

**We would like to thank the reviewer Stephen Price 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 Stephen Price (Referee #2)**

### **SUMMARY**

*This paper presents a detailed study of a proposed method for providing optimized initial conditions for ice sheet models. The method attempts to formalize ad hoc approaches proposed and applied in a number of previous studies. Because the method does not use a formal PDE constrained optimization framework (hence the description as "ad hoc"), it can be expected to be applicable to, and potentially used by, a wider range of ice sheet models (e.g., adjoint-based methods are not required for calculating gradients and minimizing cost functions).*

*In the manuscript, the authors do a generally good job of 1) carefully explaining the method (although some confusions remain in parts – see below), 2) interpreting how and why the method works, 3) demonstrating the overall success of the method as applied to a realistic Greenland ice sheet application, and 4) exploring the sensitivity to various aspects of the method. Overall, the method shows promising results and the authors are honest about its shortcomings.*

*While I have some possibly significant points for the authors to consider and address in revision (noted below in more detail), overall this paper is interesting, well written, presents significant and useful findings, and clearly falls within the scope of GMD.*

*Thank you for your positive evaluation. We hope that we address your concerns in the following.*

### **MAJOR COMMENTS**

*Where applicable, page and line numbers in comments below are referred to as "x, y:", where x = page number and y = line number.*

*1,11: "spin-up parameters" – this terminology, "spin-up" and "parameters", is confusing, and used throughout the paper. "Spin-up" is first referred to as an existing, standard method for initializing and ice sheet model (on p.2), then later it is used interchangeably to describe the new method described here. I think the two should be clearly distinguished throughout the paper. Similarly, "parameters", unless clearly distinguished, are generally going to be thought of as belonging to the dynamic ice sheet model (e.g., the sliding coefficient is often referred to as a tunable "parameter"). The method proposed here is really more of a nested iteration, and some coefficients used to specify the number of iterations that take place in each loop (more comments on this below). Starting on p. 4, section 3, it seems like it might make sense to refer to this as something other than a "spin-up" method, which has historical associations with your "free spin-up" description. Call it an iterative minimization, or something like that?*

We agree that the terminology used to describe the method in the initial version of the manuscript was confusing. In the revised manuscript we use “spin-up” only for the long-term free evolving simulations as in Goelzer et al. (2018). Following your suggestion, we referred to our method as iterative minimisation procedure or minimisation procedure.

We still use the term “parameter” to refer to the coefficients of the model but following your advice, we systematically distinguish between ice-sheet model parameters and minimisation procedure parameters.

There was also some possible confusion with the terminology for the different parameters used in our procedure.  $Nb_{iter}$  represents the duration of the period during which we compute the basal drag coefficient. During this period, the basal drag coefficient is updated at each model time step (i.e. one year in our case, specified in the revised version of the manuscript). The term “iter” for this parameter is misleading as this step corresponds to a unique continuous simulation without iterating/looping back to a previous state of the model. For this reason, we changed  $Nb_{iter}$  to  $Nb_{inv}$  in the revised version. For sake of clarity,  $Nb_{year}$  is now referred as  $Nb_{free}$ , as it corresponds to the duration of the free-evolving simulation performed within the 2<sup>nd</sup> step of the procedure (see Section 3).

*2,10-30: Here, methods 2 and 3 are discussed as distinct from one another. But in reality, does anyone ever do just 2, or do 3 without doing 2 first? It seems like these are most often combined into a single method: use a fixed topography to spin-up the temperature (and maybe also the velocity field, so that the temperature and velocity are internally consistent), and then use that temperature field along with an inverse method to calculate velocities that better match observations.*

We agree with your comment. This has also been pointed out by D. Pollard (referee 1) 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 here aim at answering this but they are not mutually exclusive. This part has been substantially rewritten with clarity in mind (From P2 L18 to P3 L15).

