

Response to Reviewer #2

We would like to thank the referee for insightful and constructive comments. Several valid points are raised, which we respond to in a point-by-point manner below (blue). We have also made substantial efforts to rework the manuscript to improve structure and clarity.

On behalf of the authors,
Henning Åkesson

General Statement

The paper presents some modelling results, which concern the growth and the retreat of the Hardangerjokulen ice cap, South Norway, from the mid-Holocene to the presentday. To do so, the authors have used the Ice Sheet System Model to simulate the dynamical evolution of the ice cap. The model accounts for internal ice dynamics (SIA), linear basal sliding and surface mass balance. Dynamical parameters (sliding and shearing of ice) are first calibrated at present-day, and secondly in transient runs. The transient simulations indicate an asymmetry between southwest and northeast section during both advancing and retreating stages. This study is in line with number of previous papers (referenced in the manuscript), which present modelling results for a specific glacial area, and compare the results to field evidence. I have no doubt that the methodology can be successfully applied to gain insights about the chronology of the advances and retreat of this particular ice cap and complement the geomorphological information already available. Unfortunately, I find the present manuscript hard to follow so that the main achievement of the paper is somehow hidden. One reason to explain that is: the paper does not follow any clear continuous line, which should bring the reader from the original investigated problem to some final conclusions.

We agree with the reviewer that the original manuscript could have been clearer and more concise. We have now reworked the Abstract, and completely rewritten the Introduction, clearly stating the scientific questions we address and the reasons for doing so. We have shortened some sections in the Methods, as suggested by the reviewer (specifically on SIA, see below). In the Discussion, subsections are now more explicitly linked together and paragraphs not following the main aims and scope of the paper have been deleted. We have also added a dedicated subsection on transferability/implications in the Discussion. In the Conclusion, we highlight our main findings more directly linked to the scientific aims outlined in the Introduction.

The paper spends a lot of sentences to discuss things of little importance/originality or already debated many times, and this strongly harms the overall reading. Unfortunately, the most interesting results arrive at the end of the paper, so that it is likely that most of readers won't reach this point (being discouraged by too many unnecessary discussions).

We thank the reviewer for highlighting this. We hope that our reorganization of the manuscript will guide readers to these also in our view important results on hysteresis and high mass balance sensitivity.

In addition, the paper shows number of inaccurate/inappropriate/awkward sentences, which harm the overall argumentation (see some examples below). I believe that the paper must be rewritten before to be reconsidered for publication. This including a substantial shortening (removing unnecessary/distracting parts, and better emphasizing the main outcomes). I hope that my next suggestions will help the authors to achieve this task. We agree with the reviewer that these sentences arise from unclear writing. We reply specifically to the examples given by the reviewer below, and have kept clarity in mind when revising the rest of the manuscript.

Major concerns:

- Section 5.2 is a typical example of section, which really slow down the reading because of a lack of originality and importance for the present study. I would recommend to remove it, or to keep the most relevant information (maybe to be merged with Section 5.1). I believe that Section 5.3 could be more efficiently and more concisely rewritten. More generally, the whole Section 5 should be “optimized”

We are thankful to the reviewer for these concrete suggestions. Section 5.2 is now greatly shortened with relevance to the present study in mind, and merged with the Discussion of the dynamical parameters in Section 5.1. Further, Sections 5.3 and 5.8 in the original manuscript both discussed mass balance. They are now rewritten and combined into one section. We also combine Section 5.5 and 5.7, because they both discuss the impact of the linear build-up phase.

- The manuscript contains hazardous/inaccurate statements, and sometimes awkward/dangerous assessments. However, I believe this is more unfortunate formulations rather than misunderstandings by the authors. For instance:
 - p. 7: "Where bed and surface topography is complex, lateral drag and longitudinal stress gradients may become important. Still, the SIA has proven accurate in representing glacier length and volume fluctuations on decadal and longer time scales." is more confusing (and even contradictory) than useful.

