We thank Lev for his constructive comments, which have helped improve the manuscript. To address his concerns, we have made substantial revisions. Specifically, we have expanded discussions about parameter uncertainty and included a table and figure of the entire ensemble of experiments performed. We have also added more discussions of our results in the context of previous reconstructions studies. Please find detailed responses to Lev's specific comments below, along with excerpts from the revised manuscript given in boxes. A bibliography of the references cited here is present at the end of the document. We would like to highlight that excerpts presented here are only a handful of the changes made to the manuscript, which are relevant to his specific comments. Alongside these, we have included many other changes designed to improve the clarity and readability of the study.

**Reviewer 2:**

*This submission presents the first asynchronously coupled ice sheet and LoveClim transient modelling results for the MIS 7 to 6d (240 to 170 ka) interval. It is therefore plenty novel and relevant for CP. Two of its main conclusions reflect those of another submission currently under CPD review examining last glacial inception (Bahadory et al, cp-2020-1, of which I'm a co-author): that 1) LoveClim appropriately coupled with an ice sheet model can within uncertainties capture major interglacial-glacial-interstadial transitions and 2) replicating such transitions can impose strong constraints on the coupled model. The third conclusion, that orbital forcing has more impact than GHG changes during glacial inception, is not surprising based on associated radiative forcings and chosen fixed forcing states but does not necessarily hold when one considers say the whole last glacial cycle (Tarasov and Peltier, JGR 1997, albeit with a much simpler 2D energy balance climate model and isothermal ice sheet model).*

*As to the quality and limitations of the scientific methods, aka model configuration and experimental design, the component PSU ice sheet model and LoveClim EMIC are well documented and well used models. They arguably remain near state-of-the-art for transient glacial contexts (though LoveClim is sorely in need of a replacement and ongoing work with transient GCM/ISM models are defining a new state of the art for very small ensembles). However, I do find some limitations that need to be made explicit in the manuscript. As shown in Bahadory et al, GMD 2018 (again from my group and curious why this paper is not cited), accounting for orographic forcing of precipitation in downscaling to the ice sheet grid and accounting for topographic changes in meltwater routing can each have significant impact on modelled ice sheet evolution. To be concrete, inclusion of the latter, for instance can, result in more than 15 m eustatic sealevel equivalent discrepancies within 4 kyr (IBID). The current submission is not even explicitly clear if the modelled surface freshwater routing changes during the glacial cycle (though it appears not to be the case). There are also a host of other sources of model uncertainty that are not mentioned, including: length of model spinup, topographic upscaling from ISM to LOVECLIM, the requirement of much higher ice sheet resolution or subgrid mass-balance accounting to adequately represent restricted glaciation over Alaska, and the dozens of poorly constrained parameters in both models that the modellers have chosen to not vary.*

A. We share Lev's concern that there remains a number of parameters which are not well constrained. This is a general problem of climate-ice sheet models which are sufficiently computationally efficient to allow for simulations spanning multiple millennia, since such models cannot resolve many key processes and hence need to rely on parameterizations. On the other hand, there has been an extensive effort to constrain the parameters in the two LOVECLIP components separately, LOVECLIM and PSUIM, and sets of standard parameters have been developed, which are the default in our coupled modeling system. While it is generally possible to optimize parameter sets for a given period in time, these optimal parameter sets tend to depend on the period chosen. Presumably, this is related to model imperfections. We agree that including the processes described in Lev's comment do have the potential to reduce some of the model imperfections, however, they also come at the cost of new parameter values which need to be constrained. So, the general caveat of imperfect models and poorly constrained parameter values remains. Despite these caveats, the fact that our model is able to reproduce many transient features of past climate reconstructions gives us confidence that these models are sufficiently realistic to start using them to address questions related to physical processes and questions underlying ice sheet trajectories. As outlined below, we have revised the text to clarify the extent to which the processes above are included or not in our present model setup.

   a. Orographic forcing is not implemented in our setup. While the authors were aware of Bahadory and Tarasov (2018), it was an oversight on our part of not citing it since it was not applied to glacial simulations. We have clarified this in *Lines 244-248* in *Section 2.3*:

Instead of using a fixed value of $\gamma$, both Roche et al. (2014) and Bahadory and Tarasov (2018) used a dynamic lapse rate, where $\gamma$ is estimated locally for the ice model grids in each LOVECLIM grid. Moreover, the lapse rate also depends on the atmospheric $CO_2$ concentration. Such dynamic lapse rate corrections are not implemented in the current setup, and neither is the advective precipitation downscaling scheme of Bahadory and Tarasov (2018).

b. The modelled freshwater in PSUIM is dynamically routed based on the actual topography. We have now clarified this in *Lines 263-266* in *Section 2.3*:

The total meltwater from basal melting and liquid runoff in PSUIM is dynamically routed based on PSUIM topography till it reaches the ocean or the domain edge, and then is routed to the nearest ocean grid point in LOVECLIM. The calving flux is channeled into CLIO's iceberg model (Schloesser et al., 2019;Jongma et al., 2009) in the Southern Hemisphere (SH) and as an iceberg melt flux (freshwater flux and heat flux) in the NH (Schloesser et al., 2019).

c. Our model was spun up for 10,000 years using orbital and GHG forcings of 240ka with the NH ice volume equilibrating to -20m SLE. This is mentioned in *Lines 282-286* in *Section 2.4*:

The LOVECLIP experiments are initialized using present day ice sheet conditions and spun up using orbital and greenhouse gas (GHG) forcings of 240 ka for a period of 10ky. The model equilibrates to an ice sheet distribution in the NH corresponding to -20m SLE, implying an open Bering Strait. Our initial ice sheet distribution at 240ka is shown in Fig. 4c and is in close agreement with that used by previous studies such as Colleoni and Liakka (2020) for 239ka and Colleoni et al. (2014) for 236ka.

d. The topography from PSUIM is upscaled to LOVECLIM using a simple weighted average. We have clarified this in *Lines 259-260* in *Section 2.3*:

LOVECLIM orography and surface ice mask are updated based on the evolution of ice sheets and bedrock elevation from PSUIM. The PSUIM topography is upscaled to LOVECLIM grid using simple weighted averaging.

e. The need for higher resolution to model mass balances and transport over regions with large sub-grid relief is a very valid concern, as mentioned in Le Morzadec et al. (2015). Coarse grid resolutions average out large sub-grid relief (tall peaks and low valleys) over some mountainous regions, such as parts of Alaska, and thus don't capture the non-linear combination of accumulation zones on the high peaks and ablations zones in the valleys. This is indeed a shortcoming of our modelling setup and we have now addressed this in *Lines 372-375* in *Section 3.2*:

Our modelling setup also does not account for sub-grid mass balances, which can be especially relevant over mountainous regions with large sub-grid relief such as Alaska (Le Morzadec et al., 2015). Coarse grids tend to average out such tall peaks and low valleys and thus don't capture the non-linear combination of accumulation zones on the high peaks and ablation zones in the valleys.

We have also added more discussion about discrepancies in the current study and further steps for model improvement in *Lines 543-577* in *Section 4*:

The simulated ice sheet volume is well within the range of reconstructions for a rather narrow range of parameters. Small changes in parameter values can produce strongly diverging trajectories, and the emergence of multiple equilibrium states may also suggest the model's dependence on initial conditions. This poses a challenge, as many ice sheet and climate model parameters remain poorly constrained. In this context, we note that parameterizations associated with hydrofracturing and cliff instability did not impact our ice sheet trajectories. These processes have provided substantial contributions to the rapid Antarctic ice sheet retreat simulated in response to future climate projections (DeConto and Pollard, 2016), and better constraining these parameterizations is important to reduce uncertainties related to future sea level trajectories (e.g., Edwards et al., 2019). Presumably, these processes did not play an important role in our present simulations, because the climate is generally too cold, suggesting that opportunities for constraining these parameters in glacial simulations may be limited. We further note that the parameter sets which allowed for the most realistic simulation of glacial inceptions during

MIS 7-MIS 6 may not necessarily be optimal for other periods. That optimal parameter sets can depend on the period over which they are optimized, has recently been shown for a similar coupled climate ice sheet model (Bahadory et al., 2020).

Our present setup has difficulties in realistically simulating both Laurentide and Eurasian ice sheets simultaneously and generates a smaller Eurasian ice sheet compared to reconstructions, which could be a model dependent feature of LOVECLIM, given it is a T21 grid with only three levels in the atmosphere, and so could vary with the choice of the climate model used. Since we use an accelerated setup, we only conserve the freshwater flux from the ice model to LOVECLIM, which could lead to an underestimation of the oceanic circulation changes due to the lesser volume of net freshwater being dumped into the ocean. Nevertheless, there is scope of further improving the current setup. For instance, we only implement temperature and precipitation bias corrections in the current setup, and including bias corrections for radiation and ocean temperature might improve our representation of ice sheets. Future research might further improve the current setup by including the advective precipitation downscaling scheme (Bahadory and Tarasov, 2018) to account for orographic forcing, which is not captured in LOVECLIM. We are also investigating the possibilities of using a dynamical, an altitude-dependent and a $CO_2$-dependent lapse rate corrections while downscaling temperature from LOVECLIM to PSUIM. This is because the atmospheric lapse rate depends on the atmospheric $CO_2$ concentration – an effect that has not been considered so far in glacial dynamics. Furthermore, improving our basal sliding coefficient map for the NH using information of sediment sizes, instead of simply using a binary coefficient map, has the potential of further improving the simulations.

Potentially more realistic results could be obtained if the simulations were unaccelerated (which would be computationally very expensive), and from using more complex climate models that include stratification-dependent mixing in the ocean for instance. Furthermore, Glacial Isostatic Adjustment (GIA) processes captured only in comprehensive full-Earth models such as forebulges are not simulated in the ice-sheet model used here. Nevertheless, we would like to reiterate that simulating a trajectory is more difficult than conducting timeslice experiments, as climate and ice sheet components work on totally different timescales and a fine interplay of parameters can add up to very different equilibrium states. And such coupled climate-ice sheet paleo-simulations offer great opportunities for constraining parameter sets for future simulations.

*The other hole in the paper for me is pervasive in paleo ice sheet modelling: a very limited exploration of the impact of model uncertainties, given the small number of ensemble parameters and limited ensemble size. This is an exploratory work, and so arguably gets a pass with this limited ensemble, but I encourage the authors to expand their set of ensemble parameters and ensemble size in future work. And the paper needs a bit more attention to discussion of uncertainties that arise from the very limited ensemble size and the potential impact thereof.*
*The paper structure is logical. The abstract is concise and appropriate. The language is fluent, though there are instances where precision is lacking (eg "reasonably well", cf detailed commments below) as are some important (to me at least..) details about model setup.*

A. We have now updated the table of experiments (Table 1) and added a figure in the supplementary showing all the experiments that were conducted as part of our ensemble (including those not discussed in the paper). We have also included discussions in the methods and results sections to further clarify the ensemble parameters, model uncertainties and future works. Answers to these questions are mentioned below as responses to the specific comments.