*4, section 3: Somewhere in here, you might discuss or mention the work of Perego et al. (2014, JGR Earth Surf., 119, p.1894), which has very similar overall goals to that discussed here, but using a formal minimization framework (e.g., your Figure 2b is analogous to their Figure 1, although the timescales are different).*

Thank you for mentioning this omission. We now mention the study of Perego et al. (2014) in the introduction and in Section 3:

*“While numerous studies are based on fitting the modelled ice velocities (e.g., Gudmundsson and Raymond, 2008; Arthern and Gudmundsson, 2010; Morlighem et al., 2010; Gillet-Chaulet et al., 2012; **Perego et al., 2014**), or both surface velocities and basal topography (**Perego et al., 2014**; Mosbeux et al., 2016), only few authors opted for fitting ice surface elevation (Pollard and DeConto, 2012; Pattyn, 2017). Here, we decided to adjust the basal sliding velocities via the adjustment of the  $\beta$  coefficient to fit the GRS ice thickness to the observed one. Similarly to **Perego et al. (2014)**, our choice is motivated by the need to refine the estimates of GRS*

*contribution to future sea-level rise without the sea-level rise signal being contaminated by unphysical transients from the initial condition. However, while **Perego et al. (2014)** adopted a formal minimisation approach (i.e. adjoint-based model) we suggest instead an ad hoc method potentially applicable to any ice sheet model.”*

*6, 4-5: “...performance in terms of trend and error in simulated ice volume compared to observations”. While you do somewhat address the mismatch between observed velocities and /or ice flux later in the paper, I think it would make more sense to bring it up here. Or even earlier, when you first discuss the metrics you are going to use here. I kept wanting to see some discussion on that and felt like it was being ignored. It would have helped if you had stated early on that you were going to look at this topic later on in the paper.*

Our method is based on fitting the simulated ice thickness to the observation while the observed velocity is not used to constrain our results. At the end of the minimisation procedure (minimal thickness error and minimal model drift), the simulated velocities are close to the balance velocities, which are, in turn, expected to be close to the observed velocities. In the revised manuscript, this point is mentioned in Sec. 3 at the end of the description of the minimisation procedure:

*“In the following, we also discuss the spatial patterns of ice thickness and ice velocity mismatches with respect to observations. 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”.*

We also dedicate a section on the simulated velocities for a range of enhancement factors in the revised manuscript (Sec. 4.2.2.c).

*6, Figure 4: I found this figure a bit confusing. A couple of ways that might help to improve it include 1) tying it to the discussion in the text more clearly (and vice versa – refer to the steps in the figure when you are describing them in the text) and, 2) drawing it as a set of nested loops instead of a left-to-right flow chart. It seems to me like what you describe is two back-to-back loops ( $Nb\_iter$  followed by  $Nb\_year$ ) that both sit inside of a larger, outer loop ( $Nb\_cycle$ ). A different figure might capture that better (it could still include parts of what you have here).*

We have completely redesigned the schematic representation of the method (Fig. 3 in the revised manuscript). Compared to the previous version, the figure is largely simplified. It still consists mostly of a left-to-right flow chart because there is a temporal continuity between the different steps: the results of the basal drag coefficient computation (step 1) feed the free-evolving simulation (step 2). However, the outer loop in which the two steps are nested appears now more clearly. We also specifically refer to this schematic representation when needed in the description of the procedure.

7, steps 1 and 2: Note that what you describe here in steps 1 and 2 is essentially identical to the iteration described in Price et al. (2011; PNAS, 108(22) – see “methods” and SI for more details), except that they are using observed and modeled velocities rather than observed and modeled ice thickness to adjust the sliding coefficient). Also, it took me a while to figure out exactly what “Nb\_iter” was. It’s not immediately clear why this is >1 (i.e., what are you iterating on?). Eventually, I guessed that you are allowing the new sliding coeff. and the model velocities to come into some sort of equilib. with one another. If that is true, you should state it explicitly!

It is true that the assumptions made to report the modification of the sliding velocity to the basal drag coefficient is essentially similar to those of Price et al. (2011). This is now acknowledged in the description of the method. However, in addition to the differences you mention, Price et al. (2011) also maintain a fixed geometry, which is not the case here. The fact that we systematically have a free-evolving ice elevation is now clearly stated in the revised version of the manuscript to avoid any confusion.

Nb<sub>iter</sub> (now Nb<sub>inv</sub>) is the duration of the period during which the basal drag coefficient is computed. It does not involve any iteration as it is simply a free-evolving simulation for which the basal drag is updated at each model time step. This is now better explained in the revised paper.

