We would like to thank the reviewer David Pollard for the evaluation of our study. Please find below the reviewer's comments in black font and the author's response in blue font.

Responses to David Pollard (Referee #2)

General comments:

This paper applies a simple method of adjusting basal sliding coefficients to obtain realistic ice thicknesses in an ice sheet model of modern Greenland. Similarly to previous simple methods used for Antarctica, the paper shows how the iterative method converges towards basal coefficient maps ("beta") that yield best-fit ice distributions. The method requires relatively short integrations, making it feasible for more complex models. The analysis is detailed and substantial, showing that the method functions well and yields meaningful results, and the paper will be of considerable interest to the modeling community.

Thank you for this comment.

My main concern is that, as described in the paper, there are large interior regions where ice thickness errors cannot be corrected due to internal deformation flow being too large, which detracts from the primary results. Additional runs to correct this are suggested below.

Following your comment, we now explore extensively the role of the enhancement factor and show that we are indeed able to correct the error for the interior regions using a lower enhancement factor. To this aim we considerably increased the number of simulations shown in the revised manuscript with respect to the initial submission. In light of these new simulations we address your comments in the following.

Main specific comment

Much of the paper's primary analysis in section 4 concerns the progress of the procedure as the overall length increases (increasing NBcycle). For given NBiter and NByear, the rms thickness error "dH" tends more or less monotonically to a minimum (Fig. 5), but total volume error "dV" overshoots zero and becomes more unrealistic again (Fig. 6a). The analysis (sections 4.2.3, 4.2.4, Table 1) is mainly concerned with finding values of NBcycle and associated NBiter,NByear, at which dH, dV (and dV/dt, but see below) are qualitatively the best (small) if the procedure is stopped at some point.

I think these results are not the most useful or meaningful, because there are substantial regions in the east-central Greenland interior where internal deformation flow is too large, producing too small ice thicknesses even with zero basal sliding. This prevents the dH and/or dV metrics from both converging to zero together as the procedure is extended indefinitely, and causes the "overshoots" in Figs. 6. This is fully described in the paper's *section 5, but only after the primary results of section 4 are presented.*

It would be better to address and fix the problem from the start in section 4, which would yield more meaningful results. The existing results, regarding the particular NBiter/NByear/NBcycle values where the dV and dV/dt metrics cross the zero lines, just reflect the influence of the problem region with excessive internal flow.

Also they depend on the choice of initial beta(x,y), which is arbitrary (as shown nicely by Fig. 3c), but if chosen further from the final state, needs more NBcycle cycles to reach the same point of evolution.

The problem is fully recognized in the paper's section 5.1, and a possible solution is implied in section 5.2, by trying different values of the enhancement factor Ef. My main suggestion is to repeat the procedure of section 4 for a range of Ef values, say Ef = 0.1, 0.5, 1, 1.5, 2, 2.5, 3. Hopefully just one long procedure would be sufficient for each Ef value, with just one set of NBiter,NByear values, and a large NBcycle of 10 or 15 (see below).

*I would anticipate that for the smaller Ef values, the persistent thickness errors in the Greenland interior can be corrected by adjusting local beta's, so both metrics dH and dV (and dV/dt) will converge towards zero and not overshoot (but see "basal temperatures", below). The main outcomes of the new section 4 would be (i) the value of Ef below which this occurs, and (ii) how long the overall procedure needs to be continued (how many NBcycle's) to reach acceptably small dH and dV. (Possibly the rate of convergence may be quicker for different ratios of NBiter and NByear, but I suspect not, and for the smaller Ef, everything depends just on the total number of years (NBiter+NByear)*NBcycle. Note that if dH converges on zero, then dV and dV/dt must too.*

This would of course require significant re-running of the model for the other Efs, and reorganizing sections 4 and 5, but would yield more useful and less arbitrary results in my opinion. One encouraging sign that it will work is how much better Fig. 10 looks (Ef=1) compared to Fig. 7b (Ef=3). (Much the same adjustment of Ef was done in Appendix B of Pollard and DeConto, The Cryo, 2012, called PDC12 here, but was not as important because their main results used a relatively low Ef).

