## Response to Reviewer 2

We are grateful to the reviewer for the friendly and constructive review supporting publication of our paper. Your valuable remarks and suggestions will greatly improve our manuscript.

The comments by the reviewer are in indented blocks and italic fonts.

## Response to general comments

> *It is not clear, at least to me, how you deal with ocean-ice interaction. Does SICOPOLIS simulate any ice shelves in your experiment? If yes, what do you choose as subshelf melting rate? Please make this clearer.*

In the model setup for this paper, SICOPOLIS does not simulate ice shelves and does not treat ice ocean interaction. Bi-directional ice ocean interaction was never planned in IGLOO, as can be seen in Fig.1. In the paper, changes in the ocean temperature are prescribed from data (Section 2.8) to the turbulent plume model. Impact of ocean temperature and subglacial ice discharge on submarine melting is examined in offline mode with the turbulent meltwater plume model. Concerning clarification about ice shelves, we will add more explanation to the paper.

> *Whilst the different model sub-components are generally well described I had difficulties in understanding what is the 1-D glacier model presented in Figure 1. My guess is that it corresponds to the coupler between the plume model and SICOPOLIS? Similarly as for your other arrows, you could add in Fig. 1 what is exchanged between the 1-D glacier model and the other components. In particular I do not understand what the right-to-left arrows stand for since the models are not interactively coupled yet.*

We will improve Fig. 1 by adding the variables for the submerge part of the outlet glaciers and for their submarine melt. Additionally, we will refer to Beckmann et al. (2018), who explains more details on the 1-D glacier and plume models, as parts of IGLOO. As noted in the paper, the coupling between the ice sheet and the outlet glaciers has not been implemented into IGLOO yet. To clarify this further, we will make the exchange arrows between the ice sheet and the outlet glaciers in Fig. 1 dashed now.

## Response to specific comments

> *P3L29-30 Please show on a map where these two glaciers are located.*

We will add a new figure and will refer to it at the suggested place in the main text.

> *P6L7 How is defined the "shape of the glacier front"?*

We meant the submerged part of the outlet glacier and will change the term accordingly.

*P7L8 "surface melt/runoff" ! It is not clear what this is. I assume it is the runoff provided by the MAR model (_rain minus retention due to refreezing?) and not surface ablation only?*

Right and understood: melt is not synonym of runoff. It is the runoff, in our case from the MAR model. We will erase the word "melt".

*P7L8-9 In your framework, you use the routing scheme of HYDRO (i.e. based on effective pressure) to route the water generated by surface melt. A fair amount of surface melt could be routed using surface gradient instead. Could you comment on how this can affect the pattern of discharge to the ocean?*

We fully agree with you that the routing scheme in HYDRO can lead to different patterns of subglacial discharge reaching the ocean compared to a scheme, which uses surface gradients only. However, in case of complex bedrock with deeply incised structures, the routing scheme in HYDRO is more accurate. Apart from surface melt, there is also basal melt, which is important for winter and which our model accounts for.

*P7L29-30 The gradients are not well defined for the accumulation regime because of precipitation that has a much more complex spatial pattern than temperature (and by extension ablation). I would guess that the vertical gradients for runoff are not well defined neither since a large part of runoff is composed by liquid precipitation. Could you comment on that?*

The complete sentence reads: "For the surface mass balance, we apply the gradient method only to the ablation regime, because the regression is in many cases not well defined for the accumulation regime (Helsen et al., 2012)." We evaluated the data on surface mass from the MAR regional climate model and made the observation that the SMB gradients in the ablation area are much better defined compared to the gradients in the accumulation area. The issue can be seen in Fig. 2 by Helsen et al. (2012) too. The data in accumulation area appears noisier compared to the data in the ablation area. Therefore, it makes sense to determine a regression line solely in the ablation area, while in the accumulation area one can just assume zero gradients. We could add more explanation on this to the paper.

*P8L8-9 How is the surface temperature elevation gradient computed?*

We compute the surface temperature gradient via representative local gradients inside a search radius, as Helsen et al (2012) did for surface mass balance. This is in further detail explained in Section 2.5. We will improve Sections 2.5 and 2.6 and will better interlink between the sections relevant for this issue.

