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The Cryosphere Discussions

Interactive comment on "Modelling the 20th and 21st century evolution of Hoffellsjökull glacier, SE-Vatnajökull, Iceland" *by* G. Aðalgeirsdóttir et al.

Anonymous Referee #2

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This discussion paper describes a very interesting and scientifically relevant modeling study of the Hoffellsjökull outlet glacier from Vatnajökull ice cap in Iceland. The manuscript is well written and structured. It is not often that a model study can rely on such a variety of field data. Thus the overall presented reconstruction and projection of ice evolution of Hoffellsjökull is well based on field data as well as good quality modeling which makes this contribution definitely worthwhile for publication in TCD. Nevertheless, parts of the manuscript need further explanation or improvement, before this manuscript can be published. In some section a higher level of detail in the descriptions of work methods is needed alongside with some clarifications. Please refer to the specific comments section for details.

Generally, this being a two dimensional study of ice evolution, I would have expected

the authors to make more use of this kind of model output. Only in Figure 6 do we get to see a 2D velocity field, even though the whole manuscript would highly benefit from the presentation of further, two dimensional modeling results. For example, Figures 2 and 3 could be expanded to include model output in either one or two dimensions. Generally most figures (1, 2, 3, 4, 5 and 7) display only input data to the model of Hoffellsjökull. I recommend to shift the focus of the figures more towards model output without starting to neglect the important model input. Surely there can be some highly interesting maps of future ice extends be included in the manuscript.

All further comments are listed in the sections below, which I hope the authors will find helpful for improving the manuscript.

Specific Comments

p1057-l3: The citation on the ice cover fraction of Iceland is somewhat old (33years), especially for a study dealing with changing ice geometries from the past to the future. Is there a more recent estimate?

p1058-l7: The authors mention an ice fall in the model geometry, but later do not explain if this fact is relevant for the performance of the chosen ice dynamics model (SIA). Please comment on that in the discussion on p1066 around line 5.

p1060-l2-5: "...recent surface DEMs assuming that although the surface has lowered significantly, the surface shape only changes slightly in the accumulation zone." The authors could show that this assumption is valid by presenting modeled surface profiles for several years in a figure similar to Figure 3. This would be a good reason to show modeled surface evolution and investigate the role of ice dynamics in this case.

p1060-l18: Please give an approximate date for these historical records. At this point the question arises if such a large bedrock change is relevant for the model setup used. Later on it is not quite obvious if the model needs to deal with any bedrock changes. In the discussion, there are also results inferred from this bedrock change. Since you present these historical records of basal change throughout the manuscript (e.g. Figure 3), also discuss if this change in relevant for the modeling efforts. In my opinion this is a minor concern to the model, as the model only simulates a retreating ice mass, starting from LIA conditions, as can be seen in Figure 9a. Thus the bedrock change already occurred before the model starts. Please note the respective comment on the discussion section below.

p1061-l23-25: The authors never explain in the manuscript how this data is used to calibrate the model. Add a paragraph on how the PDD model is acutually calibrated with this data in section 5.1

p1062-l24-28: Be more specific here. Explain how well the linear regression between T at Hólar in Hornafjörður and P at Fagurhólsmýri works, give for example any statistical measure how well this model performs. Judging from Figure 5, the temperature at Hólar in Hornafjörður is also reconstructed for most of the period between 1857 and 1924. Does this mean the linear regression of P at Fagurhólsmýri is based on reconstructed T at Hólar in Hornafjörður? How much confidence one can have in such a reconstruction is hard to judge without any more details. Thus add a paragraph describing the reconstruction methods in more detail.

p1065-l5: State the statistical measure used here to calculate these percentage values for "variation". Judging from Jóhannesson et al. (2007), variance is used here?

p1065-l7-9: "Our model studies..." Which model studies would this be? The authors do not explain more of this comparison between precipitation gradients and actual spatial precipitation patterns in this manuscript. Was this another study? If so, please cite that study, else explain in more detail what has been done. Where do the spatial precipitation patterns come from which have been used here? How is the conclusion reached that the mountains around Hoffellsjökull do not create local, orographic precipitation

which is relevant to this study? Also, please state the horizontal and vertical precipitation gradients used in this study as all other parameters for the mass balance model are reported.

p1065-l13-15: As stated above, give more detail on how the mass balance and energy balance data was used to tune for the PDD factors.

