

## ***Interactive comment on “Simulating asymmetric growth and retreat of Hardangerjøkulen ice cap in southern Norway since the mid-Holocene” by H. Åkesson et al.***

**Anonymous Referee #2**

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### **General Statement:**

The paper presents some modelling results, which concern the growth and the retreat of the Hardangerjøkulen ice cap, South Norway, from the mid-Holocene to the present-day. 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),

C1

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. 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). 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.

### **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”.
- 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:

C2

- 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.
  - l.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?
  - "SIA is viable to use if interests are climatic rather than ice dynamics." should be more accurate.
  - "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!
  - 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
  - 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?
- 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 stud-

C3

ies. I think this is not the right place, such statements should rather appear in a dedicated "perspective" closing section.

- 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.
- 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 a absolute way.
- 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).
- Figure 11 and 12 are to me the most interesting results of the paper, so that

C4

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.

### 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.
- 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.
- 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.
- l. 12 p.4: I don't think that the acronym NVE was defined so far.
- l. 4 p.5: "At c. 4000 BP" and many other places in the text: What "c." stands for?
- Eq. (4) dot is missing at the end. Punctuation (coma and dots) is sometimes missing in your equations.
- l.25 p.7: "SIA" must be "The SIA".
- l.7 p.8 "ISSM has capacities ..." this is useless information since you don't use this capability.
- Eq. (7)  $u$  should be  $\bar{u}$ .
- l. 13 p. 8: is the unit of  $M$  not  $\text{m a}^{-1}$ ? mass balance rate should rather be annual mass balance?

C5

- 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  $\beta$  gives similar RMSE: one can reduce sliding and increase shear of ice while keeping same surface velocities.
- 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  $(A, \beta)$  which would be a set of "best-fit" parameters.
- l. 20 p.13 : You haven't defined  $T_{ice}$ .
- l. 20 - l.27 p.13 : This paragraph is especially laborious to read, and can be certainly shortened.
- l. 29 p. 13: It makes no sense to refer to numerical objects (mesh node) in this Section.
- l. 33 p.13: "By imposing ... ice masses". This sentence doesn't bring anything.
- l. 34 p. 13: "cold- to warm-based" I guess you refer to basal condition? If yes, you should formulate that more clearly.
- l. 17-18 p. 17: ", since the ice present is divided ..." I don't understand the meaning.
- You should maybe rename Section 5.3 or 5.8, since it seems (from the names) that they address the same issue.
- l. 1-4 p.21: why don't you show the results?