*P8L25-27 A list of limitations of such an approach is welcome here, thanks. However I think you should expand more on the discussions of these. In particular, I think that taking into account the free-evolving topography during past cycles will have a large impact on simulated temperature profile as the ice thickness has considerably changed during the last termination (Vinther et al.,* *2009) and the stress regime will be largely different with an ice sheet extended towards the continental shelf in glacial conditions. An other limitation of the SMB anomaly method to drive the spin-up is that an artificial SMB term is used to compensate all the model deficiencies in terms of ice dynamics. A discussion on these points would be much appreciated.*

We will extent our discussion on the model limitations. The point on elevation changes during last glacial cycle is already in the list on page 8, lines 25-27. We will include Holocene in the discussion of model limitations. Concerning our method with implied SMB, we will add a paragraph to the discussion section, where we will discus the advantages (e.g. little drift due to a simulated surface elevation close to the observed surface elevation) and the deficiencies, which could be caused if the SMB correction becomes too strong. To point on this, our investigation of the impact of the relaxation constant on surface elevation and implied SMB inspects already the method. For the choice of the relaxation constant, we made a trade off to yield a surface elevation close to observations and at the same time make the correction via the implied SMB as small as possible. We will widen our discussion of the method.

*P9L27-29 Please rephrase.*

We will rephrase the sentence.

*P10L9 Can't we expect a regional freshening due to the Greenland ice sheet melt? Why this could not be tested here as well with idealised scenarios (as for temperature)?*

As IGLOO does not include interactive ocean, we are not able to test this hypothesis.

*P10L22-24 The change in resolution certainly has an impact on the stress regime simulated by SICOPOLIS. Could you compare the state of your Greenland ice sheet (internal temperature) and your inferred surface mass balance (Mimpl) for present-day at 5 and 10 km resolution? Maybe you could add a few words on why doing the spin-up at a coarser resolution is not a problem in your case. It could also be interesting to have the future projections at 10 km resolution.*

We needed the switch in the resolution to be able to perform the palaeo runs for model initialization within a reasonable computing time. We also needed many calculations to be able to adjust the model parameters. Such a switch is always a compromise between accuracy and computational time. To address your question on 5 and 10 km resolution, we attached a figure to the end of this response (Figure S1). As seen in Fig. S1, our fields with the switch to 5 km resolution show finer structures compared to the fields from the run in 10 km over the entire glacial cycle. In particular, the temperate basal regions have a much finer structure now and resolve much better the outlet glaciers and their catchment areas. It is evident that the base of outlet glaciers should be temperate in reality, what is resolved better in 5 km resolution in our model, even with the switch. For climate projection, we believe that the inaccuracy resulting from our switch in resolution (and the other switches) is small compared to the uncertainties in the scenarios and the choice of GCM forcing. For the final paper if granted, we could further investigate the problem of resolution change. For sure, we will add discussion on this point to the paper. Concerning future projections in 10 km resolution, we think that this could be problematic, because the SMB data provided by MAR just are in 5 km resolution. For the future projections, this 5 km resolution is the best possible choice if we use the MAR data. A resolution of 10 km would be less accurate. For resolving the ablation area, resolution is especially critical. In lower resolution, we will loose grid points with negative SMB, because the ablation area is rather narrow. For the temperature field used for 10 km spin-up this is less problematic.

> *P10L25-26 Similarly to the change in resolution: have you tried to switch the hybrid mode before 500 years or after? How big is the impact on the simulated Greenland ice sheet? I assume that the thermal regime might be largely affected by the change in dynamics …*

We did several computation concerning optimal points in time for the changes in regime. They led us to the conclusion that we need the last two switches (one for change in stress regime and another one for change from relaxation surface to free surface) to happen at different times, in order to avoid a model shock. On this issue, we will make investigations that are more systematic. Most probably, an earlier switch for change in stress regime will not make too much difference. Important to note, that we need at least these two switches, because we cannot effort to run the ice sheet model in the hybrid mode over the entire glacial cycle.

> *P11L4-5 Would it make sense to discuss the RMSE in SMB instead of the total difference in SMB?*

The choice of the measure is often arbitrarily. However, in our case, we used total difference in SMB, because we can compare it with total SMB from simulations with regional models of the Greenland ice sheet. Indeed, we use the difference in SMB for discussion in the discussion section.

