Response to reviewers comments on "Modelling the 20th and 21st century evolution of Hoffellsjökull glacier, SE-Vatnajökull, Iceland" by G. Aðalgeirsdóttir et al.

We would like to thank both referees for their very thorough and constructive comments, we appreciate their effort towards improving the manuscript. In the text below we answer all the raised issues.

Referee #1

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

This is a good point that we did not explain well enough in the manuscript. We did the steady state experiments with and without the trench filled and the resulting steady state volume is the same with the trench filled as with it as it was measured in 2001. We therefore concluded that the trench does not have a large effect on the modelling results.
The trench is the upper part of an over deepened valley carved into the bedrock by glaciers during the past glaciations. Before the LIA advance this valley was filled with loose sediments, similar to the sediment plains in large areas of SE-Iceland. The historical documents describe that the glacier advanced during the early 19th century over a vegetated plain (located at ~40–80 m a.s.l.). The volume difference between a modelled plane and the current Hoffellsjökull bedrock is 1.6 km^3. This volume consisted of loose fine grained sediments that are easily transported by fluvial processes at the glacier sole (e.g. Björnsson, 1996). Typically, suspended sediment loads in glacial rivers are ~1.5 kg m^{-3} (i.e. Rist, 1957, Björnsson, 1996). Assuming the average runoff from Hoffellsjökull in the past 10 years, $0.75*10^9$ m^3 (more realistic average for the past 400 years may be only half of this), and the 1.5 kg m^{-3} sediment load yields a sediment removal rate of $1.12*10^9$ m^3 a^{-1}; or ~3000 years to remove all the sediments, whereas the advance of the glacier over sediments started ~400 years ago, suggesting average sediment load of ~10 kg m^{-3} (similar to that estimated for Breiðamerkurjökull, Björnsson, 1996). Regular jökulhlaups started in ~1840 (Björnsson, 1976, Hjulström et al., 1954, 1955) with increasing frequency and larger volumes in the period 1920-1960 as the glacier lowered and retreated. Jökulhlaups are effective in removing and transporting sediments, sediment loads can be much higher than 10 kg m^{-3}. All this suggests that significant part of the sediment removal and deepening of the trench had happened by the mid 20th century. Because the trench does not have an impact on the steady state modelled volume, it was decided to run the model with the measured 2001 trench in all the model simulations.
The bedrock is not expected to change much in the 21st century as the glacier is modelled to reduce its volume and retreat and therefore no marginal lakes will form and thus no jökulhlaups are expected and the sediment transport capacity will be low.

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.

The original manuscript lacked an explanation of how the specific mass balance was calculated. We have now added text to explain how the values in table 1 are computed and a figure showing all the mass balance measurements, winter, summer and annual mass balance as a function of elevation (new Figure 5).
Since 2001, the surface mass balance have been conducted at 2 locations on the glacier, at 2 other locations very close to the ice divide and on the outlet glacier Breiðamerkurjökull at SE-Vatnajökull close to the model domain (Figure 1). Also, the mass balance was observed at 10 locations, with both good spatial coverage and dense elevation range in the 1930s (Ahlmann and Thorarinsson, 1943). The temperatures during the years 1935 to 1938 was very similar to the temperatures of the last 10-15 years. Furthermore, the observed pattern of the mass balance change with elevation at Hoffellsjökull 1935 to 1938 is the same as observed at Breiðamerkurjökull (see location of stakes at SE-Vatnajökull in Figure 1b). This information is used along with the recent two mass balance stakes on Hoffellsjökull when inferring the mass balance maps from 2000-2010, that are then integrated over the whole area to calculate the values in Table 1.
Also, the modelled mass balance as a function of elevation at 5 time intervals (1900-1909, 1950-1959, 2000-2009, 2050-2059 and 2090-2099) during the model simulation is shown on a new figure (Figure 12) that in addition shows the evolution of the 2D surface mass balance field and the glacier extent, in response to the request for more 2D model results.

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.

This is a very good point that we overlooked when writing the original manuscript. We discuss more results in the manuscript now and we have created a new 2D figure of the surface mass balance at five modelled times that also show the glacier extent at these times. As a part of this figure the mass balance as a function of elevation at these five model times is also shown, which is then comparable to the new figure that shows the available mass balance measurements as a function of elevation.

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.

