Response to Reviewer #1

We would like to thank the referee for his/her thorough review with insightful and constructive comments. Several valid points are raised, which we respond to in a point-by-point manner below, our responses in blue. We are especially thankful for the reviewer's many detailed suggestions, which have improved the manuscript greatly. We have in addition made substantial efforts to improve structure and clarity.

On behalf of the authors,
Henning Åkesson

General comments

In this manuscript Åkesson and co-authors simulate the build-up of the Hardangerjøkulen ice cap (Norway) from the Mid-Holocene (4000 years ago, when there was no ice cap) to the present-day by coupling a SIA model to a simple elevation dependent mass balance model. At first a mass balance forcing based on climate reconstructions is used (Holocene), after which a switch is made a mass balance forcing based on geomorphological evidence (LIA to 1968) and finally direct surface mass balance measurements are used (1968 to present-day). This setup, with a focus on the long-term evolution of the ice cap, is interesting to get an insight in the dynamics of this ice cap and the important role of the surface mass balance (SMB) and its feedback with elevation. However, the authors do not really dig into these concepts and most of the descriptions are too site specific. Despite some attempts to make a few generalizations, the research and concepts presented here are rather trivial and no new concepts are introduced. A few interesting elements /possible points of research focus are mentioned, but then usually a reference is made to 'potential future work' / 'behind the scope of this research' and these not further elaborated.

We think the present setup and focus (long term reconstruction/evolution of an ice cap using transient numerical modelling) is not commonly found in the literature and that our findings have implications for reconstructions and predictions of ice caps in other regions than Hardangerjøkulen. We agree however with the reviewer in the sense that the transferability and novelty was not clear in the original manuscript.

To improve this, we have now strengthened our focus on the SMB-elevation feedback. Originally in the Discussion, these findings have now been moved to the Results, to increase visibility. Simulations excluding the feedback have been added to Fig. 12. We do believe that the strong role of this feedback on the time scales we consider is relevant not only for Hardangerjøkulen but for studies of other maritime ice caps, e.g. in Norway, Alaska, Iceland and Patagonia, because of the similar hypsometries and mass balance regimes of these ice caps. In addition, we think that the dependency of initial conditions for ice caps (hysteresis), illustrated in Fig. 11, has not received much attention in the literature and is relevant for modelling and reconstructing paleo-ice caps and predicting future ice cap evolution. The out-of-phase variations of area and volume (Fig. 10) we find also have implications for such studies. We now further underline this study's relevance and transferability in the Introduction and have added a separate section on this in the Discussion.

Regarding "digging into" the ice dynamics, see responses below.

Quite a lot of comparisons with other studies are made (often for totally different settings, which is not always appropriate) to typically conclude that similar findings are found. Moreover a lot of statements and passages are simply not supported by the results presented, which is for instance the case for the parts on ice dynamics and the comparisons between the shallow ice approximation (SIA) and more complex solutions (Full-Stokes (FS) / Higher-Order (HO)) (see also my more specific contents).

We agree with the reviewer that the discussion of ice dynamics was not entirely appropriate for our study. Therefore, we have now rewritten these parts and refrain from making inferences about HO/FS since, as the reviewer rightly point out below, we have not done such comparative studies.

Regarding SIA/HO/FS, 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.

On this topic, reviewer 1 writes in the Specific comments on the Introduction: "At several points in the paper the difference between SIA and FS is minimized in your interpretation: but do not rely on your results to do this, be careful. Differences can be quite large, especially in your fast flowing steep outlet glaciers."

In contrast, in the comments on Section 5.5, the reviewer suggests that "… the effect of SIA/HO-FS is very limited compared to other errors and uncertainties. Over Holocene timescale the SMB (where uncertainties are large) will have much larger effect than dynamics on the evolution/growth."

We are not sure where the reviewer stands here, but we agree with the second comment that SIA/HO-FS differences are likely small on the time scales we are interested in. In general, 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 have been straightforward. The problem is that the parameterization of the basal friction for SIA and HO is different in ISSM; SIA parameterizes basal velocities and HO parameterizes basal stress. We therefore do not think a SIA/HO twin simulation would be informative.

I also have some strong reservations concerning some interpretations, mainly those relying on the (too) simple surface mass balance (SMB) parameterization.

See our response on the physical basis of our SMB parameterization in Specific comments, Section 5.3, below.

Furthermore the structure of the manuscript is often difficult to follow with sections in which comparisons with other studies are made, but also comparisons between earlier studies on Hardangerjøkulen and the literature. A lot of sections could be reduced, many repetitions could be avoided and the writing style can be improved.

We agree with the reviewer that the original manuscript could have been clearer and more concise. Substantial efforts have therefore gone into restructuring the manuscript. We have now reworked the Abstract, and completely rewritten the Introduction, clearly stating the scientific questions we address and the reasons for doing so. Some subsections in the Methods have been shortened. 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. In the Conclusion, we highlight our main findings more directly linked to the scientific aims outlined in the Introduction.

Under this form the paper lacks scientific novelty and many of the descriptions are very general and imprecise. Some of the methodology may have to be rethought, which is especially the case for the surface mass balance, which almost fully determines the build-up and is highly uncertain.
We agree that the SMB is uncertain and is crucial for the Holocene evolution. However, our aim is not to reconstruct SMB for the Holocene, but to assess the long-term dynamic response to a simple climate forcing. We agree with the reviewer that the SMB forcing is simple; we have made it so deliberately and view this as a strength rather than challenge. This because we would like to isolate the effect of bed topography/geometry/dynamics, given a simple, imposed (linear) climate forcing.
See also our response on the physical basis of our SMB parameterization in Specific comments, Section 5.3, below.

