Summary and comments on the revised manuscript entitled

# Application of GRACE to the assessment of model-based estimates of monthly Greenland Ice Sheet mass balance (2003-2012)

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by

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## Summary

The authors combine three model records of surface mass balance (SMB) over the Greenland ice sheet (GrIS) with mass change observations from the Gravity Recovery and Climate Experiment (GRACE). The aim is to exploit these records for validating the initialisation of a state-of-the-art ice-flow model. Major effort is therefore put in discerning regional differences in the GRACE mass loss signal by relying on improved processing of the gravimetry data. In addition, the initialisation strategy for the ice-flow model is two-fold. First, inverse techniques are used to infer ice properties as ice temperature, the viscosity parameter and the sliding coefficient. Second, the ice-flow model is run forward over ~50 kyr under constant climate to guarantee consistency between ice flow and the prescribed SMB. Prognostic output over the period 2003-2012 is compared to the regional mass change pattern observed by GRACE. Many aspects of the presented initialisation are state-of-the-art which supports that ice-flow models are able to reproduce and explain the dynamic state of the ice sheet. The results show a strong underestimation of the observed mass change (Fig. 3A), which is explained by not accounting for fast processes in the ice-flow model. This might sound excusable but I fear that this puts the entire application of the flow model into question, as the major results do not significantly differ from a pure RCM SMB and GRACE data comparison.

## Main comment

While reading the manuscript, the above mentioned concern gradually became more and more manifest. Most findings are actually comparisons between cumulated SMB and GRACE data. Any conclusion/results which include the ice-flow model are not very significant (see detailed comments below). The authors even state themselves that relevant dynamic processes are not included in ISSM. Therefore, the remaining slow dynamic effect, they see, is small on the overall mass budget (Fig. 3). It is no surprise that many conclusion basically stem from a pure comparison between SMB modelling and GRACE mass change signals. The ice-flow model, as you present it, is not able to explain the recent dynamic response of the GrIS as it is not solely driven by SMB changes and resultant geometric ad-

justment. This is in fact what you calculate. In consequence, your ISSM mass budget calculation only explains 64% of the GRACE signal (Fig. 3). Ice dynamics is key here, but you need to include a process that explains the discharge increase through the marine terminated glaciers. Otherwise the flow model only serves to redistribute mass. The fundamental problem is that the extensive introduction of the flow model initialisation insinuates that you actually gain in reproducing the GRACE observations. But as you state yourself this is not really the case.

If the authors intend to revise or re-submit their manuscript, I urge them to strongly reformulate their central objectives and/or re-structure the article accordingly. I see two options, either exclude the entire flow model application or find a defensible parameterisation to simulate the recent ice discharge increase.

## Specific comments

P1L4 '[...] scarcity of highly resolved [...] improvements in spatial resolution and noise reduction of monthly global gravity fields.' You speak of high-resolution validation data which is missing and you put it into context with the improved GRACE data. GRACE processing clearly improved but I'm not sure if you should insinuate by your wording that the 300-km GRACE posting is a high-resolution, in the light of almost automatically processed DEM and velocity data which is now available.

P1L9-P1L10 As you say that no fast dynamic processes are included in your model, you actually see difference between the GRACE signal and cumulated SMB values from regional climate models (RCM).

P1L11 You did not force your model with the SMB from three RCMs. You used one RCM during spin-up and then you added anomalies in the more recent period. This is a fundamental difference.

P1L18-L19 Differences to GRACE are the full ice-dynamic signal, which is not modelled, and uncertainties in the RCMs.

P2L6-L7 If you speak of uncertainties in SMB projections, you should also mention the relevance of melt-water retention and refreezing.

P2L7-L9 Uncertainties in projecting future ice discharge: if you are speaking of short-term variations in ice discharge, I'm not sure if the link to SMB changes is most relevant. This link evokes the notion of slow geometric adjustment and gradual flow adjustment (over millennia). This process is well understood. Fast changes in ice discharge, however, are triggered at the margin (within some tens of kilometres). SMB variations can be one factor but other processes might be much more relevant (you mention them all in the next paragraph).

P2L10 'An alternative [...]' There you raise a big point. Extrapolating trends is in fact highly speculative and I prefer a well-initialised model setup for such projections. So I would not use the word'alternative'. I would state that flow-model projections are also a sort of extrapolations but well-informed by first physical principles of ice volume evolution.

P2L18-L21 This criticism on missing processes in current ice-flow models invokes the notion that your approach will be able to address these issues. I therefore suggest reformulation because most of the criticism applies to the presented approach.

P2L25 I do not agree with you here. Most state-of-the-art ice flow model, used for the ice2sea projections, account for longitudinal stress coupling (CISM, ISSM, PISM, Pollard, Elmer/Ice, etc.). The somewhat simpler SIA models are mostly applied in paleo applications, where long time-scales justify to some degree their applicability.

P3L8 You introduce this paragraph with: 'Another pressing issue is the lack of observational data for model evaluation.' The previous paragraph is already about the sparse observational record and unresolved issues linked to it. Please reformulate.

P4L28 What are the GAE and GAF products?

P6L11 This is a nice fit between observed and modelled velocities. For quantitative comparison, could you please give some measure like a root-mean-square error. Then one can directly compare to similar assimilation strategies (the ice sheet-wide RMS is for example given in Arthern et al., 2015).

P8L17 What is this CRI filter?

P9L10-L11 Your setup does not describe all components of the MB. You miss a process that can explain fast dynamic changes along the marine terminated outlet glaciers. Otherwise, the only dynamic response comes from the slow response to the geometric adjustment resulting from the SMB changes.

P9L11-L12 It is nice to see that the seasonality of the mass change signal is reproduced, reflecting the applicability of the RCMs.

P12L31-P13L1 Here, you formulate my main concern yourself. In other words, ISSM-GrIS mass changes do not differ much from the integrated SMB. The relevant dynamic process is not captured. It is highly controversial how this acceleration is triggered so I suggest to rely on parameterisations. Often basal sliding was increased by a constant factor or linked to changes in ocean temperature.

P21L7 I repeat myself but could you please provide a RMS deviation between modelled and observed surface velocity magnitudes.

Figure 3A The figure shows the total mass budget of the Greenland ice sheet between 2003 and 2013. I understand that you want to put the GRACE signal next to the modelled mass changes. First you have the SMB-GrIS curve (dashed line), which shows a somewhat reduced signal. If I understand it correctly, SMB-GrIS is the cumulated SMB anomaly. I did not quite get if this is for any specific RCM or a mean evolution. Anyhow, my question points at the difference between the SMB-GrIS and ISSM-GrIS. Again if I understood the setup correctly, ISSM-GrIS accounts for ice discharge (D) changes. At moments in the recent decade, observed changes in D were forwarded to have explained a large portion of the mass loss from GrIS. Yet the difference between ISSM-GrIS and SMB-GrIS are almost negligible. The reason for this is that you do not account for processes which explains fast dynamic response. Your model simply redistributed mass, except at the marine ice fronts. The slight dynamic reduction of the SMB mass loss signal, which you explain by the slow geometric adjustment (slopes, driving stress, etc.), has already been analysed in depth in the literature. As the title and abstract of the manuscript do not suggest that this small dynamic (long-term) effect is the target of your study, I fear that your model setup is not chosen appropriately.