Thanks for the in-depth analysis of our results. We fully agree with your comment and this is why we performed additional experiments varying the enhancement factor from 0.5 to 5 for a given set of Nb_{inv}, Nb_{free}, Nb_{cycle} values (former Nb_{iter}, Nb_{year}, Nb_{cycle}). As a result, Sections 4 and 5 have been completely reorganized. The results of these new simulations (with Ef ranging from 0.5 to 5) are now presented in Section 4 before discussing (Section 5) the sensitivity to the initialisation procedure coefficients Nb_{inv}, Nb_{free} (former Nb_{iter}, Nb_{year}). As you suggest in your comment, we are able to show that the enhancement factor can be used to correct the ice thickness error where deformation due to vertical shearing is predominant (e.g. interior region). In particular we show that for Ef ≥ 2, a larger Ef value leads systematically to a larger ice thickness RMSE. For lower Ef values (Ef < 2), we obtain minimum RMSE for Ef between 1 and 1.5. For Ef = 0.5, the ice thickness RMSE is slightly higher (with respect to that obtained for Ef between 1 and 1.5 and we still have positive ice thickness anomalies (w.r.t. to observations) in the ice-sheet interior due, in that case, to a too slow ice flow related to vertical shearing. These results are discussed in Section 4.2.1 of the revised manuscript.

In the new section 5, we investigate the sensitivity of the method performance to the Nb_{inv} and Nb_{free} parameters. As suggested, for each (Nb_{inv}, Nb_{free}) combination, Nb_{cycle} simulations have been performed with Nb_{cycle} = 15. We show that there is a strong decrease of the ice thickness RMSE after one cycle (Nb_{cycle} = 1) but only little improvement when using $Nb_{cycle} \geq 6$. These results are discussed in details in Section 5.3. As also mentioned in our response to your comment *pg. 10, Fig. 6, and pg. 11 line 10 to top of pg. 12,* the critical duration to obtain a good performance is defined by $Nb_{inv}*Nb_{free}$ because the initial condition for the different cycles is systematically the same: only the initial basal drag coefficient for step 1 is different (see Section 3).Finally, we have also to mention that in the revised paper, the ice thickness RMSE is the key parameter to assess the performance of our method. Moreover, the ice volume trend is no longer considered. Rather, we introduce a new metric that can be considered as the ice thickness change root mean square. This allows the compensatory biases to be circumvented (see Section 4.2.2).

Related to main comment

One complication involves the basal temperature field, i.e., frozen vs. thawed basal areas. Where the base is frozen, the procedure of adjusting beta is ineffective in reducing ice thickness errors of course. This is mentioned in the paper (pg. 11, lines 1-3), but because of its importance, I suggest showing a map of modeled basal temperatures Tb(x,y), perhaps near the top of pg. 11 where basal temperatures are discussed, and assessing it versus other established Greenland Tb maps (such as the recent modeling synthesis in MacGregor et al., JGR-Earth Surface, 2016).

Such a figure is shown in the revised manuscript (Fig. 1c). In Section 3, we also provide a brief comparison between our simulated distribution of frozen/thawed bed areas (inferred from the simulated basal temperatures) and the reconstructions of MacGregor et al. (2016): *"The resulting basal temperature after this long integration, presented as a difference with respect to the pressure melting point, is shown in Fig. 1c. It shows areas with temperature largely below the pressure melting point, associated with frozen bed, and areas with temperature at the pressure melting point (red colors), associated with thawed bed. Compared to the recent synthesis of GrIS basal temperatures (see Fig. 11 in MacGregor et al., 2016), our initial basal temperature agrees generally well with the reconstructions in the northwestern and northeastern parts of the GrIS but are probably overestimated, with a too large thawed bed area, in the eastern and central parts of the GrIS (not shown). The impact of ice temperature on the minimisation procedure is discussed in Sect. 5.1"*.

Also, it would help to mention this point in the description of the procedure itself on pg. 7. In the suggested new runs above, the model's basal frozen areas will prevent the beta-adjustment procedure from fully reducing the metric dH to zero (and dV). This can be assessed in the new results.

The importance of basal temperature is explicitly presented in the description of the method (step 1): "Owing to its design, the method is only able to correct for the ice thickness mismatch where sliding occurs, i.e. where the base of the ice sheet is at the pressure melting point."