More detailed analysis and other experiments, which allow for some generalizations (i.e. findings which are less site specific), are needed for this research to be more relevant to the scientific community.
As mentioned above, we now add a separate section on transferability/applicability of our results the Discussion. We have analysed volume and area evolution further, and illustrate this in a new figure, relevant for volume-area scaling (in addition to the existing Fig. 10).

Specific comments

Abstract:

- First paragraph (l. 1-4, p.1): do you need this in abstract? Quite long abstract, so would consider removing this.
  We agree that the abstract was long and lacked focus. We have shortened the "motivation" part of the abstract to one sentence.

- l. 11: "given a linear climate forcing": the forcing was in reality not linear. You impose this. Could change this to: "Under a linear…"
  Indeed. Changed.

- l. 13: "intriguing": this is a scientific text, something cannot be "intriguing": there is a reason behind it. Rather opt for "remarkable".
  True, though we prefer to change to "distinct".

- l.16-17: in- and out-of-phase: not clear here. One has to read the manuscript to understand. Would reformulate this.
  Reformulated to "we find that for several outlet glaciers and indeed for

the entire ice cap, volume and area vary out-of-phase for multiple centuries during the late Holocene, and in-phase approaching the LIA."

- l. 18: canonical: what does this mean?
  With "canonical" we here mean an assumption that is commonly used/ recognized/established/prevailing.

- l. 19: "we provide new insights…" → would not formulate it this way. Let the reader decide whether he thinks it is new. To me most findings are site specific and there are little to no new insights on the long-term dynamics response of ice caps (e.g.1: the role of SMB-elevation feedback is something that has been analyzed far more in-depth and from a conceptual point of view (see my comments further); e.g.2: the fact that growth is not symmetrical and linear despite the linear forcing is also rather trivial)
  We thank the reviewer for this suggestion. This has been reformulated. Regarding the SMB-elevation feedback, we now assess its effect more extensively and have moved its place from the Discussion to Results. We think that the asymmetric/asynchronous, non-linear response to linear forcing deserves attention. It has implications for paleostudies aiming to reconstruct ice caps as well as for future predictions, and has in our view not received enough appreciation in the literature. We now highlight this further in our new section on transferability/applicability.

- l. 21: close to observations: of course, because this is partly imposed.
  It is true that we estimate the SMB forcing between 1600-1962 based on the length variations of two outlet glaciers. However, the forcing is not aggressively tuned (as pointed out in the manuscript). The close fit between modeled and observed ice cap margins in the second half of the 20th century is not a given, and shows that SMB plays a key role. We now reformulate ourselves stressing that not only calibrated lengths correspond well, but also ice cap extent in general.

Introduction:

- l.3-4: make reference to the new study by Huss and Hock (2015) here, which is the first to model all glaciers and ice caps explicitly.
  Thanks for making us aware of this study. We now cite it.

- l.5-6: reference(s)?
  Changed.

- l.7: do not understand. GICs response essential because ice sheets are slow? (contribution ice sheets also important in next century)
  This is indeed confusing. We now specify that both GICs and ice sheet contributions are important for 21st century sea level rise.

- l.8: 170000 GICs: reference for number?
  This number is now actually more than 211 000, according to the latest version of the Randolph Glacier Inventory by GLIMS (version 5.0,

www.glims.org/RGI). We now reference this.

- l.12-17: "For comparison… into the physics operating on these time scales": strange passage. How is this related to the rest of intro?
  We agree that this is not clear. Our reworked Introduction more clearly links this to our focus on long term transient modelling and its relevance/implications for glacier reconstructions.

- l.18: omit "so-called": they are Full-Stokes models.
  Done.

- l.18: also add a reference to Jouvet et al. (2009) here. Far more relevant than two others given the fact that you consider a small ice mass. Study of Jouvet et al. (2009) was first to really apply FS on glacier for time dependent evolution.
  We thank the reviewer for this suggestion and now cite Jouvet et al. (2009) here.

- l.20: "simpler models are generally preferred": why so? Do not agree. Must make sure that you have a certain detail in data to justify the use of complex (HO/FS) model, but if this is the case and if you have the resources to do so: more complex model is more interesting. At several points in the paper the difference between SIA and FS is minimized in your interpretation: but do not rely on your results to do this, be careful. Differences can be quite large, especially in your fast flowing steep outlet glaciers.
  One advantage of simpler models/SIA is given in the sentence after (l.21-22): simpler models allow for more extensive ensembles and longer runs, because they are cheaper. We acknowledge however that there are different schools of thought here. Therefore we now rephrase this, pointing out the advantages of simpler models without concluding whether they in general are "preferred", leaving it up to the reader to decide what he/she thinks.
  The reviewer also points on something else here: a certain detail in data is needed to justify the use of a HO/FS model. We do not believe that this data is available to us, nor are we focusing on the short-term variations where HO/FS possibly have an effect. We acknowledge however that the rationale behind simpler models was not clear and have now rewritten this passage.