Thank you for pointing this out, we have fixed the inconsistency and believe that numbers and time periods are consistent in the manuscript now.

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’.

The distances are added to the text as suggested.

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?

We mean the shape of the elevation contours, which is better described by "the spacing of the elevation contours". The text is changed to "assuming that although the surface has lowered significantly, the spacing of the elevation contours in the accumulation zone only changes slightly." And in the ablation area part shape is replaced by "the curvature of the elevation contours."

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.

The effect of the bedrock trench was estimated by running the steady state experiments with and without the trench filled (see discussion above). The resulting steady state volume is the same for both experiments and therefore we concluded that the change in cross section at the bottom of the glacier does not have a large impact on the overall size of the glacier. The trench certainly has an effect on the evolution of the lower part of the glacier, as now there is a proglacial lake forming as the glacier retreats. The effect of the lake is not taken into account in this model study but as the glacier is retreating into the trench and now calving into the lake, it should be considered in further studies.

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?

Yes, velocity has been measured at the two mass balance sites on Hoffellsjökull during the same years as the mass balance measurements. Text is changed to tell this.

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

The order has been changed, starting with the measurements from the 30s.

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 measurements?

A new figure (figure 6) has been added showing velocity profiles at DD' and the measurements at the two stakes since 2001 along with comparison to the SPOT5 and modelled velocity

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?

The method used to compute the specific mass balance in table 1 is now better explained in the text and a new figure that shows all the data used to compute the numbers in table 1; the mass balance measurements from the 1930s on Hoffellsjökull and the more recent measurements on the two stakes on Hoffellsjökull and on the neighbouring Breiðamerkurjökull outlet glacier has been created and added to the manuscript.

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?

The meteorological data are available as monthly mean values, but daily values are created from these by establishing a representative one-year data set of daily mean temperature and daily accumulated precipitation with random sampling of each day of the year from the period 1981-2000 which has daily data series available. This was done to maintain the statistical relationship between the daily temperature and precipitation, which is important for realistic mass balance modelling. This is now explained in the manuscript.

1062_24-28: Does this imply that precipitation was measured at Fagurhólsm`yri 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?

Yes, precipitation measurements at Fagurhólsmýri started in 1924. The reconstructions were done with available data and therefore these apparently random starting years. Figure 5 (now figure 8) has been change to start in 1860 for both temperature and precipitation as this is the starting time for reference period 1, used in the steady state experiments.

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?

The reconstructed precipitation correlates reasonably well with the measured temperature during winter months (September-April), but not well in the summer. When using the same data for reconstruction and testing the correlation is 0.52 for the whole year and 0.59 when only winter months are used. Correlation for each month (jan-dec) is 0.63, 0.56, 0.62, 0.57, 0,0,0,0, 0.5, 0.54, 0.61, 0.62. When every second year was used for reconstruction and the others were used for testing the correlation is 0.5 for the whole year and 0.58 for the winter months (0.5 and 0.56 when the years that were used for reconstructing and testing were swapped). The mean value of the precipitation is well reconstructed, but the annual variability is much less than in the observations, as can be understood by the fact that there is a large scatter in the precipitation that the regression construction will not capture.

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.

Text has been changed as suggested to clarify the number of scenarios in the start of the paragraph.

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

Yes, this is now included in the text

1064_7-9: I presume the values presented for the two real station locations are in fact the values modelled for the nearest grid point?

The temperature and precipitation simulated by the models was averaged over Iceland to create a single time-series intended to represent the climate development for the whole country, the text has been changed to state this

1064_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 mention the earlier period?

The text has been changed and is now in more logical order.

1064_15-16: Is this 10% compared to the 2000–2009 average?
Yes, this is made clearer in the text

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

The numbers for the linear vertical and horizontal precipitation gradients (0.5 m, 0.005 m and -0.008 m per 1000 m in vertical, east and north direction, respectively) are added to the text.

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

There are two stakes on the glaciers measured, and one close to the ice divide outside the glacier, which also was used (hence three), the text is now changed to clarify this.

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?

This was done by using the water equivalent of the melt energy calculated from the weather parameters observed at the AWSs on the glacier (now made clearer in the text).
The statement "… resulted in the same temperature gradient" is wrong (was different in our first MS draft). The DDF model uses temperature observed at non glaciated area away from the glacier along with a free air adiabatic lapse rate. The lapse rate deduced from the observed temperatures at 2 m above the melting surface (within the glacier boundary layer) is generally lower than of the free air and cannot be used in this study to evaluate the optimised free air lapse rate of the applied DDF model. Hence, our in-situ data cannot be used to evaluate the temperature gradient of the DDF model. The sentence has been changed the "By assuming the same temperature gradient, we found ddf_s = …"

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.