It is also fully discussed in the results section (Sec. 4.2), when showing the results for the different enhancement factors.

With simple adjustment procedures (as here, and in PDC12), there is a valid concern that the problem is under-determined, i.e., there are more adjustable parameters than observed constraints, so errors due to one parameter may cancel errors in another parameter or in the model physics. Multiple combinations of Ef and beta(x,y) can produce the correct ice thickness H at a given point, and this is compounded by possible errors in model ice temperatures, both basal and internal (which affect ice rheology). One alternative for this study would be to fix all ice temperatures at some best-fit or at least modern spun-up state. That would (i) reduce total integration times for the procedure because of slowly varying ice temperatures, and (ii) somewhat alleviate concerns of under-determinedness.

In the experiments presented in this revised paper, the temperature equilibrium is done only once, using a fixed topography. For this kind of simulation, the time step can be greater than that used for a free-evolving simulation because the mass conservation equation is not solved. As a result the temperature equilibrium computation is not particularly computationally expensive. During the iterations, the temperature is allowed to evolve though it could have indeed been fixed. However, because the simulations are not very long we do not think that this would have changed significantly the minimisation results.

On a related matter, we acknowledge that our simulated temperature at the end of our fixed topography spin-up does not necessarily perfectly match the observations. Tuning the initial ice temperature is not an easy task because of the limited existing constraints (which mostly consist in basal temperature) and because of various degrees of freedom for such a tuning (paleo temperature, ice flow parameters and geothermal heat flux). It is true nonetheless that if our confidence in the simulated temperature field was increased, the under-determinedness aspect of the minimisation procedure would be reduced, it would not disappear. In Section 6, we added a discussion related to the uncertainty associated with the GrIS thermal state:

"[…] the overall performance of the method is critically dependent on the basal thermal state and points out that the finding of appropriate initial conditions with a simple adjustment procedure remains an undetermined issue. Actually, multiple combinations of

the enhancement factor and the basal drag coefficient can produce a simulated ice thickness close the observed one, but this cannot discard the possibility of errors in modelled basal and vertical temperatures. However, we have shown that our minimisation procedure is able to reduce the ice thickness mismatch regardless of the initial temperature profile. This offers the possibility to tune the thermal state to be as close as possible to the observations (inferred basal temperature as in MacGregor et al. (2016), or vertical profiles at ice core locations) before running the iterative minimisation procedure. Increasing our confidence in the vertical temperature profile would therefore increase our confidence in the choice of Ef and β values".

Another possible way to improve the underdetermined aspects would be to quantitatively compare with observed surface velocities (as done qualitatively in Fig. 8 and pg. 12, lines 12-17, see comment below), and somehow combine that comparison automatically into the adjustment procedure for beta(x,y) and Ef. This is just a suggestion for future work (not for this paper!), and connections could be made with other optimization techniques that fit to observed velocities (pg. 2, lines 24-25). Another step for future work could be to add a regularization term for beta(x,y) (Pattyn, The Cryo, 2017).

These two aspects are now fully discussed in the discussion section (Section 6). In particular, we suggest the possibility of including an additional metric related to surface ice velocities:

"Finally, we have shown in this paper that the iterative adjustment of β produces modelled surface velocities that compare well with the observed ones. This suggests that future work could include an additional metric related to surface ice velocities so as to further reduce the uncertainties associated with the choice of model parameters and variables".

Concerning the regularization term, please see our response to your comment referred to as *p11, Fig.7.*

Other specific comments:

pg. 2, line 10, regarding "Three main classes of initialization techniques have been developed:". Some of the text on this page blurs the distinction between initial conditions (model variables at start of integration) and boundary conditions (externally prescribed quantities).

We have substantially reshaped the text here and we are now more specific on initial conditions with respect to boundary conditions. We clarify what the initialisation procedure for ice sheet model is at the beginning of this paragraph:

"Reliable simulations of the GrIS require a proper ice sheet model initialisation procedure to avoid an unphysical model drift which can be caused by inconsistencies between the ice-sheet model initial conditions and the boundary conditions (external forcing fields). These initialisation procedures consist in finding the initial physical state of the ice sheet (such as the internal temperature), the model parameters, and sometimes the boundary

conditions, that best reproduce the observations with a minimal model drift."