- In this study: would have been interesting to make comparison with a more complex model, especially given the fact that you work with a model (ISSM) where this can be done! Run of 4000 years with HO model with resolution 200-500 m is definitely feasible, especially given the very small extent of the ice cap (compared to ice sheets).
  As the reviewer rightfully point out, a HO model for 4000 years is indeed feasible, even an ensemble study could be done. But we are not convinced we should justify HO/FS simulations by availability rather than applicability. As described in detail before, a SIA/HO/FS comparison is

not straightforward and we do not think it would be informative. Again, we now articulate the rationale behind our simple model more clearly.

- • l.22: simple models are needed to do extensive 'ensemble experiments'. Has been done in a far more elaborate and precise way by others, in a computationally heavier setup: e.g. have a close look at the recent study by Ziemen et al. (2016) (much larger domain, over the entire Alaskan Ice Field, and with more complex model, especially when it comes to the SMB), which analyses in a very nice and in depth way the effect of many parameters (not only related to ice flow and sliding)
  We thank the reviewer for directing us to this relevant study, which contains several interesting findings and improves our knowledge of ice fields, their outlet glaciers and how to model them. Though some longer simulations are done, the main focus of Ziemen et al. (2016) is predicting the next 100 years. This focus is quite different from ours. However, we now analyze our ensemble in more depth by detailing individual runs in Fig. 6 and further discuss the effect of the dynamical parameters. Our choice of a simple SMB profile is discussed in the Specific comments, Section 5.3, below.
  We now cite Ziemen et al. (2016) in the Introduction and Discussion.

- l.24-27: you mention centuries to millennia when it comes to response time. And one of the reasons for you to study the last 4000 years is related to the long response time of the ice cap. The long-term dynamics are important, but also the shorter time scales matter. If you apply a strong warming during several decades, the long-term evolution will quickly be altered and especially the outlet glaciers (which are quite central in your story) will react to this. Would also mention the decadal time scale here (which you mention later, in your ice flow model description, p.7, l.1-3) and some related studies (e.g. Leysinger Vieli and Gudmundsson, 2004; Raper and Braithwaite, 2009; Zekollari and Huybrechts, 2015)
  We agree with the reviewer that decadal time scales are indeed important. The references Leysinger-Vieli and Gudmundsson (2004) and Zekollari and Huybrechts (2015) are cited elsewhere in the manuscript but are now referenced together with Raper and Braithwaite (2009) also in the Introduction and ice flow model description.

- l.26: which studies? Should make a reference here.
  The references were partly given in the previous sentence. We now make it clear what "Studies" we mean.

- l.29: "carry out an extensive evaluation". Do not agree. See also my comment earlier and reference to the work of Ziemen et al. (2016).
  See our response above regarding Ziemen et al. 2016.

- l. 26-29: in the end this is a passage that summarizes why "your work is better than others". Be careful with this, especially given the fact that the setup is not so unique (other long-term studies exist) and the analyses are not so in-depth (again: Ziemen et al. (2016): here the calibration is also not 'lost' (l.30))

As said before, we think that there are several important results in this study, and that long-term transient modeling/reconstruction studies of ice caps in general are rare. We do however agree with the reviewer that this can be made clearer in the manuscript. We have therefore completely rewritten the Introduction and added a new section in the Discussion, focusing on transferability/implications.

- l. 32-33: "by considering the underlying bed topography": of course: otherwise you do not have the ice cap geometry and cannot do any modelling + the uncertainty is very large and many areas without measurements. "interacting ice dynamics": do almost not have any information about this (especially when it comes to basal sliding, a process which is discussed elaborately in your manuscript)

  We are thankful that the reviewer points out this imprecise wording. Having a bedrock DEM is indeed a prerequisite for the type of modelling we do. In contrast, a bedrock DEM is not always available for glacier reconstructions, ice volume estimates (e.g. volume-area scaling for sea level rise), or other applications. By "…glacier reconstructions can be improved by considering the underlying bed topography…", we tried to convey that studies aiming to reconstruct an ice cap or glacier through time would benefit from assessing/acknowledging/quantifying the impact of the bed topography on ice flow and mass balance, and therefore on the reconstruction itself. This follows from our finding that a spatially symmetric SMB and linear climate forcing result in a spatially asymmetric, non-linear response, whose explanation include the impacts of bed topography.

  We acknowledge that the accuracy of the bed topography varies for Hardangerjøkulen, as for other ice masses, and already point this out in the Discussion (p.12, l.13; p.17, l.2; p.20, l.6).

  "…interacting ice dynamics" is indeed not appropriate, we have now changed this to "surface mass balance", since the SMB-elevation feedback together with bed topography is vital to the long-term evolution reconstructions mainly are interested in.

- p3, l.1: "model stategy": strange formulation. Rather use "metholodogy"

  Changed to "methodology".

Section 2:

- Strange sequence: present-day → LIA → Holocene: would re-arrange this.

  Good suggestion, the order is now chronological.

Section 2.1.1:

- l.9: Present-day: when is this? 2012? Quickly changes under presentday conditions. Otherwise use "about" to qualify this.

  Indeed not clear, this specific survey was in 2010, which is now stated.

- Give a lot of info about Rembesdalskaka: what about the other outlet glaciers?
  This focus reflects that SMB measurements are done on Rembesdalskåka, and nowhere else. We agree however that at least Midtdalsbreen should have been given some attention, since this is the other outlet glacier we focus on, and we have now added additional information.

Section 2.1.2:

- Which DEM is used (needed to reconstruct the bedrock elevation)? Is this the one you mention later in section 3.2.2
  The 1995 DEM mentioned in Section 3.2.2. is indeed what we use. This DEM is a result of several preceding surveys, mentioned in Section 2.1.2. We now specify this also here.

