

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.

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We would like to thank the referees for giving their time to raise important discussion points and to assess the quality of our manuscript. We believe many of the suggestions were helpful in improving the presentation of our results and the manuscript overall. Below, we address the review comments and suggested modifications. For those comments that required updates to text, we enclose a paper draft with corresponding changes in red. The main modifications can be summarized as follows:

1. The tone of the manuscript has been changed to focus on the comparison of ice sheet model estimates of Greenland mass balance to GRACE, rather than focusing on the ice flow portion of the ice sheet model.
2. All figures/analysis have been updated with new GRACE data (version 2 of the

C1

JPL mascon product), as well as new ISSM runs that implement the L1L2 formulation which includes effects of longitudinal stresses and considers the contribution of vertical gradients to vertical shear. We note that updates to both GRACE and ISSM did not change the conclusions presented.

3. Much of the technical discussion and several figures has been moved to the appendix in an effort to shorten the manuscript. Additionally, we have added many sub-headings to increase the readability of the manuscript.
4. We have included discussion on dynamic thickening in the interior of the ice sheet.

1 Anonymous Referee 1

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 then relaxing the geometry for 30 kyr. The analysis focusses on the period 2003-2012 for which GRACE observations are available.

Main comments:

C2

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.

These are valid points, and stem from our attempt to cover many topics in the same manuscript. We have attempted to narrow our focus in the manuscript, to compare GRACE-JPL against state-of-the-art model estimates (which include SMB and ISSM). As suggested, references to "ice flow" have been changed to "ice sheet" throughout the manuscript, and we have changed all references to "ice dynamics" to reflect more

C3

precise mechanisms (i.e., ice discharge or paleo-driven background dynamics).

The goal of this study was never to use GRACE to validate our model, as many processes are admittedly missing from the model itself and are clearly at play in determining Greenland MB. We chose to use the terminology "evaluate" in order to reflect this, but the tone of the manuscript did not always complement these intentions. To remedy this, we have changed the title of the paper and have expanded the introduction to include a discussion of SMB models, which are the most important element of the modeled mass balance.

We hope that the title, tone, and focus of the manuscript now better reflects these goals.

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 dynamic thickness change, on a mascon-by-mascon basis, is shown in Fig. 7A. The high resolution spatial patterns of the ice sheet model response is included in the supplement for reference, in order to better explain the mascon-scale spatial patterns presented in Fig. 7A. Because our goal here is not to validate the ice flow model (as originally suggested by our title), we have decided to focus on assessment of the model-based estimate of MB, and have decided to keep this figure in the supplement, for reference.

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

C4

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 the paper as a limitation of the steady state initialisation approach.

This is a valid point, and you bring up key limitations of our spinup process that we agree should be highlighted more in the text of the manuscript. In order to do so, we have added a paragraph in the Initialization and Relaxation section of the manuscript to discuss caveats. In addition, as suggested, we have extended our discussion of the interior thickening to include references to past studies that describe the southern interior thickening driven by the downward propagation of more rigid Holocene ice (Reeh, 1985) as well as the observed thickening in the Northeast that has recently been attributed to recent decrease in accumulation in comparison to the average Holocene accumulation rates (MacGregor et al., 2016). We have taken advantage of our steady-state spinup to estimate dynamic mass gain in the interior of the ice sheet (about 9 Gt/yr), which is focused in the Northeast. Unfortunately, the resolution and placement of the mascons in the South do not allow us to quantify "Reeh" thickening that may be occurring there (though we note that minor thickening signals in mascon 166 may be related to this phenomenon).

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.

C5

This should be verified with a model run forced with zero SMB anomalies over the same time span as the forward experiments (173 years).

We have added a dotted line to Fig. 3 that represents a control experiment, forced with \overline{SMB} during the 173 years. This line illustrates a change in ice sheet mass over that period, which is an order of magnitude less than the intra-annual variability over the historical period.

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 (P9,120-24) 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).

Such inconsistencies are, unfortunately, the downside to assimilating the best observations currently available into ISSM. They are, in fact, the very reason why the model drifts upon spinup - and why we have decided to relax the model to a virtual steady-state before forcing it with historical SMB. After relaxation, the resulting velocities, though similar to present day velocities, have changed to be in a relative steady-state with \overline{SMB} . Since we have little to no information about how the basal conditions of the ice sheet have changed over the last 173 years, we believe our best assumption is to hold the assimilated ice sheet properties (i.e. ice viscosity and basal drag) constant. At the end of the Initialization and Relaxation section, we have added a final paragraph that acknowledges the key limitations of our approach. We hope this sufficiently covers the referee's concerns expressed here.

