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The Cryosphere
Discussions

Interactive comment on "Application of GRACE to the evaluation of an ice flow model of the Greenland Ice Sheet" *by* N.-J. Schlegel et al.

Anonymous Referee #2

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I am not sure what the paper intends to achieve. As stated, the idea of the paper is to evaluate (validate?) the behavior of ISSM by comparing results from an initialisation experiment to observed mass changes as derived from GRACE. However, I understand the purpose of the paper more as trying to explain the current evolution of the Greenland ice sheet and as an attempt to decompose the observed ice-sheet imbalance into its possible contributions, and attribute the residual from subtracting the modeled trend from the observed trend to (mainly) missing ice physics at the margin. If so, I believe there are serious problems with the way the experiments have been set up.

First, the ice-sheet model is run to steady state with the 1979-1988 average SMB from the Box SMB model, and this state is then taken as initial condition for 1846. Implicit in this approach is that the ice sheet was also in steady state in 1846 and that the ice flow and ice thickness field for both periods was in equilibrium with the 1979-1988

SMB for both periods (and identical). Even though it is rather well established that Greenland ice sheet volume was overall not changing during the 1979-1988 period, this does not mean that the local mass balance was necessarily zero all over the ice sheet (which is in fact highly unlikely), i.e. thinning in certain regions could well have been compensated by thickening in other regions. The current evolution of the Greenland ice sheet is the result of the superimposition of many different signals on a multitude of time scales. Very long time scales of 10**3-10**4 years are connected to viscosity changes in the basal layers from ice temperature and ice property changes and to ice-dynamic adjustments to geometry changes that likely extend back as far as the Last Glacial Maximum. The approach taken by the authors ignores all these longer-term effects as a contribution to current mass changes of the Greenland ice sheet.

Second, I am somewhat surprised by the choice of the ice sheet model. For their study, the authors opted to use the 2D SSA version of ISSM, which ignores vertical shearing. That is fine for modeling ice streams in Antarctica with high basal sliding, and may apply in Greenland in outlet glaciers close to the coast, but much of the Greenland ice sheet is frozen to bedrock with a flow regime that is better approximated by the SIA. It is furthermore not clear whether ice temperature is evolving together with the ice flow (I guess not, it seems to be prescribed) which a priori excludes a temperature change contribution to the current ice evolution (as are e.g. changes in ice hardness related to the downward advection of the LGM/Holocene boundary, amongst possible other processes).

Third, the paper does not convince me that the difference between modeled and observed trends can be attributed well on a regional scale as the uncertainties in input and observed fields are too large (especially SMB, but also bedrock elevation, in addition to errors in the GRACE field that is moreover spatially poorly resolved) and the simplifications in the model setup and initialization procedure are too important to reach solid conclusions.

Apart from these reservations, partly confirmed by the authors when differentiating between different regions, I found the results section hard to swallow as it is much too long, not well organized, and lacks synthesis.

I don't think the problems with the paper as it stands now (focus and length of the paper, problems with the model setup and the initialization, not considering pre-1846 contributions to ice evolution, messy conclusions, ...) can be fixed with only a major revision.

A few other comments

Abstract, p.1, lines 17, 19: 'transient' processes, 'transient' dynamics: what is meant with 'transient' in this context?

p. 2, line 2: 'Sto': the referencing is somewhat sloppy. Presumably, 'Sto' is Stocker et al., 2014. This kind of referencing occurs in many other places in the manuscript. More generally, when referring to the IPCC work, it is recommended to refer to the individual chapters.

p. 4, line 16: who are 'A. et al.'?

p. 4, line 6: what is meant with 'surface mass variations'? Do the authors perhaps mean surface elevation changes? If so, are these expressed in ice equivalents, i.e. in mass changes? Or do the authors mean 'ice mass' variations as opposed to the GIA contribution to GRACE?

p. 5: why is only a 9-year period chosen for averaging SMB. Isn't that a bit short considering the inherent variability of climate conditions over the ice sheet?

p. 6, line 9: 'ISSM Greenland observed velocities': do you perhaps mean 'modelled velocities'?

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p. 29, Fig. 2: the figures are too small to distinguish the patterns. The blue-red colour scale does not allow to differentiate much.

p. 31, Figure 5: the colour legend seems to be for the difference plot (panel C) only, but not for panels A and B. 'Mascons' with the same colour do not always appear green in panel C. Otherwise there is a problem with the color scale.

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