A new figure (Figure 12) that shows the 2D mass balance field at 5 model times and the modelled mass balance as a function of elevation at 5 time intervals (1900-1909, 1950-1959, 2000-2009, 2050-2059 and 2090-2099) during the model simulation is created and added to the manuscript. The model results compare well with the available mass balance measurements that are also shown in a new figure (new figure 5).

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.

This is a good suggestion, the two methods have been given numbers MI and MII and we refer to these in the following text.

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

The values were given in figure 8, they have now been added to the text.

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.

Yes, this is true, text has been changed and is now:
"This is not an entirely realistic boundary condition as some ice flow may in fact occur across topographic ice divides and it can be expected that the ice divides of the eastern outlets of the Vatnajökull ice cap can shift as a consequence of the response of the ice cap to mass balance variations. As a first approximation, no ice flow across the topographic is assumed (consistent with the SIA formulation of the ice flow model) and fixed boundaries of the main ice flow basins may be assumed to be reasonable since the location of the ice divide is to a large degree controlled by the basal topography. However, the assumption of fixed ice divides may be expected to become increasingly inaccurate when simulated changes in the geometry of the glacier become relatively large compared with the original size of the glacier."

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

Done

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, 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.

The steady state experiments are now better separated from the 20th century runs and to reduce confusion of years we use LIA_{max} to denote the maximum volume of the glacier, rather than stating a year.

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'.

The text is changed in several places (including caption for Figure 5) to state that "the surface mass balance is close to zero", rather than "in balance" as a response to this comment.

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.

A figure showing the steady state glaciers is not added, but rather a 2D figure showing the modelled mass balance, as well as the extent of the glacier at 5 modelled times.

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

Done

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

One possible explanation is that the ddfs is affecting larger area (the accumulation area) and melting more snow will raise the ELA, which will reduce albedo and increase melt. Text is added in the paper to suggest this as an explanation.

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

The number is now changed to approximately 20% in both places

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?

This simulation is made with the calibrated model. We use the surface mass balance measurements to calibrate the degree-day model and the steady state and transient model runs compared with the observed volume and topography of the glacier (LIA_{max} and present day) to calibrate the flow parameters of the model. The fact that the transient model run simulates well the observed volume changes during the whole 20th century confirms that the model is well calibrated and gives us confidence in running the model with the future climate scenarios.

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

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

Yes, and therefore only one method was used for the future runs.

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?

Method MI was used for the future simulations. The difference between the runs using the two methods is much smaller than the difference resulting from the different climate forcing applied with the model and therefore it was decided not to repeat the runs with MII.

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.

We have added a new figure (Fig. 12), showing the surface mass balance and glacier extent in 20^{th} and 21^{st} century, from the run forced with the DMI-HIRHAM output, which is approximately in the middle of the range of the different models.

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

The comparison between the modelled volume and observed for both methods MI and MII is in Table 3. The modelled volume changes are larger, the numbers for MI are 1.9 km^3 and 2.4 km^3 for 2001-2008 and 2001-2010, respectively and for MII they are 1.8 km^3 and 2.3 km^3 These numbers and the following text are added to the manuscript "This is a larger volume change than the direct observations indicate for this decade, but it should be kept in mind that the model has been calibrated to simulate the observed volume changes for the whole century and short term variations in the volume that may be a result of mechanisms omitted by the model may not be accurately reproduced."

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.

The model was run with a constant trench as measured in 2001. Steady state model runs were done with and without the trench and the resulting volume is the same for both, indicating that the shape of the glacier "funnel" at the end of the glacier, that is small, relative to the total size of the glacier, does not have a large impact on the model results. The discussion about the trench here is deleted from the manuscript as it was confusing.

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

Done

1070_28: How much lower?

The value is 2.3 10-15 $s^{-1}kPa^{-3}$, which is about a third lower than the value recommended by Paterson (1994). This is added to the text. See discussion below about the rate factor and the newly recommended value by the new version of the glaciology text book (Cuffey and Paterson, 2010).

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.