> *P11L6-11 In the paper it could be worth discussing the spatial pattern of your difference in surface elevation and surface mass balance. From b-c we can really see that you have an important model drift at the margins: except for the NEGIS region your velocities seem too high (confirmed by Fig. 5) leading to negative surface elevation difference (compensated by artificial positive SMB anomaly). Related to this, why could you not find a Cb value minimising this model drift?*

We will add more discussion about our difference in surface elevation and surface mass balance to the paper. Indeed this is missing, as we solely focussed the text on the dependence of the field patterns on the relaxation time. Concerning impact of Cb value on implied SMB, we have not investigated this in very detail so far. As stated in the paper, we find our Cb value by minimizing the RMSE in surface velocity. We regard the surface velocity higher that 50 m/yr (as stated in paper) as the relevant measure for optimizing the basal sliding coefficient Cb, because sliding mostly affects fast flow. In our approach, we use MAR SMB and observed surface elevation to find an appropriate relaxation constant (see Fig. 3). We think that this is reasonable, because the relaxation constant affects initial fields of surface elevation and implied SMB, which we use for the projections. Furthermore, with this method the total drift (measured in ice volume) is negligible, what is relevant for our sea level rise projections.

> *P11L13-14 Do you need to spin-up HYDRO as well?*

HYDRO does not need any spin-up, as it is a diagnostic model. We will add a clarifying sentence on this matter to Section 2.2.

> *P11L24-25 The simulated surface velocity does not seem smaller to me when you look at the western flank of the ice sheet.*

We meant the ridges and not the flanks. We will reformulate like: “The simulated surface velocities (smaller than 2 m/yr) along the ridges are somewhat smaller compared to the observed ones (often larger than 2 m/yr)”. If necessary, we will further improve the paragraph.

> *P11L26-27 Please reformulate.*

We will fix the sentence.

> *P11L29-30 Why?*

In this paper, we operate SICOPOLIS in a setting, which does not treat ice shelves. We will clarify this.

> *P12L18 There is no change in ice volume visible but it does not mean that the ice thickness is not changing as you have compensating errors.*

We are aware that in principle there can always be regional biases, which compensate each other in total. In our approach, the implied flux compensates particularly the regional errors of the ice sheet model. Indeed inspection of evolution of the ice thickness field could be used to refine our approach. We are grateful for this hint. In this paper, we use ice volume as indicator for the quality of our approach. As the change in ice volume in the run forced with implied flux only is very small, we regard ice volume as indicator as far sufficient.

> *P12L20-21 How this correction has been made? Do you use a point by point correction of ice thickness or do you simply use a correction of volume based on an averaged number? If the latter, how large would have been the difference when using the point by point correction?*

We subtract the ice volume gained by the run forced with implied flux only from the ice volume resulting from the projection runs. This is done for every point in time. We will clarify this in the paper.

*P12L30 The effect is stronger with RCP8.5 when looking at the absolute value but relative changes are in fact smaller. Which is in agreement with Vizcaino et al. (2015).*

This is correct. Thank you very much for finding this. We will add some sentences about that.

*P28 Fig. 3 Maybe you could add to these the evolution of total volume and RMSE of SMB for the different relaxation time.*

As there is already a panel for the difference between the totals for simulated and observed SMB, this would not give to much new information. We would like to keep the figure as it is.

*P29 Fig. 4 Could you add more levels to your colour scale?*

We will add more levels to both colour bars.

*P31 Fig. 6 Do you have floating points? If yes, you should highlight them on this plot.*

In our model setting, the ice is restricted to land and cannot move into the ocean. Therefore, we do not have floating points.

*P32 Fig. 7 Could you comment on why you have sub-glacial lakes when the base is frozen?*

In our setting HYDRO computes the hydrological potential over the entire area of the ice sheet, although it is possible to restrict the computations to temperate basal regions. Such a restriction can lead to blocking effects and the basal water fluxes are hampered to reach the ocean in region where it should reach the ocean. If the hydrological potential is defined over the entire ice area, we are just on the safe side. Because we only allow basal sliding over temperate basal areas, it will make little difference to the ice dynamics. However, the basal water is more likely to reach its place in the ocean. By the way, our projected positions of present-day subglacial lakes correspond with findings by Livingstone et al. (2013). These positions are due to sinks in the hydrological potential. In this context, we would like to point out the great uncertainty in the determination of the thermal state of the Greenland basal ice (MacGregor et al., 2013).

