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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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 geormophological 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.

  - – 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?
    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 tunning), but not in a 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 loosing 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 \beta 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, \beta) 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 havn'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.

Response to Short Comment #1

We would like to thank Dr. Luening for his Interactive Comment, and respond to his suggestion below in blue.

On behalf of the authors,
Henning Åkesson

While studying this manuscript I could not find any hint towards the Medieval Climate Anomaly (MCA). This is a phase which was anomalously warm in the study region and which was generally associated with a major glacier melt episode. Why is the MCA not featured in the article? The most likely driver of the MCA warming is high solar activity during this phase, requiring an adequate radiative forcing to produce an effect in the theoretical models. I strongly recommend to include a discussion on the MCA in the paper. Reference to the Little Ice Age requires an analysis of the preceding climatic event, i.e. the MCA, otherwise the discussion is incomplete.

We thank Dr. Leuning for raising this concern. The Medieval Climate Anomaly (sometimes referred to as the Medieval Warm Period (MWP) in the literature) is indeed an important climatic period in the recent past which, as Dr. Leuning points out, is associated with glacier retreat.

Nevertheless, there is little information to validate Hardangerjøkulen's response to a climate/mass balance forcing during this period. The same reasoning applies to the earlier Holocene, as mentioned in our response to the two reviewers. We do not intend to add mass balance variations/variability without constraints, and we also wish to keep our mass balance forcing simple in order to isolate the effects of bed topography and the SMB-elevation feedback. We also do not consider assessment of the responsible climatic forcing(s) behind the Little Ice Age, its preceding or following climates as a central aim of our study. We therefore do not think discussing the MCA/MWP is relevant for our purposes.

Simulating ~~asymmetric growth and retreat~~ the evolution of Hardangerjøkulen ice cap in southern Norway since the mid-Holocene and its sensitivity to climate change

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Abstract. ~~Changes to the volume of~~ Understanding of long-term dynamics of glaciers and ice caps ~~currently amount to half of the total cryospheric contribution to sea-level rise and are projected to remain substantial throughout the 21st century. To simulate glacier behavior on centennial and longer time scales, models rely on simplified dynamics and tunable parameters for processes not well understood. Model calibration is often done using present-day observations, even though the relationship between parameters and parametrized processes may be altered for significantly different glacier states.~~

~~In this study, we simulate the evolution of the Hardangerjø~~is vital to assess their recent and future changes, yet few long-term reconstructions using ice flow models exist. Here we present simulations of the maritime Hardangerjøkulen ice cap in ~~southern~~ Norway from the mid-Holocene through the Little Ice Age (LIA) to the present-day~~. For both the calibration and transient experiments, we run an ensemble using a two-dimensional~~ , using a numerical ice flow model ~~with local mesh refinement. For the Holocene, we apply a simple surface mass balance forcing based on~~ combined with glacier and climate reconstructions. ~~For the LIA until 1962, we use geomorphological evidence and measured outlet glacier positions to find a mass balance history, while from 1963 until today we use direct mass balance measurements.~~

~~Given~~ Under a linear climate forcing, we find that ~~Hardangerjø~~Hardangerjøkulen grew from ice-free conditions in the mid-Holocene ~~,~~ to its maximum ~~LIA extent in a highly~~ extent during the LIA in a non-linear, spatially asynchronous fashion. During ~~the~~ its fastest stage of growth (~~2200–1200~~ 2300–1300 BP), the ice cap tripled its ~~ice~~ volume over only 1000 years. ~~We also reveal an intriguing spatial asymmetry during advance and retreat; the western ice cap and the northern outlet glacier Midtdalsbreen grow first and disappear first. In contrast, the eastern part, including the northeastern outlet glacier Blåisen, grows last and disappears last~~The modelled ice cap extent and outlet glacier length changes from the LIA until today are close to observations.

~~Furthermore, volume and area of several outlet glaciers, as well as of the entire ice cap,~~

Volume and area for Hardangerjøkulen and several of its outlet glaciers vary out-of-phase for ~~multiple~~ several centuries during the ~~late Holocene, before varying in-phase approaching the LIA. We relate this to bed topography and the mass balance-altitude feedback, and challenge canonical linear assumptions between ice cap extent~~Holocene. This volume-area

disequilibrium varies in time and from one outlet glacier to the next, illustrating that linear relations between ice extent, volume and glacier proxy records~~. Thus, we provide new insight into long-term dynamical response of ice caps to climate change, relevant for paleoglaciological studies and future predictions.~~ , as generally used in paleo-climatic reconstructions, have only limited validity.

~~Our model simulates ice cap extent and outlet glacier length changes from the LIA until today that are close to observations. We show that~~ We also show that the present-day ~~Hardangerjøkulen is extremely~~ ice cap is highly sensitive to surface mass balance changes ~~, mainly due to a strong~~ and that the mass balance-altitude feedback ~~for the gently sloping surface topography of the ice cap .~~ and ice cap hypsometry are essential to this sensitivity. A mass balance shift by +0.5 m w.e. relative to the mass balance from the last decades almost doubles ice volume, while a decrease of 0.2 m w.e. or more induces a strong mass balance-altitude feedback and makes Hardangerjøkulen disappear entirely. Furthermore, once disappeared, an additional +0.1 m w.e. relative to the present mass balance is needed to regrow the ice cap to its present-day extent. We expect that other ice caps with comparable geometry in for example Norway, Iceland, Patagonia and peripheral Greenland may behave similarly, making them particularly vulnerable to climate change.

1 Introduction

~~Recent global decline in glacier ice across all continents (Gardner et al., 2013) is viewed as one of the clearest signs of the impacts of a warming climate (Vaughan et al., 2013). Further projected accelerated mass loss (Radic et al., 2013; Giesen and Oerlemans, 20 predicted to have considerable impact on 21st century sea level rise (Marzeion et al., 2012; Church et al., 2013) and beyond (Levermann et al., 2013). Socioeconomic effects include severe changes to hydropower operations, agriculture, tourism and ecosystems.~~

~~Given the inertia of ice sheets,assessments of the response of glaciers and small~~

The 211,000 glaciers and ice caps (GICs) ~~to climate are essential. Although the 170 000 GICs~~ (Pfeffer et al., 2014; Arendt et al., 2015) in the world are relatively small compared to the Greenland and Antarctic ice sheets, but they constitute about half of the current cryospheric contribution to sea level rise (Shepherd et al., 2012; Vaughan et al., 2013), a distribution projected to remain similar throughout the 21st century ~~(Church et al., 2013).~~ (Church et al., 2013; Huss and Hock, 2015). Since areas of GICs are more readily available than their volume, scaling methods are commonly employed to estimate total ice volumes and their sea level equivalents (e.g. Grinsted, 2013; Bahr et al., 2015). Many of these GICs are ice caps, though little is known about their response to long-term climate change, how a particular ice cap geometry contribute to this sensitivity or how scaling methods perform for ice caps.

~~Accurate predictions of the future response of GICs rely on our understanding of the relative importance of ice dynamics and surface mass balance (hereafter referred to as 'mass balance'). For comparison with current rapid changes and future predictions, glacier and climate reconstructions (e.g. Karlén, 1976; Nesje et al., 1991; Bakke et al., 2005) provide an invaluable baseline. Glacier reconstructions build on glaciological assumptions, including that sediment input is a function of glacier~~

~~(erosive) area and mass balance throughput (e.g. Hallet et al., 1996). Using models in combination with independent glacier reconstructions has the potential to deepen~~

Reconstructions of past climate and glacier variations contribute to our understanding of ~~glacier-climate interactions and give insight into the physics operating on these time scales.~~

~~Recent interest in so called Full Stokes ice dynamical models (e.g. Larour et al., 2012; Gagliardini et al., 2013), accounting for the complete stress balance associated with ice sheet and glacier dynamics has led to valuable progress in process understanding. Nonetheless, simpler models are generally preferred, as long as they can accurately describe ice flow and climatic response on the time scales of interest. Due to their much lower computational cost, simple ice dynamical models allow for extensive 'ensemble' experiments, assessing the effect of a suite of parameters on modeloutcome (Alley and Joughin, 2012).~~

~~In this study, we present simulations of the Hardangerjøkulen ice cap in southern Norway since 4000 BP, using a simple ice dynamical model. Several GIC modeling studies have investigated glacier response on time scales over a few centuries (e.g Giesen and Oerlemans, 2010; Adalgeirsdóttir et al., 2011; Zekollari et al., 2014) as well as millennia~~ long-term glacier behavior. However, these studies often build on simple glaciological assumptions relating proxies, ice extent, ice volume and climate (e.g. Hallet et al., 1996). As glaciers are non-linear systems with feedbacks, such relations are difficult to constrain without a numerical model. Yet long-term reconstructions using ice flow models are rare. Most existing quantitative modelling studies of GICs are restricted to timescales of decades (e.g. Leysinger-Vieli and Gudmundsson, 2004; Raper and Braithwaite, 2009) or centuries (Jouvet et al., 2009; Giesen and Oerlemans, 2010; Adalgeirsdóttir et al., 2011; Zekollari et al., 2014; Zekollari and Huybrechts, 2 Only a very limited number of studies exist for the longer timescales (e.g. Flowers et al., 2008; Laumann and Nesje, 2014). Studies focusing on ~~the~~ glacier evolution since the Little Ice Age (LIA) (e.g. Giesen and Oerlemans, 2010; Adalgeirsdóttir et al., 2011; Zek perturb a present-day glacier or ice cap with a climate anomaly relative to ~~a modern climatology to serve as initial conditions. Here, we instead~~ the modern and do not explicitly consider the ice cap history preceding the LIA.

In this study, we use a numerical ice flow model to provide a quantitative, long-term, dynamical perspective on the history and current state of the Hardangerjøkulen ice cap in southern Norway. These results are also relevant for our understanding of the history and future stability of similar ice masses in e.g. Norway (Nesje et al., 2008a), Iceland (Adalgeirsdóttir et al., 2006), Patagonia (Rignot et al., 2003), Alaska (Berthier et al., 2010) and peripheral Greenland (Jacob et al., 2012). We present a plausible ice cap history over several thousand years before the LIA ~~, which is then used~~ (Sect. 4.1), and use this as a starting point for simulations from LIA to present-day ~~simulations. In addition, we carry out an extensive evaluation of the sensitivity to~~ (Sect. 4.2). To evaluate the sensitivity of the ice cap to the choice of dynamical model parameters, ~~which in most other studies is restricted to the calibration phase, and in a sense 'lost' when transient model simulations start.~~

~~Using the above methodology, we aim to advance our understanding of long-term (centuries to millennia)dynamic aspects of glacier change, and to~~ we perform an ensemble of simulations with different dynamical model parameters (Sect. 4.2.1). Furthermore, we quantify the sensitivity of Hardangerjøkulen ~~ice cap~~ to climatic change (Sect. 4.1). ~~We also show that glacier reconstructions can be improved by considering the underlying bed topography and interacting ice dynamics.~~

~~This paper is organized as follows. First, the~~ We find that Hardangerjøkulen ~~ice cap is described in Sect. 2, after which our model strategy and calibration is outlined in Sect. 3. Results from the mid- to late Holocene simulations, as well as~~

~~from the LIA until today, are presented in Sect. 4. We analyze the sensitivity to model parameters, mass balance and ice dynamics in Sect. 5, including the implications for our modelled Holocene evolution. Finally, we discuss the sensitivity of present-day Hardangerjø~~is exceptionally sensitive to surface mass balance changes, and that the surface mass balance-altitude feedback and ice cap hypsometry is crucial to this sensitivity. To constrain the assumptions made in glacier reconstructions and volume-area scaling applications, we assess the degree of linearity between ice cap volume and area (Sect. 4.2). We show that commonly used scaling relations overestimate ice volume, and suggest that glacier and climate reconstructions could benefit from quantifying the impact on proxy records of bed topography, glacier hypsometry and the surface mass balance-altitude feedback (Sect. 5.5).

~~kulen to future changes in mass balance.~~

2 Hardangerjøkulen ice cap

2.1 ~~Geometry~~Present-day geometry

2.1.1 Surface topography

Hardangerjøkulen ~~ice cap~~ (60°55'N, 7°25'E) has a present-day (year 2012) area of 73 km^2 (Andreassen et al., 2012) and is located at the western flank of the Hardangervidda mountain plateau. The ice cap is rather flat in the interior with steeper glaciers draining the plateau (Fig. 1). The largest outlet glaciers are Rembesdalskåka (facing W-SW; 17.4 km^2), Midtdalsbreen (NE; 6.8 km^2), Blåisen (NE; 6.6 km^2) and Vestre Leirbotnskåka (S-SE; 8 km^2). Surface elevation ranges from 1865 to 1020 m a.s.l. (Andreassen et al., 2015), with 80 % of the ice cap area, and 70 % of Rembesdalskåka, situated above the mean equilibrium-line altitude (ELA) at 1640 m a.s.l. (1963-2007 average; Giesen, 2009). Rembesdalskåka drains towards the dammed lake Rembesdalsvatnet, located ~1 km from the present-day glacier terminus (Kjøllmoen et al., 2011). Midtdalsbreen is a gently sloping outlet glacier ranging from 1380 to 1865 m a.s.l.

2.1.2 Ice thickness and bed topography

A number of surveys have mapped the ice thickness at Hardangerjøkulen (e.g. Sellevold and Kloster, 1964; Elvehøy et al., 1997; Østen, 1998, K. Melvold, unpubl. data), with the highest measurement density for Midtdalsbreen ~~(Fig. 2.12a in Giesen, 2009).~~

~~A combination of automatic and manual interpolation and extrapolation was used to produce the final ice thickness map (Elvehøy et al., 1997; Willis et al., 2012), and thereby a map of bed topography (Fig. 1).~~ (Fig. 2.12a in Giesen, 2009; Willis et al., 2012). In areas with dense measurements, ice thickness was interpolated using methods detailed in Melvold and Schuler (2008). In sparsely measured areas, ice thickness H was estimated directly from the surface slope α, assuming perfect plasticity (Paterson, 1994, p. 240)~~:~~

$$\cancel{H = \frac{\tau_0}{\rho_i g \nabla s},}$$

~~where τ_0 is the yield stress, ρ_i ice density, g gravitational acceleration and ∇s the surface slope~~. Based on detailed ice thickness measurements and ~~knowledge of~~ information on the surface slope on Midtdalsbreen, a yield stress of 150-180 kPa was used, in agreement with other mountain glaciers (Cuffey and Paterson, 2010, p.297; Zekollari et al., 2013). ~~Some manual interpolation was required in areas with small surface slopes (i.e. at~~ Over the flat areas near ice divides and ice ridges~~)~~, as well as near ice margins, manual extrapolation was required to obtain a ~~continuous decrease in ice thickness~~ smooth ice surface (K. Melvold, pers. comm.). A map of bed topography (Fig. 1) was produced by combining the final ice thickness map with a surface DEM (year 1995) from the Norwegian Mapping Authority, derived from aerial photographs.

2.2 Past geometry

2.2.1 Holocene changes

Reconstructions show that glaciers in southern Norway did not survive the mid-Holocene thermal maximum (e.g. Bakke et al., 2005; Nesje, Based on lake sediments and terrestrial deposits, Hardangerjøkulen is estimated to have been absent from circa (c.) 7500 to 4800 BP (Dahl and Nesje, 1994), although a short-lived glacier advance is documented for the southern side of the ice cap at c. 7000 BP (Nesje et al., 1994). Some high-frequency glacier fluctuations of local northern glaciers occurred during the period 4800-3800 BP, after which Hardangerjøkulen has been present continuously (Dahl and Nesje, 1994). There are few quantitative constraints on ice cap extent for the period from ice cap inception 4000 BP until the LIA. However, interpretations of lake sediments and geomorphological evidence suggest a gradual growth of Hardangerjøkulen during this period (Dahl and Nesje, 1994, 1996).

2.2.2 Outlet glacier changes since the Little Ice Age

~~The 'Little Ice Age' (LIA) maximum~~ Length changes extracted from maps and satellite imagery, moraine positions and direct front measurements are combined to derive length records for two major outlet glaciers for the period 1750-2008. For Rembesdalskåka, we use the same flowline as the Norwegian Water and Energy Directorate (NVE) use for their mass balance measurements (H. Elvehøy, pers. comm.). The NVE flowline for Midtdalsbreen was slightly modified to better correspond with the maximum ice velocities. Since changes are only made upglacier of the present-day margin, they do not interfere with the area where data of frontal changes exist.

The LIA maximum for Midtdalsbreen is dated to 1750 AD with lichenometry (Andersen and Sollid, 1971). For Rembesdalskåka, the outermost terminal moraine has not been dated, but is assumed to originate from the LIA maximum.

~~Front~~ Frontal observations for Rembesdalskåka began in ~~1917, have subsequently been performed during several periods~~1917. These have been performed for 22 of the years during the period 1917-1995, and are since 1995 done annually. For Midtdalsbreen, an annual length change record exists from 1982 onwards (Kjøllmoen et al., 2011). At present, Rembesdalskåka has retreated almost 2 km from its LIA maximum extent and Midtdalsbreen ∼1 km.