- l.27-29: need interpolation for areas with small surface slope → is this only at ice divide and ice ridges. Or also in other locations? Be more specific.
  The manual extrapolation (not interpolation) was required at ice ridges and divides. This is also detailed in Giesen and Oerlemans (2010), p.93. We now reformulate this more clearly.

- l.29-30: continuous decrease in ice thickness: towards the edge? Not fully clear, could elaborate on this.
  We now clarify that near ice margins (e.g. last km), instead of using Eq. (1), manual extrapolation of ice thickness measurements was needed to obtain a meaningful/smooth ice surface.

Section 2.1.3:

- Beginning (l. 2-6): jump from one time period to another. Consider reorganizing this.
  We aimed to describe the data chronologically (l. 1-9), and then summarize how we use it in our study (l. 11-12), but we agree that this was not easy to follow. We have rewritten this passage to obtain further clarity.

- l.7: “both outlet glaciers”. There’s more than two, confusing → “The two outlet glaciers considered..”
  Changed.

Section 2.2

- Again a strange sequence: present-day → past (Holocene + LIA) → present-day
  We have now switched to a chronological order, for consistency with the glacier data.

Section 2.2.1:

- Second paragraph (l.26-30): discuss precipitation different locations and all of a sudden in last sentence a mean annual temperature is mentioned.

Not related to this. Would omit this or start with new sentence in which the temperature is mentioned (also for other sites?).
Good suggestion, we now keep temperature in a separate sentence. Only precipitation is measured at Liset, which is now pointed out. Finse is the closest meteorological station and temperature does not vary as much spatially as precipitation does. We therefore think it is sufficient to mention the temperature at Finse.

Section 2.2.2:

- l.4: "is documented" → when formulated like this seems that there was someone 4000 years ago who saw this and wrote this down. Not the case. Would for instance use "is reconstructed".
  Changed to "is reconstructed".

- l.7: "unfavourable conditions": what is favourable/unfavourable for an ice cap? Unfavourable conditions for growth? Consider reformulating this, potentially as a function of SMB.
  Reformulated to "implying a more negative surface mass balance and thus unfavourable conditions for glacier growth"

Section 2.2.3:

- l.19-20: SMB: 45 mass balance years. How do you define the SMB years? Not sure, but period 1963-2007: would in first instance interpret this as 44 years.
  SMB years are defined from 1 Oct the previous year until 30 Sep in the year mentioned. 1963-2007 runs from Oct 1962 to Sep 2007, totalling 45 years.

- SMB: decrease at highest altitudes. Is this decrease really so strong? Any references to other glaciers where a similar decrease is measured? Explanation: by snow redistribution (l. 21-23): is this the only mechanism? No correlation to temperature (cf. Clausius-Clapeyron) or any other explanation?
  The change in SMB gradient at the ice cap plateau and the decrease at the highest elevations is a persistent feature of the winter mass balance. It is strongest in the years with large accumulation (see Fig. 5.3 in Giesen (2009), PhD thesis for specific winter balance profiles). Of the other Norwegian glaciers with winter mass balance measurements, only Engabreen in northern Norway also has a decreasing mass balance at the highest elevations, although less pronounced. What may be of influence, is that Rembesdalskåka is flowing due west, while other Norwegian ice cap outlet glaciers with observations have no or a smaller westward component. Globally, winter mass balance profiles are only available for a small number of glaciers and we are not aware of any other ice cap outlet glaciers that show a similar decrease. The suggestion by the reviewer that Clausius-Clapeyron effects may play a role cannot be ruled out, particularly because the glacier faces the dominant wind direction. However, we doubt whether Hardangerjøkulen stands out enough from

the surrounding topography to induce significant orographic lifting. We now mention specifically that the origin of the mass balance decrease is uncertain, and that long-term snow depth measurements on the other outlet glaciers are needed to identify the mechanism causing it.

- Last sentence: approximated by second-order polynomial vs. in caption of the figure that illustrates this (figure 2): third-order polynomial? Which one is it?
  We thank the reviewer for spotting this. Corrected to “third-order”.

Section 2.3.1:

- p.6, l. 2: first you say that the ice cap can considered as temperate (i.e. all ice at pressure melting point) and in next sentence you mention an outlet glacier to be cold-based (i.e. ice cap is polythermal and not temperate). Not consistent. Also not very clear what has been measured and what not.
  We have reformulated this to be more precise. Midtdalsbreen may have a locally cold-based margin, but the rest of the ice cap is temperate and we think classifying the ice cap as polythermal would mislead the reader.

Section 2.3.2:

- Very large range for velocities for lower ablation area of Midtdalsbreen: 4-40 m a^{-1} the upper part of this range is even faster than the values that you mention further for around the ELA (33 m a^{-1}): is this really the case? Could be due to local topography/sliding/…, but otherwise would expect higher velocities around the ELA.
  The large range in Vaksdal (2001) reflects the spatial variations in the lower ablation area. The front is very slow-moving, almost stagnant, perhaps due to the frozen bed mentioned above. The measurements from Vaksdal (2001) are summer velocities. In contrast, the 33 m a^{-1} at the ELA of Midtdalsbreen include both summer and most of winter; it was measured from 14 May 2005 to 18 March 2006 (Giesen, 2009, p.47). Velocities in summer are expected to be higher than in winter, which should explain the difference. In addition, there could be interannual variations. We now clearly state the different measurement periods in the manuscript.