We agree with the reviewer, and now shortly justify SIA in light of Hardangerjøkulen's characteristics, our time scale of interest and the references given.

- – 1.23 p. 8: "Even though our surface digital elevation model (DEM) has higher resolution than this (100 m), we choose the highest mesh resolution to be 200 m, since this is more in line with the assumption of the SIA" In what the mesh size and the physical model (here SIA) are connected?

We thank the reviewer for highlighting this. The stress balance of SIA is completely local. Using a very high resolution for SIA increases the risk of unphysical stress gradients/velocities due to local variations in bed topography. We avoid this by smoothing the DEM. As we already point out, the rationale behind the lower mesh resolution is also to save

computational resources. As mentioned in response to reviewer 1, we now also carry out an analysis on mesh convergence. Preliminary results show that volume varies by less than 5%; details will be given in the revised manuscript.

- - "SIA is viable to use if interests are climatic rather than ice dynamics." should be more accurate.
See above comment on SIA.
- - "By investigating a small valley glacier in the Canadian Rocky Mountains and neglecting basal sliding, Adhikari and Marshall (2013) suggested that SIA performs well in less 'dynamic' settings, while the results compared to HO/FS diverge for more 'dynamic' situations." is a striking example: of course, if there is less dynamics, then the errors related to the dynamics gets less visible!
This is a good point and we now refrain from make such general statements. As requested by reviewer 1, the Discussion section on ice dynamics is shortened, since we do not have much available data to constrain our results to, and we do not run comparative tests for SIA/HO/FS due to reasons given below.
- - l.1 p.17: "It is challenging to assess how much sliding there could be before SIA validity deteriorates, but it likely depends on the climatic and glaciological setting. " is inaccurate
We agree that this statement is rather confusing and is now removed. We now deemphasize our discussion on sliding, since we do not have the available data for validation of basal motion.
- - l. 23 p. 17 "we are aware of the limitation ... Therefore, the actual rate of advance may differ...". The actual rate of advance may differ because you are aware of the limitation of the SIA?
This is indeed unclear wording. We now change this to "...the actual rate of advance may differ ..., because SIA has limitations in the steep terrain..."
- The paper is poorly structured. Some information come repetitively in the paper (as the justification of using the SIA). The discussion (Section 5) looks more like a list of items without connections between the subsections. The last paragraphs of each subsection of Section 5 state some recommendations for future studies. I think this is not the right place, such statements should rather appear in a dedicated "perspective" closing section.
Substantial efforts have gone into restructuring the manuscript; see comments above. Regarding "future work", we integrate these more appropriately into the text. However, we do not think a dedicated "Perspective" section is necessary.
- The dynamical model is essentially based on the most simple existing model, namely the Shallow Ice Approximation. Even if this is a surprising choice (regarding to the capabilities of ISSM, and other higher order models freely

available nowadays), I find unnecessary to describe in details the well known SIA so that Section 3.1 can be strongly shortened. In addition, there are several clumsy attempts to justify the use of the SIA throughout the whole paper. The uncertainty due to mechanical simplifications cannot be quantified since no comparative tests are done with higher order solutions. As a consequence, I don't see the point of discussing so much in details this assumption, while a simple referencing to previous comparative studies (e.g. Lemeur and al) would be enough.

We agree with the reviewer that the details of the SIA theory can be omitted. However, we do believe that there is a value in justifying our choice of SIA, especially since both reviewers suggest that a HO/FS model should be used if available. As mentioned in our response to reviewer 1, the SIA is considerably cheaper and allows for ensemble and longer time scale studies. We believe that HO/FS is unnecessary for the long time scales studied here, on this ice cap lacking areas of fast flow. This is in line with what previous studies have shown (referenced in the manuscript). Even if we had attempted a SIA/HO comparison within ISSM, it would not be straightforward. The problem is that the parameterization of the basal friction is different for SIA and HO in ISSM; SIA parameterizes basal velocities and HO parameterizes basal stress. We therefore do not think a SIA/HO twin simulation would be informative.