The text has been change in response to this comment and comments from reviewer 2, we do not favour one of the two methods now, as it is not possible to determine which one is better simulating the glacier evolution. As pointed out by the second reviewer there are infinite combinations of rate factors and sliding parameters that would give similar results, and we have chosen one.

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.

Study of the energy balance of both Vatnajökull and Langjökull ice caps have been published in (Guðmundsson et al. 2003; Björnsson et al., 2005 and Guðmundsson et al., 2009a). In addition, detail comparison of the performance energy balance and degree-day models as well as the variability of degree-day parameters with surface type, elevation, season and warmer climate has been carried out by Guðmundsson et al. (2003) and (2009a). We have now added a new paragraph to discuss this (by referring to the previous work of Guðmundsson et al. and Björnsson et al.).

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

Done

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

The text has been changed to explain that the runoff increases only during the first part of the 21^{st} century. Yes, it would be very interesting to run the model without changes in the glacier area, this has been done for other glaciers in Iceland (Langjökull and Hofsjökull) and the results indicate that the first half of the 21^{st} century the runs are similar, but subsequently the glacier loses more volume when the elevation is allowed to change than when the elevation is constant.

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?

A paragraph about the comparison with other glaciers in Iceland is added to the conclusion at the end of the paper.

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.

The table is kept in the paper as it is the mean specific mass balance computed with the method described in the paper, but a figure showing the measured mass balance as a function of elevation is added to the paper.

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?

The simple names MI and MII are added to the table. The areas are not added to the table, but the extent of the glacier is shown in the new 2D figure of the SMB.

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.

(a) to (e) are removed, mean annual added and the second digit for temperature and precipitation removed.

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.

The 2001 DEM is shown in C, this information is added to the figure caption. The northern margin of the rectangle in B was a little further south than C showed, this has now been fixed.

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.

Yes, the blue area in A did not have bedrock measurements, in the meantime these measurements have been made and we have created a new figure showing the bedrock for the whole area.

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

No, the blue colour in the legend is exactly the same as 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.

Yes, this is a very nice and impressive figure because of the details it shows, that we would like to keep in the paper. The DEM from 2010 is the most accurate available, shows very detailed surface topography and has been used a reference for co-registering the SPOT5 HRS-DEM, and provides the 2010 point in figure 10 and therefore we think it is reasonable to keep the figure in the paper.

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.

Yes, the results of this run are now shown in the new figure of the 2D mass balance and glacier extent (figure 12), so we keep the highlighting of the DMI-HIRHAM scenario.

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 _T=−1_C and _P=−0.37 m/a.

We keep the figure as it is, as the runoff plot also uses the starting condition (1895) for the reference, then all figures are relative to the same reference.

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?

The simple names have been added. The runoff change is with respect to the 1895 runoff, this information is added to the figure caption.

Technical corrections

1056_5: add 'the ice cap' before 'Vatnajökull'
Done
1056_6: 'southeastern'
done
1057_18: add 'two-dimensional' before 'Shallow Ice Approximation'
Done
1057_18-22: This sentence is too long, please split in two.
Done
1058_15: 'An up to 8 m thick'
Done
1060_4-5: rewrite 'the surface shape in the accumulation zone only changed slightly'
Done
1063_2: write out 'CES' the first time it is used and provide a reference if available
Done
1063_3: write out 'AOGCM' the first time it is used
Done
1063_2: write out 'RCM' the first time it is used
Done
1063_13: remove comma before the reference
Done
1065_11: do you mean 'calibrated' instead of 'validated', since you change the ddfs based on the results?
Yes, text is changed and more detail added
1065_17: 'similar to that'
Done
1066_1: please be more specific when referring to 'dynamical complications'
The text has been changed and "dynamical complications" taken out
1066_13: 'simulated'
Done
1066_23: 'that resulted in the best simulation of'
Changed
1067_19: replace 'estimated' by 'reconstructed'
Done
1067_25-26: replace 'at that time' by 'in 2001'
Done
1067_27: 'to maintain a glacier at the 2001 extent. '
Changed
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.'
Done, thank you
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).'
Done
1069_5: In the caption of Fig. 9 the area is 234 km2, please be consistent.
234 km is correct (see Table 3). This has now been corrected.
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.
Changed to between 2000 and 2050
1077_captionTable2: ’27 August to 22 September’
Done
1084_captionFigure5: Is this annual mean temperature? Remove the remarks between brackets ’(period 1, used ...)’.
yes, mean annual added and remarks removed
1088_captionFigure7: ’2000–2010’ (or ’2000–2009’ in the figure)
Changed to 2000-2009
1087_captionFigure8: The references to Table 1 should become Table 4 and some model descriptions point to the wrong panels.
This is fixed
1088_captionFigure9: ’2000–2010’ (or ’2000–2009’ in the figure), and the last sentence can be removed.
Changed to 2000-2009

Referee #2

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.

