1 Interactive Comment from S. A. Khan (abbas@space.dtu.dk) received and published 13 May 2014

This is a very interesting and well written paper. Hopefully it will be published in its final version soon. However, I have a minor comment regarding your correction on elevation changes.

Page 2344 line8-10: "Elevation changes were corrected to remove the effect of vertical crustal motion due to Glacial Isostatic Adjustment (GIA) and variations of firn compaction rates in 2003-2009."

Please note that the correction for elastic vertical crustal motion (due to present-day ice loss) is in general much larger than GIA. The elastic rates are typically few cm/yr, while GIA rates are few mm/yr (e.g. see *Bevis et al.* [2012]; *Khan et al.* [2010a]).

To solve the problem, I have uploaded two data files for northeast Greenland. They simply contain rates of elastic vertical crustal motion during 2003-2006 and 2006-2009. Rates are given in mm/yr on a 5x5 km grid and computed as described by *Khan et al.* [2010b]. Feel free to use the data without any restrictions.

To access data use the following link: `ftp://ftp.space.dtu.dk/pub/abbas/TCD/`

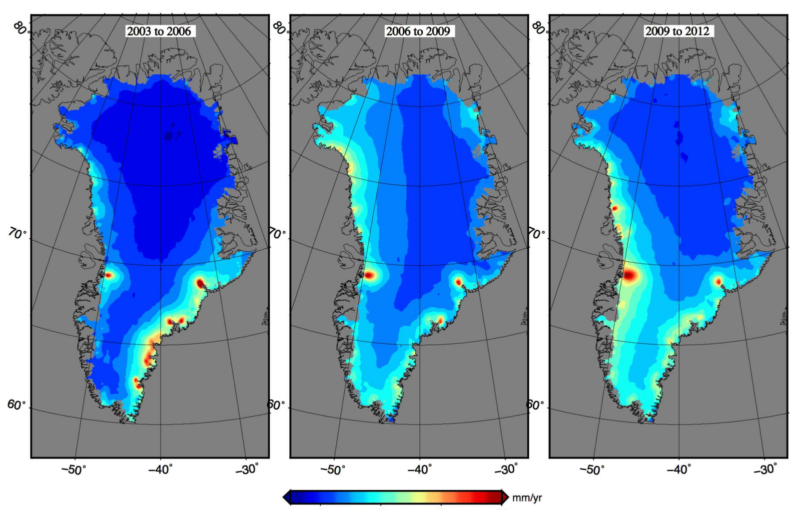

Figure 1:

We thank Dr. Khan for his positive remarks about our manuscript and for providing estimates of the elastic vertical crustal motion during 2003-2006 and

2006-2009. We agree that it is important to remove the effect of this signal from the altimetry derived elevation change estimates. However, the elastic crustal response changes very rapidly in time. Therefore, we believe that the current temporal resolution (3-year average) is not sufficient for correcting our detailed altimetry record. Hopefully, more detailed time series of elastic vertical crustal motion estimates, together with rigorous error estimates, will become available soon, allowing us to completely remove the effect of crustal deformation.

2 Review from A. Aschwanden received on 12 Jul 2014

2.1 General Comment

Initialization remains a serious challenge in ice sheet modeling, and Larour et al. address this challenge by presenting the first use of time-dependent surface altimetry data from ICESat as part of the data assimilation process. Indeed, I'm not aware of any other study to do time-dependent data assimilation. The paper is very well written and structured, it's easy to read and understand. Adding the temporal dimension to data assimilation opens exiting new capabilities along with many new questions. Therefore I don't expect the manuscript to answer more questions than the topic raises; and the authors discuss in detail what should be done next. I'm looking forward to read any follow-up papers. The study is certainly worth publishing, and I only have a few comments that I wish to be addressed.

We thank Dr. Aschwanden for his positive review of the manuscript and our approach to assimilating altimetry signals into ice-sheet models. We have taken into account all the detailed comments below, resulting in an improved manuscript. We do agree that many questions are raised by this new type of modeling, and also look forward to working on further studies relying on automatic differentiation and assimilation of altimetry.

For diagnostic (non-transient) case, using surface elevation to constrain the initialization is similar to, but more sophisticated and principled than, flux correction methods used by, e.g. *Price et al.* [2011] and *Aschwanden et al.* [2013]. A study worth mentioning is *Habermann et al.* [2013] as they present snapshots of the evolution of basal yield stress at Jakobshavn Isbrae by inverting surface velocities for a number of years, and find that the observed speed-up is possibly linked to a drop in yield stress. This could be considered as a pre-stage to transient assimilation.