~~Both outlet glaciers~~ The two outlet glaciers considered advanced in response to snowy winters around 1990. The terminus change from 1988 to 2000 for Rembesdalskåka was +147 m and for Midtdalsbreen +46 m. By 2013, ~~the two glaciers~~

Rembesdalskåka and Midtdalsbreen had retreated 332 m and 164 m respectively, from their positions in 2000 ~~, respectively~~ (Andreassen et al., 2005; Kjøllmoen et al., 2011; Cryoclim.net, 2014).

~~Length changes extracted from maps and satellite imagery, moraine positions and direct front measurements are combined to derive length records for the two outlet glaciers for the period 1750-2008. For Rembesdalskåka, we use the same flowline as NVE uses for their mass balance measurements (H. Elvehøy, pers. comm.). The NVE flowline for Midtdalsbreen was slightly modified to better correspond with the maximum ice velocities. Since changes are only made upglacier of the present-day margin, they do not interfere with the area where data of frontal changes exist.~~

2.3 Climate

2.3.1 ~~Holocene changes~~

~~Based on lake sediments and terrestrial deposits,~~

2.3.1 Holocene and Little Ice Age climate

Reconstructions for southern Norway based on pollen and chironomids suggest that summer temperatures were up to 2°C higher than present in the period between 8000–4000 BP, when solar insolation was higher (Nesje and Dahl, 1991; Bjune et al., 2005; Velle At 4000 BP, proxy studies suggest a drop in summer temperatures to 0.5 °C lower than present combined with a drier climate (Dahl and Nesje, 1996; Bjune et al., 2005; Velle et al., 2005b; Seppä et al., 2005).

Dahl and Nesje (1996) reconstructed Holocene summer temperatures for southern Norway based on former pine-tree limits. Using a well-established empirical relationship between summer temperature and winter precipitation at the ELA of Norwegian glaciers (Liestøl in Sissons, 1979; Sutherland, 1984), they estimated winter precipitation for the Hardangerjøkulen ~~is estimated to have been practically absent from c. 7500 to 4800 BP (Dahl and Nesje, 1994), though a short-lived glacier advance is documented for the southern side of the ice cap at c. 7000 BP (Nesje et al., 1994). Some high-frequency glacier fluctuations of local northern glaciers occurred during the period 4800-3800 BP, after which Hardangerjøkulen has been present continuously (Dahl and Nesje, 1994)~~area from lake sediment-derived ELAs. These reconstructions suggest a close to linear cooling and wetting trend from 4000 BP until the LIA, including a possible warm event lasting for several centuries around 2000 BP (Velle et al., 2005a).

The LIA climate in southern Norway is likely to have experieneced more precipitation (Nesje and Dahl, 2003; Nesje et al., 2008b; Rasmu was c. 0.5-1.0 °C colder than present (Kalela-Brundin, 1999; Nordli et al., 2003), although some reconstructions indicate milder summers during the first quarter of the 18th century (Kalela-Brundin, 1999).

2.4 ~~Climate and mass balance~~

2.3.1 Present climate

Southern Norway is located in the Northern Hemisphere westerly wind belt and is heavily influenced by moist, warm air picked up by the frequent storms coming off the Atlantic Ocean (Uvo, 2003). When these winds reach the mountainous west coast, orographic lifting occurs and precipitation falls as rain or snow, depending on elevation. Conversely, eastern Norway is located in the rain shadow of the coastal mountains and the high mountain plateau Hardangervidda.

This strong west-east precipitation gradient is illustrated by the mean annual precipitation for 1961-1990 over southern Norway. Precipitation in Bergen, 65 km west of Hardangerjøkulen, reaches 2250 mm a^{-1} ~~.~~ (data from eklima.no, Norwegian Meteorological Institute). In contrast, Oslo in eastern Norway receives 763 mm precipitation per year. Liset, 17 km southeast of the summit of Hardangerjøkulen receives 1110 mm a^{-1}, while Finse, 8 km northeast of the summit, experiences 1030 mm a^{-1}~~and~~. Finse has a mean annual temperature of -2.1°C~~(data from eklima.no, Norwegian Meteorological Institute).~~

2.3.2 ~~Holocene and Little Ice Age climate~~

~~Reconstructions for southern Norway based on pollen and chironomids suggest that summer temperatures were up to 2°C higher than present in the period between 8000–4000 BP, when solar insolation was higher (Nesje and Dahl, 1991; Bjune et al., 2005; Velle At c. 4000 BP, when reappearance of northern Hardangerjøkulen is documented (Dahl and Nesje, 1994), temperatures at Finse had decreased to about 0.5 °C below present-day (Velle et al., 2005b)~~, while temperature is not measured at Liset.

~~Precipitation changes are less well-known, but the mid-Holocene in southern Scandinavia was likely drier than present (Dahl and Nesje, 1996; Seppä et al., 2005), implying unfavourable conditions for glaciers.~~

~~The period from mid-Holocene to the LIA was probably characterized by a gradual warming and wetting trend (Dahl and Nesje, 1996), wherein there may have been a warming event lasting for several centuries around 2000 BP (Velle et al., 2005a).~~

~~The LIA climate in southern Norway likely had more precipitation (Nesje and Dahl, 2003; Nesje et al., 2008b; Rasmussen et al., 2010) a was c. 0.5-1.0 °C colder than present (Kalela-Brundin, 1999; Nordli et al., 2003), though some reconstructions indicate milder summers during the first quarter of the 18th century (Kalela-Brundin, 1999).~~

2.3.2 ~~Surface mass balance~~

2.4 Surface mass balance

Glaciological mass balance measurements started on Rembesdalskåka in 1963. The mean net balance for the period 1963–2010 was slightly positive (+0.08 m water equivalent (w.e.)), divided into a winter balance of +2.10 m w.e. and a summer balance of -2.03 m w.e. (Kjøllmoen et al., 2011).

For Midtdalsbreen, ~~there are only mass balance measurements for 2000-2001~~ mass balance was only measured in 2000 and 2001 (Krantz, 2002). This two-year time series is too short for a robust surface mass balance comparison between the two outlet glaciers.

Specific mass balance ~~gradients~~ profiles for the entire elevation range of Rembesdalskåka exist for 35 of the 45 mass balance years (October 1 - September 30) in the period 1963–2007. The interannual variability around the mean winter profile is similar at all elevations, while the range in summer balances increases from high to low elevations ~~(see Fig. 2.7a in Giesen (2009)).~~ (Fig. 2.7a in Giesen, 2009). The decrease in ~~(mainly winter)~~ mass balance at the highest elevations ~~can probably be explained by snow redistribution by wind, but to the authors' knowledge, no studies have quantified this effect on the mass balance at Hardangerjøkulen~~is a persistent feature of the winter mass balance, and is strongest in years with large accumulation (Fig. 5.3 in Giesen, 2009). Its origin is however uncertain and long-term snow depth measurements on several outlet glaciers are needed to identify the underlying process.

The net balance gradient has a similar shape for most years, and the relation between net mass balance and altitude is approximately linear from the terminus up to 1675 m a.s.l. (Fig. 2), with a mass balance gradient of 0.0097 m w.e. per m altitude. The net mass balance is zero at 1640 m a.s.l., marking the ~~equilibrium-line altitude (ELA)~~ELA. Above the ELA, the mass balance gradient decreases with altitude, approximated ~~in Fig. 2 by a second-order polynomial .~~ by a third-order polynomial (Fig. 2).

2.5 Ice dynamics

2.5.1 Basal conditions

Although bed conditions are not well-known, based on the sparse sediment cover in the surrounding areas (Andersen and Sollid, 1971), we assume Hardangerjøkulen to be hard-bedded, i.e. without any deformable subglacial sediments present. Given its climatic setting and ~~judging from~~ based on the radar investigations ~~mentioned~~ described in Sect. 2.1.2, Hardangerjøkulen can be characterized as a temperate ice cap. ~~Based on temperature measurements , Midtdalsbreen has been suggested to be~~ To the contrary, temperature measurements suggest that Midtdalsbreen has local cold-based ~~in its lowermost parts (Hagen, 1978; Konnestad, 1996; Reinardy et al., 2013). We however~~ areas at its terminus (Hagen, 1978; Konnestad, 1996; Reinardy et al., However, we expect that this has a minor effect on ~~ice flow for~~ the large scale ice flow of Midtdalsbreen and Hardangerjøkulen~~as a whole~~.

2.5.2 Surface velocities

~~For~~ Over the lower ablation zone of Midtdalsbreen, surface speeds of 4–40 m a^{-1} were measured during summer 2000 (Vaksdal, 2001). In addition, ice velocities ~~was~~ were derived from Global Positioning System (GPS) ~~unit recordings~~ units recording at nine locations on Hardangerjøkulen during the period May 2005–September 2007 (Giesen, 2009). One GPS was mounted on the automatic weather station (AWS) on Midtdalsbreen, the other eight were situated on stakes at the ELA of the main outlet glaciers (Fig. 1). These data show highest velocities for the largest outlet glacier Rembesdalskåka (46 m a^{-1}). Velocities at Midtdalsbreen~~(~~, measured May 2005 to March 2006, were 33 m a^{-1} at the ELA and ∼20–22 m a^{-1} at the AWS~~) are~~ , which is within the range of ablation zone summer velocities suggested by Vaksdal (2001).

Since velocities have only been measured for single years or shorter, these observations provide guidance rather than serving as calibration or validation data for our model. To the authors' knowledge, there are no high resolution velocity data derived from remote sensing ~~platforms~~covering the area of interest.

3 Model description and setup

3.1 Ice flow model

We use the two-dimensional, vertically integrated Shallow Ice Approximation (SIA) within the finite-element Ice Sheet System Model (ISSM; Larour et al., 2012)~~, a finite element ice flow model primarily developed for high-resolution, higher-order modeling of ice sheets using a parallel software architecture. There are many capabilities and modules in ISSM ; only those~~ . Only the capabilities of ISSM relevant for this paper are covered here~~. For~~ , for a complete description, including a more comprehensive section on model numerics and architecture, we refer to Larour et al. (2012) and http://issm.jpl.nasa.gov.

~~We use the two-dimensional (2d), vertically integrated Shallow Ice Approximation (SIA) within ISSM, meaning horizontal velocities correspond to averaged velocities over the ice column.~~ The SIA is based on a scaling analysis of the full Stokes stress balance (Hutter, 1983; Morland, 1984). This scaling argument ~~carries the assumption that~~ assumes that the typical glacier length L, is much larger than the typical ice thickness H. For this purpose, the aspect-ratio ϵ is defined as

$$\epsilon = \cancel{\frac{H}{L}}\frac{[H]}{[L]}, \tag{1}$$

where ϵ describes the 'shallowness' of an ice mass. An aspect-ratio much smaller than unity is required for the SIA to be valid. Generally, the smaller the ϵ, the more accurate the SIA is ~~(Le Meur et al., 2004; Winkelmann et al., 2011). For Hardangerjøkulen~~(Le Meur et al., 2004; Greve and Blatter, 2009; Winkelmann et al., 2011). Based on outlet glacier length records from the LIA until today, the characteristic horizontal scale for Hardangerjøkulen is 4 to ~~8 km, and the characteristic ice thickness~~ 10 km. Due to the highly variable bed topography, a typical vertical scale of ~200 m ~~, giving~~ is estimated qualitatively using ice thickness around the ELA. These scales give an ϵ ~~of~~ between 0.02 and 0.05~~to 0.025~~, which is acceptable for using the SIA (Le Meur and Vincent, 2003).

~~Where bed and surface topography is complex, lateral drag and longitudinal stress gradients may become important. Still, the~~ The SIA has proven accurate in representing glacier length and volume fluctuations on the decadal and longer time scales ~~(Leysinger-Vieli and Gudmundsson, 2004).~~

we are focusing on (Leysinger-Vieli and Gudmundsson, 2004). While higher order models may be needed in dynamic regions, even for paleosimulations (Kirchner et al., 2016), Hardangerjøkulen has relatively gentle surface slopes and lacks areas of very fast flow~~. The SIA is therefore a viable option when studying this ice cap on climatic time scales. Similar studies on Hardangerjøkulen (Giesen and Oerlemans, 2010), ice caps in Iceland (Guðmundsson et al., 2009; Adalgeirsdóttir et al., 2011), and glaciers in the French Alps (Le Meur et al., 2007) indicate that employing SIA models on alpine glaciers and small ice caps on climatic time scales gives satisfactory results.~~ , making the SIA a viable choice. Because of its simplicity, SIA is also computationally efficient (Rutt et al., 2009)~~. This enables modeling of ice cap evolution over long time scalesand for a wide parameter space, both key aims of this study~~, enabling ensemble simulations over longer time scales.

3.1.1 Ice deformation and sliding

The constitutive relationship relating stress ~~τ~~ to ice deformation (strain rate) is Glen's flow law (Glen, 1955), which for the special case of vertical shear stress τ_{xz} only (SIA) ~~is~~ states

$$\dot{\epsilon} = A{\tau_{xz}}^n, \tag{2}$$

where $\dot{\epsilon}$ is the strain rate tensor, A is ~~a temperature dependent~~ the flow factor accounting for ice rheology and $n = 3$ is Glen's flow law exponent. We use a spatially constant flow factor A, assuming homogeneous ice temperature T_{ice} and material properties across the ice cap.

In contrast to many other studies, where a tuned 'best-fit' parameter combination is ~~used throughout~~ selected and used in all simulations, we perform ensemble runs for a parameter space of different flow factors and sliding parameters (described below), for both the calibration procedure and subsequent model runs.

~~Consistent with SIA theory, vertically averaged ice deformational velocities $\bar{u}_d$ are calculated by~~

$$\bar{u}_d = \frac{2AH}{n+2}\tau_d^n$$

~~Since the SIA assumes driving stress τ_d to be equal to the basal shear stress τ_b, the basal shear stress τ_b can be written as~~

$$\boldsymbol{\tau_b} = \boldsymbol{\tau_d} = \rho_i g H \nabla s$$

~~where ρ_i is the density of ice, g the gravitational acceleration and ∇s the surface slope.~~

SIA is strictly only valid for a no-slip bed (Gudmundsson, 2003; Hindmarsh, 2004). However, Hardangerjøkulen is a temperate ice cap, and summer speed-ups have been observed at Midtdalsbreen (Willis, 1995; Willis et al., 2012), indicating basal motion.

~~Several previous studies (e.g. Ritz et al., 1996; Payne et al., 2000; Rutt et al., 2009) employing the SIA use~~ We introduce sliding using a linear Weertman sliding formulation (Weertman, 1964), ~~where basal velocities are a function of the basal shear stress,~~ which for the SIA ~~is equivalent to setting~~ means basal velocities u_b are proportional to the ~~driving stress~~ basal shear stress τ_b:

$$u_b = \beta\tau_b^m, \tag{3}$$

where β is a (tuning) basal sliding parameter. β can be set spatially and temporally constant, or be a function of temperature, basal water depth, basal water pressure, bed roughness or other factors, and m is the sliding law exponent, which equals one for the linear sliding law we apply. ~~The basal velocity u_b is added to the deformational velocity, so that total vertically averaged velocity becomes $\bar{u} = \bar{u}_d + u_b$.~~

In this study, the basal sliding parameter β is assumed spatially and temporally constant. In reality, sliding likely varies ~~both~~ in space and time ~~in accordance with varying basal hydrology, bed roughness and material properties~~according aforementioned factors. However, we consider it ~~too~~ speculative to apply *ad-hoc* variations in basal sliding without proper validation. ISSM has capabilities to perform inversions for basal friction based on data assimilation techniques (e.g. MacAyeal, 1993; Morlighem et al., 2010), but this requires more extensive velocity data coverage than what is available for Hardangerjøkulen at present.

3.1.2 Mass transport

For the vertically-integrated ice flow model used in this study, the two-dimensional continuity equation states

$$\frac{\partial H}{\partial t} = -\nabla \cdot (\cancel{uH}\bar{u}H) + \dot{M}, \tag{4}$$

where ~~u~~ $\bar{u}$ is the vertically averaged ice velocity (m a^{-1}) and $\dot{M}$ the surface mass balance rate (m ice equivalent a^{-1}). The basal melt rate is assumed negligible, and calving is not included in the model. Rembesdalskåka likely terminated in lake Rembesdalsvatnet during the LIA and the northwestern ice cap presently terminates in water, however we expect this to have minor effect on ice dynamics.

3.1.3 Mesh and time stepping

Following methods outlined in Hecht (2006) and Morlighem et al. (2011), an anisotropic mesh with resolution 200-500 m was constructed using local mesh refinement based on modelled velocities for a steady-state ice cap close to observed LIA extent. This ice cap was reached using our 'best-fit' deformation and sliding parameters (Sect. 3.2.1) on a uniform mesh, and a mass balance perturbation forcing the ice cap to advance to terminus positions close to the LIA extent. The anisotropic mesh adds accuracy around the LIA margins. When the glacier is smaller or larger, the accuracy is reduced (400-500 m).

