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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 #1

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General comments

This paper describes a modelling study, performed to investigate the response of Hoffellsjökull, an outlet glacier of Vatnajökull, to climate change in the 20th and 21st century. Various observations are available for this glacier and are used to calibrate and validate the models used. The model produces a realistic simulation of the 20th century evolution of the glacier, giving confidence in the projections for the 21st century. Such detailed studies of individual glaciers with extensive field data are an important supplement to future projections of glaciers on a global scale. I therefore recommend this manuscript for publication in The Cryosphere, but I would suggest several major improvements to be made.

One issue that needs further explanation is the bedrock topography used in the model.

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The bedrock elevation was measured in 2001, but the authors describe that the bedrock in the lower part changed considerably in the early 20th century, when the glacier excavated a trench. Since there is no account of a changing bedrock in the model, it seems that the authors used the 2001 bedrock for all simulations. What is the effect on the results, especially for the 20th century simulation? And is the bedrock expected not to change in the 21st century?

Furthermore, the surface mass balance deserves more attention in the manuscript. Point measurements are available, but it is not clearly described how these are used to calibrate the model parameters. Area-averaged mass balance is also presented, but seems to be calculated from a very small number of point measurements. How was this done and how large is the uncertainty in these values? A figure showing the annual modelled and measured (seasonal) mass balances for the 20th century would be a useful addition to the manuscript. Moreover, the changes in the mass balance in the 21st century would be interesting to see.

In general, I think that the manuscript should include more model results. The title promises the modelled 20th and 21st century glacier evolution, but only the last figure actually shows these results. The authors used two-dimensional models, hence I would also expect to see more figures with spatial fields, this was only done for ice velocity. Especially the modelled glacier extent in the 20th and 21st centuries would be interesting to show and compare to observations. Besides glacier extent, the authors could think of showing altitudinal mass balance profiles for different years, the 20th and 21st century surface profiles along the same lines as shown in Fig. 3, differences of modelled surface topography and the available DEMs, ice velocities in the 21st century, etc.

The manuscript is well-written and has a clear structure. However, I think the authors should pay some extra attention to a consistent use of numbers and time periods throughout the manuscript. I noticed several inconsistencies (included in the comments below), which can be very confusing to the reader.

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Further questions and remarks are specified below.

Specific comments

1057_24: How near are these stations? This is hard to estimate from Fig. 1a, please give the distance to the glacier or an indication like 'located within ... km from the glacier'.

1060_4-9: What is exactly meant with 'the shape', referred to for both the reconstruction of the accumulation and the ablation area? Do you mean the glacier outline, the elevation contours/curvature, ...? How was this information used to reconstruct the map, was for instance the spacing of the elevation contours assumed similar to more recent maps?

1060_22-27: Has the described change in the bedrock topography been accounted for in the model simulations or is the present-day bedrock topography used for the entire 20th century run? This is not described in the manuscript, while judging from the large change in bedrock topography illustrated in Fig. 3, the effect on the ice dynamics must have been considerable. If a changing bedrock was included in the model, then the method should be described and the bedrock profiles for different years should be shown together with the surface topography in Fig. 3. If the changing bedrock was not included, this should be mentioned and the effect on the results needs to be estimated.

1062_2-4: Do you mean the two mass balance sites on Hoffellsjökull with 'all'? In which years were these measurements made? For the same period as the mass balance measurements described in the previous section?

1062_9-11: It would be more logical to describe the ice velocity measurements in chronological order, starting with the 1936–1938 expedition.

1062_2-13: Only the SPOT5 derived velocities are reported and used for model validation. Please indicate how the other velocity measurements were included in the analysis, can you include an additional figure in Fig. 6 showing the point measure-

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ments?

1061_11: Table 1 appears to give the area-averaged mass balance for Hoffellsjökull. How were these values computed from the small number of stake measurements?

1062_15-17: At which temporal resolution is the meteorological data available? And which temporal resolution of the input data is used in the model, hourly, daily, monthly?

1062_24-28: Does this imply that precipitation was measured at Fagurhólsmỳri was measured from 1924 onwards? Why has precipitation been reconstructed back to exactly 1857, while the temperature has been reconstructed back to 1830 and both are used from 1860 onwards? How good is the correlation of reconstructed and measured precipitation from 1924 to the present? In other words, how reliable is the precipitation reconstruction based on temperature?

1063_2-13: After reading this paragraph, it was not yet clear to me how many future scenarios were used, although I could deduce this from the information given in the next two paragraphs. I would suggest to rewrite the first paragraph to make this clear from the beginning. Perhaps you can start with mentioning that you use 13 future climate simulations, of which 10 are directly from AOGCM runs performed for the IPCC report and 3 from RCM downscaling of AOGCM runs. Subsequently, you can describe the origin and selection of the records in more detail.