Section 3.1:

- Not fully sure about the formulation of the SIA. Typically explained more as a function of (glacier) width vs. ice thickness. What do you exactly mean by ‘typical glacier length’ (l.24)? How do you determine the ‘characteristic horizontal scale’ (l.29) for your ice cap to be 4-8 km (and the ‘characteristic ice thickness to be around 200 m’ (l.29)?)
  We agree with the reviewer that SIA validity is a function of the horizontal extent and ice thickness. The aspect-ratio ε in Eq. (2) is a measure of this, with the underlying assumption that surface slopes are small. See also Eq. (5.5), (5.6) and (5.77) in Greve and Blatter (2009), p.63 and p.77.
  We now specify that the typical horizontal scale is based on Midtdalsbreen and Rembesdalskåka’s length records from the Little Ice

Age until today (~4.5-6.5 km and ~9-11 km, respectively). The "typical" vertical scale is more challenging to quantify due to the highly variable bedrock topography and is therefore estimated qualitatively by looking at ice thicknesses around the ELA. We now also include brackets in Eq. (2), so that ε = [H]/[L], to highlight that [H] and [L] are typical values and does not represent any particular part of the glacier.

- As I indicated before, given the model you use, a comparison between SIA and HO would have been interesting (and computationally feasible)
  See previous comments on SIA/HO.

- Would recommend to also have a look at recent paper by Kirchner et al. (2016) who review in-depth the differences between models of different complexities for longer time scales. Interesting elements that you could (/should?) add when discussing the SIA / HO-FS differences (not only here, also for other parts in text)
  We are thankful to the reviewer directing us to this relevant paper, which suggests that SIA/FS differences may be larger than expected from theory and that FS may be needed in more dynamic regions (ice streams, ice shelves, areas of fast flow). We now mention this study here and in our Discussion, but as the reviewer suggest, we choose not do discuss SIA/HO/FS differences extensively since we have not performed a comparative study, as mentioned before.

Section 3.1.1:

- Be consistent in formulation with $\tau, \tau, \tau_d, \bar{u}_d, u_b, \bar{u}, u$, which is not the case at this point.
  Changed.

Section 3.1.3:

- l.22-24: really need the lower resolution? Would expect higher resolution to be computationally feasible. If opt for low resolution, would do (one) higher-resolution run for comparison also.
  We thank the reviewer for this suggestion. We are performing experiments to test convergence on mesh resolution. Preliminary results show that the total volume varies by less than 5%; details will be given in the revised manuscript.

- l.25: need such a small time step?
  We also tried longer time steps, but numerical instabilities arose already at 0.025 years, so we settled on 0.02.

Section 3.2.1:

- l.29-30: repetition (+ see earlier comment: are this 44 or 45 years of measurements?)
  We choose to keep this sentence, since we do not think it is obvious from Section 2.2.3 how and what part of the available SMB data is used in our model.

It is 45 SMB years, as stated in previous response.

- SMB forcing: very simple. Not sure about applicability for other periods in time. Cannot catch many processes that are important and probably very different under other climatic conditions (changes in albedo, changes in refreezing,…etc.)
Our choice of a simple SMB profile is discussed in the Specific comments, Section 5.3, below.

- p.9, l.3-5: elaborate. Not clear at this point.
We now elaborate this further, stating that 'The averaged 35-year specific mass balance profile corresponds to an annual mass balance for Rembesdalskåka of -0.175 m w.e. We therefore shifted this profile by +0.175 m w.e. to obtain B_{ref}.

Section 3.2.2:

- Rate factor does not only depend on ice temperature. Important, but not the sole parameter. This is for instance clear from the fact that a wide range of rate factors is used for temperate glaciers, while the temperature is always at the pressure melting point. In your discussion and rationale the focus is too much on temperatures, be careful. l.21: "corresponding to ice temperatures" → "roughly corresponding to ice temperatures".
We agree that "corresponding to ice temperatures" is confusing wording, since the rate factor does not only depend on ice temperature, as also mentioned by reviewer 2. We now also state that rate factor can depend on ice fabric and impurities (and possibly other factors).

- l.30: "Based on figure 3": cannot base yourself on figure to conclude something. You base yourself on the experiments (their outcome) and the figure illustrates this.
Good point, now clarified.

Section 3.2.3:

- Again start with a repetition: overlap with section 2.2.2: should reorganize this to make text more consistent.
We now more clearly separate data/reconstructions (Section 2.2.2) and model forcing (Section 3.2.3).

- l. 19: "adds additional uncertainty and unnecessary complexity": be more specific. Not sure some additional complexity is unnecessary, could very well be needed to capture some processes…
We agree that complexity is not necessarily negative. We now clarify that our simple, linear SMB forcing for the Holocene is not only a result of poorly known climatic/SMB conditions in the past. It is also a deliberate strategy we choose to assess/isolate any non-linear, asynchronous behaviour in a clean way.

Section 3.2.4:

- Last sentence (l.1-2, p.11): repeat yourself again. Would remove this.
  Good suggestion, now removed.

Section 4.1:

- l.5: again a repetition.
  We thank the reviewer for highlighting this. We now remove repetitions and focus on the actual results.