**Specific comments:**

*For a range of model parameters, the simulations capture the reconstructed evolution of global ice volume reasonably well # What does "reasonably well" mean. Be precise*

A. Reasonably well means within the uncertainty bounds of reconstructions. We have now replaced this in *Lines 18-19* in the *Abstract*:

For a range of model parameters, the simulations capture the evolution of global ice volume well within the range of reconstructions.

*It is demonstrated that glacial inceptions are more sensitive to orbital variations, whereas terminations from deep glacial conditions need both orbital and greenhouse gas forcings to work in unison*
*# this likely depends on your choice of fixed orbital configuration # cf Tarasov and Peltier, JGR 1997.*

A. We have clarified this to be true for our time period of consideration. This is mentioned in *Lines 19-21* in the *Abstract*:

Over the MIS7-6 period, it is demonstrated that glacial inceptions are more sensitive to orbital variations, whereas terminations from deep glacial conditions need both orbital and greenhouse gas forcings to work in unison.

*This poses a general challenge for transient coupled climate-ice sheet modeling.# on the flip side, it poses a strong constraint opportunity, cf# Bahadory et al, cp-2020-1.pdf in TCD*

A. We agree and have added this in *Lines 24-26* in the *Abstract*:

This poses a general challenge for transient coupled climate-ice sheet modeling, with such coupled paleo-simulations providing opportunities to constrain such parameters.

*which correspond to about 1.3 mm/year global sea level equivalent during the build-up phase.# that number is more than a factor too small for last glacial# inception if one goes by the cited LR04 stack*

A. We have now modified this to clarify that this value is an average and the changes were much higher during the LGM. This is in *Lines 36-39* in the *Section 1*:

One of the main obstacles in simulating variability on orbital timescales is the fact that ice-sheets are slow integrators of small imbalances between ablation and accumulation, which correspond to an average of 1.3mm/year global sea level equivalent during the build-up phase but can exceed 10mm/year for instance during the Last Glacial Maximum (LGM, 21ka).

*fig 3 captions # again mixing up ensemble with ensemble run. An ensemble is a collection # of model runs.*
*fig 3 # does this show all the model runs in the non-fixed forcing ensemble you # carried out? If so, please make this clear.*
*including multi-ensemble simulations# do you mean mult-run or did you actually carry out multiple ensembles? # If so, how large was each ensemble?*

A. We use only one ensemble of multiple runs in our simulations. We also updated Table 1 to show all the ensemble runs performed in this study. While we performed a total of 50 separate experiments, we reported only 15 of them in Figure 3 that best describe the parameter sensitivities. We have now clarified this in in *Lines 293-300* in *Section 2.4:*

Furthermore, sensitivity experiments with different GHG sensitivities (α, Sect. 2.1) and melt parameterizations ($m$, Sect. 2.2) are run with full forcing. Generally, higher α leads to a stronger sensitivity to $CO_2$ concentrations, and higher values of $m$ strengthen buildup and weaken melting of ice during interglacial climates. These experiments are presented in the first row of Table 1 (1-15) and Fig. 3. Additional simulations with different combinations of acceleration ($N_A$), GHG sensitivity (α), melt parameter ($m$), basal sliding coefficient maps over the NH ($C(x,y)$) and higher ice model resolution (0.5 × 0.25° for NH, 20 × 20 km polar stereographic for Antarctica*)* have been performed (experiments 16-50 in Table 1). The whole ensemble of simulations is presented in Fig. S3. Although we note that these experiments do not present a systematic evaluation of the full parameter space, ice sheet trajectories are consistent with and thereby support the conclusions presented in this paper.

We have also updated the text throughout the manuscript to clarify that we do not run multiple ensembles but run multiple ensemble members.

We also updated the table of experiments (Table 1):