C6

Is the mass conservation approach from Morlighem et 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.

The reference to mass-conserving has been replaced with "BedMachine bedrock", which is a more accurate description of the product. We are not using the same SMB as Dr. Morlighem, which, in part (along with a number of other data mismatches noted earlier), is why we have decided to relax to a virtual steady-state before running our historical simulation.

A number of questions for general consistency between modelled and observed quantities. How are ice thickness changes converted to mass? What density is assumed?

We have added some additional sentences in the GRACE period mass estimates section of the manuscript that now indicate that we assume the density of ice to be 917 kg/m^3 , and describe our process for translating ISSM ice thickness changes to mass within mascons: "Within the ice sheet boundary, mass changes are considered on individual elements of the ISSM mesh and outside of the ice sheet boundary, mass changes are considered on individual elements of a 10 km triangular mesh. To assess mass change within each mascon, elements within the projected mascon boundaries are summed, and elements bisected by mascon boundaries contribute to this sum proportionally (by area) to the mascons that fall within their individual outlines. This procedure is mass conserving on the continental-scale, but please note that it introduces small leakage errors along the mascon boundaries that are insignificant compared to the uncertainties considered in this study."

How do you deal with the firn layer? Did you account for the map projection error when converting between lat-lon and projected coordinates?

C7

At the end of the Models of Historical SMB section, we now explain that any processes related to firn densification are modeled by each individual RCM surface model, and beyond that calculation, ISSM assumes that the SMB provided by the RCM is ice. Any map projection errors are assessed in translation from ISSM thickness changes to mass change in each mascon. This explanation (as stated above) has been placed in the GRACE period mass estimates section and describes our effort to conserve total ice sheet mass change and to minimize leakage between mascons during this calculation.

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.

We have reworded the end of the abstract, discussion, and conclusion sections to say that continued improvements in physically-based modeling of the processes most likely to be responsible for intra-annual variability will likely improve the skill of ice sheet models, particularly in terms of decadal-scale modeling. In addition, we stress that such models which consider hydrological processes will have the opportunity to take full advantage of observations that are available on a monthly-to-seasonal timescales.

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

C8

mass changes in the centre from the more complex marginal regions and discuss them separately.

We have organized the Results section into categories, which follows the logic of our plots from a continental-scale view of the comparison down to a higher spatial resolution (regional) and higher temporal resolution (seasonal). We believe the new subsections will make it easier for the reader to navigate. We have also reorganized the Discussion section, which is now categorized by region of the ice sheet.

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').

We understand the reviewer's concern; however, we feel that the terminology presented here is self-consistent. As mentioned, 'mascon' is short for 'mass concentration'. The unique aspect of using mascons as basis functions when processing GRACE data is that they do explicitly define regions within a known latitude/longitude domain (unlike other basis functions such as spherical harmonic coefficients which are global by nature): so, each mascon explicitly defines a region. The word "region" and "mascon" are somewhat synonymous. We hesitate to use a word such as "region" when discussing the results, simply because the placement of the mascons is rather arbitrary and their boundaries do not necessarily delineate specific geographic regions of interest (i.e. drainage basins). Therefore, we prefer to keep the terminology as is.

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.

C9

We have made a distinction in the text between BOX, MAR, RACMO and the resulting ISSM runs, which we now refer to as ISSM-GrIS BOX, ISSM-GrIS MAR, and ISSM-GrIS RACMO.

P1, I13. Replace "is primarily controlled" by "is assumed to be primarily controlled"

This line has been updated as suggested, in the abstract.

P1, I18-19. What are "transient dynamics"?, rephrase.

Transient dynamics has been removed from the abstract and the last three sentences of the abstract have been reworked as a result.

P2, I2. Something not right with the reference here.

Thank you for pointing this out. Something did not render correctly by TCD, and we will make sure the reference is correct when the corrected manuscript version is uploaded.

P2, I2. It would be good to specify the current estimate for the rate of the GrIS sea-level contribution here as a reference value.

This has been added to the text. Thank you for the suggestion.

P2, I4. More important for the future sea-level contribution from Greenland are changes in the SMB, not ice flow. Clarify in the text.