- l.6-7: you "demonstrate" that growth is non-linear. Of course, this is not an idealized setting, so rather trivial that growth is non-linear. Is this really "demonstrating " something? Lines that follow: long part to say little.
  We agree that this is not appropriate wording. We "find" that the growth is non-linear.
  Only a theoretical case would be perfectly linear, so the reviewer is correct in that we expect a temporally variable response in the real case. However we do not think it is obvious that Hardangerjøkulen would grow in this stepwise manner, and even so, the timing and its relation to bed topography and the SMB-elevation feedback are interesting aspects of Hardangerjøkulen's history and have implications also for the long-term evolution of other ice caps.
  We now also improve clarity in this section by more clearly linking it to subsequent sections and Discusison.

Section 4.2.1:

- l.28: start with another repetition.
  Deleted.

- l.30-31: have a very large spread. Of course, large ensemble, most are wrong (too stiff/slow or too viscuous/fast): the range mentioned depends fully on the size of your ensemble and per se does not mean anything.
  This is a valid point. We now rather specify which range of parameter gives plausible results for the ice cap volume/extent.

Section 4.2.2:

- Very descriptive, chaotic and lacks structure. Should reorganize this and be more specific (to-the-point) to be clearer.
  We have rewritten this section focusing on clarity and now keep a chronological structure.

Section 5.1:

- l.30-31: "this is not surprising" → would reformulate this.
  Now reformulated to "This can be explained by…" We also use related advice from reviewer 2, stating that surface velocities are a function of both A and β, and the same surface velocities can be kept by a reduction of sliding and increased shear (or vice versa). However we do not

calibrate our models against surface velocities (because of poor data coverage, as pointed out in the manuscript).

- First paragraph: discussion about (basal) velocities: have very little information (especially when it comes to basal velocities) (as you mention yourself) → discussion is not really relevant.
  It is true that little is known about (basal) velocities. We now therefore only explain the model behaviour itself, and stress that more velocity data would be needed to assess deformation/sliding in more detail.

- l.6-11: Rate factor is not only related to temperature (see earlier comment). → l.14: "corresponding to -3°C": directly relating to temperature is probably not relevant/correct.
  We agree that this was not appropriate, see response to earlier comment (Section 3.2.2).

- l.20-26: weak description. Many words to say little. In the end you say: if fast → thin / if slow/stiff → thick
  We now reduce and clarify this section significantly.

- l.30 (p.13) → l.2 (p.14): mention something interesting. Would do this here. At this point the manuscript introduces a model and a (pretty straightforward) calibration/validation (and the evolution for this specific ice cap): what is the added value of this study compared to earlier studies?
  We believe that we perform a robust calibration with the data we have available. The available (velocity) data are not sufficient to constrain the dynamic parameters to a narrower range.
  We agree with the reviewer that the implications of our study were not clear. As mentioned above, our new, dedicated subsection on transferability/applicability improves this.

Section 5.2:

- Long section about sliding: do almost not have any information. Based on your modeling → cannot really learn anything new about sliding for this ice cap. Results are simply related to your model setup and in the end your finding (which you mention further: that a lot of different combinations for your rate factor and sliding parameter are possible) is logical (as both flow and sliding have similar spatial patterns in your setup) and this was already demonstrated in earlier studies.
  We agree with the reviewer here, and have strongly shortened this section, as also suggested by reviewer 2.

- Comparison with other studies on ice sheets. Is this relevant? Totally different setting, other mechanisms for water to reach the bed (/being locally produced).
  Good point, we now focus on other ice caps and outlet glaciers.

- l.18:"It is therefore not surprising" → change

Changed to "...which probably explains why Hardangerjøkulen is more sensitive to the sliding parameter value than Langjökull."

- l.26-29: relationship sliding and geometry: from theoretical perspective. This is not a "finding" from your study..
  We are not sure what the reviewer means here, studies in l.24-27 are model studies of paleo-ice sheets. We do get a thinner ice cap with increased sliding, and we find value in highlighting previous work. We have now however omitted some of the details, since the papers cited studied ice sheets and not ice caps.

- l.28: "Thus, for whatever the cause,.." → If you want to know the cause: have a look into ice flow theory.. + not kind of language expected in scientific text ("for whatever the cause"..)
  We agree that this was not appropriately phrased and have deleted this formulation.

- p.15, l.3-4: indeed. A whole section to say very little..
  Now shortened.

Section 5.3:

- l.6-9: repeat yourself.
  Good point, now removed.

- l.10-13: SMB vs. elevation: too simple here. What about albedo, refreezing and for instance insolation (expect very different SMB vs. elevation for a surface oriented to the South and one oriented to the North…)
  We appreciate that the reviewer suggests several relevant processes for the SMB. However, we deliberately chose to use a simple mass balance formulation, to focus on ice dynamical, long-term response to spatially homogeneous changes in the forcing. We justify this formulation based on results presented in Giesen (2009) and Giesen and Oerlemans (2010). They simulated the ice cap evolution through the 20th century with the simple SMB profile used here, as well as with a spatially distributed mass and energy balance model. Differences in ice volume and outlet glacier lengths at the end of these simulations are present, but small. Even when including an albedo scheme, a spatial precipitation gradient, and aspect and shading effects on insolation, the modelled lengths of Rembesdalskåka and Midtdalsbreen cannot both be matched with the observations. This suggests that this should not be attributed to the SMB, but to other factors.
  As Giesen (2009) and Giesen and Oerlemans (2010) already studied spatial variations in the SMB, our aim is not to repeat their analyses. Instead we include the results relevant for our study in this Section. Hardangerjøkulen has a gently sloping surface and is not surrounded by high mountains. Therefore, topographic effects on the insolation result in small spatial variations of the SMB are between -0.1 and +0.1 m w.e. for the vast majority of the ice cap, only two outlet glaciers oriented south show larger deviations locally. Under a realistic 21st century scenario,

Giesen and Oerlemans (2010) show that lowering the ice albedo from 0.35 to 0.20 only leads to a 5% larger volume decrease of the ice cap. Furthermore, even in a considerably warmer climate with a smaller ice cap (with continuously updated topographic effects on solar radiation), the SMB gradient with elevation was close to the present-day value. We conclude that using a SMB profile only dependent on elevation is a good approximation for Hardangerjøkulen, even in a different climate with a smaller or larger ice cap.