- All what concerns the calibration to ice flow and sliding parameters should be strongly shortened since this problem has been presented many times so that only the result matters. Also, I am not really convinced by the "best-fit" pair of parameters, which is chosen among all those which minimize equally the RMSE. If I understand correctly, the 'best-fit' parameters are chosen according to the temperature of the equivalent rate (Arrhenius) factor A , which should correspond to temperate ice. This is a very weak argument, which cannot be used to constrain A . Most of ice flow models are tuned through enhancement factors, this indicates that one cannot rely directly on the exponential formula for $A(T)$ given in (Cuffey and Paterson, 2010, p. 73). Say differently, the formula works in a relative way (after tuning), but not in an absolute way.

The discussion on the dynamical parameter calibration can certainly be shortened. The inability to find a unique parameter combination is important but indeed not new. However, many studies do not keep their parameter ensemble during transient runs like we do, and risk losing some important information on parameter sensitivity, so we still believe that our approach requires some attention.

We completely agree that the Cuffey and Paterson $A(T)$ formula can only be used after tuning, not in an absolute way. We also agree with the underlying statement that assuming an A according to the table value corresponding to temperate ice is a weak argument. To be clear however, we do not assume temperate ice. Instead, we tune A without any *a priori* assumption about ice temperature and use the RMSE for ice thickness as a constraint. We arbitrarily pick an A (which corresponds to $T=-1$ C) in the middle of the region of similar RMSE's (dark blue region in Fig. 3). Differences in RMSE within this region are within 1 m, further underlining the motivation behind

keeping our ensemble after the calibration. A comparison with a map of ice velocities (which is not available for this ice cap) would more strongly constrain A, and we try to convey this in the paper by stating that several parameter combinations give similar RMSEs for ice thickness. A key result of the paper is also that the impact of A on ice volume is large during our transient simulation over several centuries (Fig. 6), while relatively small at calibration (Fig. 3). This disparity suggests that small differences in model rheology at calibration propagate with time. This time-dependency has implications for other model studies of long-term dynamics of glaciers and ice caps.

- I don't see in what the spatial asymmetry is intriguing or unexpected since you mention several times that precipitation are asymmetric (west-east precipitation gradient).
We should have been clearer on this. In *reality*, there is likely a W-E precipitation gradient, due to the prevailing SW-W wind direction. In the *model*, there is no such gradient. Instead, SMB is prescribed as a function only of elevation. In our case this neglected horizontal SMB gradient is an advantage, since we know that the spatial differences during growth and retreat we find by definition cannot be due to an asymmetric SMB forcing. This is why we consider our found asynchronous growth and retreat an interesting and perhaps unexpected finding.
- Figure 11 and 12 are to me the most interesting results of the paper, so that they deserve to be better highlighted. However, the authors should clarify in what these results are different from the Figures 6.7 and 6.8 of (Giesen, 2009, PhD dissertation), which is already based on the SIA.
We thank the reviewer for these encouraging words. We now make these results more visible by moving them to the Results, and link them to the aims of the rewritten Introduction. Giesen (2009) indeed used SIA, although with a different sliding and ice deformation formulation, as well as different numerical methods (finite difference and not finite element model). Figs. 6.7 and 6.8 from Giesen (2009) have not been published in a peer-reviewed journal. Since the results in our present study support Giesen (2009), but are not identical and derived from a different model, we think these findings are worth highlighting. What is new in the present study is also the inclusion of our parameter ensemble, providing an estimate on the effect of parameter uncertainty on the relationship between SMB anomalies and steady-state ice volumes.