<table>
<tr><th>Expt Number</th><th>Orb Forced</th><th>GHG Forced</th><th>$N_A$</th><th>α</th><th>$m$ (Wm$^{-2}$)</th><th>$C$ (myr$^{-1}$Pa$^{-2}$)-NH</th></tr>
<tr><td>1 (BLS)</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>125</td><td rowspan="15">Binary distribution<br><br>1.<u>Ocean:</u><br>$C(x,y)$=10$^{-6}$; representing deformable sediments<br>2.<u>Land:</u><br>$C(x,y)$=10$^{-10}$; representing non-deformable rock.</td></tr>
<tr><td>2</td><td>N</td><td>N</td><td>5</td><td>2</td><td>125</td></tr>
<tr><td>3</td><td>Y</td><td>N</td><td>5</td><td>2</td><td>125</td></tr>
<tr><td>4</td><td>N</td><td>Y</td><td>5</td><td>2</td><td>125</td></tr>
<tr><td>5</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>125</td></tr>
<tr><td>6</td><td>Y</td><td>Y</td><td>5</td><td>1.8</td><td>125</td></tr>
<tr><td>7</td><td>Y</td><td>Y</td><td>5</td><td>2.2</td><td>125</td></tr>
<tr><td>8</td><td>Y</td><td>Y</td><td>5</td><td>2.5</td><td>125</td></tr>
<tr><td>9</td><td>Y</td><td>Y</td><td>5</td><td>3</td><td>125</td></tr>
<tr><td>10</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>80</td></tr>
<tr><td>11</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>100</td></tr>
<tr><td>12</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>120</td></tr>
<tr><td>13</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>130</td></tr>
<tr><td>14</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>140</td></tr>
<tr><td>15</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>150</td></tr>
<tr><td>16-20</td><td>Y</td><td>Y</td><td>5</td><td>1.5</td><td>120,125,130,140,150</td><td rowspan="7">Binary</td></tr>
<tr><td>21-24</td><td>Y</td><td>Y</td><td>5</td><td>3.5</td><td>80,100,120,125</td></tr>
<tr><td>25-27</td><td>Y</td><td>Y</td><td>1 (30ky run)</td><td>2</td><td>110,120,130</td></tr>
<tr><td>28-30</td><td>Y</td><td>Y</td><td>2 (30ky run)</td><td>2</td><td>110,120,130</td></tr>
<tr><td>31-33</td><td>Y</td><td>Y</td><td>10</td><td>2</td><td>110,130,150</td></tr>
<tr><td>34-36</td><td>Y</td><td>Y</td><td>10</td><td>2.5</td><td>110,120,130</td></tr>
<tr><td>37-38</td><td>Y</td><td>Y</td><td>20</td><td>2.5, 3</td><td>125</td></tr>
<tr><td>39-41</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>125</td><td rowspan="3">Tertiary<br>1.<u>Ocean:</u><br>$C(x,y)$=10$^{-6}$;<br>2.1 <u>Land (soft tills):</u><br>$C(x,y)$=10$^{-7}$,10$^{-8}$,10$^{-9}$ over northeastern North America<br>2.2 <u>Land (hard bed):</u><br>$C(x,y)$=10$^{-10}$</td></tr>
<tr><td>42-44</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>150</td></tr>
<tr><td>45-47</td><td>Y</td><td>Y</td><td>5</td><td>2.5</td><td>125</td></tr>
<tr><td colspan="7">High Resolution Runs: 0.5 × 0.25° for NH, 20 × 20 km polar stereographic for Antarctica</td></tr>
<tr><td>48-50</td><td>Y</td><td>Y</td><td>5</td><td>2</td><td>110,130,150</td><td>Binary</td></tr>
</table>

**Table 1: List of all ensemble runs performed for the study study (shown in Fig. S3). The first 15 experiments are discussed in Sect. 3.1 and shown in Fig. 3. Values in bold represent the difference from the baseline simulation (BLS, experiment number 1). $N_A$ represents the PSUIM vs LOVECLIM acceleration factor (Sect 2.3). α represents the GHG sensitivity scaling factor (Eq. 1, Sect. 2.1) and $m$ represents the constant parameter in the surface energy balance equation (Eq. 3, Sect. 2.2). $C$ represents the basal sliding coefficient map used for the NH (Eq. 7, Sect. 2.2). All experiments are run at $1 \times 0.5°$ resolution for the Northern Hemisphere and $40 \times 40$ km polar stereographic resolution for Antarctica. The experiments in italics (16-50) are not presented here but were also performed to better constrain the parameter sensitivities.**

And added a figure in the supplementary, Fig. S3, showing all the ensemble runs used in our study:

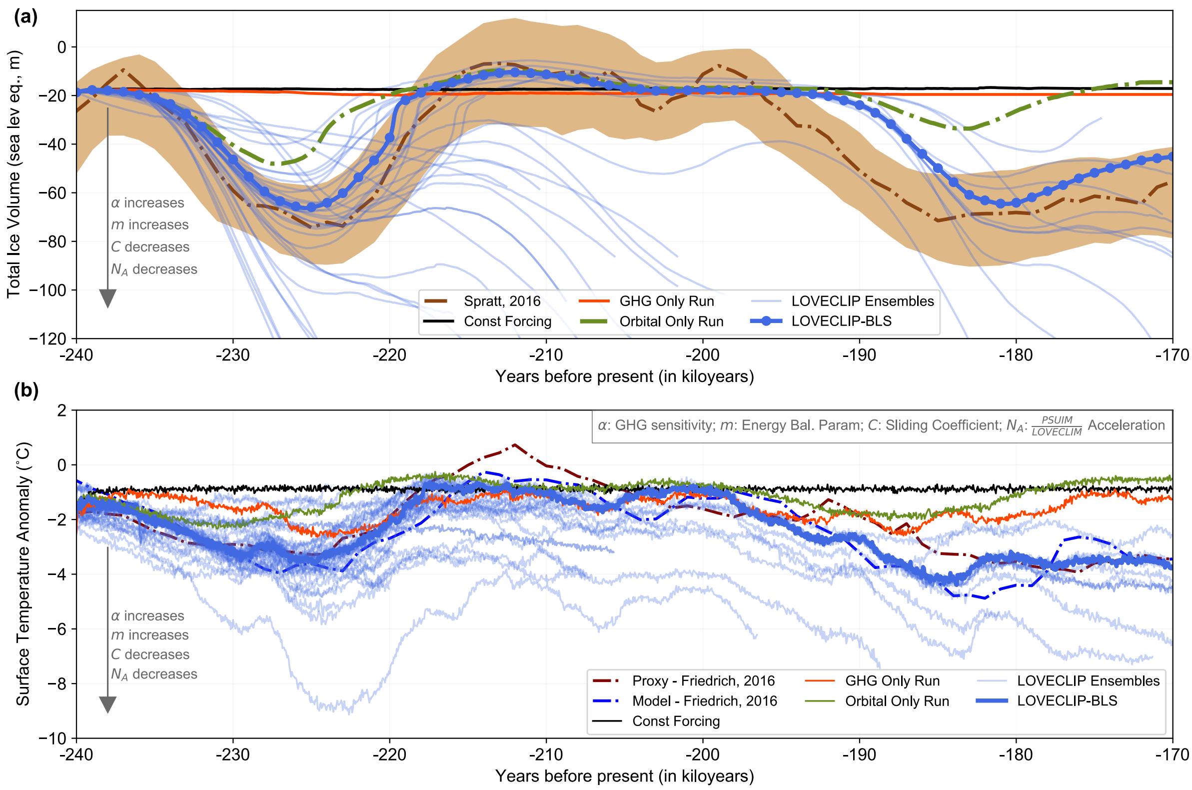


**Figure S3: Transient LOVECLIP ensemble simulations over MIS7 with varying GHG sensitivities ($\alpha$ = 1.5-3.5), energy balance parameter ($m$ = 80-150Wm$^{-2}$), basal sliding coefficient ($C$ = 10$^{-6}$-10$^{-8}$ myr$^{-1}$Pa$^{-2}$) and PSUIM-vs-LOVECLIM acceleration factor ($N_A$ = 1,2,5,10,20). The best results are obtained for $\alpha$=2, $m$=125 Wm$^{-2}$, binary sliding map (ocean: $C$=10$^{-6}$ myr$^{-1}$Pa$^{-2}$ and land: $C$=10$^{-8}$ myr$^{-1}$Pa$^{-2}$) and $N_A$=5 (experiment 1 in Table 1, BLS).**

*The effect of CO2 variations with respect to the reference CO2 concentration (365ppm) on the longwave 120 radiation flux is scaled up by a factor α, to account for the low default sensitivity of ECBilt to changes in CO2 concentrations (Friedrich and Timmermann, 2020;Timmermann and Friedrich, 2016). α is determined based on transient past and future simulations.# Please provide the pCO2 ECR for alpha=2 with your setup. This would # let reader better judge how consistent this resultant sensitivity is# compared to that of IPCC grade GCMs. Also, it would be worthwhile# comparing your \alpha to that found based on 1D radiative-convective # modelling (Ramanathan et al, 1979 JGR).*

A. Our ECS is 3.69K for $CO_2$ doubling for α=2, well within the ranges for LOVECLIM only simulation reported in Friedrich et al. (2016) and Friedrich and Timmermann (2020). We have now mentioned this in *Line 129* in the *Section 2.1*:

For reference, the equilibrium climate sensitivity for $CO_2$ doubling is 3.69K for $\alpha$ of 2.

*2.2 PSUIM surface mass balance description, eq 1 and 2 # on what timestep is this carried out? If longer than 1 hour # (presumably), what accounting is there for diurnal variations?*

A. The surface mass balance is calculated at 3-hourly timesteps. The monthly data is interpolated to daily values using a weighted average of values across the adjacent months and then a sinusoidal cycle with max temperatures at 1400 and minimum at 0200, with peak-to-peak amplitude of 10˚C, is superimposed to account for daily variations.

We have now clarified this in *Lines 168-173* in the *Section 2.2*:

This surface mass balance is calculated at timesteps of 3 hours. Monthly surface air temperature ($T$) and surface incoming shortwave radiation ($Q$) (obtained from LOVECLIM in the current setup, discussed further in Sect. 2.3) are interpolated into sub-daily values in two steps. Firstly, the monthly values are interpolated to daily values using a weighted averaging of the values across two adjacent months. Next, a sinusoidal cycle with max temperature at 1400 and minimum at 0200, with a peak-to-peak amplitude of 10˚C, is superimposed on the daily data to account for diurnal variations.

*after eq 4 : with r = maxJ0, min[1, (T* + 3)/3] = maxJ0, min[1, (T* + 3)/3]J0, min[1, (T* + 3)/3][1, (T* + 3)/3]* + 3)/3] # Based on my on examination of ice sheet model horizontal basal # temperature between along flow adjacent grid cells (which provides # logical upper bound for the transition range), 3 C is a wide # transition range for warm based sliding. How is this justified?*

A. The 3˚C range was chosen during model development of PSUIM to improve the realism of modern ice thicknesses and flow over certain regions of Antarctica, and avoid ubiquitous frozen beds over the Transantarctic for instance (Pollard and DeConto, 2012). Instead of using temperatures between adjacent along-flow grid cells as upper bounds, the authors use this 3˚C temperature range to represent the sub-grid variations in basal temperatures within a single grid cell, due to small-scale variations of bed properties, roughness and topography.

*For the NH, a binary sliding coefficient map ... low sliding over present-day land (C(xJ0, min[1, (T* + 3)/3], y) = ... representing non-deformable rock).) = ... representing non-deformable rock). # Much of Southern Canada and Northern USA (regions of glacial ice # cover) is covered by tills, not hard beds and this can significantly # influence ice sheet evolution (eg Tarasov and Peltier, 2004 # QSR). How do you justify making all this hard bedded?*

A. While we apply only a binary basal sliding coefficient map in the current study, we did try a tertiary map (with intermediate sliding over the northeastern North America) and the ice sheet evolutions were similar. Given this is the first study using this coupled setup, we presented results using the simple binary sliding coefficient map distribution. We are currently working on using an improved sliding map based on the sediment size data. We have added this as discussion of future work in in *Lines 567-569* in the *Section 4*:

> Furthermore, improving our basal sliding coefficient map for the NH using information of sediment sizes, instead of simply using a binary coefficient map, has the potential of further improving the simulations.