We have added a sentence here to specify that the largest source of uncertainty in future sea level rise is SMB, and secondly ice sheet discharge into the ocean.

C10

P2, I7. 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.

Throughout the text, we have replaced the reference to "ice flow models" with "ice sheet models", where we refer to total mass balance of the ice sheet (or SMB+discharge), as suggested by the referee.

P2, I8. Please give some references for these models here or refer to past initiatives (searise and ice2sea).

Here, we have added reference to both searise and ice2sea as sources that showcase a collection of ice sheet models and various sea level-based experiments.

P2, I9. Should say here why this alternative is most promising: because the models are physically based.

We have added a reference to the fact that these models are physically based.

P2, I10-15. 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.

The introduction of the manuscript has been reworked, and now includes a discussion of ice sheet models (including an acknowledgement to efforts that have been successful in matching current Greenland mass balance trends), the most current SMB

C11

products derived from Regional Climate Models, and an explanation of GRACE. We believe this summary, which now focuses on all the products we are assessing in this study, is more comprehensive.

P2, I21. Please be more specific what you mean by "ice flow dynamics".

"Ice flow dynamics" has been updated to read, "ice discharge into the ocean". We believe this statement is more accurate.

P4, I5. What does "inversion" refer to here?. Clarify.

We have removed the text referring to the "inversion" as it was unnecessary to make our point.

P4, I16. Incorrect reference A et al.

Actually, this reference is correct. Mr. A had the very unfortunate life circumstance of having a last name that was uniquely given by the first letter of the alphabet! We share in your sentiments that this is quite unfortunate for Mr. A.

P5, I31. Where does the number of years 25 come from?

This sentence has now been updated to read "from 1970 to 2000".

P5, I33. 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.

C12

Yes, this is correct. We have updated the paragraph to read "The total SMB forcing for each RCM product is equal to \overline{SMB} plus the monthly SMB anomalies derived for that particular product beginning in 1979."

P6, I8. "to highlight the regions where the modeled ice sheet *mass trend* differs from GRACE", or similar.

We have updated this sentence to state that our goal is to compare ISSM simulations that are "forced with three different high-resolution RCM-derived SMB products, against the monthly GRACE-JPL product, in order to highlight the regions where modeled ice sheet mass trend and annual amplitude differ from GRACE".

P6, I8-9. Topography is not a surface feature of the ice sheet.

We have changed "surface features" in this statement to read "surface properties".

P6, I9-12. This description pertaining to init and relaxation may be better placed in Section 3.2.

Thank you for this suggestion. A portion of this description has been moved to Section 3.2, and the second part, describing forward model SMB forcing has been moved to the beginning of Section 3.3.

P6, I11. Mention here that basal melting is ignored and why.

The formula for MB (now in Sect. 3.2), has been updated to include basal mass balance, and we explain that this term is ignored because ISSM does not simulate basal hydrology.

C13

P6, I12. 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.

We have updated this statement to reflect that our simulation is in a "virtual" steady-state and have added "nearly" before "equal" to reflect that mass balance is near zero after relaxation. In addition, in Section 3.2, we have added a sentence to clarify that the relaxed ice sheet is taken as an assumed state of the ice sheet in 1840.

P6, I16. 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.

We have added GRACE-JPL to the list and have mentioned that we have attempted to quantify errors in SMB and GRACE where possible. We do not feel it is necessary to expand upon the limitations in model spinup, as we have added a paragraph in Section 3.2 listing the limitation and assumptions with the steady-state spinup procedure.

P6, I25. Add "over time" after "RACMO" to avoid confusion.

We have added "over time" to the end of this sentence.

P6, I26. Add "anomalous" before "SMB forcing".

We have added "anomalous" here, as suggested.

P6, I26. Maybe "Next, we sum mass changes simulated by ISSM for the BOX ..."

C14

Thank you for this suggestion. The change has been incorporated.

P6, I27. 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.

The sentence in question has been updated to read, "This mass signal represents the ISSM model estimate of ice sheet mass balance through time and is comprised of the anomalous SMB forcing and the dynamic response to SMB changes since the year 1840."

P7, I7. Replace "directly" by "is the only component that"

The manuscript has been updated as suggested.

P8, I26. 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.

We agree. This sentence has been removed for clarity.

P9, I1. 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.

C15

We have split this sentence into three and have reworked these sentences to better motivate why we are interested in evaluating the model spin-up procedure.

P9, I5. 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.