~~Even though our surface digital elevation model (DEM)has higher resolution than this (100 m), we choose the highest mesh resolution to be~~ The stress balance of SIA is local. Using a very high resolution for SIA hence increases the risk of unphysical stress gradients and velocities due to local variations in bed topography. We avoid this by smoothing the surface and bedrock DEM's to 200 m~~, since this is more in line with the assumptions of the SIA. It~~. This mesh resolution also enables us to carry out Holocene runs and our ensemble study at lower computational cost. Tests on mesh convergence using uniform 150 m and 200 m meshes indicate that total volume varies by less than 5 % compared to our anisotropic 200-500 m mesh.

We use a finite difference scheme in time, where a time step of 0.02 years was found low enough to avoid numerical instabilities.

3.2 Experimental setup and calibration

3.2.1 ~~Mass balance forcing~~

~~A vertical reference mass balance gradient B_{ref} is derived from observed specific mass balance gradients, which exist for 35 of the 45 years spanning 1963–2007 (Fig. 2). Mass balance $B(z,t)$ for any point in time is calculated by shifting B_{ref} by a~~

~~mass balance anomaly $\Delta B(t)$ at all elevations (Oerlemans, 1997a):~~

$$\cancel{B(z,t) = B_{ref}(z) + \Delta B(t)}$$

~~B_{ref} is defined to represent zero mass balance for the present-day surface topography. The measured mean net balance for Rembesdalskåka was -0.175 m w.e., and B_{ref} is therefore derived by shifting the mean observed gradient by +0.175 m w.e. (Giesen, 2009).~~

~~A mass balance-altitude feedback is included in the model by recalculating the mass balance $B(z,t)$ at a specific point for each time step according to the updated surface elevation. The elevation of the maximum net mass balance is not adapted to changes in the ice cap summit elevation, as the effect on modelled ice volume is minor (Giesen, 2009).~~

3.2.1 Ensemble calibration of ice deformation and sliding parameters

To calibrate model parameters governing ice deformation and basal sliding, we use the 1995 surface DEM as the initial condition. ~~This topography is based on aerial photographs taken by the Norwegian Mapping Authority (Statens Kartverk), and corresponds to the period when most of the bed topography was mapped.~~

We run the model with constant climate forcing, using our reference mass balance gradient (~~i.e. with~~ $\Delta B(t)$ = 0 in Eq. 6 below), until a steady-state is reached.

Since we run the model with a mass balance gradient averaged over several decades, it is important that there was no large climate-geometry imbalance for this period. Indeed, the ice cap was in close to steady-state between the early 1960s and 1995, since surface elevation change from 1961 to 1995 was ± 10 m (Andreassen and Elvehøy, 2001).

In reality, an ice cap is never in exact steady-state, but it is still a useful concept to understand model sensitivity (Adalgeirsdóttir et al., 2011). To investigate model sensitivity to deformation and sliding parameters, and to find a 'best-fit' combination for our historic runs, we run an ensemble of 24 possible parameter combinations, well enclosed by values used in the literature. ~~For the~~ The flow factor A ~~,~~ depends on ice temperature, as well on ice fabric, impurities and possibly other factors. Without an *a priori* assumption of ice temperature, we investigate values from $A = 0.95 \times 10^{-24}$ to 2.4×10^{-24} s^{-1} Pa^{-3}, ~~corresponding~~ roughly corresponding to $T_{ice} = 0$ to ~~ice temperatures of $T = 0$ to~~ -5 °C (Cuffey and Paterson, 2010, p.73). For the sliding parameter, we perform runs using β = 4 × 10^{-12} to 1 ×10^{-13} m ~~a~~s^{-1}Pa^{-1}.

The 'best-fit' combination is obtained by minimizing the Root Mean Square Error (RMSE) between the modelled (H_{mod}) and observed (H_{obs}) ice thickness:

$$RMSE = \cancel{\sqrt{\frac{\sum_{i=1}^{k}(H_{mod}-H_{obs})^2}{k}}}\sqrt{\frac{\sum_{i=1}^{k}(H_{mod}-H_{obs})^2,}{k}} \tag{5}$$

where k is the number of vertices for which the RMSE is calculated.

Since the outlet glaciers Midtdalsbreen and Rembesdalskåka are of primary interest, we use the combined RMSE along their flowlines as the most important metric (Fig. 3). As an additional check, we also calculate the RMSE for ice thickness over the entire ice cap (not shown here). ~~Based on Fig. 3, we~~ We consider our 'best-fit' parameter combination to be $A = 2.0315\times10^{-24}$ s^{-1} Pa^{-3} ~~($T_{ice} = -1$°C)~~ and $\beta = 2\times10^{-12}$ m ~~a~~s^{-1}Pa^{-1} ~~, though several parameter combinations produce similar RMSEs, as further discussed in Sect.5.1.~~ (Fig. 3).

3.2.2 ~~Holocene~~

3.2.2 Mass balance parametrization

A vertical reference mass balance gradient B_{ref} is derived from observed specific mass balance gradients, which exist for 35 of the 45 years spanning 1963–2007 (Fig. 2). Mass balance $B(z,t)$ for any point in time is calculated by shifting B_{ref} by a mass balance anomaly $\Delta B(t)$ at all elevations (Oerlemans, 1997a):

$$B(z,t) = B_{ref}(z) + \Delta B(t). \tag{6}$$

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}.

A mass balance-altitude feedback is included in the model by recalculating the mass balance $B(z,t)$ at a specific point for each time step according to the updated surface elevation. The elevation of the maximum net mass balance is not adapted to changes in the ice cap summit elevation, as the effect on modelled ice volume is minor (Giesen, 2009).

3.2.3 Holocene mass balance

~~Lake sediment studies by Dahl and Nesje (1994)~~ Reconstructions (Sect. 2.2.2) suggest that Hardangerjøkulen has been continuously present since c. 3800 BP, with ~~some small~~ smaller local glacier activity during the millennium before. We therefore choose 4000 BP, with no ice cap present, as the starting point for our simulations.

~~Dahl and Nesje (1996) reconstructed summer temperature based on former pine-tree limits from southern Norway, as well as winter precipitation for the Hardangerjøkulen area based on lake sediment-derived ELAs and a well-established empirical relationship between winter precipitation and ELAs for Norwegian glaciers (Liestøl in Sissons, 1979; Sutherland, 1984). These reconstructions suggest a close to linear cooling and wetting trend from~~ Temperature proxies indicate a positive mass balance anomaly at 4000 BP~~until the LIA, with some fluctuations superimposed (Velle et al., 2005a).~~

~~At 4000 BP, reconstructions (Dahl and Nesje, 1996; Velle et al., 2005b; Seppä et al., 2005) suggest temperatures 0.5 °C lower than present (favorable for glacier growth), and a drier climate (unfavorable for glacier growth). The combined effect implies a mass balance similar to present.~~

~~Based on this~~, while precipitation reconstructions point to more negative mass balances (Sect. 2.2.2). Combined, these suggest mass balance conditions similar to present-day. Accordingly, we start from $\Delta B(t)$ = 0 and thereafter linearly increase mass balance to 0.4 m w.e. over the period 4000 BP to 400 BP (1600 AD). The final value of 0.4 m w.e. is chosen to produce an ice cap sized between the present-day and LIA extent. For this simulation, we use our 'best-fit' deformation and sliding parameters obtained from the calibration ensemble.

It is possible to refine or alternate this simple forcing in several ways. However, applying such changes based on poorly ~~known~~ constrained past climatic and mass balance conditions adds additional uncertainty~~and unnecessary complexity.~~. Our deliberately simple, linear forcing also allows us to isolate any non-linear, asynchronous behaviour in a clear manner.

3.2.4 ~~Little Ice Age until present-day~~Historic mass balance

Using our Holocene run ending at 1600 AD as initial conditions, we aim to reproduce the history of Hardangerjøkulen from the LIA until present-day, as well as to assess model sensitivity to choice of deformation and sliding parameters. For these purposes, we run the same parameter ensemble as used in the calibration process.

Since the mass balance record from Rembesdalskåka starts in 1963, mass balance has to be reconstructed for the period prior to this. A plausible mass balance history is ~~therefore~~ found from 1600 AD, through the LIA maximum in 1750 up to 1963, using a dynamic calibration (Oerlemans, 1997a, 2001). This approach is based on matching the model against the moraine evidence and length records of the outlet glaciers Midtdalsbreen and Rembesdalskåka, while adjusting $\Delta B(t)$ accordingly. ~~As a starting point, the~~ We use a slightly modified mass balance history as obtained for Hardangerjøkulen by Giesen (2009)~~is tested. This history is then modified slightly to match the outlet glacier length records. However, we employ minimal tuningof the mass balance history~~, using minimal tuning, since a key aim is to investigate parameter sensitivity, and mass balance is arbitrary before 1963.

~~As a further constraint, modelled ice cap extents are compared with known past extents from maps and aerial photographs from the 1900s, and the modelled ice cap surface is compared with the 1995 DEM. The sliding parameter β might change over time, since basal velocities may change with changes in surface melt, subglacial hydrology, basal roughness and other factors. However, we keep our ensemble intact in both space in time , avoiding *ad-hoc* changes without physical foundation and validation, as explained in Sect. 3.1.1.~~

3.2.5 Mass balance sensitivity and hysteresis

To investigate the sensitivity of present-day Hardangerjøkulen to changes in mass balance, steady-state experiments are performed with present-day ice cap topography as the starting point. These experiments are performed starting from the steady-state ice cap obtained with the 'best-fit' parameters and no mass balance anomaly. From this state, we perturb the mass balance by anomalies between -0.5 and +0.5 m w.e., and run the model to a new equilibrium.

To investigate the role of the mass balance-altitude feedback in the ice cap response, we perform additional experiments excluding this feedback by keeping the spatial mass balance field fixed in time to the present-day surface topography.

Finally, we investigate dependence on initial conditions (hysteresis), by running experiments using ice-free initial conditions, with the mass balance-altitude feedback included.

4 Results

4.1 Mid- to late Holocene evolution of Hardangerjøkulen

~~Starting from a present-day reference mass balance forcing at 4000 BP, we use our simple yet empirically based, linearly increasing mass balance (Fig. ??a) from 4000 BP (~~ Using a linear mass balance increase from 0 m w.e. ~~anomaly relative to present-day) to 1600 AD (~~at 4000 BP to 0.4 m w.e. ~~). We demonstrate that the~~ at 1600 AD (Fig. 4a), we find an ice volume evolution for Hardangerjøkulen during the mid- to late Holocene ~~was~~ that is far from linear and ~~differed~~ different between outlet glaciers (Fig. ~~??~~4c). Starting from ice-free conditions, ice cap volume increases ~~rapidly during the first ∼200 years (4000–3800 BP), then close to linearly between c. 3800–2300 BP (period A–B). Subsequently, starting at around 2300 BP,~~ in a step-wise manner, with Hardangerjøkulen ~~triples~~ tripling its volume over a period of 1000 years (~~B–C~~c. 2300–1300 BP), before stabilizing at the end of the period~~(C–D)~~.

~~Snapshots at times A–D~~ Simulated snapshot thickness maps reveal patterns of ice cap growth (Fig. 5). Initially, ice grows on high bedrock ridges above the ELA (Fig. 5a, also see Fig. 1). During the period of linearly increasing ice volume (~~A–B~~4000–3800 BP), Rembesdalskåka and Midtdalsbreen advance at similar rates. At this stage, Rembesdalskåka occupies an area with a gently sloping and partly overdeepened bed (Fig. 6).

After passing the lower edge of this overdeepening, Rembesdalskåka advances ∼3.5 km in 400 years (2300–1900 BP), corresponding to a length increase of 60 % (Fig. 6). In contrast, Midtdalsbreen is already at an advanced position in 2300 BP, and changes only modestly during this period.

Ice volume grows rapidly ~~during~~ from 2300–1900 BP, however the advance and thickening of Rembesdalskåka ~~can not alone~~ alone can not explain this ice volume increase. Rather, the bulk of Hardangerjøkulen's volume increase during this period is due to ice cap growth in the east and southeast, where deep bedrock basins are filled with ice up to 400 m thick (Fig. 5d, see also Fig. 1).

We tested alternative mass balance forcings (faster rate of linear increase, and constant mass balance equal to the final value), and found the spatial pattern of ice cap growth ~~robust for different forcings~~to be robust.

At the end of the spinup period (c. 1300–400 BP), outlet glaciers stabilize their frontal positions, and ice volume increase flattens out.

4.2 Hardangerjøkulen since the Little Ice Age

4.2.1 Parameter ensemble

~~The modelled state in 1600 AD is reached using our 'best-fit' parameter combination from the Holocene run.~~ From 1600 AD, we ~~run the model with~~ continue the Holocene run using our ensemble of sliding and deformation parameter combinations, for one specific mass balance history.

The ensemble modelled ice volumes at the LIA maximum (1750 AD) range from c. 12.7 to 17.4 km^3, and vary between 6.9 and 13.4 km^3 for the present-day (2008 AD; Fig. ~~??~~4d).

4.2.2 ~~Simulation using 'best-fit' parameters~~

~~Within the ensemble, the closest match with observed ice volume~~ Parameter combinations including rate factors $A(T=-1$ °C) all give results $\pm$ 10 % from the observed ice volumes in 1961 and 1995. Using faster sliding and stiffer ice, or vice versa, it is possible to get close to the observed ice volume also for other rate factors. However, only using ice volume for validation is not sufficient. A simulated ice volume close to observations does not imply accurate ice extent and surface topography. The $\sim$100 m spread in estimated surface elevation for the ice cap interior in 1995 ~~(black dots in Fig. ??d) is obtained by our independently calibrated 'best-fit' parameter combination. Modeled and observed ice volume in 1961 and 1995 differ by 0.10 and 0.22 km^3, respectively, or 1.1 and 2.3 % of total observed ice volume, respectively~~(Fig. 7), illustrates the impact of parameter uncertainty on the dynamics and hence ice cap hypsometry.

4.2.2 Simulation using 'best-fit' parameters

The LIA maximum ice volume using the 'best-fit' parameter combination is modelled to 14.8 km^3 (Fig. ~~??~~4d). ~~The simulation shows that Hardangerjøkulen has lost one-third of its volume between 1750 and present-day.~~

~~The simulated continuous ice volume history of Hardangerjøkulen, from 4000 BP through the LIA until today, including our ensemble from 1600 AD onwards, is shown in its entirety in Fig. ??cd.~~

~~The mass balance forcing used in Fig. ?? is derived by matching modelled and observed length variations of the outlet glaciers Midtdalsbreen (NE) and Rembesdalskåka (SW). For the LIA maximum, it is~~ It is not possible to obtain correspondence to observed lengths for both outlet glaciers simultaneously, not even by altering the dynamical parameters (Fig. 7). The mass balance history giving optimal results for Midtdalsbreen was chosen since its LIA maximum extent has been dated to 1750 AD, while no dates exist for Rembesdalskåka. In addition, bed topography is more ~~accurately known for Midtdalsbreenthan for Rembesdalskåka. When the model is calibrated against Midtdalsbreen's front variations~~accurate for Midtdalsbreen. Using this setup, the LIA maximum length agrees reasonably well with moraine evidence, whereas Rembesdalskåka is too short (Fig. 8).

~~Both modelled outlet glaciers are too short during the early 1900s, but the difference for Midtdalsbreen is only slightly larger than the model resolution (200 m). After 1960, the model-observation match is good, with differences being within the size of one mesh element (Figs. 7 and 8).~~

Consistent with the results for Midtdalsbreen and Rembesdalskåka, the lengths of the southwestern outlet glaciers at the LIA maximum are underestimated in the model (Fig. 9a)~~. The~~, while the extent of the northeastern outlet glaciers agrees well with moraine evidence.

~~While the outlet glaciers~~ During the early 1900s, outlet glacier lengths are too short ~~around 1930~~ (Fig. 8 and 9b), ~~the~~ but the difference for Midtdalsbreen is only slightly larger than the model resolution (200 m). The ice cap margin after 1960 is reproduced with a high degree of detail (Fig. 9cd). Most, but not all discrepancies are close to the model resolution. One exception is the too small northwestern ice cap~~, however~~. However, ice thickness in the missing area is small ($<$ 50 m), so this mismatch contributes little in terms of total ice volume.

The closest match with observed ice volume in 1961 and 1995 (Fig. 4d) within our ensemble is by the 'best-fit' parameter combination obtained from calibration (Fig. 3). Modeled and observed ice volume for these years differ by 0.10 and 0.22 km^3, respectively, or 1.1 and 2.3 % of total observed ice volume, respectively. Modelled thickness in 1995 is generally in good agreement with the data, though the ice cap interior is somewhat too thin and the thickness along the eastern margin is overestimated (Fig. 9e).

The simulated continuous ice volume history of Hardangerjøkulen, from 4000 BP through the LIA until today, including our ensemble from 1600 AD onwards, is shown in its entirety in Fig. 4cd. The simulations show that Hardangerjøkulen has lost one-third of its volume between 1750 and present-day.