*Preliminary experiments (not shown) with different acceleration factors suggest that model results do not change significantly when N <= 5. # Please be more precise by what "significantly" means.*

A. We wanted to convey that the simulated ice volume evolutions were similar over these low acceleration experiments. We have now replaced this in *Lines 215-217* in the *Section 2.3*:

> Preliminary experiments (not shown) with different acceleration factors suggest that the simulated ice sheet evolution is relatively insensitive to $N_A$ for $N_A \leq 5$. Therefore, $N_A = 5$ is used for the simulations presented in this paper, providing a good compromise between the objective to simulate realistic ice sheet evolution and computational efficiency.

*Furthermore, for surface temperature T, a lapse-rate correction of 8 ̊C km−1 is applied to account for differences between LOVECLIM orography and PSUIM topography and precipitation is multiplied by a Clausius–Clapeyron factor of 2^.. with $\Delta T$ being the temperature lapse-rate correction, to account for the elevation desertification effect (DeConto and Pollard, 2016).# How do you justify using a lapse rate that is inconsistent with# the lapse rate LOVECLIM uses internally? For future work, I would # strongly advise inclusion of orographic forcing given the impact# thereof missed in a coarse grid EMIC (cf eg Bahadory# and Tarasov, GMD 2018)*

A. Firstly, both the models LOVECLIM and PSUIM, and the parameterizations therein, were developed independently. Secondly, we use a higher lapse rate in PSUIM because we are more interested over the high latitudes in terms of ice sheet evolution. These regions are also drier and would have a higher lapse rate compared to the environmental lapse rate used in LOVECLIM. We are currently investigating the possibilities of using a dynamic lapse rate following Roche et al. (2014) and Bahadory and Tarasov (2018), an altitude dependent lapse rate as in Colleoni and Liakka (2020), and a $CO_2$ dependent lapse rate. We have added more discussion around this in *Lines 219-248* in *Section 2.3*:

> PSUIM uses surface air temperature ($T$), precipitation ($P$), solar radiation ($Q$), and ocean temperature at 400m depth ($T_o$) as inputs from LOVECLIM. These are downscaled using a bilinear interpolation approach. The surface temperature and

precipitation outputs from LOVECLIM which are used for the PSUIM surface mass balance are bias-corrected in the coupler, following Pollard and DeConto (2012b), Heinemann et al. (2014) and Tigchelaar et al. (2018).

$$T(t) = T_{LC}(t) + T_{obs} - T_{LC,PD} \quad (8)$$

$$P(t) = P_{LC}(t) \times P_{obs}/P_{LC,PD} \quad (9)$$

where $T$ is monthly surface air temperature and $P$ is monthly precipitation forcing from LOVECLIM at timestep $t$. Subscripts '$LC$', '$obs$' and '$LC,PD$' refer to LOVECLIM chunk output, observed present day climatology, and LOVECLIM present day control run, respectively. The observed present day climatology is obtained from the European Centre for Medium-Range Weather Forecasts reanalysis dataset, ERA-40 (Uppala et al., 2005). These LOVECLIM biases are calculated for PD simulations using an LGM bathymetry. We did compare the biases between using a PD or LGM bathymetry, and while there were regional differences, the large-scale structure was found to be similar (not shown). The annual mean of the monthly mean bias correction terms $T_{obs} - T_{LC,PD}$ and $P_{obs}/P_{LC,PD}$ are presented in Fig. S1. Temperature biases in LOVECLIM for boreal summer (JJA) and austral summer (DJF) are shown in Fig. S2 for reference, since summer temperatures are more crucial for ice sheet growth and decay. Furthermore, a lapse-rate correction of 8˚C km$^{-1}$ is applied to account for differences between LOVECLIM orography and PSUIM topography for the interpolated temperature, $T(t)$, and precipitation is multiplied by a Clausius–Clapeyron factor of $2^{\frac{-\gamma.\Delta H}{10^{\circ}C}}$, with $\gamma.\Delta H$ being the temperature lapse-rate correction, to account for the elevation desertification effect (DeConto and Pollard, 2016):

$$T_{PSUIM}(t) = T_{interp}(t) - \gamma.\Delta H \quad (10)$$

$$P_{PSUIM}(t) = P_{interp}(t) \times 2^{\frac{-\gamma.\Delta H}{10^{\circ}C}} \quad (11)$$

where $T_{PSUIM}$ and $P_{PSUIM}$ are the final temperature and precipitation inputs for PSUIM, $T_{interp}$ and $P_{interp}$ are bias corrected LOVECLIM temperature ($T$, Eq. 8) and precipitation ($P$, Eq. 9) interpolated to PSUIM resolution, $\gamma$ is the lapse rate (8˚C km$^{-1}$), and $\Delta H$ is the altitude difference between PSUIM grids and the corresponding LOVECLIM grid. Colleoni and Liakka (2020) used a similar fixed atmospheric lapse rate correction during downscaling temperature to their ice model, GRISLI, with $\gamma$ as 3.3˚C km$^{-1}$ for annual mean and 4.1˚C km$^{-1}$ for summer mean. And they reported slightly smaller ice sheets on using an elevation dependent lapse rate, going all the way up to 7.9˚C km$^{-1}$. Instead of using a fixed value of $\gamma$, both Roche et al. (2014) and Bahadory and Tarasov (2018) used a dynamic lapse rate, where $\gamma$ is estimated locally for the ice model grids in each LOVECLIM grid. Moreover, the lapse rate also depends on the atmospheric $CO_2$ concentration. Such dynamic lapse rate corrections are not implemented in the current setup, and neither is the advective precipitation downscaling scheme of Bahadory and Tarasov (2018).