Techniques #1 and #2 discussed on this page are intrinsically concerned with initialization, but I would argue that beta is a boundary condition, and procedures to adjust it are a distinct type from #1 and #2. (For instance, #3 could first be used to produce a map of beta, and then #1 or #2 could be used with that map to produce an initial model state).

We agree with this comment. This has also been pointed out by S. Price (referee) and we acknowledge that the initial version was not clear. The aim of the initialisation procedure is to find: the physical state of the ice sheet and the model parameter and/or the boundary conditions that reproduce the observations and allow for a minimal model drift for prognostic experiments. The three methods discussed in the first version of the paper aim at answering this but they are not mutually exclusive. This part has been substantially rewritten with clarity in mind.

pg. 2, line 30, or elsewhere: Note that, as well as PDC12, Pattyn (The Cryo., 2017) applied the method in his Antarctic model, using it both with Weertman sliding (as here) and Coulomb friction laws. Also note that linear sliding (n=1, Eq. 2 here) is not a requirement, and the procedure can be applied essentially as is to non-linear sliding (n>=2), as in the above papers).

Thank you for this information. We have thus added reference to Pattyn (2017) and specified the possibilities of applying the method using both linear or non-linear sliding laws: "*Here, we present a new iterative minimisation procedure that relies on the same basic principles as those developed by Pollard and DeConto (2012) (referred to as PDC12 in the following) and applied by Pattyn (2017) for the Antarctic ice sheet using linear and non-linear sliding lows."*

pg. 2, line 29-30. The preceding text on this page mentions disadvantages of methods

 # 1 and 2. Disadvantages of the simple inverse method could also be mentioned here:

- *A) there are (probably) cancelling errors in the model physics hidden by errors in the basal coefficient map, and*
- *B) the method as in sections 3 and 4 cannot fix ice thickness errors where the bed is frozen.*

We agree with this. We added: *"However, methods that choose to invert the basal drag coefficient only are not able to correct ice thickness errors in regions where there is no sliding (i.e. where bed is frozen). Moreover, while inverse methods are designed to produce an ice sheet state close to observations, the inferred basal drag coefficient may cancel errors coming from erroneous simulated basal temperatures and/or model physics shortcomings. Yet, as outlined by Pollard and DeConto (2012), the risk of cancelling errors is of lesser importance compared to those related to inconsistencies between internal conditions and surface properties that will likely to be considerably reduced with expected* *future improvements in ice-sheet models and better observations of basal conditions".*

pg. 4, line 5: In most places, beta is appropriately called a "basal drag coefficient", i.e., larger for stickier beds, smaller for slipperier beds. Here it is called a "basal sliding coefficient" which suggests the opposite sign. To help readers, check that "drag" is used throughout.

As recommended, we now call β the "basal drag coefficient" throughout the revised paper.

Pg. 7, Eq. 5: ... + Uˆsli is in error, I think, should be ... + Uˆdef.

Thanks for noticing, the error is now corrected.

pg. 7, Eqs. 3-7, and Fig. 4: After careful reading, I think I understand the procedure details, but am not sure. First, it would help to state earlier whether NBiter, NByear and NBcycle are years, or number of iterations (on pg. 7 around line 18; it is done at top of pg. 8, but earlier would help). As a suggestion, a numbered list of sentences might help to communicate the procedure, something like:

1) *Eqs. 3-7 are applied at the end of every model timestep, adjusting beta iteratively for the next timestep. The model is run in this way through NBiter years.*
2) *The model is then run in "free" mode, i.e., with beta unchanged from its state at the end of (1), through NByear years.*
3) *Steps (1) and (2) are repeated NBcycle times.*
4) *Finally the model is run for an additional 200 years in "free" mode with beta unchanged.*

I am not sure if all the above is correct, especially step (4). Possibly the extra 200 years is run after every cycle of (1) and (2), i.e., as part of every NBcycle cycle. That seems to be implied by Fig. 4, because the upper black arrow for the NBcycle cycle includes everything including the 200-year integration. But if that were the case, it would be puzzling because it would be the same as tacking 200 years onto every NByear integration (my step (2)), i.e., just increasing the value of NByear by 200 and having no final step (4).