p1066-l11-21: Aðalgeirsdóttir et al. (2006) report a value of $6.0 \times 10^{-15} s^{-1} kPa^{-3}$. Also stating "...is on the order of..." does not warrant a precision like $6.8 \times 10^{-15}\ s^{-1}\ kPa^{-3}$. Thus state "...is on the order of $6 \times 10^{-15}\ s^{-1}\ kPa^{-3}$. Regarding the citation of the recommended value for A in the case of temperate ice, there is newer literature than Paterson (1994). In the updated version of Cuffey and Paterson (2010, p. 73) a significant lower value of $2.4 \times 10^{-15}\ s^{-1}\ kPa^{-3}$ is stated for temperate ice, which is a mean of 5 models, including the model of Aðalgeirsdóttir et al. (2000). Why do the authors not take the updated value for A into consideration in this manuscript? The two cited papers at this part of the manuscript are from before 2010, so the discussion here is okay, but further on it is not at all clear why the old value of A for temperate ice is used for one model realization, especially since this value is considered by many as too soft and unrealistic.

p1066-l21: Jarosch et al. (2007) do not consider basal sliding at all, so why is this paper cited?

p1066-l22-23: Here a general but brief discussion should be added explaining the non-uniqueness of tuning A (deformation parameter) and C (sliding parameter) to fit measurements. As the authors correctly point out, both of their model realizations (one with a very soft ice and one with stiff ice as well as sliding) produce very similar outcome (c.f. Figure 8b and 9). In principle there is a large number of different combinations for A and C, which result in the same model behavior. The authors report two of them but do not explain how they decide which combination of A and C to use and how the evaluation of the "best simulation" is done. The measurements presented in this manuscript are not enough to distinguish any fitting combination of A and C to be better, as the authors implicitly state on p1071-l6-8. Again here more detail on the work method is needed in the manuscript.

p1067-l8-10: The application of a directly measured geometry as a starting point for such a model is problematic for even more reasons. In addition to the stated issues there is the very likely possibility that the measured geometry is not in balance with the climate forcing of the past few years. So starting with a given, measured geometry and applying a measured climate to such a model will always create transient behavior. It should also be noted that in reality the forcing and the response of the glacier are always transient and that a steady state does never exist. The authors should expand the discussion here to include the possibility of an imbalance with the current climate.

p1067-l15: Did the spin-up start with an ice free geometry?

p1067-l26: A wetter climate is also a possibility, which the authors demonstrate in Figure 8a. Using these results from the steady state simulations to infer some climate conditions needed is problematic in this case. The steady state conditions are reached for any given reference climate after several hundred years of simulation (c.f. Figure 8). Only after those long term steady state (climate forcing) simulations the differences in glacier volume become apartment. These results contradict the results of the transient run shown in Figure 9, which perfectly manages to simulate the measured ice volumes along the way. The success of the transient model run also underlines the comment from above that glaciers reacts to a transient climate.

p1068-l5-7: The importance of cyclonic storms arriving from the southeast for maintaining the volume of Vatnajökull and its outlet glaciers (even just for Hoffellsjökull) can not really be demonstrated by the steady state experiments. Again, the steady state results demonstrate that a fixed climate with no precipitation change but a temperature increase will lead to a 25% smaller ice volume after several hundreds of years when compared with the reference run (red line, Figure 8a). They do not demonstrate that

an extra precipitation source is required in reality to maintain the measured ice volume. The important point here is to not confuse a sensitivity study of the model, driven by long term fixed climate scenarios, and what processes are at work in reality. First of all the switch between the two climate states happens in reality in a transient manner and secondly this transition takes only a bit more than hundred years (1890 - 2000). Figure 8 clearly demonstrates that no steady state with any climate forcing is found in hundred years and thus perfectly demonstrates again the transient nature of the problem. The steady state sensitivity tests the authors make are very worthwhile to understand the behavior of the model but I do not think it is possible to infer any climate conditions in reality from this set of experiment. I suggest, if the authors wish to make such conclusions, they change the climatic forcing in the transient runs over actual time spans, which promises to produce very useful results. In the steady state experiments, the integration time is just too long to make any such conclusions, especially since climate conditions are never constant for such long time scales (hundreds of years).

p1068-l11-14: The functional form of the equations is one reason, but the authors should clearly describe here the non-unique nature of tuning for A (deformation parameter) and C (sliding parameter), c.f. comment p166-l22-23.