We thank the reviewer for pointing out these references. We have modified the introduction accordingly to include such references.

I'm not an expert in inverse methods myself so I'm not able to judge any technical aspects of the methods presented in the manuscript. It appears all sound to me, but maybe another reviewer could provide more insight.

Dr. Heimbach does indeed provide more feedback on the inversion methodology, which we address in the third review later on.

2.2 Technical Comments

- Equations need proper punctuation (mostly commas are missing after an equation).

  We are not sure how to properly handle this comment, as we do not believe that there is missing punctuation in our equations. We will wait for the editor input on this, may be it is TC policy to add punctuation in the equations?

- P. 2335, L. 14-22: Change "in the first section" to "in the next section", and adjust the remainder of the paragraph accordingly.

  Done.

- P. 2340, L. 24 You've already used n for the exponent of the flow law. It's clear from the context, but you may want to use a different variable.

  Done. Replaced the variable by m. Thank you for catching this.

- P. 2344, L. 20 Maybe I'm missing the obvious, but how can a firn compaction rate be negative?

  Negative rates correspond to cases where pore-space increases relatively, by addition of fresh new snow.

- P. 2347, L. 20-23 I'm not sure I understand what you mean with "adjust the overall mean of the entire time series so as to center it...". Could you clarify and add a sentence on how this influences the results?

  We refer to a very similar comment by Dr. Heimbach on this subject.

- P. 2348, L. 26. "Matches" sounds very strong and assumes that both variabilities are exactly the same. Are they?

  Indeed this statement was too strong. We have reworked this sentence accordingly.

- P.2350, L. 12. "significantly well" is awkward.

  Took out the "significantly".

- P.2351, L. 17. "iteration on iteration" is awkward.

  Replaced with "after each iteration".

- P.2352, L. 6. "check units, it should be kg.m^{-3}.

  Thank you for catching this typo. Corrected accordingly.

- P.2352, L. 7. remove "therefore".

  Done.

- P.2352, L. 23-26. Split into two sentences.

  Done.

- Fig. 1 I believe it's EPSG:3413.

  We double checked, and indeed believe it's EPSG:3411

- Figures. For readers who are not so familiar with NE Greenland, I suggest to indicate on the figures the locations of the outlet glaciers discussed in the manuscript (such as Storstrommen).

  We added the locations of 3 outlet glaciers: Storstrommen, 79 North and Zachariae Isstrom.

3 Review from P. Heimbach received on 21 Jul 2014

3.1 Main Comments

1. The first general comment is with regard to the choice of control variables relative to the model solved. Eqn. (1), (2) suggest that at every time step, a steady state momentum balance is being solved. The time-dependence enters exclusively through the continuity equation, expressed here as mass/volume conservation equation. This is common practice in ice sheet modeling, but the implication for formulating the control problem should be exposed:

   Allowing for a time-varying alpha amounts to adding a time-varying source term in eqns (1), (2), but which are assumed to be steady-state equations. The authors should discuss the interpretation or implications of their approach. It seems to me that the model may be problematic in representing the impact of a time-varying alpha on a time-varying stress balance. This may explain why the optimization of J using the gradient w.r.t. alpha is of limited success. I don't expect the authors to make changes to their simulations, but to address this issue in the model formulation and in the discussion.

   Related to this, I assume that the rationale for making alpha time-varying is that it might be physically connected to time-varying basal lubrication, e.g. through basal melt water (either via seasonal surface melt or geothermal flux or shear heating). I suspect that the main source of time-variability is the expectation that seasonal melt water at the bed would lead to intermittent (early-season) decrease in friction. This is supported by the discussion on p. 2353 (l. 2-14) of the relationship between basal hydrology and basal stress. However, this is not borne out by the inversion (see Fig. 6a,d). The question then is, what is the physical explanation for time-varying alpha? Alternatively, is the steady-state stress balance appropriate when using time-varying alpha?

Still related, I agree with the interpretation on p. 2349 (l. 14-19) of a "clear equivalence between SMB and surface thickening rate, while basal friction is a direct forcing to the stress-balance equations (Eq. 1) and (Eq. 2), which have no direct bearing on the surface thickening rate", but think this statement needs to be stronger (to re-iterate): The time-varying nature of alpha introduces a time-varying term in steady- state eqns (1), (2), a small inconsistency which the optimization may not be able to handle consistently.