Specific comments:

- Abstract and later: I don't understand why you say that your model is 2D. The SIA provides a 3D velocity field. To me, your model is 3D. In a 3D model, velocities are calculated explicitly for the x-, y- and z- directions, which is not the case here. We use vertically-averaged

horizontal velocities, thus we do not resolve vertical variations in horizontal velocities. Therefore we view the model as being 2D.

- Abstract and later: You often emphasise the capabilities of ISSM to perform mesh refinements, but you never say what does it brings to the study. If this is not relevant, it should be in the abstract.
Good point. It adds better accuracy (200 m) around the LIA margins due to the mesh refinement there, but when the glacier is smaller/larger the accuracy is reduced (400-500 m). This is now pointed out in Methods.
- Eq. (1): I don't think it is necessary to repeat the formula used to reconstruct the ice thickness (or equivalently the bed). Referencing would be enough.
Referenced only.
- l. 12 p.4: I don't think that the acronym NVE was defined so far.
Written out.
- l. 4 p.5: "At c. 4000 BP" and many other places in the text: What "c." stands for?
It stands for circa/about/around. We now write this out the first time.
- Eq. (4) dot is missing at the end. Punctuation (coma and dots) is sometimes missing in your equations.
Fixed.
- l.25 p.7: "SIA" must be "The SIA".
Changed.
- l.7 p.8 "ISSM has capacities ..." this is useless information since you don't use this capability.
We think mentioning this motivates/justifies our approach to basal sliding. We have rewritten this to: 'While ISSM has the capacity to ..., this method could not applied to Hardangerjøkulen because of the limited velocity data coverage'
- Eq. (7) u should be \bar{u} .
Changed.
- l. 13 p. 8: is the unit of M not $m a^{-1}$? mass balance rate should rather be annual mass balance?
Indeed, changed.
- l. 31 p. 12: "both depend on driving stress", ok but I think one can explain much more easily why several pairs of A and β gives similar RMSE: one can reduce sliding and increase shear of ice while keeping same surface velocities.
Rephrased in a more concise way as suggested.

- l. 10 p. 13. As I said before, "We therefore exclude". I don't think you can use this argument to eliminate pairs of parameters. But instead, it sounds more reasonable to keep going with several pairs (A, β) which would be a set of "bestfit" parameters.
Fair point. We now show individual runs with focus on rate factors in Fig. 6, using the rate factors from Cuffey and Paterson's table. We agree that we cannot use the argument mentioned by the reviewer and *a priori* exclude $A(T=-5)$ rate factors. However, because we see that the smaller $A(T=-3)$ rate factors deviate significantly from the observed ice volumes, and $A(T=5)$ deviate even more, we choose only to show a smaller range of simulations in Fig. 6.
- l. 20 p.13 : You haven't defined T_{ice} .
We now define T_{ice} in Methods (l. 15 p.7 in original manuscript)
- l. 20 - l.27 p.13 : This paragraph is especially laborious to read, and can be certainly shortened.
We have shortened and clarified this paragraph.
- l. 29 p. 13: It makes no sense to refer to numerical objects (mesh node) in this Section.
Changed.
- l. 33 p.13: "By imposing ... ice masses". This sentence doesn't bring anything.
We agree, and now omit this.
- l. 34 p. 13: "cold- to warm-based" I guess you refer to basal condition? If yes, you should formulate that more clearly.
Indeed, now specified.
- l. 17-18 p. 17: ", since the ice present is divided ..." I don't understand the meaning.
Changed to "split up into several separate glaciers"
- You should maybe rename Section 5.3 or 5.8, since it seems (from the names) that they address the same issue.
These sections are now shortened and merged.
- l. 1-4 p.21: why don't you show the results?
This is a good point. We have added simulations without the SMB-elevation feedback in Fig. 12. One of our main conclusions is that Hardangerjøkulen is so sensitive because of this feedback, and we now also illustrate this in the new Fig. 12 as suggested by the reviewer. We have also moved it to the Results to increase visibility.