- l.19-20: what do you mean? Be more specific.
  We now specify that such studies would need to be coupled reconstructions of (winter) precipitation and glacier variations, on both sides of the ice cap. We leave it up to the reader to decide exact what type of proxy methods would be best suited for such reconstructions; their details can be found in the cited paper.

- l.23-24: indeed. Could this not be done?
  Further snow and SMB studies aiming to quantify the spatial accumulation variability require laborious efforts. Since the interannual variability in SMB in general and winter accumulation in particular is large (Giesen, 2009; Giesen and Oerlemans, 2010), such a campaign would have to run over several years. We now specify that with "further snow and mass balance studies" we mean field measurements.

- l.31-33: snow redistribution. Could indeed have an effect. But probably smaller effect than the large errors induced by your other approximations.
  Good point. We now make clear that here we explain the observed SMB rather than our model results, and combine this with the paragraph above.

- p.16, l.3-8: not convinced that this error is that large compared to the magnitude of errors induced by your simple modelling..
  In our opinion this error is large. However, because it only applies to the last years of our simulation period, the effect is small. We think this SMB data correction from the Norwegian Water and Energy Directorate (NVE) is worth to include.

- l.9: "works well": not sure..
  See above comments. As mentioned previously, our goal is not to reconstruct SMB for the Holocene and LIA, but to assess the long-term dynamic response to a simple climate forcing.

- l.13-17: of course, so would need albedo in model! Does not have to be a very complex model where a lot of data is needed for validation/calibration (e.g. model solving the full energy balance): this can be done in a rather simple way, but which is very effective (e.g. PDD model, T index model, simple energy balance model,…) (e.g. Braithwaite, 1995; Hock, 2003; Oerlemans, 2001)

Including any kind of albedo scheme would indeed add detail to the simulations. However, we do not aim to reconstruct/project the mass balance details of the ice cap changes. Our approach is to force the model with mass balance anomalies and not with temperature and precipitation records.

Since concern about the SMB forcing arises at several places, we have summarized the effects in our new discussion of SMB. As mentioned above, even with a full surface energy balance model (Giesen and Oerlemans, 2010), changes in the SMB vertical gradient are small, so the profile we use is probably also a good approximation for SMB in different climates. Of course, there will be effects of all the processes not included, but they will be second-order.

- l.21-23: Holocene changes in climate are strongly influenced by changes insolation, so this should be taken into account. Could be done with simple parameterization also.
  We believe that we have justified our choice not to include insolation changes in the original manuscript, l. 19-23. See also above comments on radiation.

Section 5.4:

- Discussion on ice dynamics, while you do not really have the material to discuss this. This is mostly a reference to the literature. A pity, given the fact that your model can be run in HO and a comparison can be made…
  As said before, we agree with the reviewer that we put too much focus on SIA/HO/FS, since we do not perform comparative tests (for reasons mentioned before). We therefore have strongly shortened this section.

- p.17, l.1-4: you discuss the effect of sliding and the deterioration of the SIA as this increases. Is indeed true. Then say that because you do not necessary have information → cannot draw conclusions. This is true, but I think that the main reason why you cannot draw conclusions is simply because you do not have a 'reference run' (a HO/FS run) to compare to.
  See previous responses on SIA/HO/FS.

- l.5-13: this is not a discussion of your results.
  Good point, we now avoid making general statements and only discuss our own results and their implications.

Section 5.5:

- l.23: effect SIA. Of course true, but the effect of SIA/HO-FS is very limited compared to other errors and uncertainties. Over Holocene timescale the SMB (where uncertainties are large) will have much larger effect than dynamics on the evolution/growth.
  We agree that SMB is more important than SIA/HO/FS on Holocene time scales, which we mentioned before. We now state this clearly in the text.

Section 5.6:

- • l.4-9: growth → very descriptive and site specific. What is added value for reader?
  We agree that this was unclear. We now discuss the non-linear, asynchronous growth in our new transferability/implications subsection. We also analyze the volume-area variations (Fig. 10) in more detail and in light of volume-area scaling relations in the literature.

- l.21-22: "this asymmetry illustrates that proxy records representing different parts of an ice cap may lead to substantially different conclusions about ice cap size through time" → of course. Rather trivial.
  While intuitive, we do not think this is appreciated in the literature of glacier reconstructions, where conclusions about past glacier activity and climate are sometimes drawn from a single outlet glacier of a larger ice mass.
  We realize that there is not much input from any of the reviewers on the proxy/paleoglaciological relevance of the study, for example no comments on the opposite asymmetry during growth and retreat. Since we think these aspects are important, we should have emphasized them more and thus now make the long-term Holocene evolution and the effect of SMB-elevation feedback more visible in the Results and the Discussion.