We understand the reviewer's concern regarding whether our inversion can readily handle the time variable nature of the basal shear at the ice/bedrock interface, and we believe that one of the main goals of our manuscript is to actually demonstrate that time varying basal friction can be inferred accurately from time varying surface altimetry. However, we do not agree that equations (1) and (2) are steady-state equations onto which a time varying basal friction term has been added. Both equations originate from the full stress-balance equation, including acceleration terms which are negligible (which however does not make this a steady-state equation) and time variable stresses. Equations (1) and (2) derive from the stress-balance collapsed using assumptions (1) to (4) (described at lines 2336:14-19) where the time variable term due to friction (τ_{bx} and τ_{by}) comes from the vertical integration of the $\partial\sigma_{xz}/\partial z$ term in the stress balance. We therefore do not agree with the statement underlying the premise of comment #1 that "allowing for a time-varying alpha amounts to adding a time-varying source term in eqns (1) and (2)". This source term was not added, it is intrinsically part of the stress balance equation, and appears so when formulating the stress-balance according to the SSA formulation. This in our opinion does not explain why the optimization of J using the gradient w.r.t. to alpha is of limited success. Rather, we stand by our assessment in the discussion that the limited success is due to the inherent numerical bias of the mass-transport equations towards SMB and the bias of the stress-balance equations towards surface velocity and friction.

The rationale for making alpha time-varying is indeed that it might be physically related to time-varying lubrication and water pressure. However, it was not our intention to clearly demonstrate a link between seasonality of water runoff (which can transfer into seasonality of the water pressure at the base) and some type of seasonal variability in the evolution of the basal friction. We therefore do not agree with the reviewer's statement that absence of seasonality in the inferred temporal friction is proof that our approach is not numerically sound. It rather demonstrates that surface altimetry is a complex metric which is the result of many complex processes that cannot be easily inverted for.

Once again, we are not trying to avoid the difficulty in trying to demonstrate why our basal friction inversion would not be as efficient as the SMB one. We believe, as the reviewer states, that there is a clear bias in the inversion where surface altimetry will essentially translate into physically realistic inversions of SMB, and surface velocities will translate into physically realistic inversions of friction. We believe we have explained this quite thoroughly in the discussion.

2. I caution the authors to refer to "improved" surface heights (e.g. caption to Fig. 6), or "improved" alpha, M_s. Fig. 6b suggests that S is not improved throughout. Whether all changes in alpha, M_s lead to "improved" values is not clear. A better term would be "adjusted", i.e. the optimal values of alpha, M_s are adjusted such as to yield a minimum least-squares misfit function J. In some cases the adjustment will indeed be improved estimates, in other cases, they will compensate for other model or estimation errors.

   We agree with the reviewer's statements, in particular the fact that a better fit could indeed be the result of model and/or estimation errors being compensated for by the parameter being inverted for. We have modified the text accordingly to remove references to "improved" surface heights, in favor of "improved" best-fit to surface heights, or "adjusted" surface heights to observations.

3. p. 2342: The following statement: "... showing a computation time for the gradient of the cost function with respect to either alpha or M_s on the order of 4 times the computation time for the forward model." simply cannot be true, unless some very significant shortcuts have been taken. It is contrary to all accepted wisdom of algorithmic differentiation using operator overloading versus source-to-source transformation approaches for complex models. Please either revise this statement, or provide a description of which shortcuts have been taken, or provide a model setup that enables testing of this statement by outsiders. (Even if that factor should turn out to be much larger than 4 times, the author's achievement is still very significant).

   We do agree with the statement that our factor of 4 cannot be true, if we assume that the model is completely scalable. Herein lies the critical difference: we are here dealing with an SSA formulation that relies on the MUMPS direct solver, which accounts for 90% or more of the computation time [*Larour et al.*, 2012]. Therefore, during the AD phase of the computation, the solver still accounts for the majority of the computation time, which makes our ratio tend towards lower numbers than accepted wisdom as stated by the reviewer. We now allude to this in the manuscript, so that AD specialists are not confused by our statement.

4. p. 2342: l. 17/18: The sentence : "the fact that we do not rely on the adjoint-state but rather on AD to compute the gradient, and that the inversion is temporal in nature." is unnecessary and wrong (or a misconception of what AD does). The code generated via AD *does* compute the adjoint state at each time step (no matter which form of AD is used). Therefore, you *do* (have to) rely on computing the transient adjoint state. The only thing you have avoided is having to hand code the adjoint model of your time-varying model that computes this state. AD is only a shortcut for avoiding hand-coding the adjoint model, not a shortcut for avoiding computing the adjoint state.

