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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 #1

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Introduction:

Schlegel and co-authors present a methodological paper that deals with the important question how an ice flow model (here of the Greenland ice sheet) could be validated with observational data. Measured changes in the gravitational field as recovered from the GRACE satellite mission are utilised to estimate mass trends on a relatively small regional scale (300 km) and are compared with mass changes derived from a combination of three SMB models with an ice flow model (ISSM). The ice flow model is initialised to a steady state with the average SMB from the Box model for the period 1979-1988, by first inverting for observed velocities and than relaxing the geometry for 30 kyr. The analysis focusses on the period 2003-2012 for which GRACE observations are available.

Main comments:

The paper is of good presentational quality and overall well written. I see a couple of problems with the proposed methodology and the drawn conclusions, but I believe major revisions addressing these concerns can make the manuscript an interesting contribution to The Cryosphere.

While the technical efforts that go into this work in terms of spinning up the model and performing the analysis are in themselves impressive and state of the art, I have my doubts whether the presented methodology would actually succeed in validating the ice *flow* model, as suggested by the title. The given approach is more likely to validate the output from the SMB models (which are taken as pre-existing products) rather than the ice flow model itself. Notably, a large part of the discussion is dealing with the SMB results. Validation of the ice flow results proper seem only possible where the SMB can be assumed to be sufficiently adequate for residual arguments. Even then, the given analysis (in terms of validating ice flow) is mainly limited to explaining remaining mismatch to observations with some missing processes not included in the model. The authors largely follow the argument that the SMB may be trusted, where they can explain the seasonal cycle well, However, some of the missing processes can be expected to have a seasonal cycle as well, which renders the attribution problem underdetermined. I believe the title of the paper and other passages claiming validation of "ice flow" or "ice dynamics" should be modified to reflect that limitation. It should be clearly distinguished what the contribution of ice dynamics is in the modelled trends to make clear what can be expected to be validated with the given observations.

It is regrettable and maybe symptomatic that the main plot that shows the effect of the ice flow model in the presented analysis is displaced to the appendix (Figure S3 A). It is important to realise, that the dynamic thickness change presented here is what needs to be validated (if the aim of the paper would really be validating the ice flow model!). The dominant signal including all seasonal variations are governed by the prescribed SMB forcing (Fig 2D).

The Greenland ice sheet responds on multiple time scales (seasonal to millennial) to changes in its SMB, and on the long time scales also to ice temperature and bedrock changes. This implies that changes observed today can have their origin in recent changes in SMB as well as processes set in motion hundreds to thousands of years ago. The present study by construction (steady state initialisation) only accounts for the effect of the anomalous SMB history of the last 173 years. Anything outside of this range is omitted in the model, but will still be imprinted on the observed mass changes to some extent. This is notably the case for dynamic thickening of the interior (Reeh, 1985; Huybrechts, 1994; and recently brought up again by Colgan et al 2015), which should be discussed in in the paper as a limitation of the steady state initialisation approach.

Since biases originating from the initialisation should be excluded from the analysis, what is the remaining background trend of the model after initialisation? It is important to show with an adequate control experiment that the model response is dominated by the anomalous SMB forcing and not by background model drift due to the initialisation. This should be verified with a model run forced with zero SMB anomalies over the same time span as the forward experiments (173 years).

There appears to be an inconsistency for the initialisation, because SMB_bar (1979-1988, assumed to be in equilibrium) is combined with observed velocities at 2012 (which already show some acceleration in them). While probably of minor importance for the results, this should be clearly stated. Also mention in the text (P9,l20-24) that the spin-up procedure implies that modelled mass changes over the period 2003-2012 are governed by SMB changes over that period itself, ice dynamic changes forced by SMB changes 1840-2012 and a background trend that you estimate from a control run (see point before).

Is the mass conservation approach from Morlighem at al. performed with the same SMB_bar as in the present approach. If not, I doubt that it can be called mass conserving at all. Please clarify.

A number of questions for general consistency between modelled and observed quantities. How are ice thickness changes converted to mass? What density is assumed? How do you deal with the firn layer? Did you account for the map projection error when converting between lat-lon and projected coordinates?

I disagree with the conclusion (p17, l33; p18, l27) that seasonal variations in ice flow are important features of an ice flow simulation in terms of sea-level contributions. Furthermore, I don't see any reason why an ice sheet model that does not exhibit any sub-annual variations could not be validated by GRACE data. Alternatively, you may want to discuss the risk of overfitting when including processes with a large amount of (tuned) unknown parameters to better match (seasonal) observations.