We acknowledge that the description of the procedure was not clear. Actually, it is based on points (1) to (3) you mention. We have substantially rewritten the description of the minimisation procedure with clarity in mind. In particular we have also added a bullet-point summary as you suggested. We have also modified the schematic representation of the iterative procedure.

pg. 7, Eqs. 6 and 7: What if Ucorrˆsli in Eq. 6 is zero or negative, and so yields infinite or negative beta's in Eq. 7? Physically this would occur when the internal deformation velocity alone is greater than the required total velocity, so the sliding velocity would have *to be negative. This is presumably handled by imposing maximum limits on beta, as mentioned later on pg. 15, line 7 (occurring in the Greenland interior where Ef is too high). It would help to describe the use of maximum (and minimum?) limits on beta in section 3 as part of the procedure.*

You are right, we effectively put limits on the value of the basal drag coefficient (from 1 to 5 10^5 Pa yr m^{-1}). We added this precision in the revised manuscript: *"It should be noted that $\overline{U_{corr}^{sli}}$ can be lower or equal to 0, leading to infinite or negative basal drag coefficient. This can happen when the velocity due to vertical shearing U^{def} is greater or equal to $\overline{U_{corr}}$. In this case we artificially impose a no-slip condition by assigning to the basal drag coefficient a maximum value set to 5 10^5 Pa yr m^{-1}. On the other hand, in case of too small U^{def} velocity, β may be as low as 1 Pa yr m^{-1} to facilitate ice sliding".*

pg. 10, Fig. 6, and pg. 11 line 10 to top of pg. 12. In my opinion the ice volume trend dV/dt is not fundamental. In the new suggested runs with lower Ef values (see main point above), I think the convergence of dV and dV/dt towards zero would be smooth, and the size of dV/dt would just indicate how far along (how many NBcycle's) the procedure has been run. If that is true (bearing in mind the caveat related to basal frozen areas above), then the final dV/dt can be made as small as needed simply by continuing the procedure longer (for instance to provide a near-equilibrated initial ice-sheet model state for subsequent experiments).

The problem with dV/dt is that there are compensatory biases that can lead to a near zero dV/dt while the ice sheet is far from equilibrium. You are right nonetheless: the longer the model runs, the smaller dV/dt is. However, the initial condition for the different cycles is systematically the same, only the initial basal drag coefficient for step 1 is different. As such, considering more cycles does not mean necessarily getting closer to the ice sheet equilibrium and the critical duration for convergence is only defined by $Nb_{inv}*Nb_{free}$.

In the revised version of the manuscript, the total ice volume is no longer considered as a criterion of the method performance, and its evolution for the different enhancement factors is only discussed to introduce the idea of compensatory biases. To circumvent the problem of compensatory biases and, to assess the model drift, we compute a new metric (instead of dV/dt in the initial version of the manuscript) defined as the root mean square ice thickness change:

$$\xi(t)= [< (H(t)-H(t-1))^2 >]^{1/2}$$

pg. 12, lines 12-17. Regarding Fig. 8, it might be worth pointing out that if ice thicknesses are correct, and if the surface mass balance is realistic, then for an ice sheet in equilibrium, total velocities must be correct. So a comparison with surface velocities is, in principle, just a test of the model's split between total and surface velocities.

We agree with this comment. This is now explicitly mentioned in the description of the method (Sec. 3) and when presenting the ability of the model to simulate realistic ice velocity for different enhancement factor (Sec. 4.2.3).

Section 3: *"Our method does not use the observed surface velocity as a constraint. However, at the end of the minimisation procedure (e.g. minimal thickness error and minimal drift), the simulated velocity tends nonetheless to approximate the balance velocity, that is the depth-averaged velocity required to maintain the steady-state of the ice sheet".*

Section 4.2.3: *"Our iterative minimisation procedure aims at simulating an ice thickness as close as possible to observations. Hence, the observed ice velocity is not used as a target by the model. However, because our procedure generates an ice sheet at quasi-equilibrium (trend ξ close to 0), the simulated velocities are close to the balance velocities, which in turn are supposedly close to present-day observations".*

pg. 11, Fig. 7: The narrow (red) bands with too thick ice around southern and central margins, where flow is in deep valleys and fjords through coastal mountains, are similar to errors in PDC12 over the Transantarctics. The discussion there about under- resolved bed temperatures may be relevant here, and a modified Tb based on sub-grid bed roughness may be a possible solution. (Related discussion is on pg. 17, lines 30-33).