It true that velocity changes have only been included as a reference for showing how ISSM models the change in ice flow over the 10 year period of our study. Therefore, we have moved this figure to the supplement and have removed extended discussion pertaining to this figure from the main text of the manuscript

P9, I7. There are no outlet glaciers in the interior of the ice sheet. Please correct this sentence.

Thank you for pointing this out. We have changed "interior" to now read "along the margins".

P9, I15-18. 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.

As suggested, we have updated the difference plots for ISSM velocity and thickness against observations, so that they now show the difference between the relaxed ISSM "steady-state" and observational datasets. We agree with the referee that this is a

C16

much clearly comparison, especially since most of the differences are due to the model relaxation. These figures are now located in the supplement.

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

We have updated this sentence to read "are offset".

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

We have reworded this sentence to say we estimate "sinusoids with frequencies of once and twice per year".

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

Thank you for this suggestion. The manuscript has been updated as suggested here.

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

We have added more text in this paragraph, to explain why muted annual amplitudes are likely related to runoff-induced thinning during the summer months. Since we are focusing on assessing annual amplitudes and seasonal cycles, we believe it is important to point out how the ice sheet model affects results on monthly to seasonal timescales. Therefore, we have left this description in the results section. However, as suggested for P13, l11-14, we have removed the extended discussion of this point, as we agree that it is confusing to the reader.

C17

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

The reference has been updated to refer to 2B instead of 2C.

P10, l25. Insert "the" before same.

Thank you; this has been updated in the manuscript.

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

This sentence has been reworked, and the manuscript has been updated to read, "Overall, this behavior increases the modeled MB".

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.

These lines have been updated to state that the behavior decreases MB "similarly for all three simulations, ultimately resulting in a better agreement between ISSM-GrIS BOX, ISSM-GrIS MAR, and ISSM-GrIS RACMO".

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

This typo has been corrected.

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

C18

"Factor" has been changed to "role" as suggested.

P13, I10. Clarify where these numbers come from. The model could distinguish between SMB and dynamics, but the model does not agree with the GRACE data.

We have modified this text and have moved it to Section 6.1. There, we state the percentage of mass loss that the model captures relative to GRACE. In addition, in order to address this comment, we have made an effort to reference a figure or the table when placing a number in the text.

P13, I11-14. 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.

This discussion has been moved to Section 6.3 and has been updated. The statements, now in the Results section, have been updated to point out that the decrease in discharge is a direct consequence of the SMB-driven thinning of the margins, and that the model simulates this change in discharge because ice thickness changes through time.

P13, I15-17. See my comment P10, I29. 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.

C19

However, I don't see any causal relation between the apparent change in spread and this statement.

This is a good point, and we have updated the manuscript accordingly. The statement about the initial model state has been removed from these lines, and the statement about model spread (now in the Results, Section 6.3.) has been updated to read that the marginal thinning serves to reduce "the MB of all the simulations in a similar way, resulting in better agreement between ISSM-GrIS BOX, ISSM-GrIS MAR, and ISSM-GrIS RACMO than between the SMB products themselves."

P13, I17. 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.

We have removed this sentence and consolidated this discussion to the results, Section 6.3. In the discussion Model Assessment section, we also make the point that the marginal thinning results in an overall decrease in mass trend for the mascons along the margins.

P13, I20. Not clear what "acceleration in ice dynamics is not a trivial component" means. Please reformulate.

This sentence has been removed from the discussion, and we now indicate that the changes to ice discharge modeled by ISSM are "minor in comparison to the direct contribution from the SMB forcing itself," in the Model Assessment section.

P13, I22. 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.

C20

Along with the previous sentence, this sentence has been reworded to clarify that over the short time period analyzed for this study, that the ice responses captured by the model are minor compared to the changes in the SMB forcing itself.

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.

We have updated the verbiage in this passage and throughout the paper, to not include discussion of uncertainty, as it confuses the issue. Our goal here is to assess the importance of including an ice sheet model in the calculation of "ice dynamics" typical for GRACE-based studies (i.e. subtract SMB from GRACE to get a value for ice dynamics). We have tried to make this clearer in the paragraph noted here, by eliminating the uncertainty terminology and adding the wording suggested by the referee.

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

This part of the discussion has been removed from the manuscript.

P14, l3. Remove "results suggest that"

These words have been removed from the manuscript.

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

C21

This has been changed to "general underestimation of surface melt runoff".

