

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

Anonymous Referee #1

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Summary:

This paper combines altimetry, gravimetry, and GPS data in a Bayesian least squares adjustment procedure to produce mass loss estimates for the West Antarctic Ice Sheet. The contribution of different processes (GIA, surface mass balance, ice dynamics, and firn compaction and elastic uplift) to the mass loss estimates are isolated and discussed.

General Comments:

The paper is well written and makes an advancement to the state of knowledge of the processes governing mass balance on the Western Antarctic Ice Sheet. The method developed appears to offer a powerful and flexible tool to assimilate future data (models

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and observations) into the estimation process. There are a few points which require further explanation and attention, however, as listed below in the specific comments.

Specific Comments:

- The process which is described is a least-squares adjustment on pre-defined state parameters. Specifically, these parameters include the 2003-2009 trend for GIA, SMB, ice dynamics, and firn compaction coupled with elastic uplift. I was curious why it was decided to estimate a trend, instead of, for instance, monthly changes. A trend is a good model for some of the processes (i.e. GIA), but perhaps is a poor choice to represent processes such as surface mass balance since there can be significant interannual variability. Could you please comment on this?

- Section 2.2. It is unclear exactly how uncertainties and correlations are prescribed for the GRACE data. Specifically, as you mention, it is quite important to accurately capture the spatial correlation statistics among the mascon estimates. I do not exactly understand the procedure used to calculate this. Why not explicitly use the posteriori correlations from the mascon solutions themselves? Additionally, in Luthcke's solution, a 2000 m elevation cutoff is dictated in the spatial correlation constraints. Is this explicitly considered in your analysis? I believe using the formal posteriori covariance matrix would be more favorable.

- I believe a nice addition to the paper would be a Table which succinctly captures the processes and methodology. For instance, in this table it would be nice to list the following: 1. Observations (altimetry, GRACE, gps, etc) 2. State parameters (trend for GIA, ice dynamics, etc) 3. Weighting information on the observations (both diagonal and off-diagonal components) 4. Assumed a priori information on the state parameters (both diagonal and off-diagonal components). This would allow the reader to quickly assess exactly what is being done and what assumptions are being made.

- Truly, there is some dependence on your solution with the choice of 3) and 4) in the above comment. Could you please remark on this, or provide some analysis on how

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sensitive the solution is to these choices? Additionally, there was no discussion of relative data weights. Do you weight any observations higher than others? What is the relative weight of the apriori information on the state relative to the observations? Some discussion of these matters would be appropriate.

- The results that you presented left me wondering how well you are fitting the data. What are the RMS of the residuals? How does the misfit to each observation type look spatially? Is your estimate fitting to one specific observation better than another? For instance, does your estimate agree better with altimetry than GRACE? If so, perhaps this was reflected in the initial choice of weights on the observations and choice of apriori information. This type of analysis would provide more credibility to the results presented.

- Figure 4. These results indicate that near MEDLEY and SEAT in the figure, that either runoff or evaporation/sublimation or blowing snow is dominating over precipitation in this region. What physical processes could allow for a negative SMB in this region? Could you please comment more on this, and why this result is believable? If this is not a result which is plausible, perhaps the introduced apriori information is bad, or the choice of weighting on the data is not optimal.

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

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