- l.23-32: long passage with little information.
  l.23-28 contains some in our view relevant previous studies worth mentioning, and we think our findings about overdeepenings and glacier advance complements/build on these.
  We agree that l.29-32 was vague and is now shortened and more to-the-point.

- p.19, l.1-27: many words about response time to in the end say very little. Do not have experiments to elaborate on this. Could spend a few words on this, but not whole section.
  We agree with the reviewer, and now keep it short and specific with regards to our results.

- l.28 → p.20, l.1-2: not sure that your results support this. Rather speculative.
  We have not studied erosion, sediment transport and deposition in our study, so we agree with the reviewer that we should be careful about drawing specific conclusions on sediment-based reconstructions. Still, we think our out-of-phase evolution of volume and area for many centuries (l. 22-23; Fig. 10) suggests that linear assumptions between basin size (area), ice volume (mass balance), climate, and their proxies should be challenged.
  We are now more specific on this and less speculative when it comes to sedimentation.

Section 5.7:

- l.7: effect proglacial lake. Can have an effect, but expect this again to be much smaller than other model uncertainties.
  We agree, and now point this out.

- l.14: "in our view a step forward": not sure. Even if would be the case, you should maybe not write this down and let the reader decide for himself whether he thinks this is new/novel/better than methodology applied in other studies. First focus should be a carefully calibrated/validated and robust setup, supported by field data, and not sure whether this is the case in this study.
  We agree with the reviewer that this was not appropriate wording. We think however that our methodology, results and their implications have value for other studies, which we also highlight in our new transferability/implications subsection mentioned above.

Section 5.8:

- l.25: you "show" that ice cap is very sensitive to change in climatic conditions. Trivial: of course, it is an ice cap. Importance SMB-elevation feedback. Has been analyzed in (far greater) depth and from theoretical point in the past. Have a look at some of the 'classic' papers on this (Lee and North, 1995; Mahaffy, 1976; North, 1984).
  We now analyse the SMB-elevation feedback in more detail and have moved it to the Results. The sensitivity to SMB is exceptionally strong for Hardangerjøkulen, the feedback is crucial to this sensitivity. See previous comments on SMB-elevation feedback.

- p.21, l.1-4: again rather trivial. What's new about this finding?
  Perhaps it is trivial that the relation is linear without including the feedback, but we mainly use this experiment to illustrate that it is indeed the feedback that makes the ice cap so sensitive. To illustrate this difference, we now show the modelled transient ice volume evolution in response to SMB perturbations of the present-day ice cap, without the SMB-elevation feedback, in Fig. 12. We have also moved it to the Results section.

- l.9: 750 years to disappear. Too precise. Would change this to "around 750 years"
  Changed as suggested.

- "As evident from Collins et al. (2013), we expect a warming scenario": strange formulation.
  We agree, and have changed this to "Future projections suggest a warming scenario for southern Norway"

- l.11-21: do not really discuss your own results, not based on your simulations.
  To keep the focus on our own results, we now exclude most of the paragraph about the future.

- l.22-28: what's new?
  Now specified that our study provides new detail on the transient evolution/growth and retreat during Holocene and its relation to bed topography/SMB-elevation feedback. We now also consider the reconstructed disappearance into perspective of mass balance sensitivity. We choose to keep the line about future warming and refer to Giesen and Oerlemans (2010) for future projections.

Conclusions:

- Start from ice-free in Holocene: do you also get this if would start simulations earlier and force with a palaeoclimatic record? Would be an interesting experiment..
  This would indeed be interesting, but it is not within the scope of our study. We see this as a suggestion for a future study, where a (simplified) mass and energy balance model is used to study the full Holocene evolution of Hardangerjøkulen, forced with paleoclimatic records of temperature, precipitation and insolation. However, a challenge would be what ice cap state to start with, since no good estimates on ice volume/extent of Hardangerjøkulen exist prior to the mid-Holocene ice-free period. In this study, we start from ice-free conditions, because this is the most robust route to study the Holocene ice cap evolution from 4000 BP onwards.

- p.22, l.3-6: this is not something new. Not a finding from this study.
  We agree with the reviewer and now deemphasize this. Still we would like to encourage other studies to keep calibration ensembles during transient simulations, so we have kept this in our conclusion.

- l. 9-14: SMB-elevation feedback exists for ice cap. You show this, but do not really add anything new to the theory related to this.
  These lines do not specifically refer to the SMB-feedback, and we think what is mentioned is relevant for other studies of ice caps, as mentioned before.

- l.15-17 + l. 24-26: site specific → what is the more general interest?
  This is indeed not clear. We have completely reworked the Conclusion aiming to be clearer about our findings and what the value/implications are.

- l.27-31: strange way to end your conclusion..
  We agree and have integrated this into the Conclusion.

Figures:

- Nice and clear figures in general.

We thank the reviewer for this. We have made some improvements anyway:

- Fig. 2: swapped red/blue colours in legend, as they did not correspond to the lines in the figure.

- Fig. 6: plotted individual simulations to indicate the distribution. Used different colors for different rate factors, same color for different sliding parameters within the same rate factor.
- Fig. 10: Added a related figure showing the relationship between volume and area, with relevance for volume-area scaling methods
- Fig. 12: included simulations excluding the SMB-elevation feedback, to add detail to this feedback in the manuscript, as suggested by the reviewer.

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