Figures 5 and 6: The labeling of the legend should be changed here to “Nb\_year” rather than “Nb\_iter”. It’s too easy to confuse what you are varying here as currently labeled. It takes careful reading to understand that Nb\_iter is actually held fixed while you vary Nb\_year. You could use Nb\_year instead and just mention in caption that the value of Nb\_iter is the same for all.

This notation is no longer used in the revised manuscript and the sensitivity to Nb<sub>free</sub> (former Nb<sub>year</sub>) and Nb<sub>inv</sub> (former Nb<sub>iter</sub>) is assessed in a dedicated section (Sec. 5.3).

End of p.9 to start of p.11 – It took me a few readings to understand the explanation here. I think it could be written a bit more clearly. The point is that the volume metric needs to be used carefully because it cannot discern compensating errors (overall too thin in the interior and too thick at the margins cancels out and looks like a good match), and thus one either needs to look at the spatial pattern of thickness errors or include some other metrics.

This was indeed the idea behind this section. However, we now discuss this point when presenting the results for a range of enhancement factors. In doing so, the compensating errors appear more clearly as we show 2D maps of ice thickness mismatch. We would also like to draw your attention to the fact that the ice volume, as well as ice volume trend, are no longer used as metrics in the revised manuscript. This avoids artefacts related to compensating errors. Rather, we use the ice thickness root mean square error and the ice thickness changes root mean square error. The latter is a metric of the drift of geometry and is defined as (see Sec. 4.2.2.b):

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

12, 12-17: *This discussion of the model fit to observed velocities is appreciated. I think it would make sense to mention much earlier in the paper that you are going to look at this. The lack of discussion of the importance of getting both the thickness AND velocity state and trends correct (and hence the flux correct) early on in the paper made me wonder how useful the method could be. At the same time, while the fit to observed vels looks good by eye, I think it would be appropriate to give a slightly more quantitative measure for how well the final initial condition matches observed velocities (e.g., RSME of speed). I don't think a relatively poorer match to the velocities (relative to the thickness) really speaks poorly of the method as there are times when having a near steady-state initial condition might be more important than matching the velocities better. But overall, it would be good to know how easily a good match to velocities follows a good match to the thickness / volume.*

As mentioned above (see our response to your comment referred to as 6, 4-5), we added the following at the end of the method description (Sec. 3): *“Our method does not use the observed surface velocity as a constraint. However, at the end of the minimization 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”*

We agree on the fact that a discussion about the ice velocity RMSE could have been included. However, from our experience, this would have been not very informative because of two main reasons:

- i) Ice velocities are highly spatially variable and present their maximum values at the ice sheet margins. This means that small errors in the simulated extent of the ice sheet lead to important discrepancies with observations. As such, marginal regions, which represent a small fraction of the ice sheet, have more weight for metrics such as the RMSE.

- ii) The ice streams have generally a very fine structure (~100 m), and the aggregation of this fast moving ice with neighbouring slow moving ice is not necessarily meaningful at 5 km resolution.

We have nonetheless computed the RMSE of velocity for the different enhancement factors considered in this revised version. The evolution of the ice velocity RMSE as a function of the number of iterative cycles ( $Nb_{\text{cycle}}$ ) is shown in the Supplementary Material (Fig. Supp. Mat. 1). This figure confirms the conclusions drawn from the 2D maps (Fig. 11): for large  $E_f$  values, the agreement with observations is poorer than for low  $E_f$  values. In addition, performing more cycles does not improve the RMSE. This conclusion is valuable for both ice thickness and ice velocities.

*Section 4.2.4: Do you have any physical explanation for the lack of sensitivity to the value of  $Nb_{\text{iter}}$ , or why  $Nb_{\text{iter}}$  is better at smaller values?*

$Nb_{\text{inv}}$  (former  $Nb_{\text{iter}}$ ) does play a similar role to  $Nb_{\text{free}}$  (former  $Nb_{\text{year}}$ ) on the computed RMSE: a longer  $Nb_{\text{inv}}$  leads to a smaller RMSE. In the original version of the manuscript, we discarded the simulations with large  $Nb_{\text{inv}}$  because the volume difference w.r.t. observations was larger than for small  $Nb_{\text{inv}}$ . This was due to the use of an enhancement factor of 3 leading to too high deformation-driven velocities and thus to negative ice thickness biases in the interior of the ice sheet. We fully discussed this in the revised manuscript.  $Nb_{\text{inv}}$  has nonetheless a smaller impact than  $Nb_{\text{free}}$ , probably because of the

chosen values ( $Nb_{\text{free}}$  varies from 50 to 400 years while  $Nb_{\text{inv}}$  varies from 20 to 160 years) and also because of a greater change induced in  $\overline{U_{\text{corr}}}$  at each iteration for large  $Nb_{\text{free}}$  values.