5 ~~Discussion~~

4.1 ~~Sensitivity to sliding and deformation parameters~~

~~Running our parameter calibration ensemble , we aim to minimize the Root Mean Square Error (RMSE) between observed and modelled present-day surface topography, yet several parameter combinations give similar RMSEs~~

4.1 Mass balance sensitivity and hysteresis

We find that Hardangerjøkulen at present is exceptionally sensitive to mass balance changes (Figs. 10 and 11a). In particular, the ice cap is bound to disappear almost entirely for mass balance anomalies of -0.2 m w.e. or lower. Our parameter ensemble suggests a disappearance for anomalies between -0.5 to -0.1 m w.e., though this range is likely smaller as explained in Sect. 4.2.1. Our simulations show a close to linear relationship between positive mass balance perturbations and ice volume response (Fig. ~~3). This is not surprising, since the parameters for ice deformation (A) and sliding (β)both depend on driving stress (Flowers et al., 2008; Zekollari et al., 2013). This highlights the challenge of picking a 'dynamically ideal' or even unique combination without empirical knowledge about their relative importance, as noted by previous studies (Le Meur and Vincent, 2003; Adalg Given such ambiguities, and the fact that the impact of the deformation and sliding parameters may differ for varying mass balance regimes, we choose to keep our parameter ensemble intact from calibration to our historic runs, where we assess parameter effects on transient behavior from~~ 10), while the ice cap melts away partly or completely for the negative anomalies.

Further experiments show that the mass balance-altitude feedback is vital in explaining Hardangerjøkulen's high sensitivity to climate change. Without the feedback, the ~~LIA until today~~ice cap responds close to linearly to mass balance perturbations and thus is far less sensitive to climate change (Fig. 10b). ~~Within this ensemble, we investigate one 'best-fit' parameter combination (Table 1) in more detail.~~ For example, half of present-day ice volume (4.9 km^3) is still present for a mass balance anomaly of -0.5 m w.e., while with +0.5 m w.e., ice volume increases by ~35 %. In stark contrast, when including the feedback, the ice cap disappears completely for the corresponding negative anomaly, and ice volume almost doubles (+92 %) for the positive anomaly (Fig. 10a).

~~Based on calibration (Fig. 3), model-observation misfit is sensitive to the choice of A when using β values corresponding to little sliding. Conversely, sensitivity to ice deformation is lower for faster sliding. With the lack of comprehensive observed velocities for validation, it is challenging to judge what range of the ensemble is more 'likely' or 'realistic', though we consider deformation parameters corresponding to temperatures of -5 °C less likely, since we expect this rheology to be too stiff for a temperate~~ Starting from ice-free conditions and including the mass balance-altitude feedback, we find that the Hardangerjøkulen's climatic response depends on the ice cap's initial state. For mass balance anomalies close to our reference mass balance for 1963–2007, between -0.2 and +0.1 m w.e., large differences occur between ice volumes reached from present-day and ice-free conditions (Fig. 10). When starting from a situation without ice, present-day mass balance conditions produce an ice cap that has only 20% of the volume of today's ice cap. In addition to Hardangerjøkulen being bound to disappear almost completely for a slight decrease in the mass balance, this result implies that a positive mass balance anomaly is needed to regrow the ice cap to its present-day extent, once it has disappeared.

4.2 Volume-area phasing and scaling

Our Holocene simulation showed that the ice volume evolution for three of the outlet glaciers (Rembesdalskåka, Midtdalsbreen, Blåisen) is asynchronous (Fig. 12). Midtdalsbreen's ice volume increases linearly over time, while Rembesdalskåka and Blåisen have distinct jumps in ice volume, related to their bed topography. The importance of bedrock troughs and overdeepenings is further illustrated by Hardangerjøkulen's non-linear volume increase c. 2300–1300 BP, a period when volume increases faster than area (Fig. 12). During this period, ice is thickening rather than expanding horizontally, which can largely be explained by ice growth in subglacial valleys in the eastern and southeastern parts of the ice cap (Fig. 1). These bed depressions fill up quickly because ice flow converges into them from surrounding high bedrock ridges, and the mass balance-altitude feedback amplifies the ice thickening.

We compare our steady-state mass balance perturbation experiments (Sect. 4.1) with volume-area scaling relations for steady-state ice caps from the literature (Fig. 13a), of the form $V = cA^{\gamma}$ (Bahr et al., 1997). For a consistent comparison, we group our perturbation experiments into those which produce a fully developed ice cap, and those where ice is mainly present on high ridges, and thus cannot be classified as a glacier or ice cap. We ~~therefore exclude simulations using this temperature from our historic ensemble. The magnitudes of modelled velocities for several parameter combinations are similar to the observed velocities available (Sect. 2.5.2) , though there are too few measurements to constrain the parameters~~find that ice cap scaling relations from the literature overestimate the ice volume of the full-grown ice cap. Both the exponent and the scaling factor found for Hardangerjøkulen (γ = 1.3738 and c = 0.0227) are closer to literature values for valley glaciers (e.g Bahr et al., 2015).

~~The ensemble spread for ice volume in the historic run from LIA until today is large~~ During the first half of the Holocene simulation, a full ice cap has not developed, and volumes are up to 60 % smaller than ice volumes predicted from the volume-area relation derived from our steady-state experiments (Fig. ~~??). However, 22% of the ensemble spread for~~ 13b). Approaching the LIA and up to today, when Hardangerjøkulen has a more developed shape, our steady-state derived volume-area relation fits well with simulated volumes. We discuss these results and their implications in Sect. 5.5.

5 Discussion

5.1 Sensitivity to sliding and deformation parameters

Running our parameter calibration ensemble, we aim to minimize the Root Mean Square Error (RMSE) between observed and modelled present-day ~~(year 2008)can be attributed to an ice rheology corresponding to -3°C, which may not be soft enough~~ surface topography. Several parameter combinations give similar RMSEs (Fig. 3). Since both the rate factor (A) and sliding parameter (β) depend on driving stress (Flowers et al., 2008; Zekollari et al., 2013), one can keep the same surface velocities by reducing one parameter and increasing the other. Hence it is challenging to pick a unique combination without more empirical knowledge about their relative importance (Le Meur and Vincent, 2003; Adalgeirsdóttir et al., 2011; Zekollari et al., 2013). This underlines the motivation behind keeping our ensemble after the calibration. A comparison with an ice velocity map, which is not available for Hardangerjøkulen~~'s temperate ice~~ , would more strongly constrain A and β.

Notwithstanding data deficiencies, a notable finding is that the impact of A on ice volume is relatively small at calibration (Fig. 3), but large during our transient simulation over several centuries (Fig. 4d). This disparity suggests that small differences in model rheology at initialization can propagate significantly with time. This time-dependency has implications for other model studies of long-term dynamics of glaciers and ice caps. With growing availability of data, such studies may consider a 'dynamic' or 'transient' calibration (e.g. Oerlemans, 1997a; Davies et al., 2014; Goldberg et al., 2015), as opposed to a 'snapshot' calibration. The 'transient' method uses several sets of observations to infer model parameters, ideally at dynamically and climatically different states.

During the years ~~subsequent to~~ following 1600 AD, ~~after the change~~ when including the ensemble of dynamical parameters, the ice cap response is a combined effect of climate forcing and adjustment to new parameter values. ~~However, the~~ The period 1600–1710 AD can be viewed as ~~an additional~~ a short spinup phase for the historic simulation, ~~since we keep~~ where the mass balance is kept constant at the end value of the Holocene simulation ($\Delta B(t)$ = 0.4 m w.e.)~~during this period~~.

For the historic run, ~~we observe that~~ the ensemble spread in surface elevation is larger in the vicinity of the ELA than at the ~~periphery~~ margins (Fig. 7). ~~This phenomenon can be explained by the fact that a change in β or T_{ice} leads to either an increase in ice velocity (~~Recall that the continuity equation (Eq. 4) requires that thickness change occurs ($\frac{\partial H}{\partial t} \neq 0$) if ~~$\beta$ and/or T_{ice} increases) or a decrease in ice velocity (if β and /or T_{ice} decreases). When the velocity increases, it takes a shorter amount of time for the ice to flow from the summit to the ELA, and therefore the ice thickness at the ELA is smaller, since it has not spent as much~~ ice flow and mass balance are not balanced ($\nabla \cdot (\bar{u}H) \neq \dot{M}$). Therefore, softer ice or higher sliding cause ice thickness to decrease, meaning ice spends less time in the accumulation zone. ~~On the other hand, ice will also flow faster downstream and will therefore spend less time from the ELA to the terminus for the same ablation rate. Similarly, for slower velocities, we expect that ice is thicker at the ELA, but the deviation in ice thickness decreases as we reach the glacier terminus.~~ Similarly, faster flow downstream of the ELA also requires thinning. The insensitivity of the frontal positions is likely due to high ablation near the margins overwhelming other effects, and for 1995 also frontal positions pinned by bedrock topography.

~~In agreement, the ensemble spread for the front position itself is small. The latter is also due to a combination of a highly negative mass balance at this elevation, and ice not flowing fast enough (in the order of 50 m a^{-1} or less) to replace the mass lost, preventing the front from moving to the next mesh node (which lies 200–300 m ahead).~~

~~A future expansion of this work, outside the scope of this study, would be a multiple regression of the dynamical parameters for Hardangerjøkulen and its outlet glaciers. This could disentangle whether their importance changes over time, for example depending on mass balance regime or whether the glacier is retreating or advancing.~~

~~By imposing changes in dynamical parameters and exploring their model sensitivities, we can estimate how dynamical changes affect ice masses. For example, ice may go from cold- to warm-based or vice versa, or transient changes in basal conditions may change sliding speeds. Given recent advances in data assimilation, including methods to estimate basal slipperiness for present-day ice sheets (e.g. Morlighem et al., 2010), it would be interesting to see how stable the obtained friction maps are through time. Such issues should be of concern for model studies of future as well as past ice sheet behavior.~~

5.2 ~~Sliding in previous studies~~

~~Many previous studies (e.g. Payne, 1995; Ritz et al., 1996; Payne, 1999; Payne et al., 2000; Flowers et al., 2008; Le Brocq et al., 2009; Gi the SIA have used a spatially and temporally fixed sliding parameter in a Weertman-type sliding law (Weertman, 1957).~~

~~Le Brocq et al. (2009) found that the sliding parameter β for West Antarctica varied over five orders of magnitude in response to available water, ranging from 1×10^{-5} to 1×10^{-1} m a^{-1}Pa^{-1}. In our study, the 'best-fit' β in the ensemble is 6.3$\times 10^{-5}$ m a^{-1}Pa^{-1}, while the ensemble ranges from 3.16 $\times 10^{-4}$ to 1.26 $\times 10^{-5}$ m a^{-1}Pa^{-1}. The sliding parameter we use for Hardangerjøkulen is thus in the lower range of what Le Brocq et al. (2009) suggested, corresponding to areas away from ice streams, which is the type of environment we would expect most similar to Hardangerjøkulen. Compared to the values Payne (1995) and Payne (1999) used to model ice sheets, the Hardangerjøkulen ensemble values are 1-2 orders of magnitude lower (i.e. less slippery). However their deformation parameters A are 1-2 orders of magnitudes higher than ours (i.e. softer), and since both the sliding and deformation relation used are linear with respect to velocities, the combined effect is similar.~~

Flowers et al. (2008) simulated Holocene behavior of the Langjökull ice cap on Iceland using β = 2.5 $\times 10^{-4}$ m a^{-1}Pa^{-1}, which is within our ensemble range. Somewhat in contrast to this study, they noted a low sensitivity to β. ~~They attributed this insensitivity to the lack of a seasonally driven surface velocity cycle. Conversely~~However, seasonal speed-ups are absent at Langjökull while they have been observed at Hardangerjøkulen (Willis, 1995; Willis et al., 2012)~~. It is therefore not surprising that Hardangerjøkueln is more sensitive than Langjökull to the choice of sliding parameter.~~

~~In contrast to this study, some previous studies of smaller ice masses have used a non-linear Weertman-type sliding (Le Meur and Vincent While a direct comparison of our sliding parameter β is not possible, they note that several combinations of their sliding and deformation parameter give similar results, in line what we find here, as discussed in Sect. 5.1.~~

~~Hubbard et al. (2006) used field evidence to constrain model experiments for the Last Glacial Maximum in Iceland, and~~ , which probably explains the differing sensitivities. In line with our study, Hubbard et al. (2006) obtained a shallow, dynamic ~~ice sheet~~Icelandic ice sheet at the Last Glacial Maximum, associated with high sliding. ~~Using similar methods for the~~ Similarly, Golledge et al. (2008) obtained a thin, more extensive Younger Dryas ice sheet in Scotland ~~, Golledge et al. (2008) noted subtle~~

~~but consistent patterns when varying sliding values. Specifically, their modelled ice sheet became thinner but more extensive~~ with increased sliding~~, consistent with Hubbard et al. (2006)'s and our findings. Thus, for whatever the cause, high sliding seems to be associated with, perhaps sometimes a prerequisite for~~. As also explained above from a theoretical perspective (mass continuity), a shallow geometry is associated with high sliding.

~~It is possible to adjust the basal sliding parameter over time~~

A future expansion of this work would be a multiple regression of the dynamical parameters for Hardangerjøkulen and its outlet glaciers. This could disentangle whether their importance changes over time, for example depending on mass balance regime or whether the glacier is retreating or advancing. However, ~~in the *ad-hoc* formulation used, factors like surface meltwater supply, thermal regime, bed roughness, and the type of drainage system are lumped together in the parametrization and may change differently over time. We therefore consider transient adaptation of basal slipperiness in this study speculative rather than insightful. Alternative approaches to our sliding formulation include a non-linear Weertman sliding law (e.g. Pattyn, 2002; Le Meur an effective pressure-dependent sliding (e.g. Schoof, 2005; Tsai et al., 2015), or relating β to surface melt or another climate variable (Greve and Otsu, 2007; Clason et al., 2014).~~

the available (velocity) data are not sufficient to constrain the dynamic parameters to a narrower range, thus more data would be needed to make such an analysis insightful. Better knowledge of the bed properties at Hardangerjøkulen by means of radar, seismics or borehole studies, along with modeling of the subglacial drainage system, would also be steps toward understanding the (transient) behavior of basal slipperiness.

5.2 ~~Uncertainties in mass~~ Mass balance parametrization

~~For the LIA maximum, the terminal moraine at Midtdalsbreen is dated to 1750 AD, while the moraine at Rembesdalsk~~ We deliberately chose to use a simple mass balance formulation, to focus on first order ice dynamical responses to spatially homogeneous changes in the forcing. The evolution of Hardangerj~~å~~ø~~ka is not dated, but assumed to be formed at the same time. The true maximum for Rembesdalskåka may however have had a different timing, though the model resolution is probably too coarse to investigate details of such asynchronous advances, let alone the uncertainty in the climatic forcing.~~

~~The challenge to accurately model Rembesdalskåka and Midtdalsbreen simultaneously during the LIA (Fig. 7) may be related to the mass balance formulation used . As implemented here, mass balance is only a function of elevation.~~ kulen through the 20th century has been simulated by Giesen (2009) using the simple mass balance profile used here, as well as with a spatially distributed mass and energy balance model (Giesen and Oerlemans, 2010). Differences in ice volume and outlet glacier lengths produced with the two mass balance configurations were present, but small, justifying the use of the simple mass balance profile. In this section, we discuss some of the results presented in Giesen (2009) and Giesen and Oerlemans (2010) that are relevant for our study.

Similar to the present study, Giesen and Oerlemans (2010) was not able to match both the modelled lengths of Rembesdalskåka ~~is facing the prevailing westerly wind direction and is expected to receive more snow than Midtdalsbreen~~ and Midtdalsbreen with modern observations. Since they used a sophisticated mass balance model including an albedo scheme, a spatial precipitation gradient, and aspect and shading effects on insolation, this suggests that the mismatch should not be attributed to the mass balance forcing, but to other factors. ~~To account for a west-east gradient in winter accumulation, a potential improvement would be to let mass balance vary spatially.~~

~~Unfortunately, the~~ The two single years (2001-02; Krantz, 2002) with mass balance measurements on Midtdalsbreen are not enough to systematically assess differences in the mass balance regimes of Rembesdalskåka and Midtdalsbreen~~, though~~. Nonetheless, differing mass balance regimes ~~are suggested by Andreassen and Elvehøy (2001), who calculated surface elevation change~~ were suggested based on surface elevation changes from 1961 to ~~1995. West-east mass balance gradients have also been proposed~~ 1995 (Andreassen and Elvehøy, 2001), and also served as an explanation for differing glacier reconstructions between the southwestern ~~margin (Nesje et al., 1994) and the northeastern margin~~ and northeastern margins of the ice cap ~~(Dahl and Nesje, 1994). Further glacier~~ (Dahl and Nesje, 1994; Nesje et al., 1994). Coupled glacier and precipitation reconstructions based on multiproxy approaches on lacustrine sediments (e.g. Vasskog et al., 2012) could give more insight into differing continentality of the outlet glaciers of Hardangerjøkulen.

~~A horizontal precipitation gradient at Hardangerjøkulen was assumed by Giesen and Oerlemans (2010). This effect was added artificially based on meteorological station data on respective sides of the ice cap rather than from *in situ* measurements of mass balance. Quantification of the spatial variability of accumulation through further snow~~ Snow and mass balance ~~studies would~~ field studies covering the entire ice cap would also be valuable to better understand the ~~climatic response of Hardangerjøkulen~~spatial mass balance variability.