This is a good point, also raised by referee 1, we have added 3 new figures, one showing the measured mass balance as a function of elevation (new Figure 5), one showing the measured and modelled surface velocity (new Figure 6) and finally one showing the modelled 2D surface mass balance fields, as well as the glacier extent at 50 years intervals modelled with method MI and the DMI HIRHAM ECHAM5 climate scenario (Figure 12). We show also in Figure 12 the modelled mass balance as a function of elevation, which is comparable to the measurements in (new) Figure 5. The observation data set for this glacier is exceptionally good and we therefore want to give those data good space in the paper.

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?

Yes, a newer reference has been added and the older removed.

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.

The discussion on the selection of flow model on p1066 has been changed to discuss that the focus of the study is not to model the detailed flow in the area around the ice fall, but the advance and retreat of Hoffellsjökull and therefore it is appropriate to apply a SIA model.

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.

After some consideration it was decided not to add a figure showing the modelled surface at different times, but rather the surface mass balance and the glacier extent, as well as the smb as a function of elevation (new figure 12). The performance of the model can be judged from how well the volume evolution of the 20th century is simulated, as shown in Figure 11.

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.

The historical records describe the land use during 18^{th} and 19^{th} century, this is added to the text. The model has been run with the trench as measured in 2001. We did steady state model runs with the trench filled and got the same steady state volume and conclude that the trench does not have a significant impact on the result (see discussion in response to comments from reviewer 1). This is now explained in the paper.

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 actually calibrated with this data in section 5.1

Text has been added to the manuscript to explain how the PDD model is calibrated in section 5

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.

The reconstruction method is explained in more detailed now and the correlation values in winter and summer are given. As explained in the answer to reviewer 1 above, the correlation is much better between temperature and precipitation in the winter months, than in the summer months. We have good confidence in the reconstructed temperature, which has been created using iterative expectation-maximization (EM) algorithm (Dempster *et al.*, 1977) with temperature records from 8 stations around Iceland. We do regression for the temperature and precipitation for the monthly values for the period 1931-2009 and get correlation of about 0.5-0.6 during winter months, the annual variability of the precipitation constructed with this method is smaller than observed.

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?

Yes, variance is used, text has been changed.

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.

The horizontal and vertical precipitation gradients have been determined in another study for the whole of Vatnajökull ice cap (Johannesson, 2007) and we use these gradients in our study. The values for the gradients are added to the text p1064 line 5. The paragraph about the precipitation pattern is changed and statement about the mountains around Hoffellsjökull not creating local precipitation pattern has been removed.

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.

The text has been changed and more details on how the surface mass balance model is calibrated added.

p1066-l11-21: Aðalgeirsdóttir et al. (2006) report a value of 6.0×10^{-15} s^{-1} kPa^{-3}. Also stating "...is on the order of..." does not warrant a precision like 6.8×10^{-15} s^{-1} kPa^{-3}. Thus state "...is on the order of 6×10^{-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 2.4×10^{-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.

This is a good point and discussion of the updated lower rate factor for temperate is added to the manuscript, with a reference to Cuffey and Paterson (2010). In a model study for Hofsjökull ice cap (Aðalgeirsdóttir et al., 2006) the best fit for the shape of the ice cap was found with the soft ice parameter (implicitly including basal sliding) and therefore we use that value also in the present study. As discussed below there is not a unique set of parameters, A and C, which will give the best fit and therefore the simulation for the 20th century carried out with two sets of parameters. With the data available it is not possible to determine the value, so we take out that discussion in the conclusion section.

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

Reference taken out here, it is used in the discussion of model studies of the recovery of depressions in the surface of Vatnajökull page 1070.

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.

A brief discussion has been added here to explain that many combinations of A and C will fit the observations and we select one by fitting to volume evolution of the 20^{th} century the best. The preference of one combination of A and C on p1071 is taken out.