1063_11-13: Was this additional temperature increase added as a constant factor over the season and the total period?

1063_7-9: I presume the values presented for the two real station locations are in fact the values modelled for the nearest grid point?

1063_9-11: In the previous sentence you intend to compare the scenarios with the 2000–2009 climate and here you suddenly compare with the 1981–2000 climate, while in the next sentence you mention the 2000–2009 period again. Would it not be more logical to first describe the comparison with the 2000–2009 climate and subsequently

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mention the earlier period?

1063_15-16: Is this 10% compared to the 2000–2009 average?

1064_25: Please provide numbers for the horizontal and vertical precipitation gradients.

1065_12: Only two mass balance sites were mentioned in Section 3.2 and indicated in Fig. 1, where is the third stake located?

1065_10-15: How were the available measurements used to define the optimal values for the model parameters? Although calculated energy fluxes are available, you only need the melt energy or total melt for calibration of the degree-day factors. You do not list measured temperature to be used in the validation, but the temperature records at both AWSs must have been used to check the temperature lapse rate?

1065_7-15: I would like to see a figure with the validation of the mass balance model results, preferably in combination with the modelled mass balance for the entire 20th century.

1066_15-17: Please name these two approaches, for example with a number or letter and use those in the remainder of the manuscript, this would considerably improve the readability of the manuscript.

1066_23: Please provide the optimal values for the flow and sliding parameters.

1066_24-28: Furthermore, the ice divide might not be exactly vertical, but shifted with respect to the surface divide at levels below the surface.

1067_12-14: Move this sentence to the next section (7.2) where you start the non-steady state model simulations.

1067_12-18: Throughout the manuscript both 1890 and 1895 are used to indicate the LIA maximum, which I think is confusing. The confusion mainly arises because the year 1890 has an observed glacier geometry and is associated with the LIA maximum,

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while the simulations start in 1895. I can understand the authors' decision to start in 1895, because the climate variations are large around 1890. Since the difference is only five years, the glacier geometry in 1895 will not be very different from the 1890 observations and no large errors are introduced. But then again the 1860–1890 climate is used to model a steady-state glacier around 1895, while also the climate until 1895 could have been used. To present a more consistent method, I would suggest to more clearly separate the steady-state and 20th century runs. Then you can use the 1860–1890 climate to simulate a steady-state glacier corresponding to the 1890 maximum extent and compare it to the observed geometry. And the 20th century simulation can start in 1895, from either the modelled or observed ice cap geometry.

1067_21: In the introduction you refer to the period 1960–1990 as a period with relatively small glacier changes, while here 1981–2000 is used. In general, please be careful with balance states, since also according to your own simulations, the glacier needs a much longer time to reach dynamical balance. Furthermore, the glacier extent at a given time is a response to the climate in the preceding period, not at that specific time. It would be safer to say that 'the extent of most Icelandic ice caps changed little'.

1067_15-27: I would like to see a figure showing the steady-state glaciers for the two baseline periods, preferably compared to the observed glacier extents in 1890 and 2001.

1068_8-11: Here you can refer to the simple names for the two sliding methods.

1068_16-17: Do you have an explanation why a change in ddf_s has a larger effect? Is there a feedback mechanism involved?

1068_21: In the abstract, the number given is 21%.

1068_21-24: Is this simulation different from the optimal calibration run? Otherwise it is not very surprising that the volume reduction matches the observations... Or were different criteria used for the calibration of the model parameters?

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1068_24-26: Here you can refer to the simple names for the two sliding methods.

1068_26-28: Do both sliding approaches also result in realistic surface profiles?

1069_9-10: Which of the two sliding methods was used for the future simulations? Have you tested whether changing the method has an effect on the results?

1069_8-24: I would like to see more spatial plots of the model results, exploiting the value of using a two-dimensional model. Especially the evolution of the glacier extent during the 21st century would be of great interest, for one future scenario or perhaps two extreme cases.

1070_8: How does modelled volume compare to these numbers?

1070_11-13: I do not understand the argument about the trench, I thought it was excavated well before the 1981–2000 period? And it is still not clear to me whether the bedrock topography in the model included a changing trench or not.

1070_15-22: Here you can refer to the simple names for the two sliding methods.

1070_28: How much lower?

1071_8-12: This conclusion needs better argumentation, because in the previous paragraphs both sliding methods were claimed to give similar results. The two curves in Fig. 9a are also very similar, so I do not understand how these can be used to favour one of the two methods.

1071_20-22: However, the question is whether the degree-day parameters are also appropriate in the future climate with a possible different partitioning of the energy fluxes or a lower albedo. Please include a comment on this issue.

1071_23-26: Figure 9b has been presented and discussed before, please limit the discussion to issues that have not been addressed before.