*Figure 8: I am actually quite surprised to see that this method somehow “gets” the NEGIS in the modeled velocity field. Can you confirm if this is still the case when you start the iteration from a uniform value of beta? It seems like it would be very hard for the iteration to form this subtle feature in the model without some direct connection between the sliding coefficient and the velocity field (the topography is too subtle and it doesn’t seem like the metrics being used could possibly discern the necessary variations in the sliding coefficient based on the subtle changes in ice thickness). I’m curious if it is somehow a “relict” feature that exists primarily because of the initial sliding coefficient field you started with (which, for ice2sea, may have been tuned somehow to reproduce the NEGIS).*

Having a good representation of the NEGIS could indeed be a reminiscence of the initial 3D fields as the 30,000-yr temperature equilibrium has been computed using the Ice2Sea basal drag coefficient, which is itself derived from the inversion of ice velocities. However, it seems to be a robust feature of the minimisation procedure since the NEGIS is well reproduced even when starting from a homogeneous basal drag coefficient.

We have added this discussion in the revised manuscript (Sec. 4.2.3):

*“Interestingly, the extent of the NEGIS is particularly well represented, in particular for lower enhancement factors (Fig. Supp. Mat. 2). This can be a relic of the long temperature equilibrium performed with a time constant basal drag coefficient taken from Ice2Sea experiments (Edward et al., 2014), in which the NEGIS is well delimited (Fig. 2a). However, because this feature is still present when starting the iterations from a spatially homogeneous basal drag coefficient (see Sec. 5.2), it can also suggest that there is some topographic control of this feature as the adjustment of our local basal drag coefficient is very effective in reproducing the observed velocity in this area. Having a good representation of the NEGIS is an encouraging sign for the performance of our minimisation procedure, especially since most models fail to achieve this (Goelzer et al., 2018)”.*

*16, 5.2: I was also glad to see this section, as it seemed like a logical next step given the limitations of the method for adjusting the ice speed and ice thickness in the interior. However, I was expecting at least maybe the suggestion that one could combine the method of tuning the sliding coefficient with a similar method for tuning  $E_f$  where the ice was determined to be frozen to the bed. It seems like the exact same method could be used to iterate on the value of  $E_f$  that is used to iterate on the value of the sliding coefficient. Have the authors thought of trying this? It seems relevant to at least speculate on, or comment on as a logical next step.*

For the revised manuscript, we did not use an iterative method (similar than that applied to the basal drag coefficient) to adjust the enhancement factor, but we performed the minimisation procedure for various values of the enhancement factor ranging from 0.5 to 5 to examine the impact on deformation rates. The results are now presented in Section 4 (instead of Section 5 as in the initial version) and are also discussed in terms of basal

thermal state (thawed vs frozen bed areas, see Section 4.2.2). Moreover, we have also addressed this point in the Discussion section (see Sect. 6).

*17, 5-9: It would be interesting to see a 1:1 plot of the sliding coefficient values for the two different initial conditions. This would be a nice visual way of convincing the reader that there really is little sensitivity to the initial value of the sliding coefficient. As noted above, it would be very nice to see a comment here on whether or not the NEGIS is still an “emergent” feature when starting from a uniform sliding coefficient.*

Such a figure is shown below (Fig. 1). It confirms that the final adjusted basal drag coefficients (obtained when starting from Ice2Sea and from  $\beta=1$ ) are quite similar despite persisting local differences that make the plot to appear noisy. Note that the ice thickness RMSE and the ice thickness trend obtained with both initial basal drag coefficients are almost identical. Moreover, the ice thickness and surface velocity differences remain very small (see Fig. S3b and Fig. S4b). These results have been presented in Section 5.2.:

*“Using  $Nb_{inv}=20$ ,  $Nb_{free}=200$ , and  $Nb_{cycle}$  varying from 1 to 15 with  $E_f=1$ , we obtain a minimum ice thickness RMSE of 49.9 m and a trend  $\xi$  of 15.1 cm yr<sup>-1</sup>. While there are some minor spatial differences in terms of the inferred basal drag coefficient (Fig. 2c), the aggregated metric such as the RMSE and the trend are identical to the results presented in Tab. 1. In the same way, the simulated ice thickness and surface velocities obtained with  $\beta = 1$  present very small differences with those obtained when starting from the Ice2Sea basal drag coefficient (Figs S3 and S4). This illustrates the robustness of the method and shows that it does not depend on the chosen initial distribution of the basal drag coefficient”.*

Fig. S4 also shows that the NEGIS ice velocities differences are negligible, despite slightly higher in the  $\beta = 1$  case, demonstrating that the NEGIS is still an emergent feature.

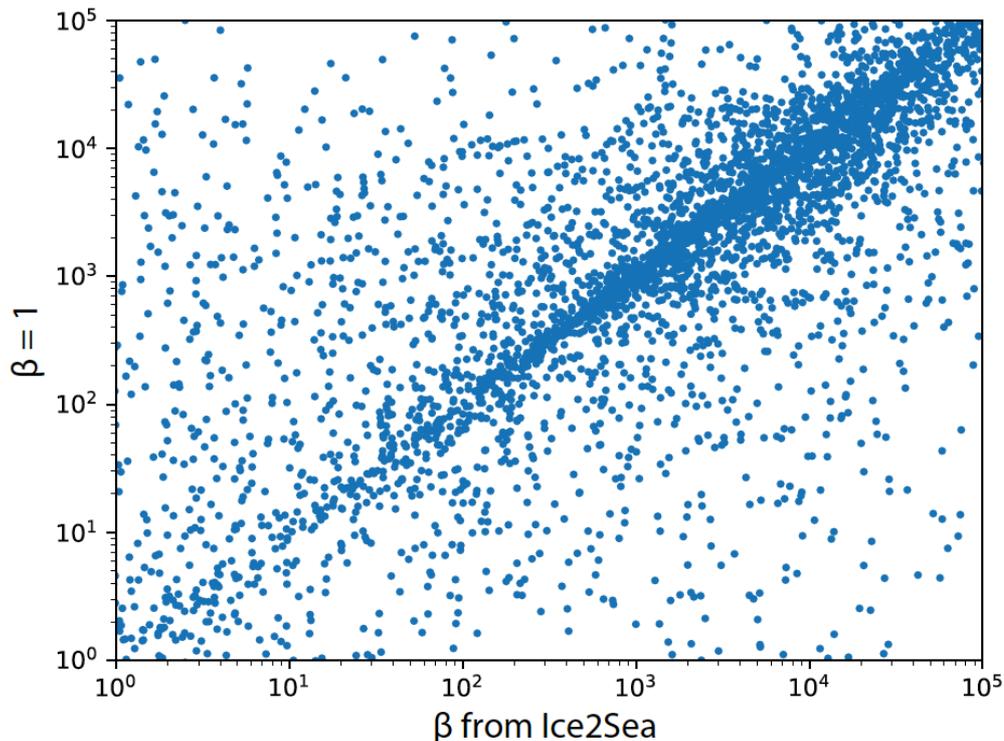


Figure 1: Basal drag coefficient ( $\beta$ ) 1:1 scatter plot between uniform  $\beta = 1$  and  $\beta$  from Ice2Sea (Edwards et al., 2014) in Log10 Pa yr m<sup>-1</sup>.