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

The suggested change has been made on P15, l20.

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

"Temporal" has been changed to "temporally" as suggested.

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

As suggested, we have included a discussion of dynamic thickening of the interior. Such a discussion strengthens this manuscript, and we are thankful for the suggestion.

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

This typo has been updated in the manuscript.

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

As suggested, we have moved "from 2003-2012" to the earlier part of the sentence, after "simulations".

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

C22

The manuscript has been updated as suggested.

P18, I25. I don't think you can make such firm statements about processes that are not modelled, not studied and not analysed.

We have reworked the end of this concluding paragraph to state that (in reference to the high-frequency variations that appear in the GRACE-JPL signal), that hydrological and ocean-driven processes are strong candidates for those processes that could account for such a signal.

Figures:

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

Yes, this is a problem with the overlay. The figure has been updated to take the overlay into consideration and the colors should match now. Thank you for bringing this up to us.

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

C23

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

All the panels for Fig. 2 have been moved to the supplement, because, as discussed above, these figures are for reference and do not pertain directly to the mascon-by-mascon comparison we are addressing here. In the supplement, the figures are now much larger, 2 panels per page, so the colors and spatial patterns are more visible for the reader. As discussed above, a mascon version of the dynamic thickness change plot is already in the manuscript (Fig. 6). The dynamic plot in the supplement is now enlarged, so that it shows more detail. For the velocities and thickness comparisons against observations, we now use the relaxed model as the reference. For the velocity change from 2003-2012, we now use annual averages of velocities for these years.

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.

We have included a light gray line in this plot, which represents the seasonal SMB forcing. Thank you for pointing this out.

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).

Again, thank you for this suggestion. Unfortunately, the largest "Reeh" thickening occurs in the Southern ice sheet, and most of our observed thickening is taking place in the Northeast. (The spatial resolution in the south is not refined enough to separate

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the interior from the margins, so any background trend is indistinguishable in magnitude over the mascon.) We have noted this process as a possible explanation for the thickening, however, and have also discussed other theories for background thickening in the Northeast.

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2 Anonymous Referee 2

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.

As suggested by referee 1, we have reworked the manuscript, including the title, tone, and focus to address the confusion about validation of an ice sheet model that is missing physical processes that play a significant role in dictating Greenland MB. We agree that, instead, our goal is to use GRACE to assess the dynamics that are not captured by current model-based estimates of Greenland MB (comprised of SMB and ice sheet model), and we hope that the new version of the manuscript makes this goal clearer.

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 the 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.

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This is a very good point, and part of the assessment of this study is to determine where our assumption might be invalid, through comparison with observations. Overall, we find that in order to study mass balance variations over such a short timescale, the results are not strongly affected by our spinup assumptions. For instance, in the supplement, we include a number of variations in model spinup, using different MAR products for the reference climatology (for example MAR2, MAR 3.5.2, and MAR 3.5.2 with NCEP boundary conditions), and each product has different spatial patterns in the spinup climatology. We find (as mentioned in the manuscript) that our results are not strongly sensitive to the initial spatial patterns of our spinup climatology. Additional experiments have also shown that our results for the short period 2003-2012, would not change if a climatology between 1840 and 1900 were used for the spinup. In fact, to the first order, the ice sheet model responds almost exclusively to the anomalies in SMB, and these responses are small compared to the SMB internal variability. We expect any second-order responses due to our choice of steady-state climatology to be even smaller, and would not change the conclusions presented here.

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 icedynamic 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.

The spinup procedure does ignore these background effects, and this is a limitation that we discuss in the initialization section of the manuscript. However, because we have chosen a steady-state spinup procedure, we are confident that our ice sheet model is responding to only changes in SMB. In terms of dynamic background trends in the ice sheet, this is an advantage, as pointed out by referee 1. As suggested, by removing

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the model results from interior, we are able to isolate signals of interior thickening. We believe the addition of this component to our results and discussion strengthens the manuscript.

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).

For the newly revised version of the manuscript, we have updated and reprocessed all of our results to use the L1L2 version of ISSM (which considers vertical shear to the same extent that SIA does, but also considers longitudinal stresses), in order to address your concerns. This update does not change any of our main discussion or conclusions.

Over the short time period we investigate here, changes to ice temperature are not expected to cause any significant changes to our results (*Seroussi et al.*, 2012). Because of this, we believe that we are justified in holding the ice temperature (ice viscosity) constant throughout the simulation. As discussed above, since dynamic thickening due to past climate forcing of the ice sheet is not included in our simulation, we expect it to be captured in the difference between GRACE-JPL and the model simulations.