1071_25-26: It is too simple to state that runoff increases with temperature, because

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later in the 21st century, runoff decreases again while temperature keeps increasing (this is also mentioned in lines 28-29). The total runoff depends on total precipitation, mass balance and glacier area, with the mass balance effect dominating in the first half of the 21st century, while the glacier area is the limiting factor in the second half. It would be interesting to separate these two factors, for example by performing a 21st century simulation without changing the glacier area, then the mass balance effect on runoff can be determined.

1071_8: The manuscript introduction gives a general introduction on the Icelandic ice caps. How do the results for Hoffellsjökull relate to other Icelandic ice caps, are they also expected to disappear by the end of the 21st century? How do the results obtained in this manuscript compare to future simulations for other ice caps or glaciers, either in Iceland or in similar climates?

1076_Table1: I would suggest to replace this table by a figure showing the measured mass balance (winter, summer and net) together with the modelled mass balances for the entire 20th century.

1078_Table3: If you add simple names for the two sliding methods to (c) and (d) in the table, then you can remove (a) to (d) and simply write out the symbols in the caption. Can you also add the simulated area to this table?

1079_Table4: Like in Table 3, I do not see the use of having (a) to (e) in the table. I would suggest to remove them and explain the symbols in the table caption. Is the temperature the annual mean value? Considering the uncertainties, especially for reconstructed meteorological variables, including one digit for temperature and precipitation seems to be precise enough.

1080_Figure1: Which DEM is shown in C? The rectangle in B does not seem to correspond exactly to the area shown in C, in particular the most northern mass balance stake is not included in the rectangle in B.

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1081_Figure2: What is the blue surface in A? Area where no bedrock was measured? Since it is also not included in the legend and the caption mentions that blue colours represent elevations below sea level, I would suggest to replace this with plain white space or something similar.

1082_Figure3: Do I see it correctly that blue line in the legend is a different colour (lighter) than the blue in the figure?

1083_Figure4: This is a very nice picture, but it does not add much to the topic of the manuscript and therefore I would suggest to omit it.

1086_Figure7: Why is the DMI HIRHAM scenario highlighted? It is not used for any specific purposes and the argument that it is near the middle does not hold for precipitation in the second half of the 21st century. However, if you would show results for individual runs (e.g. DMI HIRHAM) in additional figures, then the highlighting would make sense.

1087_Figure8: I would suggest to present the present-day climate (red line) as the reference, since it is present in all three panels. Then the LIA climate would have ΔT=–1°C and ΔP=–0.37 m/a.

1088_Figure9: Include simple names for the two sliding methods in the figure legend, now they have exactly the same description (observed climate). Panel B shows 'runoff change', with respect to which period was the change defined?

Technical corrections

1056_5: add 'the ice cap' before 'Vatnajökull'

1056_6: 'southeastern'

1057_18: add 'two-dimensional' before 'Shallow Ice Approximation'

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1057_18-22: This sentence is too long, please split in two.

1058_15: 'An up to 8 m thick'

1060_4-5: rewrite 'the surface shape in the accumulation zone only changed slightly'

1063_2: write out 'CES' the first time it is used and provide a reference if available

1063_3: write out 'AOGCM' the first time it is used

1063_2: write out 'RCM' the first time it is used

1063_13: remove comma before the reference

1065_11: do you mean 'calibrated' instead of 'validated', since you change the ddfs based on the results?

1065_17: 'similar to that'

1066_1: please be more specific when referring to 'dynamical complications'

1066_13: 'simulated'

1066_23: 'that resulted in the best simulation of'

1067_19: replace 'estimated' by 'reconstructed'

1067_25-26: replace 'at that time' by 'in 2001'

1067_27: 'to maintain a glacier at the 2001 extent. '

1068_5-7: rewrite, e.g. 'This demonstrates that the low pressure cyclonic systems, frequently arriving ... amounts of precipitation, are important to maintain this glacier.'

1068_8-11: rewrite, e.g. 'The results of ... indicate that implicitly taking ... account gives a similar result as including basal sliding explicitly (Fig. 8).'

1069_5: In the caption of Fig. 9 the area is 234 km^2, please be consistent.

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1069_7: Better say 'between 2000 and 2010', since the runoff decreases again later in the 21st century, which is visible in the same figure.

1077_captionTable2: '27 August to 22 September'

1084_captionFigure5: Is this annual mean temperature? Remove the remarks between brackets '(period 1, used ...)'.

1088_captionFigure7: '2000–2010' (or '2000–2009' in the figure)

1087_captionFigure8: The references to Table 1 should become Table 4 and some model descriptions point to the wrong panels.

1088_captionFigure9: '2000–2010' (or '2000–2009' in the figure), and the last sentence can be removed.

Interactive comment on The Cryosphere Discuss., 5, 1055, 2011.