## **Summary and Conclusions:**

*There is the suggestion here that the method could work better at higher resolution. However, I don't think this will actually be the case. This is because this method can only adjust the value of the sliding coefficient point-by-point; each grid point is adjusted independently of every other one. Once you get down to a grid spacing of a few ice thicknesses or less, this will cease to work very well, because the change in sliding coefficient at one grid point will lead to changes in ice speed at that point AND at neighboring points, via horizontal stress gradients. When this happens, the iteration ceases to make further improvements because it doesn't have a way to avoid the "noise" that local adjustments cause at neighboring points (I have some experience with this problem, based on the similar iteration described in Price et al. (2011; PNAS paper prev. referenced). This is one reason that, at high resolution, it starts to become difficult to use ad hoc methods like this for very precise tuning and one may need to turn to more formal optimization methods.*

Thank you for this comment. We addressed this issue in the Discussion section (Sec. 6):

*"[...higher resolution models can also better account for the dynamics of small-scale outlet glaciers and for their interactions with floating ice that strongly influence the ice-sheet mass balance (e.g., Aschwanden et al., 2016). However, due to the elliptic character of the SSA equation (e.g., Quiquet et al. 2018), the local adjustment of the basal drag coefficient impact the ice velocity of neighbouring points. As a result increased resolution may increase the noise, unless introducing a smoothing function that filters the high frequency noise (Pattyn, 2017)".*

*Some speculation on future directions would be appreciated. For example, could you also include a metric on ice velocity, so that your iteration was scored by the weighted mean of the fit to thickness AND the velocity? This would also be a good place to speculate on iterating on the value of  $E_f$  in areas where the bed is frozen.*

These comments have also been raised by D. Pollard (Referee 1). In Section 6, we now suggest the possibility of including an additional metric on surface ice velocity:

*"Finally, we have shown in this paper that the iterative adjustment of  $\beta$  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".*

Moreover, we have changed the structure of Section 4 and 5 in the revised manuscript, and we now investigate the impact of the enhancement factor for a wide range of values (from 0.5 to 5). Corresponding results are presented in Section 4.2.

## **MINOR COMMENTS**

*1,6: "to infer reliable initial conditions of the ice sheet". This is not really true. Most inverse methods applied to ice sheet models currently only really "work" well if you are only interested in a snap-shot of the ice sheet velocity. Without other considerations, you might get a model snap-shot that does a great job of mimicking observed velocities, but it will likely suffer very badly from the problem you aim to address here (that is, large, unphysical*

transients).

The abstract has been considerably modified to match with the new structure and content of the paper. The point you raise here has been addressed by including the following sentence in the new abstract: *“Most often such approaches allow for a good representation of the mean present-day state of the ice sheet but are accompanied with unphysical trends”*.

1, 11: *“. . . to minimize errors in sea-level projections”*. This is misleading, as it’s not really one of your criteria here. We can’t know that this will minimize errors in SLR projections can we?

This part of the abstract has been completely reformulated (along with the target criteria of the minimization procedure: *“The quality of the method is assessed by computing the root mean square errors in ice thickness ice thickness changes”*).

2,1: Be explicit – the *“unrealistic evolution”* you are talking about is large, unphysical transients in ice thickness.

We changed the text for: *“Reliable simulations of the GrIS require a proper ice sheet model initialisation procedure to avoid unphysical model drift which can be caused by inconsistencies between the initial conditions of the ice-sheet model and the boundary conditions (external forcing fields)”*

2,5: *“GrIS characteristics”* -> GrIS *“state”*?

Changed for *“GrIS current state”*.

2,5: *“the major source of uncertainty”* -> *“a major source of uncertainty”*

Corrected.

2,6: *the vertical temperature profile is not part of the “basal properties”, as this sentence implies (probably just poorly written).*

Following your comment, we have changed the sentence as follows to avoid any confusion: *“... offer only a partial description of the GrIS current state and a major source of uncertainty lies in the poor knowledge of the basal properties (e.g. water content in the sediment or basal dragging) and of the internal thermomechanical conditions (e.g. temperature and deformation profile).”*

2,15-18: *“significant mismatch . . . topography”*. I would use *“state”* here instead of *topography*, since it is much more than just the topography (velocity, flux, etc.). For *“Such spin-up methods”* it seems relevant to mention why only low cost models can do this, because the spin up is order 10,000-100,000 yrs long.

