We would like to thank the reviewer's (James Lea and Anonymous Referee#2) for their constructive review and feedback on our paper. Below you will find our response (in black text) to the reviewer comments (in blue text) and the details regarding changes made to the paper following their recommendations.

Reviewer #2, Anonymous Referee

Cuzzone et al. present have simulated the deglaciation of the Godthàbsfjord region using different climate forcings (temp, precip.) including a calving laws. Their results show that the Holocene deglaciation was primarily dominated by changes in surface mass balance, whereas calving is less important. The simulations are compared to geological reconstructions of the deglaciation. Overall, the paper reads well and presents some new and interesting aspects on how to simulate ice sheet evolution (as far as I can tell as a none-expert in ice sheet modelling). I only have a few major comments and some additional minor comments to the manuscript.

Thank you for your review of our work. Below we address your concerns and identify in text where changes have been made.

Major comments:

I am surprised that it is not quantified how much of the deglaciation is forced by SMB and calving. Most places it is vague formulated like "significant influence", "strongly impacted by" or "less important contribution from". Is it possible to be more specific i.e. say xx% from SMB and yy% from calving?

We understand the concern and interest in trying to partition the relative contribution to the deglaciation from SMB and iceberg calving. Unfortunately, this is very difficult to do as the SMB and calving are interconnected. For example, as the ice surface elevation changes in response to SMB or calving, the ice dynamics will change, impacted calving and SMB alike.

Our experimental setup and implementation instead allowed us to capture the influence of iceberg calving on the deglaciation across this domain in a relatively simple manner. By running the transient simulations with and without the iceberg calving turned on, we can directly compare the simulated deglaciation ages. These experiments conclude that iceberg calving played a negligible role on the deglaciation across the KNS forefield as captured by the geologic constraints.

It is unclear to me if they account for changes in sea surface temp in the fjords? Changing SST should influence the calving rate, but it is unclear to me if this is accounted for in the model. It is known from paleoclimate data that the SST changes significantly during the Holocene and this could have played in significant role on the ice retreat in particularly in fjord settings like the Godthàbsfjord region (see review by Axford et al2021).

While SST records exist and indicate variations across the Holocene, the ice front would be more susceptible to changes in sub-surface ocean temperatures, of which data is limited and lacking. Likewise, estimating basal melt rates is difficult although approaches are being implemented in ice flow modelling currently. Because of these limitation in data, we did not include variation in ocean temperature, and discuss this in Section 5.3 (Model Limitations). We added an additional sentence (Line 647: "While proxy records indicate changing sea surface temperatures during the Holocene proximal to our model domain Axford et al. (2021), due to a lack of constraints on the long-term subsurface ocean thermal forcing needed to implement undercutting in our simulations, we opted to disregard this.”). We also discuss here that although we do not include the influence of melt rates on the vertical calving face of the ice front, we adjust the von misses stress threshold to a lower value (600 kPa) which would likely account for the possible role of oceanic melt and undercutting. While lowering this threshold showed no appreciable influence on the deglaciation or mass loss, it is not absolutely conclusive that the ocean and variations in sub-surface temperature played a negligible role. We are currently addressing this as part of a much larger project, where we use a model PICOP coupled to ISSM (Pelle et al., 2019) to estimate melt rates from far field temperature and salinity changes.

To this end we included this statement in the limitations (Line 656: “Future work will use a basal melt-rate parametrization (PICOP; Pelle et al. 2019), employed in ISSM currently, to estimate oceanic melt rates from far field variations in Holocene subsurface temperature and salinity in order to more robustly estimate the impact of oceanic warming Holocence deglaciation across the GrIS.”)

Pelle, T., Morlighem, M., and Bondzio, J. H.: Brief communication: PICOP, a new ocean melt parameterization under ice shelves combining PICO and a plume model, The Cryosphere, 13, 1043–1049, https://doi.org/10.5194/tc-13-1043-2019, 2019.

The 9.3 ka and 8.2 ka re-advance events have been recorded north of the study areas around Jakobshavn Isbræ (e.g. Young et al2011). In the Godthaÿbsfjord region there is no evidence of a readvance during these two cold events – neither in the geological data or in the simulations (as far as I can tell from the figures). It would be interesting if it could be discussed why the ice sheet in this Godthaÿbsfjord region did not react to these events. One possibility is that the ice sheet retreated far inland during the Early Holocene and that the 9.3 and 8.2 ka re-advances was minor and did not pass the LIA extent. However, according to the simulations the ice margin did not retreat far inland of the present extent. How could this be explained?

Figure 7 (Ice volume) shows a pause or slight increase in ice volume during the 9.3 ka and 8.2 ka event. We just want to reiterate that these are simulated ice margins from the model experiments, not necessarily what occurred in reality. Thus, slight pauses or increases in ice volume at 9.3 and 8.2 ka indicate that our model is capturing/responding to the 9.3 and 8.2 ka events, but the ice-margin response is rather limited. Thus, the simulations do show a response to these events, and the ice margin (not shown in the paper plots) does stabilize and in some location’s advances (although very minor).

The reviewer is correct in that in the immediate KNS region, geologic constraints reveal that the ice margin retreated behind the eventual historical maximum position between ~10.3 and 9.5 ka (Fig. 2) so therefore any expression of the 9.3 and 8.2 ka events would not be preserved on that landscape. Rather, the LIA/historical advance overran any potential 9.3 or 8.2 ka moraines

Line 26 delete “novel”

done

Line 29 change bedrock with fjord

We decided to change it to 'terrestrial' to indicate land above sea level.

Line 29 what do you mean with "above sea level"?

See above

Line 34 and throughout the manuscript: capitalize "early", "middle" and "late" Holocene.

done

Line 47: capitalize smb and use consistently in MS

done

Line 63, 86: (s)outhwestern

done

Line 264: ) is missing.

done

Line 287: . is missing after et al

done

Table 1 is not really needed.

We prefer to leave this in as a reminder for the set of experiments needed, but are willing to remove if it is absolutely needed.

Line 481: Format reference.

done

Line 630: Both "Isbræ" and "glacier" have been used.

done

Line 633: change "ford" to fjord.

done

Line 642-656: Mostly not conclusions and could be omitted.

Perhaps stylistic, but this paragraph was intended to set a tone for the conclusions that follow. Again, we would prefer to keep this, but are fine omitting if the reviewer/editor find it absolutely necessary.

Line 741-744: check format

done

Line 783-786: check format

done

Line 793-802: check format
done