This issue has been addressed in the Discussion section (see Section 6):

"Another limitation of the method may come from the model resolution. The succession of higher/lower ice thickness due to the succession of valleys/ridges in mountain areas may be poorly resolved. Owing to the insulation effect of the ice, this may lead to an erroneous representation of the basal temperature patterns, and SSA regions may be erroneously interpreted as frozen bed regions and vice versa (Pattyn, 2010). This drawback is clearly illustrated in our study in Figure 6 (Ef=1). Indeed, the simulated ice thickness obtained with the inversion procedure is generally less than 50 m in most GrIS areas, but can be greater than several hundred meters in coastal mountain ranges such the central eastern margin area where ice flow occurs in deep valleys. An alternative solution consists in correcting the basal temperature to account for bedrock roughness and, similarly to what was done in PDC12 to improve their inversion procedure in the Transantarctics".

pg. 5, line 7: Maule et al. (2005) has geothermal heat flux maps only for Antarctica, not for Greenland, I think.

For the SEARISE project a geothermal heat flux for Greenland was provided by Mike Purucker (co-author of the Fox Maule et al. (2005)) and colleagues. Because it has remained unpublished, they recommended at the time to cite Fox Maule et al. (2005) when using this data. Here is the link to the data:
http://websrv.cs.umt.edu/isis/index.php/Greenland_Basal_Heat_Flux

pg. 5, line 10: Should be Fig. 3a, not Fig. 2a.

You are right, although Fig. 2 is now the one in which we show the basal drag coefficient, so it actually is Fig. 2a in the revised manuscript.

pg. 6, Fig. 3a. Just for interest, where do the finely spaced N-S lineations in basal drag coefficients in western Greenland come from, in the GRISL1 ice2Sea simulations?

We did not investigate specifically this. In fact, these lineations are present in all our inversion results, even if they are sometimes less visible. We guess that it could be an artefact related to the interpolation of the original ice thickness from Bamber et al. (2013) to the GRISLI grid at 5km.

pg. 7, line 19: change to "let the model freely evolve".

This has been rephrased as: "The second step consists in running a new free-evolving simulation but this time using a time constant (but spatially varying) basal drag coefficient, i.e. the last inferred basal drag coefficient of the first step".

pg. 9, Fig. 5: To be consistent with pg. 8, line 3, the labels in the key in the top right hand corner should be "NBiterˆ20 - NByearˆ50", "NBiter_20 - NByearˆ100", etc., (where ˆ means superscript). Same for Figs. 6 and 7. Also, for consistency throughout, use either NB... or Nb...

We no longer use this notation in the revised version of the manuscript.

pg. 9, line 11: ~10000 Gt: It looks more like -12000 to -13000 in Fig. 6a.

This number no longer appears in the revised manuscript.

pg. 10, Fig. 6 caption: It seems a bit confusing to have total volume in Gt, and total ice volume trend in mm yr-1. (Presumably the latter is an average over all ice surfaces). It may be clearer to have the latter in Gt yr-1.

As mentioned earlier, we no longer present the trend in ice volume. Our new metric, the root mean square ice thickness change, is expressed in cm yr^{-1}.

pg. 11, Fig. 7 caption, last line. Nbcycleˆ4 should be Nbcycleˆ5 or Nbcycleˆ7, I think, from Fig. 5.

True. This figure does no longer appear in the revised manuscript though.

pg. 14, Fig. 8 caption: Is there a reference for this RADARSAT surface ice velocity map?

In the first version of the paper, we used the surface ice velocity map from Joughin at al. (2010). This dataset has been updated in the revised manuscript and we now use data taken from Joughin et al. (2018). This reference has been added in the Fig. 10 caption (former Fig. 8).

pg. 16, line 4: Change to "allows us to...", or "allows the deformation to decrease and thus..."

This sentence has been moved to Section2 in the description of the GRISLI model when introducing the role of the enhancement factor. It has been changed in: “*Lower enhancement factors lead to lower deformation rates and as such to slower ice velocities”.*