   We do completely agree with the reviewer on this aspect, and have modified the manuscript accordingly.

5. p. 2345: l. 18: The sentence: "Assimilating altimetry data into a forward transient ice flow model presupposes that the model itself is spun-up in a way that more or less closely matches observations for the time period considered." is misleading or wrong. Nothing prevents an assimilation problem to be formulated in such a way that initial conditions and model parameters are adjusted such as to correct a poorly spun-up initial state (e.g., Goldberg and Heimbach, 2013). In fact, "data assimilation" in its most common usage in numerical weather prediction (NWP) is synonymous with finding initial states which lead to optimum fit to observations at analysis time (and optimum forecasts).
   A more accurate statement might be: "Since our assimilation method does not adjust initial conditions of the model, we have to rely on a spun-up model state which more or less closely matches observations for the time period considered. In general, the success of inverse methods applied to nonlinear problems often relies, in practice, on initial guesses of the independent variables that yield states that are not too far from observations."

   We totally agree with the reviewer, and have actually used the more accurate statement offered.

6. p. 2347: l. 20/21: "Because the model spin-up does not reach a configuration that matches the altimetry time series within a 1 standard deviation, we are still forced to adjust the overall mean of the entire altimetry time series so as to center it on the modeled surface height in 2006." I'm not sure I understand what this means. I think what is being said is that a time-mean bias (spatially constant or spatially varying?) is removed such as to obtain a better initial misfit? This needs to be described more clearly so it is more transparent to readers what is being done. Ideally, a figure should be added, depicting the true mismatch without the adjustment.

   This concern has been relayed by the first reviewer also, and we agree that a better explanation is needed. We have reworked the paragraph to show that a spatially varying time-mean has been removed from the altimetry time series, with a figure to more clearly depict it.

7. p. 2347: end of section 3.2: A description is needed regarding the exact nature of the time-variation of alpha and M_s. Is the period between two consecutive adjustments the same as the model time step (i.e. two weeks), or is it longer-period? This has repercussion on the dimensionality of the control vector. if Nx*Ny is the dimension of a 2-D field, then the control space would have dimensionality Nx*Ny*nUpdates. nUpdates could be either the number of time steps (roughly [2009-2003+1]*365/14), or a coarser partition of the integration period. Another question is why the inversion for alpha and M_s have been performed separately (l. 19,20). A formal inversion would invert for both parameters jointly.

   We follow the advice of the reviewer and improve the description regarding the nature of the time-variation of alpha and M_s. The period between two consecutive adjustments is the same as the model time step (2 weeks). The number of updates we rely on here are therefore the number of time steps in the model. In terms of carrying out the inversion for alpha and M_s separately, we do not yet have the framework required to do multi-parameter inversion, and do not believe we will have it in the foreseeable future, so it is intrinsically a framework restriction. Indeed, multi-parameter inversions would be more effective, and this will be hopefully carried out in further studies.

8. p. 2348: l. 2/3: I am not sure how it can be inferred from Fig. 4 that "best-fit to observations can only be improved by varying forcing over the entire space and time domain." All that Fig. 4 shows is that the gradients are space-time dependent. This, in turn, is a consequence of the nature of the observations. To see this, note that for a cost function of form:
   J = 1/2 (F(x) - obs)^2,
   the gradient is of the general form:
   dJ/dx = (dF/dx)^T * (F(x) - obs),
   i.e. the gradient is "driven" by the (linear) model (F(x)) vs. data (obs) misfit. To the extent that (F(x) - obs) is time-space varying, so will be the gradient.

   We agree with Dr. Heimbach and thank him for the explanation of how the gradient variability is intrinsic to the nature of the cost function we chose for the given temporal inversion. We improved the paragraph accordingly by showing how the results indeed convey the expected variability, and replicated the equation demonstrating the time-space variability of the gradient for a quadratic cost function.

   Related, p. 2348, l. 12-15 and p. 2349, l. 5-7: "For dJ/dalpha, this can be largely explained by the fact that basal friction is much higher there than near the coastline, making it much harder for equivalent variations in basal friction to impact ice flow dynamics and surface heights."