We agree with you. The sentence has been changed in:

*“Even if model parameters can be chosen to reduce the mismatch between modelled and observed present-day ice sheet state (e.g. topography, velocity), this approach may lead to important errors. In addition, due to the long integrations needed (>10 000-100 000*

*year long), such spin-up methods can only be used with low computational cost models, which are often unable to properly capture fast ice flow processes.”*

2,22: *“inconsistencies between . . . “. You could be more explicit here. The problem is that the modeled flux divergence is nowhere close to being balanced by the sum of the surface and basal mass balance terms.*

Thanks for clarifying this point. We have now explained in the revised paper why the fixed topography spin-up method could lead to an artificial drift when the free evolving topography is restored: *“In this case, because the simulated ice flux divergence is generally far from being balanced by the net mass balance (i.e. surface and basal mass balance), an artificial drift arises when free evolving topography is restored (Goelzer et al., 2013).”*

3, 10: *Clarify that hybrid model refers to the momentum balance?*

Since the velocity computation is described later in the text we prefer to remove the reference to the fact that GRISLI combines the SIA and SSA velocities in this sentence.

3,11: *“velocity fields” -> “ice dynamics” ?*

Changed.

3.15: *and equation 1 – clarify that  $U_{\text{bar}}$  is a 2d vector field?*

In the revised manuscript, we use a bold font for the vector fields.

3,20-21: *Clarify that the SIA and SSA solutions are summed heuristically, and point to a reference where you describe what that heuristic is?*

We changed the text for: *“In the model, the velocities are computed as the heuristic sum of the SSA and the SIA components, as in Bueler and Brown (2009) but with no-weighting function (Winkelmann et al., 2011).”*

3,23: *“linear till” -> “linear viscous till”; note that there’s a missing assumption here (in eq. 2) about the thickness of the till layer being uniform everywhere.*

Thanks, we have added this additional information: *“In the model version used in this study, we assume a linear viscous till with a uniform thickness”.*

3,29: *What is value of  $E_f$  used here?*

In the initial version, we used  $E_f=3$ , except in Sec. 5.2. Now we run a whole range of  $E_f$  values (results discussed in Sect. 4.2) to assess the importance of this parameter.

3,32: *The calving criterion is not clear as written. Do you mean that everywhere floating ice is <250 m is thickness it is assumed calved?*

Floating ice at the front with a thickness < 250 m is calved, yes. We rephrased as follows: *“Calving physics is not explicitly computed, but if a grid point at the ice-shelf front fails at maintaining a thickness threshold, it is automatically calved (Peyaud et al., 2007). The ice thickness cut-off threshold is set to 250 m.”*

4,6: *“either the simulated . . . velocities or the ice sheet geometry” . . . what above both? See*

comment above about Perego et al. (2014) paper

We have reformulated: “[...] in order to reduce the mismatch between the simulated surface ice velocities **and/or** the ice-sheet geometry and the observed ones.” More details are provided in the next paragraph of the revised manuscript.

5,2: “Our choice is motivated by . . . sea-level rise.” add, “without that sea-level rise signal being contaminated by unphysical transients from the initial condition.” (or something to this effect)

Added, thank you for the suggestion.

5,9: It’s not clear if you hold the temperatures fixed during the iterative process discussed here.

No, the temperature is allowed to change. It is now clarified in the revised manuscript. However, because the restart conditions used are systematically the same from one iteration to another, we do not think that the change in temperature can make a big difference. We have clarified this: “[...] GRISLI is run forward (free-evolving surface elevation and temperature) starting from the present-day observed ice thickness...”

8, 5: 4.1 “is the spin-up needed” – again, suggest using something else to describe this (“iteration”?) rather than spin-up, to avoid confusion with the common understanding of spin-up.

This terminology has been avoided. This section is now entitled “The importance of the initialisation procedure”

12, 8: “RMSE” -> “thickness RMSE”

This paragraph has been removed in the revised manuscript.

Table 1: I assume the commas are analogous to periods in the numbers listed? Is this standard? Should periods be used instead?

Sorry for this misunderstanding: the commas within numbers are the French standard for a dot. The text editor made an automatic replacement of the numerical dots by commas. We have made sure that the numbers are correctly written in the revised version of the manuscript.

The paper is reasonably well organized (aside from some suggestions noted above) and written. There are a fair number of minor edits and corrections that could be made, related to English language use. I do not point those out here explicitly but instead suggest the authors enlist a native English speaker / writer to provide a careful editing before the submission of a revised version.

We apologize for English mistakes. In the revised manuscript, we made our best to correct them.