~~Besides imposing horizontal mass balance gradients,~~ Apart from spatial variations in the mass balance profile, temporal changes in climate or ice cap geometry may affect the mass balance ~~maximum in the vertical profile (∼1775 m a. s.l.) can been adapted in time as~~ gradient. For example, solar insolation patterns may change with strongly altered ice cap geometry~~changes. However, Giesen (2009) showed that temporally shifting the mass balance maximum according to summit elevation at~~, by shading effects of valley walls. However, Hardangerjøkulen ~~plays a minor role when mass balance is slightly positive, but gives a more sensitive ice capfor negative mass balances. Our approach with a static mass balance vertical profile should therefore be regarded as conservative when it comes to mass balance sensitivity. Moreover, we found that including a mass balance-altitude feedback was a crucial feature of the mass balance formulation (Sect. **??**), in agreement with Giesen (2009)~~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 mass balance 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. Even in a considerably warmer climate with a smaller ice cap, with continuously updated topographic effects on solar radiation, the mass balance gradient with elevation remained close to the present-day value. Furthermore, solar irradiance at 4000 BP, when we start our simulation, was at most 5% larger in the summer months than today (Giesen, 2009), and is therefore expected to have a minor effect on mass balance. In addition, Giesen and Oerlemans (2010) show that lowering the ice albedo from 0.35 to 0.20 under a realistic 21^{st} century scenario only leads to a 5 % larger volume decrease of the ice cap. We conclude that using a mass balance profile only dependent on elevation is a good approximation for Hardangerjøkulen, even in a different climate with a smaller or larger ice cap.

It is not clear why ~~observed~~ *observed* mass balance decreases at the uppermost elevations (Fig. 2), but a likely explanation is snow redistribution by wind. Effects of snow erosion and redeposition may be parametrized based on surface curvature, which is a good indicator of regions with wind-induced snow redistribution (Blöschl et al., 1991; Huss et al., 2008). Giesen (2009) tested a surface-curvature approach for Hardangerjøkulen, however the plateau was too flat for snow redistribution to occur in the model.

Glaciological measurements of mass balance have inherent uncertainties and biases, related to instrumentation, survey practices and techniques (Cogley et al., 2011). Andreassen et al. (2015) performed a reanalysis of glaciological and geodetic mass balance for Norwegian glaciers, including Rembesdalskåka. For the period ~~1995-2010~~1995–2010, they found a more negative geodetic mass balance (-0.45 m w.e.) than the glaciological one used in this study. ~~An~~ We performed an additional simulation with this more negative mass balance for the final years of our simulation (1995–2008) ~~shows~~ and found that the effect on ice volume is c. 0.5 km^3, or 5.3 % of modelled ice volume in year 2008.

~~Though the simple mass balance formulation used in this study works well, there are uncertainties associated with applying present-day observed mass balance profiles in different climates.It is reasonable for smaller ice cap changes, but may be less accurate when Hardjangerj~~

5.3 Mass balance sensitivity and hysteresis

Hardangerjøkulen is found to be particularly sensitive to mass balance changes: the ice cap disappears completely for the -0.5 m w.e. anomaly forcing, and almost doubles in volume for +0.5 m w.e. Similar experiments for Nigardsbreen, southwestern Norway (Oerlemans, 1997a), and Franz Josef Glacier, southwestern New Zealand (Oerlemans, 1997b) show much smaller responses (∼20-25 %). Our results are consistent with those of Giesen (2009), who also used a SIA model (Van Den Berg et al., 2008), but with different implementation of dynamical parameters and numerical methods.

Hardangerjøkulen's high sensitivity can be explained by its hypsometry and surface topography. Nesje et al. (2008a) noted that the difference between Hardangerjøkulen~~grows or shrinks considerably. Surface topography changes may affect accumulation patterns, especially if~~ 's ELA and maximum elevation is particularly small (∼180 m) compared to other glaciers and ice caps in Norway. Furthermore, the ice cap ~~splits into individual outlet glaciers, something not captured in our implementation.~~ is relatively flat with little area distribution in altitude. A comparison with Franz Josef Glacier, New Zealand (Woo and Fitzharris, 1992), Nigardsbreen, Norway (Oerlemans, 1997a), and Vatnajökull, Iceland (Aðalgeirsdóttir et al., 2003) confirms that Hardangerjøkulen's has the most extreme hypsometry (Fig. 14a). Furthermore, the present ELA is located close to the altitudes where area is large, resulting in an unusually vulnerable ice cap. For example, an ELA increase of 100 m at Hardangerjøkulen is equivalent to a 16.9 % decrease in area. Corresponding values for Nigardsbreen (9.9 %), Franz Josef Glacier (1.5 %) and Vatnajökull (6.1 %) are much smaller, confirming this explanation (Fig. 14b).

~~In a warmer climate, like that~~

The high sensitivity to mass balance changes found for Hardangerjøkulen supports abrupt changes inferred from lake sediment records for the Holocene for both the northern and southern side of the ice cap (Dahl and Nesje, 1994; Nesje et al., 1994). One example is the so called *Finse event*, when an advance to a maximum extent beyond that of present-day of the ~~mid-Holocene, the melt season will also be longer. This implies that surface albedo will be lower for alonger time period every year, because an earlier melt season onset will expose bare ice earlier in the summer (Oerlemans and Hoogendoorn, 1989). This is a positive~~

~~feedback, since lower albedo means that more melt occurs at a given temperature. Since our mass balance forcing is derived from~~ northern Blåisen outlet glacier $\sim$8300 BP was followed by a complete disappearance of this glacier within less than a century. Our results show that for a mass balance anomaly of -0.5 m w.e., the present-day ~~climate, our simulations may underestimate the climate sensitivity during the warm mid-Holocene and~~ ice cap disappears in $\sim$300 years. Depending on the ice cap volume at the Finse event, we find that an anomaly between -2.0 to 2.4 m w.e. melts away Hardangerjøkulen within a century. Nonetheless, the advanced ice cap at the ~~predicted warm future.~~ Finse event was likely not fully grown and in a steady-state, so an anomaly of $\sim$1.5 m w.e. is more likely.

~~Finally, solar insolation patterns may also change with strongly altered ice cap geometry, for example by shading effects of valley walls. Nevertheless, Giesen and Oerlemans (2010) accounted for longer melt seasons and solar insolation changes when applying a spatially distributed energy balance model until 2100 AD. They did not find very large changes in the mass balance gradient, indicating that the transferability of today's mass balance profile is robust on these time scales.Furthermore, solar irradiance at 4000 BP, when we start our simulation, was at most 5% larger in the summer months than today (Giesen, 2009), and will thus not have considerable effect on mass balance .~~

5.4 ~~Impact of ice dynamics~~

~~This study supports previous glacier modeling exercises (e.g. Le Meur et al., 2007; Guðmundsson et al., 2009; Giesen and Oerlemans, 201 as well as theoretical comparisons between SIA and Full Stokes (FS) models (Leysinger-Vieli and Gudmundsson, 2004; Hindmarsh, 2004; in that SIA is viable to use if interests are climatic rather than ice dynamics.~~

~~SIA inaccuracy is a candidate for explaining model-observation differences in regions with steeper bedrock slopes at~~ Given a mass balance sensitivity of around -0.9 m w.e. K^{-1} (Giesen and Oerlemans, 2010) and no change in precipitation, the air temperature increase responsible for the ice cap disappearance after the Finse event must have been at least 1.5 K. Reconstructed summer temperature after the Finse event suggest a sharp increase of 1.0-1.2 K (Dahl and Nesje, 1996). A 10 % precipitation decrease would compensate for this difference, since the sensitivity to precipitation for Hardangerjøkulen ~~, conditions where SIA has been shown to be inaccurate in idealized studies (Le Meur et al., 2004).Work by Hindmarsh (2004) and Gudmundsson (2008) using idealized glacier geometries showed that the SIA accurately represents large scale flow, in the absence of significant basal sliding. By investigating a small valley glacier in the Canadian Rocky Mountains and neglecting basal sliding, Adhikari and Marshall (2013) that SIA performs well in less 'dynamic' settings, while the resultscompared to HO/FS diverge for more 'dynamic' situations.~~ is around +0.3 m w.e. K^{-1} (Giesen and Oerlemans, 2010). Despite uncertainties in the reconstruction and model simulations, it is encouraging that both give consistent results, suggesting that ice flow models coupled with reconstructions may be used to constrain past climate conditions.

~~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. Moreover, bed topography data used in many studies are uncertain. Care should therefore be taken before drawing too many conclusions on SIA accuracy based on the bedrock slopes in different areas of~~ We can also view our results on mass balance sensitivity in light of future climate change. The mean mass balance in the last decade was -0.3 m w.e. Since Hardangerjøkulen ~~.~~

~~Given our interest in the climatic response, and the lack of fast-flowing areas on~~ was in approximate balance over the preceding decades, this decrease primarily reflects changes in meteorological conditions, and not dynamical adjustments. With the mass balance of the last decade, our experiments suggest that Hardangerjøkulen ~~, we consider SIA to be a valid choice for this study. Nevertheless, since we have not compared the SIA with HO/FS for~~ disappears within 750 years (Fig. 11). However, future projections indicate further warming for southern Norway. Giesen and Oerlemans (2010) imposed future climate scenarios on a surface energy balance mass balance model coupled to a SIA model, suggesting that Hardangerjøkulen ~~, we cannot conclude how accurate SIA is~~ will vanish almost completely before 2100. Similar conclusions have been reached for glaciers in Iceland (Adalgeirsdóttir et al., 2006; Guðmundsson et al., 2009; Adalgeirsdóttir et al., 2011), French Alps (Le Meur et al., 2007), Swiss Alps (Jouvet et al., 2011) and Canadian Rocky Mountains (Clarke et al., 2015). Given the aforementioned temperature and precipitation sensitivities for Hardangerjøkulen~~in particular and small ice caps in general.To understand the significance of higher-order ice mechanics, further similar studies are needed for different dynamic, climatic and topographic settings.~~, our found -2.2 m w.e. to remove the present-day ice cap in 100 years translates to a temperature increase of ~2.7 °C, given a 10 % increase in precipitation. This is close to future projections for southern Norway (Hansen-Bauer et al., 2015).

~~Finally, because of the relatively poor process knowledge on processes at glacier beds, uncertainties regarding spatial and temporal patterns of basal sliding may be larger than the difference between a simple (SIA) and a physically more complete (HO/FS) ice flow model. To understand glacier behavior under climate change, we advise that ice flow model intercomparisons of *real* glaciers and ice caps should consider the sensitivity to both ice deformation and basal sliding (and of course, mass balance).~~

5.4 ~~Implications of modelled~~ Holocene ~~evolution~~to LIA build-up

In the early part of the modelled period (c. 4000 - 3800 BP), ice grows preferentially on high bed topography, and ~~earlier on Midtdalsbreen /~~Midtdalsbreen and Blåisen ~~than in the present-day basin of~~ start to develop earlier than Rembesdalskåka (Fig. 5, also see Fig. 1). While the model resolution here is coarse (300-500 m), we expect that ice dynamics at this stage plays a minor role, since the ice present is ~~divided over several small~~ split up into several small separate glaciers (< 2 km long, < 100 m thick)~~glaciers~~. Instead, the initial ice growth at high bed ridges is due to build-up of ice above the present-day ELA, which is used as initial mass balance forcing~~at 4000 BP~~.

~~Reconstructions around southern Norway show that glaciers did not survive the mid-Holocene thermal maximum (e.g. Bakke et al., 2005 In agreement, pollen-based reconstructions from western Norway suggest a drop in summer temperatures at 4000 BP (Bjune et al., 2005).~~

~~We are aware of the limitations of the SIA~~ The actual rate of advance may differ from what is modelled here because the SIA has limitations in the steep terrain (Le Meur et al., 2004) where Rembesdalskåka terminates during the period of fast ice volume increase (c. 3800–2300 BP, Fig. ~~??~~4c). ~~Therefore,~~ However, the effects of ice flow mechanics are likely small compared to those of the ~~actual rate of advance may differ from what is modelled here. However, we expect that this section~~

~~of Rembesdalskåka's bed fosters a more dynamic glacier than the overdeepened central part, so the simulated fast advance in this area is not surprising~~mass balance on the long time scales considered here.

During the period of modelled rapid ice cap growth (c. 2300–1300 BP), reconstructed precipitation in western Norway is slightly lower than the general increasing mass balance trend applied here (Dahl and Nesje, 1996; Bjune et al., 2005)~~, coincident with~~. At the same time, glacier reconstructions from southern Hardangerjøkulen ~~indicating~~ indicate a slight decrease in glacier size (Nesje et al., 1994). ~~In our mass balance forcing, we deliberately smoothed out any variability around the general trend, since our aim is to understand the first order aspects of ice cap growth through time. Moreover,~~ Unfortunately, there is to our knowledge no geomorphological or other evidence that can be used as tie points for modelled ice cap extent or volume during this period. ~~Imposing short variations in mass balance would therefore add further uncertainty rather than improve our understanding of the behavior of~~

Our simulated preferential ice cap growth on the northern and western side, illustrated in Fig. 5b at 2300 BP, is in line with reconstructions showing an early glacierization of the north (Dahl and Nesje, 1994) versus the south (Nesje et al., 1994).

We are aware that bed topography for Hardangerjøkulen ~~and the first order impact of bed topography.~~

5.5 ~~Non-linearity, asymmetry and paleoclimatic relevance~~

~~Comparing the ice volume evolution for three of the outlet glaciers (~~is uncertain in places, though less so for Midtdalsbreen and Rembesdalskåka, ~~Midtdalsbreen, Blåisen), we find that they do not grow in the same fashion (Fig. ??c). Midtdalsbreen's ice volume increases linearly over time, while~~ which are of prime interest. Moreover, the proglacial lake in front of Rembesdalskåka ~~and Blåisen have distinct jumps in ice volume, related to their bed topography. The importance of bedrock troughs and overdeepenings is further illustrated by~~ may have modulated LIA frontal behavior, as suggested for Icelandic glaciers (Hannesdóttir et al., 2015). We however expect this effect to be minor compared to other model uncertainties.

Further data for model validation is required to add more detail to our modelled history of Hardangerjøkulen~~'s non-linear volume increase c. 2300–1300 BP, a period when volume increases faster than area (Fig. 12). In other words, ice during this period is thickening rather than expanding horizontally. This can largely be explained by ice growth in subglacial valleys in the eastern and southeastern parts of~~. However, given the limited knowledge about ice cap activity between the ice-free conditions at 4000 BP and the LIA maximum around 1750 AD, we consider our continuous model reconstruction to be a good first estimate of how Hardangerjøkulen grew from nothing to its maximum extent during the LIA.

Moreover, we have provided a plausible ice cap history over several thousand years as the starting point for our simulations from the ~~ice cap (Fig. 1). These bed depressions fill up quickly because ice flow converges into them from surrounding high bedrock ridges, and the mass balance-altitude feedback amplifies the ice thickening~~LIA until today, in contrast to several previous studies (e.g. Giesen and Oerlemans, 2010; Adalgeirsdóttir et al., 2011; Zekollari et al., 2014) reaching desired initial LIA conditions by perturbing a present-day ice cap.

5.5 Non-linearity, asymmetry and their implications

The initial present-day mass balance forcing ($\Delta B(t)$ = 0 m w.e.) at 4000 BP likely explains the rapid increase in ice volume over the first few hundred years, since this forcing essentially represents a step change in mass balance at 4000 BP. However, this effect diminishes after a few hundred years, after which the response is due to the linear mass balance forcing. $\Delta B(t)$ = 0 m w.e. starting from ice-free conditions produces a steady-state ice volume of only $\sim$2 km^3 (Fig. 10), a volume ~~which is~~ exceeded at 3300 BP, so any additional ice volume cannot be explained by the initial step change in mass balance at 4000 BP. Most importantly, the non-linear ice volume response between 2300–1300 BP is thus entirely forced by the linear mass balance increase during this period.

Analogous to the Holocene simulations, we ~~also~~ performed experiments with a slowly *decreasing* mass balance over multiple millennia (from $\Delta B(t)$ = 0.4 to 0 m w.e.), allowing the ice cap to dynamically adjust, starting with the 1600 AD ice cap state. We find that the western ice cap disappears first, while ice in the eastern part of the ice cap is more persistent~~(not shown here).~~

~~It is striking that~~. Hence, the western and northern parts of the ice cap grow first and disappear first, whereas the eastern part grows last and disappears last. ~~This asymmetry~~ Further, our experiments show that a gradual (linear) climatic change results in a non-linear change in ice volume. This non-linear, asynchronous growth and retreat illustrates that proxy records representing different parts of an ice cap at different times may lead to substantially different conclusions about ice cap size through time.