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.

The text has been changed to include both points

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

No, we started with the measured LIA_{max} geometry, as shown in Figure 8 (new figure 10).

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.

Wetter climate is added to the text. It is a good point that the steady states are reached after several hundred years of simulation and therefore do these runs not really indicate what climate conditions are needed to maintain the glacier. The text is changed and reads now "This result indicates that the measured glacier in 2001 is still responding to the colder and/or wetter climate than the average of 1981-2000 and is adjusting to the changes in the climate forcing."

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).

Good point and we totally agree. This is in fact the reason why we did start our simulation with observed geometry (reflecting transients) rather than steady state spinup (that smoothes out all transients). The sentence about the cyclonic storms is taken out of the discussion of the steady state experiments and following added: "This model result shows the relative importance of the precipitation forcing for maintaining the glaciers in this area"

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.

This paragraph is changed and discusses now the non-unique nature of the tuning for A and C

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.

A new figure (figure 6) is added, showing the measured and modelled velocities at profile DD` and at the mass balance stakes.

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.

This paragraph is changed and reflects the changes in the manuscript.

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.

The model does not simulate the excavation of the trench and the discussion in the paper is changed accordingly. This statement is taken out of the paper.

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.

There is not enough data to choose one set out of the large number and that statement is now taken out

p1071-l8-12: Again it was never presented in the manuscript how the authors come to the conclusion that $A = 4.6\times10^{-15}$ s^{-1} kPa^{-3} and $C > 0$ is better than the equally performing model with $A = 6.8\times10^{-15}$ $s^{-1}kPa^{-3}$ 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}.

The text has been changed to state that the parameter selection is non-unique

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.

The text has been changed, random is taken out, the point is that the large interannual variability of the climate forcing results in interannual variability in the runoff that is of similar magnitude as the increase due to increased temperature, the sentence now reads "It is found that until ~2030, the large interannual climate variations lead to interannual runoff variations of a similar magnitude as the average runoff increase with respect to the period 1981–2000."

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

The model velocities are shown in the new figure 6

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.

The errors of the volume estimations can be divided into i) errors of each surface DEM (due to bias of each DEM) and ii) bias of the bedrock map (same for all the years). The bedrock bias shifts all points (volume numbers) by the same amount in the same direction while the DEM errors are of random nature between different maps. Due to this different nature of the errors, they cannot be mixed, and need to be displayed and interpreted independently. The table caption has been modified to make this point more clear.

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.

A new figure showing the extent of the glacier as well as the modelled surface mass balance as 2D field and as a function of elevation is added to the paper, therefore it was decided not to show the glacier geometry as well.

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.

We highlight the DMI-HIRHAM model output because the results from that run is now used to demonstrate the model output in new Figure 12, "in the middle" is taken out. We do not agree with the suggested change of the figure. We think there is interesting information in the actual variability of the applied scenarios in this figure, which gives a better impression of the uncertainty in the model output (shown in Figure 11) than the average with standard deviations.

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 runoff

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?

Again, we think that it is more informative to show the variability of the model output with the different scenarios rather than showing only the statistics. It gives a better impression of the uncertainty due to the different climate scenarios.
The runoff anomalies are calculated relative to the runoff at the start of the simulation (1895) and therefore negative runoff is not accumulation, but lower value than this reference value. The caption is changed to explain this.

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
Done
p1060-l1: "...of the older maps..." replace with "...of available older maps..."
Done
p1063-l1: Write out abbreviations like CES and AOGCM once when introduced and check the manuscript for more instances.
Done
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.
Changed and is now consistent
p1065-l25: Add e.g. into the citation as the papers of Aðalgeirsdóttir et al. are not the only once using SIA.
Done
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.
Reference to Glen, 1955 and Cuffey and Paterson, 2010 is added.
Figure 2: Remove the extra red lines indicated ice divides outside the model domain for Hoffellsjökull.
The lines and the colour as suggested by Reviewer 1 have been changed in the Figure
Figure 5: Enhance the gray areas. On low contrast screens this shading is not visible.
The gray shading has been enhanced.
Figure 7 and 9: The average climate legend text (2000-2010) does not correspond with the caption text (2000-2009). Correct.
Corrected
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.
This was wrong, caption is now fixed
Figure 9: "20th and 21th Century", change to ""20th and 21th Century".
Changed