And have added these as possibilities of further improvement in *Lines 561-569* in *Section 4*:

. Nevertheless, there is scope of further improving the current setup. For instance, we only implement temperature and precipitation bias corrections in the current setup, and including bias corrections for radiation and ocean temperature might improve our representation of ice sheets. Future research might further improve the current setup by including the advective precipitation downscaling scheme (Bahadory and Tarasov, 2018) to account for orographic forcing, which is not captured in LOVECLIM. We are also investigating the possibilities of using a dynamical, an altitude-dependent and a $CO_2$-dependent lapse rate corrections while downscaling temperature from LOVECLIM to PSUIM. This is because the atmospheric lapse rate depends on the atmospheric $CO_2$ concentration – an effect that has not been considered so far in glacial dynamics. Furthermore, improving our basal sliding coefficient map for the NH using information of sediment sizes, instead of simply using a binary coefficient map, has the potential of further improving the simulations.

*Basal melting and liquid runoff from PSUIM is discharged via LOVECLIM's runoff masks in both hemispheres; # do these masks account for changing topography? And if so, what # accounting is there for critical subgrid gateways for southern # drainage from the NA North American) ice complex (cf eg Tarasov and # Peltier, QSR 2006).*

A. The net meltwater in PSUIM is dynamically routed based on the actual topography. The current setup does not account for the subgrid pathways needed for southward drainage from the North American ice sheets. We have clarified this in *Lines 263-266* in *Section 2.3*:

The total meltwater from basal melting and liquid runoff in PSUIM is dynamically routed based on PSUIM topography till it reaches the ocean or the domain edge, and then is dumped into the nearest ocean grid point in LOVECLIM. The calving flux is channeled into CLIO's iceberg model (Schloesser et al., 2019;Jongma et al., 2009) in the Southern Hemisphere (SH) and as an iceberg melt flux (freshwater flux and heat flux) in the NH (Schloesser et al., 2019).

*Increasing the value of $m$ (Eq. (1)) # as a reader, it is a pain to flip back 5 pages to find out what m # is, please add a few descriptive words (surface energy offset term # or some such) ditto for \alpha*

A. Done.

*3.2 Ice sheet evolution # this section would be strengthened with more contact with the # (albeit limited) glacial geological litterature. The key relevant # data are Late Pleistocene glacial limits. Does your model respect # them everywhere? If not, what are the main discrepancies? The only # regions I see that could be at issue are your Alaskan incursion and # Northern Siberia.*

A. While we could not find many reconstructions over this period, we have now added discussions comparing our simulations with other modelling and reconstruction studies. Some excerpts of these are mentioned under from *Lines 344-376* in *Section 3.2*:

In the context of previous modelling studies and geological records over this MIS 7-6 period, our ice sheet distribution at MIS 7c (212ka, Fig. 4g and 219.5ka, Fig. S7) is very similar to that reported in Colleoni and Liakka (2020). However, we simulate a stronger inception compared to that of Colleoni et al. (2014b) over the corresponding 236-230ka period. They also reported a bifurcated but connected North American ice sheet at MIS 6 (157ka) from both their control (100km) and high resolution (40km) experiments. Our simulation results in separate Laurentide and Cordilleran ice sheets but generates neither a Eurasian nor a Siberian ice sheet, albeit at 170ka. On a side note, our North American ice sheet distribution at 180ka (Fig. 7) is closer to that of Colleoni and Liakka (2020) at 157ka. Studies of NH reconstructions during MIS 6 such as Svendsen et al. (2004), over 160-140ka, Rohling et al. (2017), around 140ka, and Batchelor et al. (2019), over 190-132ka, have all reported glacial geological records to indicate a larger extent of the Eurasian ice sheet at MIS 6 glacial maximum compared to the LGM, while our simulations only show a persistent Fenno-Scandian ice sheet and a relatively small Eurasian ice sheet at 170ka. More recently, Zhang et al. (2020) reported the existence of a Northeast Siberia-Beringian ice sheet at MIS 6e (190-180ka) using NorESM-PISM simulations validated by North Pacific geological records. However, our model does not simulate any ice over Alaska, Beringia and northeast Siberia over MIS 7-6.

Our model's difficulty in simulating the Eurasian ice sheet can be attributed to the competition between Laurentide and Eurasian ice sheet growth, which makes it arduous to realistically simulate them simultaneously alongside generating the right atmospheric patterns. Some previous studies have suggested that teleconnections from stationary wave patterns induced by a large Laurentide ice sheet could lead to warming over Europe and influence Eurasian ice sheet evolution (Roe and Lindzen, 2001;Ullman et al., 2014). The Laurentide building up first in our simulations could have changed the storm tracks and dried out Eurasia. It is also worth reiterating that LOVECLIM has a coarse T21 grid with a simple 3-layered atmosphere. While the circulation changes reported here maybe model dependent, Lofverstrom and Liakka (2018) reported that at least a T42 grid was needed in their atmospheric model (CAM3) to generate a Eurasian ice sheet using SICOPOLIS, albeit for the LGM. They attribute this discrepancy to lapse rate induced warming due to reduced and smoother topography and higher cloudiness leading to increased re-emitted longwave radiation towards the surface. These teleconnection patterns are further discussed in Sect. 3.6. Our LOVECLIM setup also uses a fixed lapse rate for downscaling LOVECLIM surface temperatures (Eq. 10 and 11), while both Roche et al. (2014) and Bahadory and Tarasov (2018) used a dynamic lapse rate, which is estimated locally for the ice model grids in each LOVECLIM grid. Bahadory and Tarasov (2018) reported ice thickness differences up to 1km on using the dynamic lapse rate scheme compared to a fixed 6.5$^{\circ}$Ckm$^{-1}$. Nevertheless, for runaway trajectories, our model can build up a Eurasian ice sheet for ice volumes greater than -200m SLE once the Laurentide growth slows down (not shown). Our modelling setup also does not account for sub-grid mass balances, which can be especially relevant over mountainous regions with large sub-grid relief such as Alaska (Le Morzadec et al., 2015). Coarse grids tend to average out tall peaks and low valleys and thus don't capture the non-linear combination of accumulation zones on the high peaks and ablation zones in the valleys. These shortcomings could explain the lack of Eurasian, Siberian and Beringian ice sheets in our simulations.