Previous work has highlighted glacier hypsometry, overdeepenings and proglacial lakes in altering glacier *retreat* to climate forcing (Kuhn et al., 1985; Jiskoot et al., 2009; Adalgeirsdóttir et al., 2011). Adhikari and Marshall (2013) and Hannesdóttir et al. (2015) showed that overdeepened basins loose mass by thinning rather than retreat. Here we suggest that a similar behavior applies to an *advancing* glacier. In particular, overdeepened areas delay frontal advance and lead to preferential glacier thickening. However, note that the effect of higher order stresses, not captured by our simplified dynamic model, may be more important for an advancing glacier (Adhikari and Marshall, 2013).

~~Notably, our experiments show that a gradual (linear) climatic change results in a non-linear change in ice volume. Given that we would like to understand past climates and perform future predictions, our results clearly pinpoint that we should assess underlying mechanisms and resulting feedbacks, rather than extrapolate a climatic forcing and glacier change concurrently through time.~~

~~In general, steeper outlet glaciers with a high ablation rate adjust more rapidly to climate than thick, gently sloping ice masses with a lower melt close to the terminus (Johannesson et al., 1989; Harrison et al., 2001). Elsberg et al. (2001) and Harrison et al. (2001) dis time scales of response in light of different glacier characteristics. Consistent with our findings, they suggest that the effect of surface elevation is critical for the response of gently sloping ice masses.~~

~~Based on theoretical considerations accounting explicitly for mass balance changes, and implicitly for ice dynamics, they find a useful ratio for glacier response time to be~~ $\cancel{\dot{G}_e \frac{H}{\dot{b}_e}}$~~, where~~ $\cancel{\dot{G}_e}$ ~~is the (linear) mass balance rate of change with altitude,~~ $\cancel{H}$ ~~is an characteristic thickness scale and~~ $\cancel{\dot{b}_e}$ ~~an 'effective' balance rate close to the terminus. They suggest that if this ratio approaches unity, the sensitivity as they have defined it becomes high, and errors can become large in the calculated response, because surface elevation and area feedbacks nearly cancel each other out.~~

~~For~~

Regarding volume-area scaling (Sect. 4.2), Bahr et al. (2015) argues that the fundamental difference between valley glaciers and ice caps, and hence the reason for different scaling exponents (γ), is the influence of bedrock topography, specifically that ice thickness is large compared to the relief of underlying topography. The bedrock topography below Hardangerjøkulen~~, mass balance does not vary linearly with elevation for the entire elevation range, but typical values for the 1963-2007 period for the linear part of the gradient we use are $\dot{b}_e$ = -6.5 m ice eq. a^{-1} and $\dot{G}_e$ = 0.0097 a^{-1}. A characteristic thickness scale for~~ consists of deep subglacial valleys and high ridges controlling the ice flow, as also noted by Laumann and Nesje (2016) for other Norwegian ice caps. In fact, our simulations confirm that bed topography is vital in controlling the growth and retreat of Hardangerjøkulen~~is 150–200 m. Using the formula from Harrison et al. (2001), this gives a ratio of around 0.2 to 0.3. For a LIA situation, the characteristic thickness may have been 250 m, with a weaker ablation rate at the terminus, giving ratios of 0.5 to 0.6, assuming the same vertical mass balance gradient. These values are all well below Harrison et al. (2001)'s problematic ratio close to unity. However, it should be noted that their theory assumes that area can react instantaneously to volume, thus remaining in-phase, something we do not observe for several outlet glaciers and periods of Hardangerjøkulen's late-Holocene history. Giesen (2009) attempted to calculate response times~~. The relatively thin ice at the ice cap summit does not correspond to the classical ice cap with the thickest ice in the center, which explains why volume-area exponents for valley glaciers (γ = 1.375) rather than ice caps (γ = 1.25) are found for Hardangerjøkulen~~and its outlet glaciers, but could not define a characteristic response time because of the high sensitivity and variation between outlet glaciers.~~. However, the overestimation of c by commonly used volume-area scaling relations for ice caps is more surprising. The low c we find compared to literature values for ice caps suggests that literature volume-area scaling parameters may not be accurate for relatively small ice caps.

~~Our simulated preferential ice cap growth on the northern and western side, illustrated in Fig. 5b at 2300 BP, is in line with reconstructions showing an early glacierization of the north (Dahl and Nesje, 1994) versus the south (Nesje et al., 1994), though there is potential for further studies investigating the spatial asymmetry in more detail.~~

Importantly, glacier reconstructions using proglacial lake sediments are generally based on assumed changes in glacier (erosive) area rather than volume (Hallet et al., 1996), while we show that volume and area can become decoupled for several ~~hundred years at a time~~ centuries (Fig. 12)~~, for example when the largest outlet glacier Rembesdalskåka was situated on overdeepened parts of its bed.~~. We also demonstrate that the degree of volume-area coupling varies for different outlet glaciers, implying that each outlet glacier should be considered individually. For example, a differing response to identical climate forcing is illustrated when Midtdalsbreen advances only modestly from 2300–1300 BP (Fig. 5b-d), while Hardangerjøkulen triples its ice volume during the same period due to ice growth ~~occurs~~ elsewhere (mainly in the east, south and southwest).

~~The wider implication of our results is that glaciers have different climate sensitivities depending on where the ice margin is located and what is underneath it. It follows that increased proglacial lake sediment input may not indicate an advancing glacier, but merely a thickening and increasingly erosive glacier occupying an overdeepened part of its bed. Similarly, assuming that changes in sedimentary input reflect area change rather than volume , and extracting climatic signals from a preferentially thinning glacier with a stagnant front, may be challenging at best.~~

~~Our study proposes a~~ Our non-linear response and out-of-phase volume and area calls for reassessment of some glacier ~~reconstruction methodologies, in particular those using sediments from proglacial lakes. We advise that such studies should infer past climatic and glacier states not exclusively using a linear assumption between sediment input and glacier basin size. Ideally such records would be accompanied by (modelled or empirical)knowledge of the interaction between past ice dynamics, sedimentation, mass balance and geometry.~~

5.6 ~~LIA maximum and initial conditions~~

~~Our simulations show that Hardangerjøkulen has lost one-third of its volume since 1750 AD. The modelled LIA maximum volume of $\sim$14.8 km^3 (Fig. ??)is challenging to validate, since we do not know the surface topography at this time.~~

~~We are aware that bed topography at Hardangerjøkulen is uncertain in places, though less so for Midtdalsbreen and Rembesdalskåka, which are of prime interest. Moreover, the proglacial lake in front of Rembesdalskåka may have modulated LIA frontal behavior, as suggested for Icelandic glaciers (Hannesdóttir et al., 2015).~~

~~The exact history of Hardangerjøkulen as a whole will thus unavoidably differ from our model results. However, given the limited knowledge about ice cap activity between the ice-free conditions at 4000 BP and the LIA maximum around 1750 AD, we consider our continuous model reconstruction to be a good first estimate of how~~ and climate reconstruction methodologies. To extract a climate signal, linear assumptions between ice extent (area), ice volume (mass balance), climate, and their geomorphological or proxy signal are commonly assumed. However, we find that these assumptions does not hold for Hardangerjøkulen ~~grew from nothing to its most extensive state during the LIA.~~

~~Moreover, we have provided a plausible ice caphistory over several thousand years as the starting point for our simulations from the LIA until today. This is in our view a step forward from several previous studies (e.g. Giesen and Oerlemans, 2010; Adalgeirsdóttir that reach desired initial LIA conditions by perturbing a present-day ice cap.~~

5.6 ~~Mass balance sensitivity~~

~~To investigate the sensitivity of present-day Hardangerjøkulen to 'future' changes in mass balance, steady-state experiments were performed with present-day ice cap topography as the starting point. The ice flow model is first forced without a mass balance anomaly until steady-state, using our 'best-fit' parameters from the calibration (Sect. 3.2.1). From this state, we perturb the mass balance by anomalies between -0.5~~ and its outlet glaciers. For a growing ice cap, two scenarios may arise for which the linear assumption between area (proxy) and ~~+0.5 m w.e., and run the model to a new equilibrium.~~

~~These experiments show a close to linear relationship between mass balance perturbation and ice volume response (Fig. 10) , until the point when the mass-balance feedback becomes too strong and the ice cap disappears entirely for more negative anomalies.~~

~~Our simulations show that Hardangerjøkulen is highly sensitive to climatewarming (Figs. 10 and 11) . In particular, the ice cap is bound to disappear almost entirely for a mass balance anomaly of -0.3 $\pm$ 0.2 m w.e., relative to the mean mass balance over the period 1963-2007. These results are consistent with those of Giesen (2009), who used a SIA model (Van Den Berg et al., 2008) with different implementation of basal sliding and ice deformation and without local mesh refinement. Similar experiments with~~

~~Nigardsbreen, southwestern Norway (Oerlemans, 1997a), and Franz Josef Glacier, southwestern New Zealand (Oerlemans, 1997b), both located in maritime climates, show much smaller volume and length responses, illustrating the extreme mass balance sensitivity of Hardangerjøkulen.~~

~~To investigate the role of the mass balance-altitude feedback in the ice cap response, we performed additional experiments excluding this feedback by keeping the mass balance field fixed at the present-day surface topography. Using this setup, a close to linear relationship between mass balance changes and steady-state ice volume was found, and the ice cap was less sensitive to mass balance changes. For example, without the feedback, half of present-day ice volume (4.9 km^3)is still present for a mass balance anomaly of -0.5 m w.e., relative to 1963–2007 (not shown here) . In contrast, when including the feedback, the ice cap disappears entirely for this mass balance anomaly.~~

~~We can view our results in light of future climate change. Compared to the period 1963-2000, with a mass balance close to zero, the mean mass balance in the last decade has decreased to -0.3 m w.e. Since Hardangerjøkulen was in approximate balance over the past decades, this decrease primarily reflects changes in meteorological conditions, and not dynamical adjustments. This mass balance change is small compared to the interannual variability in the net mass balance. Even if the mass balance does not become more negative in the future, Hardangerjøkulen is bound to disappear, although it will take 750 years (Fig. 11).~~

~~As evident from Collins et al. (2013), we expect a warming scenario for the future. Giesen and Oerlemans (2010) used an energy balance model to simulate the mass balance of Hardangerjøkulen for the next 100 years, using climate scenarios for southern Norway. Their modelled future mass loss largely exceeds that observed for the last decade. For example~~ volume (climate) fails: (i) area changes faster than volume (first few hundred years of our Holocene simulation), meaning the interpreted signal becomes biased towards a climate favorable for glacier growth (wetter/colder), ~~for a 'realistic' scenario with a temperature rise of 3°C and 10 % rise in precipitation relative to the normal period 1961-1990, the net mass balance of Hardangerjøkulen as a whole was estimated to be -4.10 m w.e. in 2086, i.e. a mass balance more than 10 times more negative than observed for the last decade. They also coupled their mass balance model to a SIA model (Van Den Berg et al., 2008), and suggested that Hardangerjøkulen will vanish almost completely before 2100. The rate of mass balance change in the 21st century is so large that the role of the mass balance-altitude feedback and ice dynamics in the modelled ice volume change is relatively small~~or (ii) volume changes faster than area (2300–1300 BP in our simulation), and the climate signal is missed or underestimated because the preferential thickening is not translated into a corresponding frontal change. We expect that ice caps with comparable geometry in for example Norway, Iceland, Alaska, Patagonia and peripheral Greenland may display similar behavior.

~~Similar conclusions have been reached for glaciers in Iceland (Adalgeirsdóttir et al., 2006; Guðmundsson et al., 2009; Adalgeirsdóttir et French Alps (Le Meur et al., 2007), Swiss Alps (Jouvet et al., 2011) and Canadian Rocky Mountains (Clarke et al., 2015).~~ These results highlight the need for model-data integration in paleostudies. Ice sheet modelers require glacier records for calibration and validation, and climate reconstructions for model forcing. Based on our experiments, we advise that glacier-derived climate records are tagged with explicitly stated glaciological assumptions and associated uncertainties. In particular, we would like to recommend future model-data studies which directly constrain geometric contributions to the glaciological uncertainties involved in sedimentary glacier proxies.

~~The high sensitivity to mass balance found for Hardangerjøkulen supports changes inferred for the Holocene. Abrupt changes are evident from lake sediment records of glacial activity both at the northern (Dahl and Nesje, 1994) and the southern (Nesje et al., 1994) sides of the ice cap. One example is the so called *Finse event*, when an advance to a maximum beyond that of present-day of the northern Blåisen outlet glacier ~8300 BP was followed by a complete melt-away of this glacier within less than a century. Since future warming is projected to be much larger than the changes during this period, a complete disappearance of Hardangerjøkulen is likely. Further studies on similar ice masses, integrating proxy data and modeling efforts, are needed to shed light on the relevant processes involved in such abrupt changes of ice caps and glaciers to climate change.~~

6 Conclusions

We have used a two-dimensional ice flow model with ~~mesh refinement~~ a simple mass balance parametrization to simulate the evolution of Hardangerjøkulen ice cap since the mid-Holocene, from ice-free conditions up to the present-day. Until the LIA, the model is forced by a ~~mass balance~~ linear mass balance increase based on reconstructions of temperature and precipitation. From the LIA onwards, an optimized mass balance history is employed, and direct mass balance measurements are used after 1963.

~~We used the Shallow Ice Approximation (SIA) for ice flow and~~ We used an ensemble approach to assess sensitivity to sliding and ice deformation parameters during both calibration and transient runs. We find that small differences in model ice rheology at the calibration stage increase significantly with time. This time-dependence has implications for other model studies of long-term dynamics of glaciers and ice caps. Such studies would benefit from using a 'transient calibration' rather than a 'snapshot' approach, and thereby reduce temporal biases arising from data quality issues, or a particular dynamic or climatic state. More data in both space and time is needed to further constrain the dynamic model parameters and mass balance for Hardangerjøkulen.

~~We show that the effect of the sliding parameter depends on what deformation parameter is used; the softer the ice , the more important is the sliding parameter. Moreover, we find it challenging to pick a unique, 'dynamically ideal' parameter combination. We therefore suggest that parameter ensembles used to calibrate models may as well be kept for transient simulations in the way presented here, if computationally feasible.~~

Our simulations ~~show~~ suggest that Hardangerjøkulen evolved from no ice in the mid-Holocene to its LIA maximum in different stages, where the fastest stage (~~2200–1200~~ 2200–1300 BP) involved a tripling of ice volume over only 1000 years.

Notably, our linear climate forcing during this time gives a non-linear response in ice cap volume and area. This growth occurs in a spatially asymmetric fashion, where Midtdalsbreen reaches its maximum first, while advances of Rembesdalskåka and the eastern ice cap are delayed. ~~In contrast, an opposite spatial asymmetry is found for a disappearing ice cap. This response is linked to~~ These different responses are caused by local bed topography ~~; in particular, we highlight that the presence of an overdeepening delays glacier advance. We also illustrate that the degree of volume-area coupling for outlet glaciers as well as the whole ice cap varies both temporally and spatially.~~ and the mass balance-altitude feedback.

~~Our simulations thus provide new insight relevant for paleoglaciological and -climatic studies assessing the influence of past ice dynamics and geometry. These considerations are also important for future predictions, though ice dynamics may become less important if the rate of future mass balance change is large.~~

~~Instead of perturbing the present-day ice cap as in previous studies, we reach initial conditions for our historical simulations starting at the LIA by modeling the Holocene ice cap history.~~ Following the simulated Holocene growth of Hardangerjøkulen, we successfully reproduce the main features of the LIA extent of the main outlet glaciers, given temporal and spatial uncertainties in moraine evidence. In the early 1900s the simulated glacier positions are slightly underestimated, whereas the ice extent closely resembles the observed margins available starting from 1960, and the surface topography fits well with the 1995 surface survey.

Hardangerjøkulen is found to be highly sensitive to mass balance changes~~, consistent with previous studies of both the past and the present. A shift by only -0.3 ±~~. A reduction by 0.2 m w.e. or more relative to the ~~1963–2007 reference mass balance~~ mass balance from the last decades induces a strong mass balance-altitude feedback and ~~completely melts away~~ lets the ice cap disappear completely. Conversely, an anomaly of +0.5 m w.e. almost doubles total ice volume.

Volume and area for Hardangerjøkulen and several of its outlet glaciers vary out-of-phase for several centuries during the Holocene. This disequilibrium varies in time and among the outlet glaciers, showing that ice cap reconstruction methodologies carrying linear assumptions between ice extent and volume may not hold. Based on the non-linear, asynchronous response we find for Hardangerjøkulen, these paleoglaciological studies may decrease their uncertainty by (i) quantifying the effect of bedrock topography on ice flow and mass balance, using a numerical model; (ii) performing reconstructions on at least two outlet glaciers, preferably with distinct dynamics and bedrock topography, and (iii) reporting glaciological assumptions and proxy uncertainties to ice sheet modelers using their data.

~~Several factors may affect the validity of the model for situations which differ largely from the~~ Our experiments further suggest that the present-day ~~situation, including mass balancedistribution for a greatly altered geometry, ice albedo feedbacks and glacio-hydrological changes. More work is needed to better constrain the time scale and relative importance of these aspects.~~ ice cap is in a mass balance regime where it will not regrow once it has disappeared. We expect that ice caps with comparable geometry elsewhere may display similar sensitivity and hysteresis. By combining our modelled sensitivities with past climatic and glacier information, we also illustrate that ice flow models can further constrain past climates and glacier states. This highlights the need to understand the long-term history of glaciers and ice caps and calls for further integrated model-data studies.