## Technical corrections

We will address the technical corrections appropriately.

## References

Beckmann, J., Perrette, M., Beyer, S., Calov, R., Willeit, M., and Ganopolski, A.: Modeling the response of Greenland outlet glaciers to global warming using a coupled flowline-plume model, The Cryosphere Discussions, pp. 1–32, doi:10.5194/tc-2018-89, in review, 2018.

## Figures

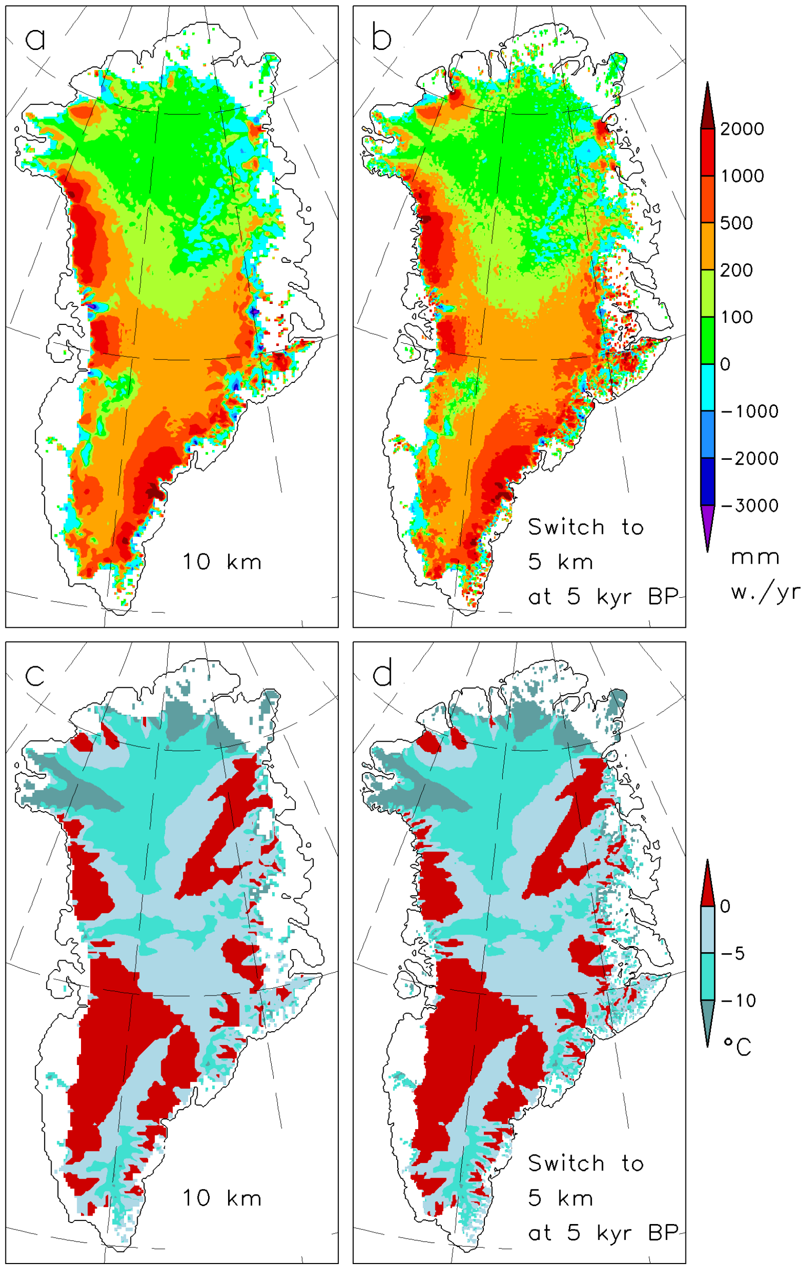


*Figure S1: Present-day fields of implied SBM (panels a and b) and of basal temperature corrected for melting point (panels c and d). Left: 10 km × 10 km horizontal resolution over the entire glacial cycle. Right: Switching from 10 km × 10 km to 5 km × 5 km at 5 kyr BP.*