Other comments:

The Results and Discussion sections are a bit difficult to navigate, due to the lack of any subdivision. It should help to group the results and discussion into different themes or regions and introduce subsections. One could e.g. distinguish between results for mass changes in the centre from the more complex marginal regions and discuss them separately.

Confusingly, the term mascon is used throughout the manuscript in two different interpretations. While it is introduced as a short form for 'mass concentration', it is later used to refer to the regional subdivision of areas in which mass changes are measured, modelled and compared. 'mascon' seems to me like a technical slang term in the second interpretation and should be replaced by something meaningful (maybe simply 'region').

The terms BOX, MAR and RACMO are used to describe the SMB products and the ice sheet runs they are based on. Better to be distinguished.

P1, l13. Replace "is primarily controlled" by "is assumed to be primarily controlled"

P1, l18-19. What are "transient dynamics"?, rephrase.

P2, l2. Something not right with the reference here.

P2, l2. It would be good to specify the current estimate for the rate of the GrIS sea-level contribution here as a reference value.

P2, l4. More important for the future sea-level contribution from Greenland are changes in the SMB, not ice flow. Clarify in the text.

P2, l7. Replace "ice flow models" by "ice sheet models" or otherwise make clear that SMB has to be included. An ice flow model in itself is not an alternative to the extrapolation methods because it misses the most important mass change component (SMB). Please also apply for the rest of the document.

P2, l8. Please give some references for these models here or refer to past initiatives (searise and ice2sea).

P2, l9. Should say here why this alternative is most promising: because the models are physically based.

P2, l10-15. The given interpretation of the current state of ice sheet modelling is a bit simplistic and should be extended. There are recent examples of models that do capture the observed trends: Fürst et al. (2015) for Greenland and Ritz et al. (2015) for Antarctica.

P2, l21. Please be more specific what you mean by "ice flow dynamics".

P4, l5. What does "inversion" refer to here?. Clarify.

P4, l16. Incorrect reference A et al.

P5, l31. Where does the number of years 25 come from?

P5, l33-. I suppose SMB anomalies are calculated against the mean SMB (1979-1988) of the same product and then added to the mean reference SMB_bar of the BOX model. This should be mentioned.

P6, l8. "to highlight the regions where the modeled ice sheet *mass trend* differs from GRACE", or similar.

P6, l8-9. Topography is not a surface feature of the ice sheet.

P6, l9-12. This description pertaining to init and relaxation may be better placed in Section 3.2.

P6, l11. Mention here that basal melting is ignored and why.

P6, l12. Include "ideally" before "in a steady state" and "nearly" before "equal". This conditions is never strictly fulfilled in any ice sheet model I know of. Also add here that this is the assumed initial state for the year 1840.

P6, l16. Add "errors in GRACE-JPL" to the list of possible explanations for the mismatch. The background trend after initialisation (see point on control experiment) could be compounded in "limitations of our model spinup", but may need extra mention if significant.

P6, l25. Add "over time" after "RACMO" to avoid confusion.

P6, l26. Add "anomalous" before "SMB forcing".

P6, l26. Maybe "Next, we sum mass changes simulated by ISSM for the BOX ..."

P6, l27. Maybe "This mass signal represents the ISSM model estimate of ice sheet mass balance through time and is comprised of the anomalous SMB forcing at the time and the dynamic response to SMB changes since the year 1840." If a background trend from the control experiment is not negligible and not removed beforehand, it should be mentioned here as an additional contribution.

P7, l7. Replace "directly" by "is the only component that"

P8, l26. Not clear what you mean by "Regional climate model SMB products are considered to be more mature than ice dynamic models on decadal time scales". I certainly don't see the causality between this statement and the next. Please clarify.

P9, l1. Too much information combined in this sentence makes it confusing. Revise and consider splitting in two. Also, topography is not a surface feature of the ice sheet.

P9, l5. I have not understood why velocity changes over this period are an important quantity to look at and what role they play in the interpretation. Maybe you could add a sentence to motivate that.

P9, l7. There are no outlet glaciers in the interior of the ice sheet. Please correct this sentence.

P9, l15-18. I find it confusing to discuss panel C and especially F here, in relation to panels B, D and E. The model-observed thickness (F) must be largely the results of the relaxation and (assuming small model drift) changes relatively little over the spinup. It would be much clearer to discuss a version of C and F, with modelled thickness and velocity after relaxation as the steady state of the ice sheet. Any changes afterwards can then be attributed to the historical SMB forcing and the dynamic response to that.

P9, l24. Replace "are fixed" by "are corrected"

P10, l4. What are "annuals" and "semiannuals"? Maybe "sinusoidals with an annual/semiannual cycle"?

P10, l5-6. "suggesting that the seasonal variability of SMB and its spatial distribution are well represented by the three forcing products"

P10, l23-24. "suggesting that the effect is related to melt". Could you explain? Also see comment (P13, l11-14.)