~~Although our simple mass balance implementation may be refined and physical complexity may be added to our sliding formulation or ice dynamical approximation, we consider our findings robust on the climatic time scales studied here.~~

Acknowledgements. We wish to acknowledge NVE for access to mass balance data, glacier outlines, and surface and bed DEMs. Thanks also to Norwegian Meteorological Institute for providing climate data (eklima.no). We are also grateful to Atle Nesje for sharing detailed knowledge about Hardangerjøkulen, and for reading the manuscript prior to submission. The research leading to these results has received funding from the European Research Council under the European Community's Seventh Framework Programme (FP7/2007-2013) / ERC grant agreement 610055 as part of the ice2ice project. ~~H.~~This research is also supported by the Research Council of Norway via the NOTUR project NN4659K "Models of past ice and climate". HÅ ~~.~~was supported by the Research Council of Norway (project no. 229788/E10), as part of the research project Eurasian Ice Sheet and Climate Interactions (EISCLIM), and has also received travel support from BKK AS. HÅ, KHN, RG and MM designed the research, HÅ performed the model runs with significant input from KHN and RG, MM provided the ice sheet model and added necessary implementations together with HÅ. HÅ created all figures and wrote the paper, with substantial contributions from the other authors. The authors declare that they have no conflict of interest.

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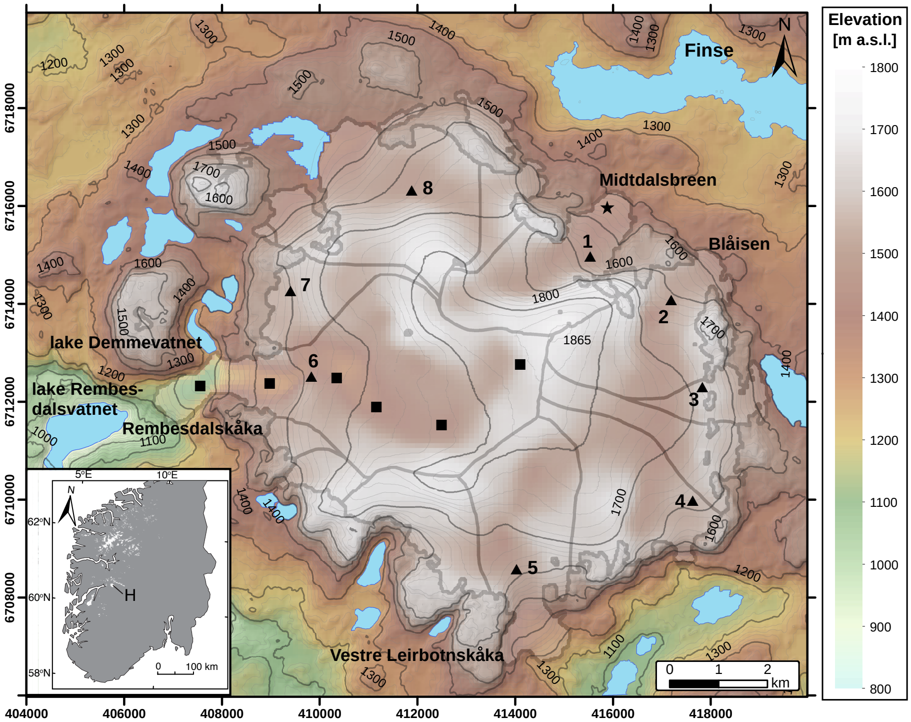

Figure 1. Bed (coloring) and surface (contours) topography of Hardangerjøkulen ice cap. Contour interval is 20 m and created from a digital elevation model by Statens Kartverk, 1995. The reference system is UTM zone 32N (EUREF89). Ice cap outline and drainage basins from 2003 are indicated (data from Cryoclim.net), as well as surrounding lakes (drawn after Statens Kartverk N50 1:50 000). Shown are GPS positions for velocity measurements (numbered triangles), mass balance stakes from NVE (squares) and location of the automatic weather station (star). Inset: map of southern Norway showing the location of Hardangerjøkulen (H).

~~Modelled surfaces from 4000 BP to 1600 AD, starting with no ice cap, shown every 50 years from older (dark blue) to younger (yellow). BP ages are relative to 2008 AD. Note that the top of Rembesdalskåka (Hardangerjøkulen's summit) does not coincide with the top of Midtdalsbreen's flowline (see Fig. 9d).~~

~~Modelled ice thickness at (A) 3800, (B) 2300, (C) 1900 and 1300 BP using our 'best-fit' model parameters obtained from independent calibration. Shown are also ice cap extent in 1995 AD (black thick line) and corresponding drainage basins for outlet glaciers Rembesdalskåka (SW) and Midtdalsbreen (NE; black thin lines)~~

~~Ice volume evolution for selected mass balance perturbations (-0.5 to 0.5 m w.e.) relative to the mean mass balance 1963-2007, using our 'best-fit' dynamical parameter combination. A mass balance anomaly of -0.2 m w.e. is added for greater detail of when Hardangerjøkulen disappears.~~

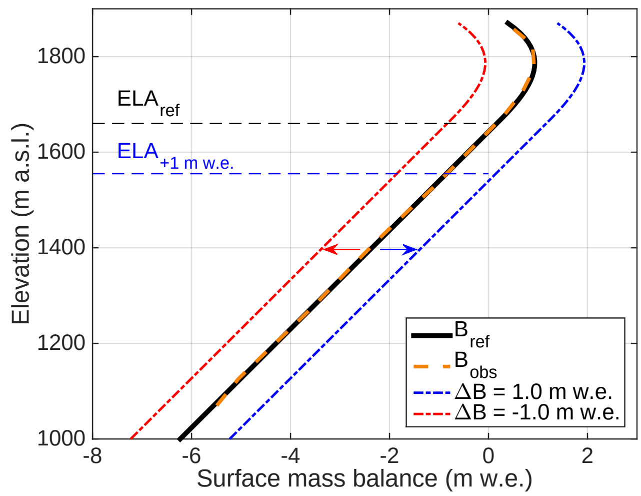

Figure 2. Reference net surface mass balance (B$_{ref}$) profile used in the model runs, based on the mean observed (B_{obs}) profile for 35 of the 45 years 1963-2007. At lower elevations, a linear gradient is used; for the highest elevations, a third-order polynomial is fitted to the observed values. Shown are also $\Delta B(t)$ = -1.0 and +1.0 m w.e., examples of how temporal mass balance changes are imposed (Eq. 6), along with corresponding ELA's. For -1.0 m w.e., mass balance is negative at all elevations, thus ELA is above the summit. Data from NVE.

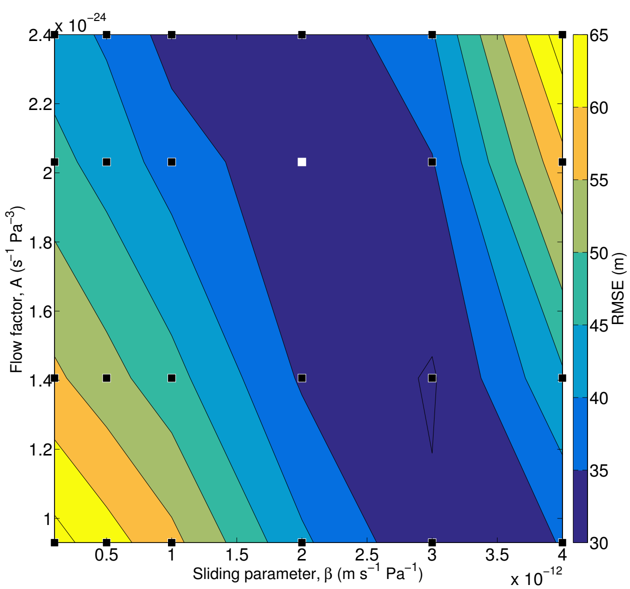

Figure 3. Root mean square error (RMSE) between modelled and observed present-day ice thickness along the flowlines of Midtdalsbreen and Rembesdalskåka, using an ensemble of sliding (β) and rheology (A) parameters. Shown are parameter combinations (black squares) and the 'best-fit' parameter combination used in subsequent runs (white square).

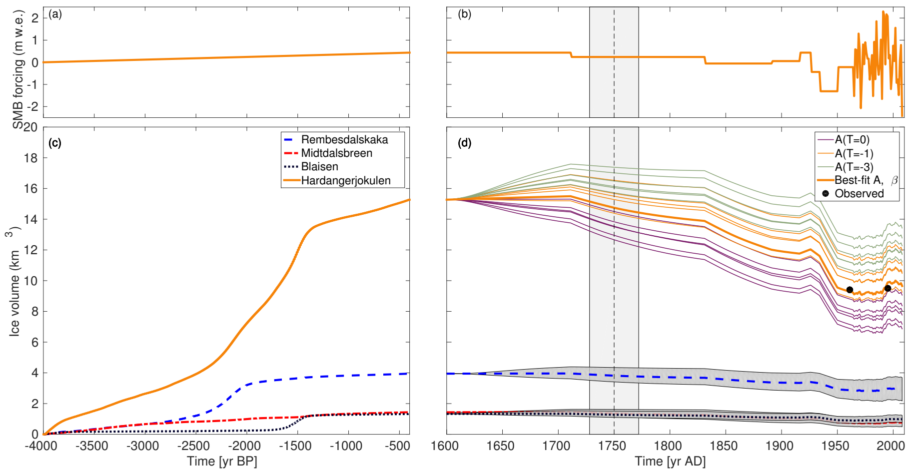

Figure 4. (a) Mass balance forcing for mid- to late Holocene (spinup period), and for (b) 1600 - 2008 AD. (c) Ice volume response for mid- to late Holocene and for (d) 1600 - 2008 AD using an ensemble of sliding and deformation parameter combinations (dark shading) and our 'best-fit' combination obtained from independent calibration. Colors represent different outlet glaciers and the whole ice cap. The LIA maximum, as dated at ~~Mitdalsbreen~~ Midtdalsbreen(dashed line), and its temporal uncertainties (light shading) is also shown, as well as ice volume observations from 1961 and 1995 (black dots). For details, see text.

Table 1. Constants and parameter values used in this study.

Parameter	Symbol	Unit	Value
Ice density	ρ_i	kg m^{-3}	917
Gravitational acceleration	g	m s^{-2}	9.81
Flow factor	A	s^{-1} Pa^{-3}	0.95×10^{-24} to 2.4×10^{-24}
Sliding parameter	β	m s^{-1} Pa^{-1}	$4\times\ 10^{-12}$ to $1\ \times 10^{-13}$
Sliding law exponent	m		1
Glen's law exponent	n		3
Mesh resolution	Δx	m	200-500
Time step	Δt	a	0.02

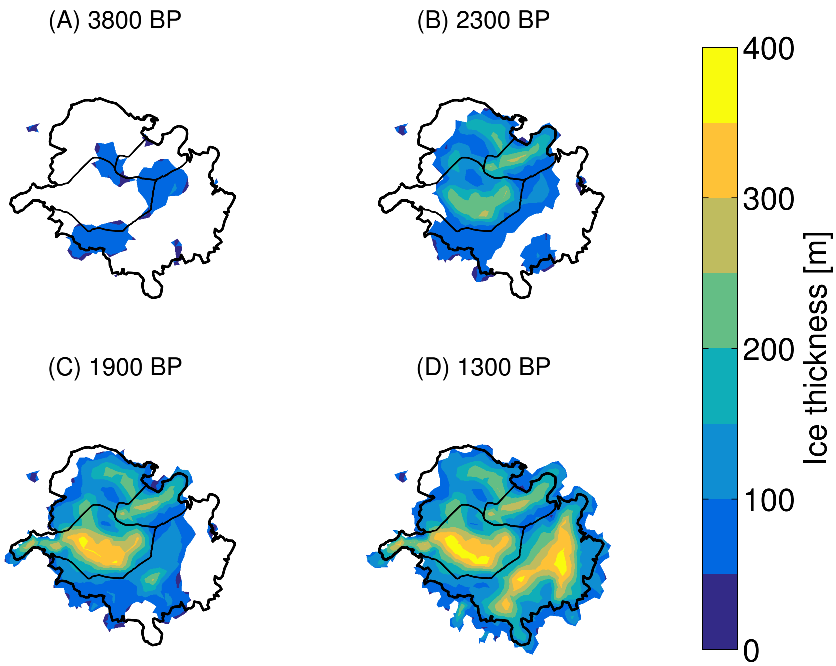

Figure 5. Modelled ice thickness at (A) 3800, (B) 2300, (C) 1900 and 1300 BP using our 'best-fit' model parameters obtained from independent calibration. Shown are also ice cap extent in 1995 AD (black thick line) and corresponding drainage basins for outlet glaciers Rembesdalskåka (SW) and Midtdalsbreen (NE; black thin lines)

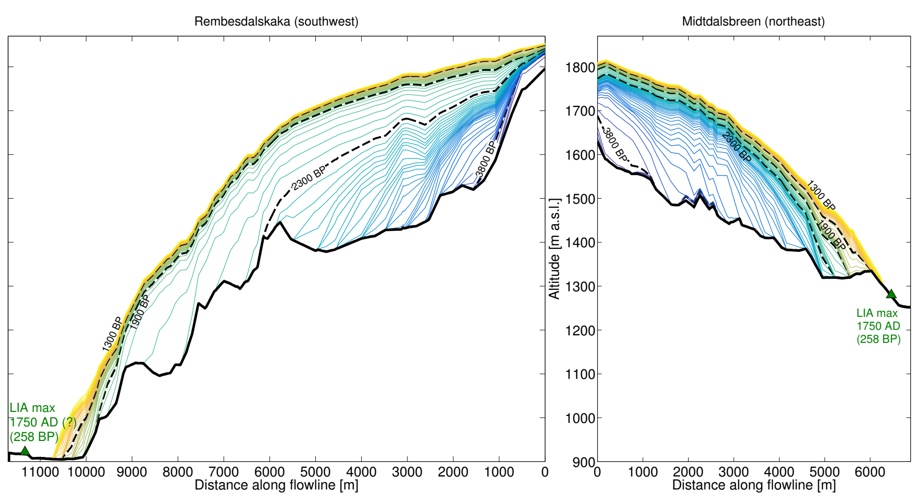

Figure 6. Modelled surfaces from 4000 BP to 1600 AD, starting with no ice cap, shown every 50 years from older (dark blue) to younger (yellow). BP ages are relative to 2008 AD. Note that the top of Rembesdalskåka (Hardangerjøkulen's summit) does not coincide with the top of Midtdalsbreen's flowline (see Fig. 9d).

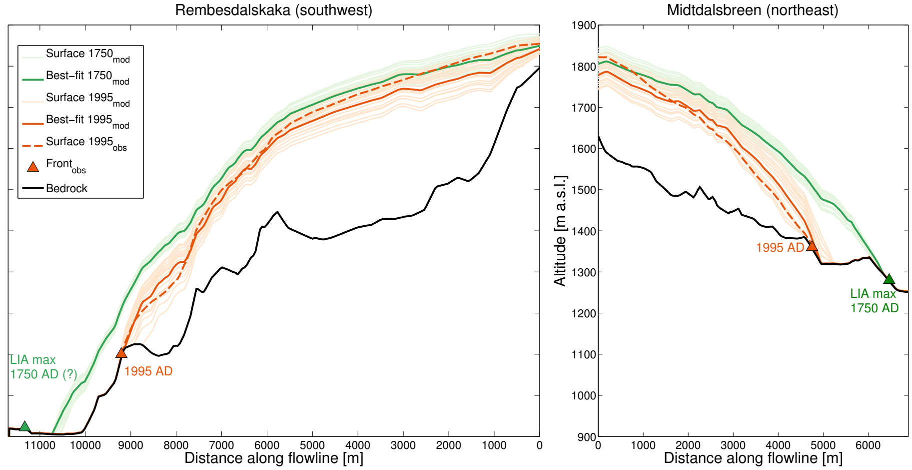

Figure 7. Modelled surfaces for 1750 (light green) and 1995 AD (light orange) for Rembesdalskåka and Midtdalsbreen, using an ensemble of different dynamical parameter combinations. Modelled surface using our 'best-fit' parameter combination is also shown for 1750 (green) and 1995 (orange), as well as observed surface in 1995 (dashed orange). Outlet front positions as known from dated (Midtdalsbreen) and assumed contemporary (i.e. not dated; Rembesdalskåka) terminal moraines are indicated with triangles. Note that the top of Rembesdalskåka (Hardangerjøkulen's summit) does not coincide with the top of Midtdalsbreen's flowline (Fig. 9e).