   This may be the case, but is an interpretation not readily borne out by

the analysis. The simplest explanation that is supported by the analysis is the same as above, i.e. the fact that:
dJ/dx = dF/dx * (F(x) - obs)
implies that for small misfits (F(x) - obs), which is the case inland, the gradient is small, no matter what the size of x (here = alpha), unless (dF/dx)^T itself would be very large (but which too would require demonstration) . Linking the smallness of dJ/dalpha to the largeness of alpha itself requires further scaling analysis.

We agree with the reviewer, and again thank him for the clear explanation of why gradients inland are expected to be smaller. As Fig 8a and 8d indeed shows, the misfit pre-inversion is already small inland, which implies that this will also be the case for the gradient. We therefore agree that linking the smallness of dJ/dalpha to the largeness of alpha itself requires further scaling analysis. We refined our statement in p.2348, l. 12-15 accordingly.

Still related, the "controlling mechanism" invoked on p. 2349 (l. 5-7) can instead be simply explained by the small residual model-data misfit in the regions suggested upward of the suggested demarcation.

We agree that the "controlling mechanism" is more simply explained by the low initial misfit. We modify the text accordingly.

9. p. 2350, l.9 onward: Figures 6c, g suggest that the optimization "corrects" winter mass balances for both positions I, II to be solidly negative, compared to their first- guess values which are near zero or slightly positive. Is this expected? The implication would of a negative mass balance not just during summer months but throughout the year would seem significant.

   This is indeed not expected, and is due to the fact that we have not constrained the input M_s to be within a certain error margin of its original value, hence leading to winters where there can be melt-rates, and summers that freeze. This is an issue in the current implementation of the method, which will be corrected in further studies. We modified this paragraph to make sure that the implication that there is a negative mass balance throughout the year is not inferred from our study.

3.2 Details

- p. 2332 l. 20: It seems more prudent to refer to the common terminology Global Mean Sea Level (GMSL) rise. Alternatively, refer to "sea level change", since regional sea level trends may be negative (i.e. sea level drop) over the last 20 years.

  Agreed, we now use Global Mean Sea Level instead of Sea Level.

- p. 2332 l. 21: Update to IPCC AR5 (plus relevant reference)

Done. We now refer to the Physical Science Basis [*Stocker et al.*, 2013].

- p. 2335: l. 14-21: correct all section numbers (section N -¿ section N+1)

  Done. Thank you for spotting this issue.

- p. 2335 l. 15: Here and throughout the manuscript (e.g., p. 2336, l.5; etc.) it would seem "nicer" and consistent with the estimation/control theory literature, to refer to "objective function" instead of "diagnostic".

  Agreed. We have referred to "objective function" throughout the manuscript in accordance with the estimation/control theory literature.

- p. 2336: l. 8: Replace "Ice flow on the NEGIS" -¿ "Flow of the NEGIS" (seems to me that the ice doesn't flow *on* the NEGIS, and "Ice flow" of the "Ice Stream" seems redundant).

  Done.

- p. 2339: l. 2: The cost function sums the SQUARED differences.

  Indeed. Thanks for spotting the issue. Corrected.

- p. 2339 l. 16: Here, and later in the manuscript (e.g., p. 2342) the notation J = F(alpha(t),M_s(t)) is not well defined, or misleading. If F indeed refers to the model (defined how? I guess the system of eqns. (1) to (6)) then J is not scalar-valued. Instead, I think what you mean is: J = J(F(alpha(t),M_s(t)))

  Indeed this is what was meant. We corrected accordingly.

- p. 2340: l. 8: Replace "adjoint theory" by "adjoint method"

  Done.

- p. 2342: l. 8: Reword: "... we can AD-compute dJ/dalpha, gradient of..." to "... we can compute dJ/dalpha, the gradient of..."

  Done.

- p. 2342: l. 12: It might be more conceptually more transparent to distinguish between first- guess α_0 and optimized $\alpha = \alpha_0 + \Delta\alpha$, i.e. write: "... we can infer an update $\Delta\alpha$ to α_0, such that $\alpha = \alpha_0 + \Delta\alpha$ leads to a simulated surface height evolution that minimizes the cost function".

  Thank you for the suggestion, which we integrated as is into the text.

- p. 2342: l. 13/14: Not the "inverted" alpha itself best fits the data, but the state computed with the adjusted alpha does.

  Done.

- p. 2342: l. 22, 24/25: "Here, we do not assimilate both forcing alpha and M_s." This statement is wrong, it mixes up dependent and independent variables. Observations are assimilated, not input variables. What you mean is either: "we do not invert for both ..." or "we do not adjust both ...". Likewise, the sentence: "which parameter assimilates existing altimetry observations most efficiently" is ill-worded.