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

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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.

The strategy of our manuscript was to quantify the uncertainties in GRACE-JPL and the uncertainties in SMB, because they indeed do have significant uncertainties at a 300 km spatial scale/monthly temporal scale (i.e., the spatiotemporal scales of interest for our study). It is of utmost importance that these errors are rigorously defined. Upon comparison, it is clear that there are regions, and during specific times of the year, where observations and the model results differ outside the bounds of the calculated uncertainty, which allows us to then probe into attribution of this signal. We believe that the methods presented here are among the most rigorous that have been applied for this type of comparison, and that our conclusions are justified based on our calculated uncertainties.

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.

Based on suggestions from referee 1, we have removed many of the technical details from the main text of the manuscript, have organized the results and the discussion into sections, and have reworked the introduction to focus on the three key components of this comparison (SMB models, ice sheet model, and GRACE). We believe that these changes address the concerns mentioned here.

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

C29

contributions to ice evolution, messy conclusions, ...) can be fixed with only a major revision.

During manuscript revision, we have reduced the technical details in the main text of the paper and have tried to better focus on our goal of assessing regional mass variability in Greenland (both observed and modeled). We do not believe that our model setup has any notable problems, but we do admit that there are limitations as a result of our assumptions. These limitations are discussed in detail in the manuscript, and we believe the steady-state spinup offers an opportunity to quantify ice sheet changes in response to pre-1840 climate forcing of the paleo ice sheet, particularly in the interior of the ice sheet. Such background trends should manifest as trends in GRACE, and not in temporal variability. By separating trend and annual amplitudes, our methods should expose significant shifts in trend that are related to dynamic background trends. For example, we discuss that the Northwest is out of balance, as GRACE-JPL exhibits mass loss even during the winter months.

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

The term transient has been changed to "temporally evolving", which we believe is a more clear description of the processes we are referring to here.

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.

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As we have responded to referee 1, we do not know why this reference did not render correctly during the TCD upload process. This has been updated, and the reference has been updated to refer to the specific chapter of interest (*Church and White, 2011*).

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

As noted to referee 1, this reference is actually correct. Mr. A had the very unfortunate life circumstance of having a last name that was uniquely given by the first letter of the alphabet! We share in your sentiments that this is quite unfortunate for Mr. A.

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?

We believe the referee is referring to a phrase that is located on p. 4, line 16. GRACE observes total mass variations: these include mass variations on the surface of the earth (water/ice), above the surface of the Earth (atmosphere), and below the surface of the Earth (glacial isostatic adjustment). By 'surface mass variations', we mean exactly that: mass variations on the surface of the Earth. These include not only ice mass variations, as you suggest, but also mass variations due to snow and water. By removing glacial isostatic adjustment from the GRACE signal, we remove the solid Earth mass variations, and are left with only "surface mass variations". We prefer to keep the verbiage as is.

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?

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We have reworded Section 3.2 to be more explicit about why this 10-year period was chosen as our reference period. Most importantly, it is the overlap period of all 3 SMB models, that also happens to exist during the 1971-1988 steady-state period noted by *Rignot et al. (2008)*. This is bound by the beginning of the ERA-I reanalysis period. In order to avoid inconsistencies between continuity strategies for the SMB products, we decided not to use any products forced with ERA-40. We were satisfied that the ice sheet remained in a near steady-state during the 1840-1900 ISSM simulation, suggesting that decade is reasonably similar in climatology to the first 60 years of simulation. Overall, we feel the choice is justified and that the mean climatology chosen is reasonably representative of the mean SMB for the beginning of the historical spin up period.

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

We believe the referee is referring to a phrase that is located on p. 9, line 6. If so, thank you for pointing this out. This was an important mistype that has not been fixed as suggested.

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

All the plots in this figure have been enlarged (and are now located in the supplement), so that we only present 2 panels per page. We believe that these figures, now being larger, are easier to distinguish in terms of the color bar and spatial patterns.

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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We doublechecked Figure 5 and found consistency in A, B, and C. Colors are consistent between the three plots. Figure 5 is now updated, as we have updated all our results to include a new version of GRACE and the L1L2 version of ISSM results. We have ensured that the colors are also consistent in this new version of the manuscript.

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