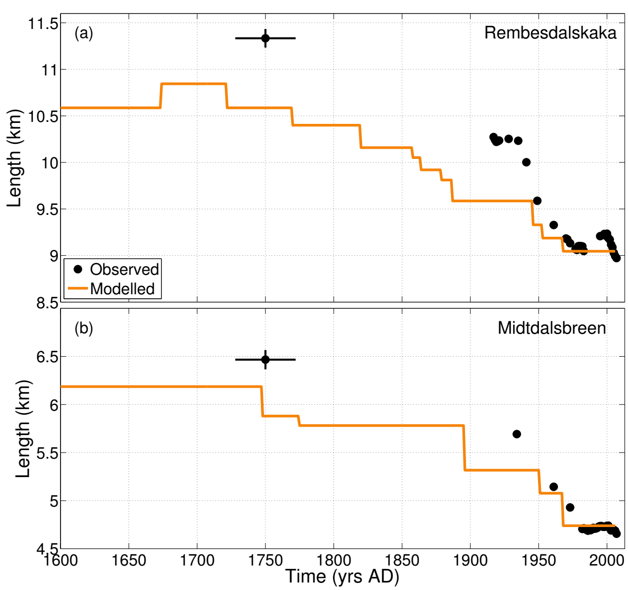

Figure 8. Modelled and observed length of outlet glaciers (a) Rembesdalskåka and (b) Midtdalsbreen. Temporal uncertainty for 1750 is indicated based on a 10 % age error (Innes, 1986) on the dated moraine at Midtdalsbreen (Andersen and Sollid, 1971), and assuming that the Rembesdalskåka moraine is contemporary. Uncertainties in measured lengths in the 1900s and 2000s are smaller than the marker size.

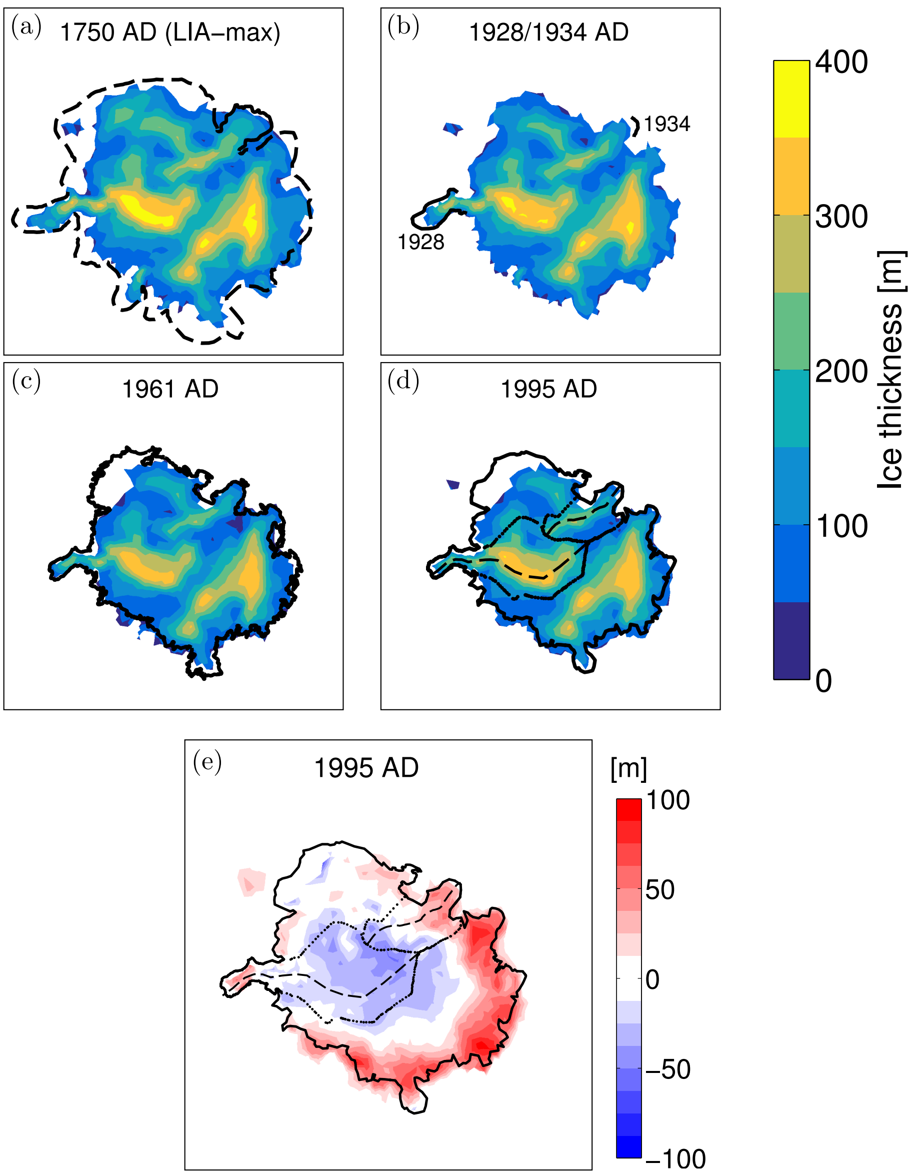

Figure 9. Modelled ice thickness of Hardangerjøkulen in (a) 1750, (b) 1928, (c) 1961 and (d) 1995 AD. Shown is also the difference between modelled and observed surface in 1995 (e), where positive (negative) values indicate that the model overestimates (underestimates) surface elevation. Observed ice cap extents (Andersen and Sollid (1971); Sollid and Bjørkenes (1978); A.Nesje, pers. comm; H. Elvehøy, pers. comm; Cryoclim.net/NVE) for corresponding years are shown where available. For 1750, assumed LIA extent from geomorphological evidence (dashed line) and dated LIA extent (solid line) is shown. For 1928/1934, the modelled thickness displayed is for 1928, though the observed front shown for Mitdalsbreen is from 1934. Drainage basins and flowlines of Rembesdalskåka and Midtdalsbreen are shown for 1995.

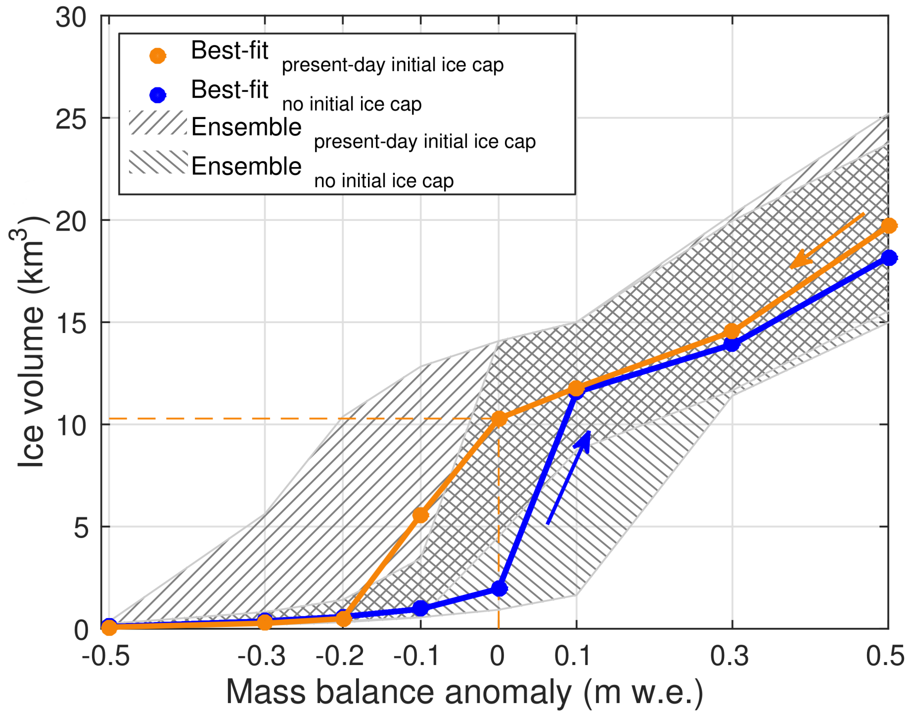

Figure 10. Steady-state ice volumes reached using step perturbations of the 1963-2007 mass balance, using an ensemble of dynamical parameter combinations, starting from the present-day ice cap and ice-free conditions.

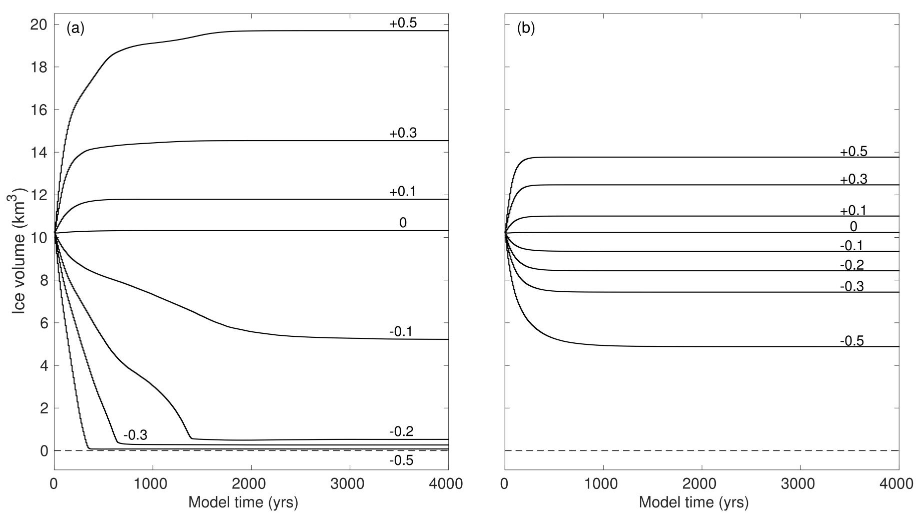

Figure 11. Ice volume evolution for selected mass balance perturbations (-0.5 to 0.5 m w.e.) relative to the mean mass balance 1963-2007, using our 'best-fit' dynamical parameter combination, for (a) with and (b) without a mass balance-altitude feedback. A mass balance anomaly of -0.2 m w.e. is added for greater detail of when Hardangerjøkulen disappears.

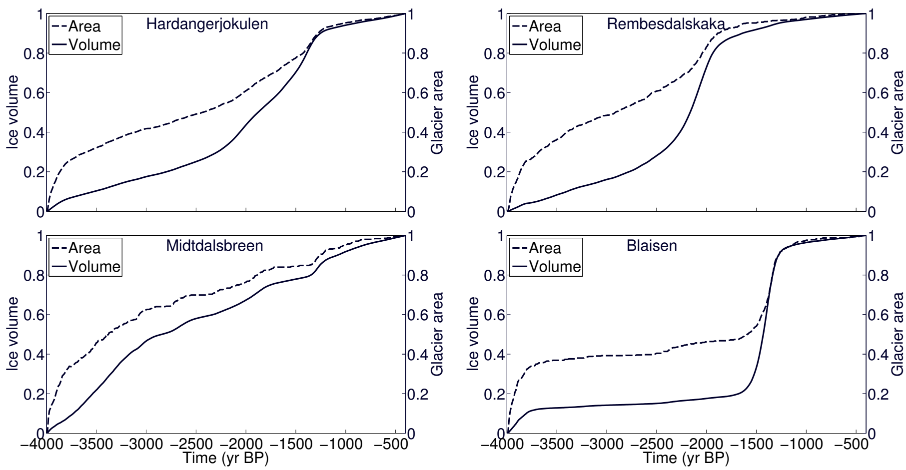

Figure 12. Simulated ice volume and area evolution for (a) Hardangerjøkulen, and the outlet glaciers (b) Rembesdalskåka, (c) Midtdalsbreen, and (d) Blåisen, from 4000 to 400 BP (1600 AD). Quantities are non-dimensionalized relative to final volume and area in year 1600 AD, respectively.

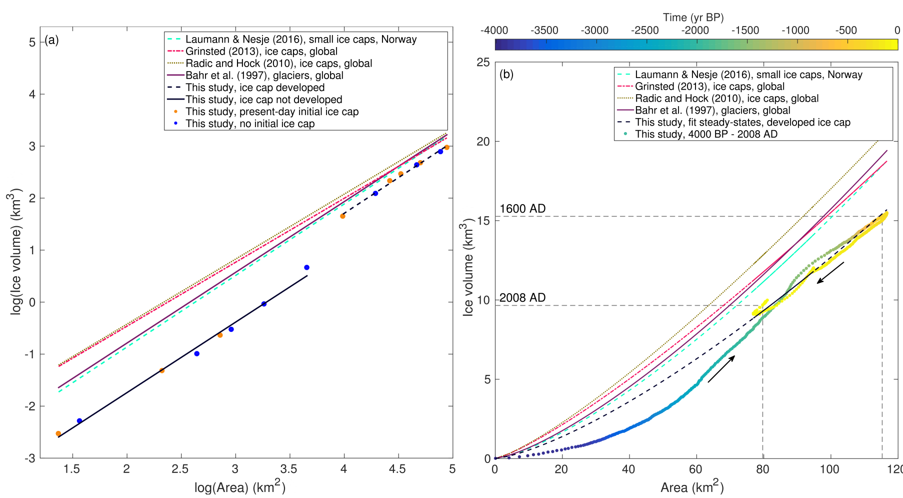

Figure 13. (a) Logarithmic values of volume and area for steady-state experiments using mass balance anomalies within -0.5 to +0.5 m w.e. relative to the 1963–2007 AD reference mass balance. Both steady-states reached from the present-day ice cap and from ice-free conditions are shown. Steady-states are grouped into two cases, depending on whether an ice cap has developed or if ice is only present on high ridges. Commonly used volume-area relations from the literature are also shown (Bahr et al., 1997; Radić and Hock, 2010; Grinsted, 2013; Laumann and Nesje, 2016). (b) Volume and area combinations for our simulation from 4000 BP to 2008 AD, along with the volume-area relation derived from simulated developed steady-state ice caps in (a).

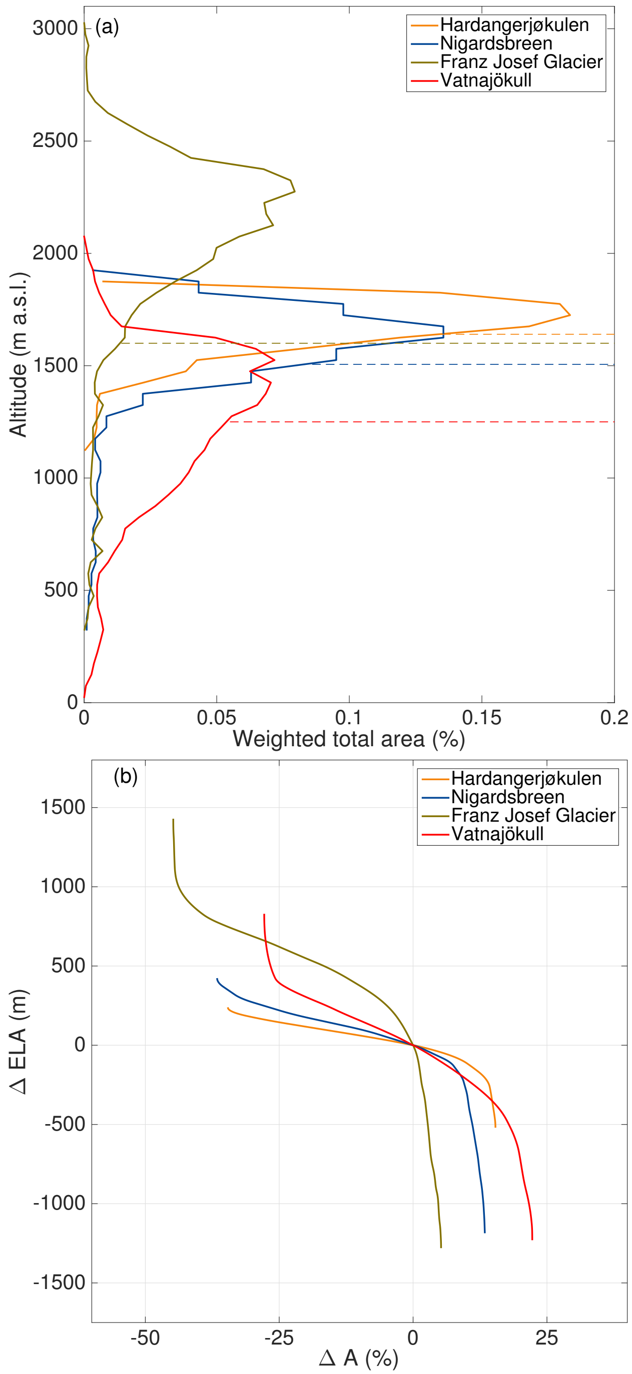

Figure 14. ~~Steady-state ice volumes reached using step perturbations~~ (a) Hypsometry of ~~the 1963-2007 mass balance~~present-day Hardangerjøkulen (Giesen and Oerlemans, 2010) and Nigardsbreen, ~~using an ensemble of dynamical parameter combinations~~Norway (Oerlemans, 1997a), ~~starting from~~ Franz Josef Glacier, New Zealand (Woo and Fitzharris, 1992) and Vatnajökull, Iceland (Aðalgeirsdóttir et al., 2003). Respective ELAs are indicated with dashed lines. Areas are weighted by the ~~present-day ice cap~~ total area, and ~~ice-free conditions~~altitude bins are 25 m. (b) Effect of a step change in ELA on area for respective glacier.