  Thank you for catching this inconsistency. We amended the manuscript accordingly.

- p. 2343: l. 6: "by a simultaneous reconstruction of the surface topography" Use of *simultaneous* makes sense for reconstruction of A *and* B. A is surface topography. What is B? Otherwise drop *simultaneous.

  Indeed. We dropped simultaneous.

- p. 2346 l. 1/2: "instantaneous spin-ups" This seems a bit of an oxymoron (or the term "spin-ups" misleading), so perhaps add "or snapshot inversions". Also, in the following reference list, it seems warranted to add *Petra et al.* [2012].

  Indeed it is an oxymoron, but I don't believe the terminology "snapshot inversion", though more accurate, is used in the Glaciological Community. I would ask that we do not change this term. We added *Petra et al.* [2012] to the list of references.

- p. 2346 l. 6-9: It may be true that: "However, this approach relies on a steady-state thermal regime for the ice sheet, which is not realistic, ..." but the same is true for the approach presented here, see p. 2888, l. 3-5: "The thermal regime of the ice is not captured in our transient ice flow model ... We believe this approximation to be realistic". It would seem that the approximation made holds equally well in both cases. By the same token, the statement "usually leads to lumping any mismatch between model and observations into the inversion itself" is equally valid in both cases, to the extent that it refers to the thermal regime.

  Indeed, the approach presented here also relies on a steady-state thermal regime, we therefore cannot use this argument. Same holds for the error lumping argument. The difference actually lies in the fact that the instantaneous spin-up relies on a steady-state also for the mechanical stress-balance. We added "mechanical" to "thermal steady-state" to explain our rationale.

- p.2346 l. 21: "followed by a relaxation of the ice sheet/ice shelf over a period of 50,000 years" I am not sure what this means, or whether this is a common numerical method. I suggest describing what the "relaxation" involves (in fact, some authors refer to "relaxation" as a simple form of data assimilation, but I suspect this is not implied here?).

  By relaxation, we mean a transient run where all the forcing are kept constant. We added this description in the paragraph.

- p. 2346 l. 22: "The climate forcing is constrained by an SMB taken equal to ...". I don't understand what is meant here by *constrained*. I suspect the authors simply mean: "The climate forcing is represented by the time-mean SMB between 1971 and 1988". Similarly, it is somewhat unclear to me why the period 1971-1988 is chosen as "climatology". The Box et al. (2013) time series goes back to 1840, so why not taking 1850-1988 as a more representative climatology (i.e. a better average over decadal variability), or any other start date between 1840 and 1971? If the 1971-1988 time-mean is used for the integration prior to 1971, it would seem more likely that SMB undergoes an artificial jump in 1840 (the time at which the Box et al. time series is applied) than using a time-mean SMB which is more representative to 1840(?)

  We understand the rationale of the reviewer here, but we believe that the 1971-1988 is more representative of a steady-state ice sheet that would be better suited for a relaxation algorithm. There will indeed be an artificial jump when we connect the relaxation over 50,000 years to the LIA-present period, but this will avoid having to trust data that is less reliable the further back we go in the *Box et al.* [2013] time series. We added some description to this effect.

- p. 2348, l. 28 / p. 2349, l.1/2: I have difficulties seeing the "clear demarcation line" and "abrupt transition in ice thickness". I'd suggest adding corresponding isolines/ contours to Figs. 4 and 7 that delineate the transitions in question.

  We color coded the 1000 m contour level in Fig. 4 (now Fig. 5) to help delineate the transition. We now refer to the contour in the manuscript.

- p. 2349, l. 19/20: Reword "... between both methods ..." to "... between varying alpha or M_s ..."

  Done.

- p. 2353, l. 1: "... exhibits high variability ..." In space or time, or both?

  Both indeed. Done.

- p. 2353, l. 28/29 and p. 2354, l. 3-6: "Here, we propose..." A good proposition, one that has already been formulated by Heimbach and Bugnion (probably others before), and that has already been explored by Goldberg and Heimbach (2013), who used time-varying altimetry and surface velocities with inhomogeneous temporal sampling (to reflect heterogeneous InSAR vs. ICESat sampling, albeit in a synthetic experiment) to constrain a transient ice flow model and simultaneously infer best-estimate initial conditions and basal sliding.

  Apologies for repeating some idea already published, we now make reference to both suggested references in the manuscript.

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