P10, l24. Should refer to (Fig. 2B) instead of (Fig. 2C) here.

P10, l25. Insert "the" before same.

P10, l28. I hope the model is conserving ice (in the sense of mass conservation).

Anyway, please reformulate.

P10, l29. "reduce the spread" is a technical interpretation. My guess is that this is not true for the relative spread. But even if there is a non-linearity in the dynamic effect, that should be the interpretation, not the pure numbers.

P11, l3. Replace "in" by "is" before "driven"

P12, l14. "be a factor" or "play a role"

P13, l10. Clarify where these numbers come from. The model could distinguish between SMB and dynamics, but the model does not agree with the GRACE data.

P13, l11-14. Since your modelling approach excludes seasonal effects from basal lubrication by melt water and ocean forcing of outlet glaciers, it is on first view somewhat surprising that your dynamic response shows any significant seasonal signal at all. Given that reduced ice discharge due to marginal thinning is the declared responsible mechanism, it seems important to mention that this is a 'passive' dynamical effect and direct consequence of the SMB forcing. In other words, the dynamics in themselves have no seasonal signature other than the one imprinted directly by the SMB change.

P13, l15-17. See my comment P10, l29. about reduced spread above. I strongly hope "that the model state ... play[s] a role in dictating the results of the three different simulations", since otherwise there would be no need to run an ice sheet model at all. However, I don't see any causal relation between the apparent change in spread and this statement.

P13, l17. As stated above, I believe the seasonal aspect of the dynamic dampening of mass loss is not really relevant. I find it generates confusion in the distinction between the two processes.

P13, l20. Not clear what "acceleration in ice dynamics is not a trivial component" means. Please reformulate.

P13, l22. While I agree generally that dynamic changes likely represent "a minor source of uncertainty" compared to uncertainty in SMB, I have quite some difficulty to see how that can be derived from the presented comparison. Please clarify.

P13, l23. Some of these marginal processes that are excluded from the modelling could certainly compensate for errors in SMB and/or included dynamics, especially since they can be assumed to have a seasonal signature by themselves. I therefore find the attribution of model error and uncertainty very much complicated, if not rendered practically impossible. This should be discussed and be reflected in the degree of certainty in the statements. E.g. replace "are responsible" by "may be assumed to be responsible" and similar.

P14, l1. "Seasonal snow cover on tundra, bare rock, ..."

P14, l3. Remove "results suggest that"

P14, l13. Please reformulate "not enough melt in relaxation SMB".

P15, l15. Replace "it is possible to quantify", by "it may be possible to quantify"

P16, l31. Maybe "both temporally and spatially"?

P16, l35. Include a discussion on interior dynamic thickening here (see comment above).

P17, l28. Remove "the" before "not well understood"

P18, l4. Move "from 2003-2012" to just after "simulations" to avoid confusion over which period SMB products are applied (namely also before 2003).

P18, l10. Insert "is" after "it"

P18, l25. I don't think you can make such firm statements about processes that are not modelled, not studied and not analysed.

Figures:

Fig 1 The colors in the legend do not match with the ones on the figure. Probably because of the gray overlay.

Fig 2 The model mesh is hardly visible at the size of panel A. An inset for one prominent region could maybe help to visualise the grid.

For my eyes panels D and E are indistinguishable. I would therefore suggest to show the difference from S3 panel A here rather than practically showing the same figure twice. Clearly, the dynamic thickness change is one of the most important variables when considering the dynamic changes and should not be hidden in the appendix. It represents the added benefit and justification for performing your analysis with an expensive ice sheet model.

Please consider using a non-linear scale for the panels B-F. It should for example become better visible that there is a positive SMB anomaly in the centre in D and E.

Why are velocities in panel C given for December 2012? Is that the reference date for the observations? If not, maybe an annual average would be more appropriate.

Fig 3 If the grey curve gives the SMB forcing for the model, shouldn't it also show seasonal variations? Please clarify and correct if necessary.

Fig 4 Maybe some of the interior mass gain could be explained by "millennial-scale ice-sheet thickening is an anticipated result of the downward advection through the ice sheet of the transition between relatively 'soft' Wisconsin ice and relatively 'hard' Holocene ice." (Colgan et al., 2015).

References

Colgan, W., Box, J. E., Andersen, M. L., Fettweis, X., Csatho, B., Fausto, R. S., Van As, D. and Wahr, J.: Greenland high-elevation mass balance: inference and implication of reference period (1961–90) imbalance, Annals of Glaciology, 56(70), 105–117, doi:10.3189/2015AoG70A967, 2015.

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