p1069-l3: It would be great to see the modeled velocities in Table 2 for the mentioned locations. This would add to the visual comparison of velocities in Figure 6.

p1070-l9-11: Change the respective section in the discussion to reflect the changes made in the manuscript as a response to comments p1067-l26 / p1068-l5-7. Further explain the nature and meaning of the steady state sensitivity runs vs the transient runs in more detail.

p1070-l11-14: Does the model simulate the excavation of the trench (rises again the question how the bedrock is treated in the model) and does the surface lower due to this change. Else this is really speculative and should be formulated as such clearly, or left out.

p1070-l15-24: The discussion of the results with different sets of A and C should include how the authors choose these sets out of a large number of possible solutions leading to the same model behavior.

p1071-l8-12: Again it was never presented in the manuscript how the authors come to the conclusion that $A = 4.6 \times 10^{-15}\, s^{-1}\, kPa^{-3}$ and $C > 0$ is better than the equally performing model with $A = 6.8 \times 10^{-15}\, s^{-1}\, kPa^{-3}$ and $C = 0$. Especially since the currently recommended and accepted value for temperate ice is $A = 2.4 \times 10^{-15}\ s^{-1}\ kPa^{-3}$.

p1072-l3-8: It is really hard to identify these conclusions in Figure 9 (especially why the climate should be random) and so it is not clear if one can draw these at all. Please refer to the comments for Figure 9 below.

Table 2: Include the velocities from the transient model runs in here.

Table 3: The captions states "Displayed errors (random for each year)..." and it is not clear why the errors should be random, are they? Be more specific here.

Figure 3: Could include future glacier geometries from the model. It is probably best to create a somewhat different graph showing measured profiles vs modeled ones.

Figure 7. The term "in the middle" is not scientific. Did you mean the "mean" of 13 climate scenarios? This plot should be presented in quite a different manner. Plots which show just the output of different climate models as individual lines are hard to read. Especially if one model is picked out and it is claimed that this model is "in the middle". I strongly recommend to change the graph to display the actual mean of all models as a thick line and than display one and two standard deviations from the mean in different shadings. This makes the plot more readable and is commonly done in climate science. In this case you need to list all used climate models somewhere in the text.

Figure 9. I would suggest to display the mean of all results driven by different climate scenarios along with one and two standard deviations in shading instead of the

"spaghetti" plots (c.f. comment for Figure 7). In Figure 9b, explain what negative run-off means, because this plot looks more like a mass balance graph (judging from the units). Does negative run-off mean accumulation? If so, state it. The caption is misleading in this case. It states "specific runoff changes (precipitation and glacier melt)", which hints to the display of an anomaly, but what is the reference value in this case?

Technical Corrections

p1057-l25: Replace "...is carried out and the ensemble of climate change scenarios..." with "...is carried out and an ensemble of climate change scenarios...". I am sure there are more ensembles of climate change scenarios besides the one of Jóhannesson et al., 2011

p1060-l1: "...of the older maps..." replace with "...of available older maps..."

p1063-l1: Write out abbreviations like CES and AOGCM once when introduced and check the manuscript for more instances.

p1065-l23: Write Shallow Ice Approximation capitalized as has been done in the introduction. Also it is already defined what SIA means, so please be consistent with the introduction and use of abbreviations.

p1065-l25: Add e.g. into the citation as the papers of Aðalgeirsdóttir et al. are not the only once using SIA.

p1066-l10: Also cite the original paper of Glen (Glen 1955 The creep of polycrystalline ice, Proceedings of the Royal Society of London.) here. Be aware that there is an updated version of the Paterson book, now by Cuffey and Paterson (2010), so the authors might want to use that source instead.

Figure 2: Remove the extra red lines indicated ice divides outside the model domain

for Hoffellsjökull.

Figure 5: Enhance the gray areas. On low contrast screens this shading is not visible.

Figure 7 and 9: The average climate legend text (2000-2010) does not correspond with the caption text (2000-2009). Correct.

Figure 8 l6-8: This part of the caption is confusing. For example a fixed value of $A = 6.8 \times 10^{-15}\ s^{-1}\ kPa^{-3}$ is stated to be used in figure (A) and (B), but in figure (B) the parameter A is varying. Make this part easy to read and correct.

Figure 9: "20th and 21th Century", change to ""20th and 21th Century".

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