GIA uplift rates seem very smoothed...much more than the 100km smoothing constraint mentioned on p2004 ln17 would suggest. Please comment.

4) A great deal of detail has been skipped regarding the methodology. The reader is not really left with a sense of how the whole system works. I realize you can't reproduce everything from earlier Zammit-Mangion et al paper, but I believe more can be done to describe the methodology. For example, the parameter layer isn't explained. And it's unclear how you go from the three layers to the FE mesh of the different processes to a final "statistically sound" result. How are you able to effectively separate the four different processes discussed in the Results section. Please consider adding some

more explanation, figures, etc. in this section. At the moment, the methodology is very much a black box.

5) Comparisons with ice core data is presented in support of the SMB results derived. Given the variability seen in the SEAT cores, it's difficult to accept any conclusions from the MEDLEY result, which represents just a few cores. What if the MEDLEY trend was an anomaly like SEAT 10-5? The Ligtenberg et al 2011 paper, which discusses the FDM derived using RACMO, made comparisons with 48 ice cores and looks to show good agreement with these cores. Many of these cores were in the WAIS, so there looks to be many other ice cores in the region that could be used to validate your model.

6) The abstract suggests it would be easily scalable for the whole of Antarctica. If so, then why was only the WAIS explored? All of the GIA/SMB/ice-mass change comparison studies (e.g., King et al, 2012, Shepherd et al, 2012, etc.) cover the AIS, so the same comparisons could be made. It would have made for a more complete comparison.

There were also a handful of other items that the authors may wish to consider:

1) p2997, ln14: the Velicogna & Wahr is a bit dated, and their later papers show a lower proportion of GIA error. Consider updating reference.

2) P2998, ln5: What if the SMB models have more than just systematic biases in them? For example, if the SMB variations themselves are modeled incorrectly (over/under estimated), then this would necessarily impact the spatial relationships used in the combination. This gets back to the earlier comment regarding the error analysis.

3) p3000, ln16: I assume these are formal errors on the trend. These tend to be optimistic, so I would recommend in the future applying some sort of error adjustment (bootstrapping, scaling, etc.) to make them more realistic.

4) p.3001, ln2: Considering you are using a RL04 GRACE mascon solution, which is

a now dated release, it would have been very insightful to see how the results were affected when only the GRACE component was changed to, say, the CSR RL05 fields. In addition, how do the mascons relate to the FE mesh? The mascon discs won't be aligned with the mesh triangle boundaries, so how is this treated (if indeed it's even a problem)?

5) p3001, ln 20: The concern here is that the correlations would be more accurate if the mass loss was only due to surface mass changes, but a considerable amount of the observed mass change is related to GIA, which may have a different spatial signal. Plus, you're correlating mass variations using volume/height estimates. Most areas will have some correlation, but the degree of correlation will certainly vary, and introduce error. If this correlation between mascons is important, which I assume it is, it would be useful to see a more in-depth treatment of the error from the altimetry-based correlation, and its potential impact on the solution.

6) p.3001, ln23: What do you mean here by "averaging strength"?

7) p.3001, ln26: Should read "Thomas et al (2011)" instead of "Thomas and King (2011)". This occurs in other places as well.

8) p.3006, ln9: It's not completely without prior information because the mass loss due to dynamics is constrained by the ice velocities described on p 3004, ln 16. This is equivalent to applying a type of forward modeling approach where all of the mass loss in essentially forced to go to regions of high velocity.

9) p.3007, ln25: If elastic effects are removed from the GPS displacements, wouldn't this impact your firn/elastic estimates?

10) p3008, ln4: Why wasn't ICE-5G or the new ICE-6G included in the comparison analysis?

11) p3008, ln 8: Isn't agreement with the GPS data nearly guaranteed since it is one of the input data sets?

12) p3008, ln21: Wouldn't this agreement be mostly attributed to the smoothed nature of the RATES and AGE-1 solutions? Neither solution predicts GIA rates above 4mm/yr. Comparing a smoother solution to one with higher resolution and signal variation (W12a, IJ05-R2, and Gunter14) is an apples-to-oranges comparison, since they have different spectral content. Also, discussions of agreement should be done with uncertainties involved. Are the differences statistically significant? What are the uncertainties of the various components?

13) p3012, ln5-25: It should be noted here that proper uncertainties of the input data sets is key towards generating reliable results. If, for example, the SMB estimates had 2-3cm uncertainties, then this would be reflected in the final estimate, i.e., the GIA rates would have large error bars. The same goes for the other data sets (altimetry, gravimetry, GPS). It's only a problem if the errors in the input data are too optimistic, i.e., lower than they are in truth.

14) p3012, ln26: The agreement with AGE-1 has been stated a couple of times, but when I visually compare the RATES and AGE-1 results, I don't see that much similarity, mainly because the RATES results have smoothed out most of the features. You might consider having a discrete color scale to better visualize the variations in Fig 7.

15) GPS station names are not shown in any of the other figures, so it's difficult for the reader to know which stations were used or excluded in the analysis. Perhaps the station names can be added in one of the figures (e.g., Fig 8?).

Interactive comment on The Cryosphere Discuss., 8, 2995, 2014.