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.

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

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. 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.

Specific comments

Abstract:
- First paragraph (l. 1-4, p.1): do you need this in abstract? Quite long abstract, so would consider removing this.
- l. 11: “given a linear climate forcing”: the forcing was in reality not linear. You impose this. Could change this to: “Under a linear…”
- l. 13: “intriguing”: this is a scientific text, something cannot be “intriguing”: there is a reason behind it. Rather opt for “remarkable”.
- l.16-17: in- and out-of-phase: not clear here. One has to read the manuscript to understand. Would reformulate this.
- l. 18: canonical: what does this mean?
- 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)
- l. 21: close to observations: of course, because this is partly imposed.

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.
- l.5-6: reference(s)?
- l.7: do not understand. GICs response essential because ice sheets are slow? (contribution ice sheets also important in next century)
- l.8: 170000 GICs: reference for number?
- l.12-17: “For comparison… into the physics operating on these time scales”: strange passage. How is this related to the rest of intro?
- l.18: omit “so-called”: they are Full-Stokes models.
- 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.
- 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.
- 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).

- 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)
- 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)
- l.26: which studies? Should make a reference here.
- 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).
- 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))
- 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)
- p3, l.1: “model stategy”: strange formulation. Rather use “metholodogy”

Section 2:

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

Section 2.1.1:

- l.9: Present-day: when is this? 2012? Quickly changes under present-day conditions. Otherwise use “about” to qualify this.
- Give a lot of info about Rembesdalskaka: what about the other outlet glaciers?

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
- 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.
- l.29-30: continuous decrease in ice thickness: towards the edge? Not fully clear, could elaborate on this.

Section 2.1.3:

- Beginning (l. 2-6): jump from one time period to another. Consider reorganizing this.
- l.7: "both outlet glaciers". There's more than two, confusing → "The two outlet glaciers considered.."

Section 2.2

- Again a strange sequence: present-day → past (Holocene + LIA) → present-day

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?).

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".
- 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.

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: 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?
- 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?

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.

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.

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)?)
- As I indicated before, given the model you use, a comparison between SIA and HO would have been interesting (and computationally feasible)
- 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)

Section 3.1.1:

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

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.
- l.25: need such a small time step?

Section 3.2.1:

- l.29-30: repetition (+ see earlier comment: are this 44 or 45 years of measurements?)
- 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.)
- p.9, l.3-5: elaborate. Not clear at this point.

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”.
- 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.

Section 3.2.3:

- Again start with a repetition: overlap with section 2.2.2: should re-organize this to make text more consistent.
- 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…

Section 3.2.4:

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

Section 4.1:

- l.5: again a repetition.
- 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.

Section 4.2.1:

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

Section 4.2.2:

- Very descriptive, chaotic and lacks structure. Should reorganize this and be more specific (to-the-point) to be clearer.

Section 5.1:

- l.30-31: “this is not surprising” → would reformulate this.
- 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.
- 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.

- l.20-26: weak description. Many words to say little. In the end you say: if fast → thin / if slow/stiff → thick
- 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?

Section 5.2:

- Long section about sliding: do almost not have any information. Based on your modelling → 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.
- Comparison with other studies on ice sheets. Is this relevant? Totally different setting, other mechanisms for water to reach the bed (/being locally produced).
- l.18:"It is therefore not surprising" → change
- l.26-29: relationship sliding and geometry: from theoretical perspective. This is not a "finding" from your study..
- 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"..)
- p.15, l.3-4: indeed. A whole section to say very little..

Section 5.3:

- l.6-9: repeat yourself.
- 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…)
- l.19-20: what do you mean? Be more specific.
- l.23-24: indeed. Could this not be done?
- l.31-33: snow redistribution. Could indeed have an effect. But probably smaller effect than the large errors induced by your other approximations.
- p.16, l.3-8: not convinced that this error is that large compared to the magnitude of errors induced by your simple modelling..
- l.9: "works well": not sure..
- 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)

- 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.

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…
- 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.
- l.5-13: this is not a discussion of your results.

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.

Section 5.6:

- l.4-9: growth → very descriptive and site specific. What is added value for reader?
- 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.
- l.23-32: long passage with little information.
- 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.
- l.28 → p.20, l.1-2: not sure that your results support this. Rather speculative.

Section 5.7:

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

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).
- p.21, l.1-4: again rather trivial. What's new about this finding?
- l.9: 750 years to disappear. Too precise. Would change this to "around 750 years"
- "As evident from Collins et al. (2013), we expect a warming scenario": strange formulation.
- l.11-21: do not really discuss your own results, not based on your simulations.
- l.22-28: what's new?

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..
- p.22, l.3-6: this is not something new. Not a finding from this study.
- 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.
- l.15-17 + l. 24-26: site specific → what is the more general interest?
- l.27-31: strange way to end your conclusion..

Figures:

- Nice and clear figures in general.

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