*the glaciation 235 into MIS 6 is delayed by ~3ky (191ka instead of 194ka). # Do you really believe that temporaly uncertainty in inferred# sealevel is < 3 kyr that far back?*

A. We agree with Lev on this and have now added this to our discussion of results in *Lines 309-313* in *Section 3.1*:

The model captures the overall trajectory of ice volume evolution reasonably well. Specifically, the model stays within the uncertainty range for the extreme glaciation-deglaciation event of MIS 7e-7d-7c. Larger differences only exist as the glaciation into MIS 6 is delayed by ~3ky in the simulation (191ka instead of 194ka). A possible explanation for this discrepancy may be related to the temporal uncertainty in reconstructions themselves, since a similar lag occurs in other modeling studies (e.g., Ganopolski and Calov (2011); Ganopolski and Brovkin (2017).

*After a relatively stable interglacial state till MIS 7a, the system moves into the next glacial and reaches a glacial equilibrium state. # This description does not accurately reflect your figure 3, I see no # sign of a "glacial equilibrium"*
*...Batchelor et al. (2019), have suggested a larger Eurasian ice sheet over the MIS 6 period (160-140ka), # "suggested" does not accurately nor precisely reflect the # inferences. Be more accurate: eg glacial geological record indicates # that the asynchronous maximal MIS 6 ice margins are outside of MIS 2 # ice margins.*

A. We have now removed the phrase about "glacial equilibrium" and have clarified MIS 6 ice sheet reconstructions in *Lines 350-356* in *Section 3.2*:

Studies of NH reconstructions during MIS 6 such as Svendsen et al. (2004), over 160-140ka, Rohling et al. (2017), around 140ka, and Batchelor et al. (2019), over 190-132ka, have all reported glacial geological records to indicate a larger extent of the Eurasian ice sheet at MIS 6 glacial maximum compared to the LGM, while our simulations only show a persistent Fenno-Scandian ice sheet and a relatively small Eurasian ice sheet at 170ka. More recently, Zhang et al. (2020) reported the existence of a Northeast Siberia-Beringian ice sheet at MIS 6e (190-180ka) using NorESM-PISM simulations validated by North Pacific geological records. However, our model does not simulate any ice over Alaska, Beringia and northeast Siberia over MIS 7-6.

*# leading to temperatures low enough (Fig. 6d) to avoid ablation even if # the Laurentide extends equatorward There is always seasonal ablation on an northern ice sheet. Be more precise.*

A. We have now rephrased these in *Lines 440-442* in *Section 3.4*:

This can be attributed to the low $CO_2$ value (<200ppmv) leading to lower temperatures (Fig. 6d) and reduced ablation even if the Laurentide extends equatorward (Fig. 6g). Furthermore, the southern extent of the Laurentide can lead to changes in circulation patterns that can alter the SMB (discussed in Sect. 3.6).

*Figure 7: # makes it a lot easier for the reader if subplots have descriptive # headings on the plot. Having to visually jump between each subplot and a large # caption disrupts reader assimilation of the plots.*

A. Done.

*Fig 7 caption two ensembles of # do you mean two ensemble members?*

A. Yes. We have updated ensembles to ensemble members throughout the study.

*Fig 7f-I # I find the colour scheme has insufficient and distorting colour range. Eg # for 7h the 0.3:0.5 colour is just a shade darker than the -0.3:-0.1 range # colour. Furthermore, it makes no sense that the plot has regions where # these colour border each other without any intermediate ranges showing.*

A. We have now used a better colormap to plot negatives in blue and positives in red throughout the manuscript.

*Fig 7: # I am a bit confused why there is such limited glaciation east of the # Canadian Cordillera, given the northwesterly (and therefore relatively colder) # absolute winds and rainfall anomalies that match (within the colour # scheme) other sectors with significant ice cover. Is this due to # the temperature bias correction or limited rainful or ? On that note, # a short discussion on the impact of the bias correction would aid # interpretation of its role in your results.*

A. We suspect the reduced glaciation east of the Canadian Cordillera because of low net precipitation (not anomalies). The precipitation bias over this region is almost 1 (Fig. S1). While Fig. 7 shows the *anomalies* in mass balance terms with reference to those at 240ka, the figure underneath (Fig. R1) shows the *absolute* values of these mass balance terms. Fig. R1 (h) and (i) show that the precipitation just east of the Cordilleran is very low. To make these patterns clearer, we have now added a figure in the supplementary showing the initial patterns of the mass balance variables at 240ka for comparison (Fig. S8 now) with the anomalies presented in Fig. 7 and Fig. S9.

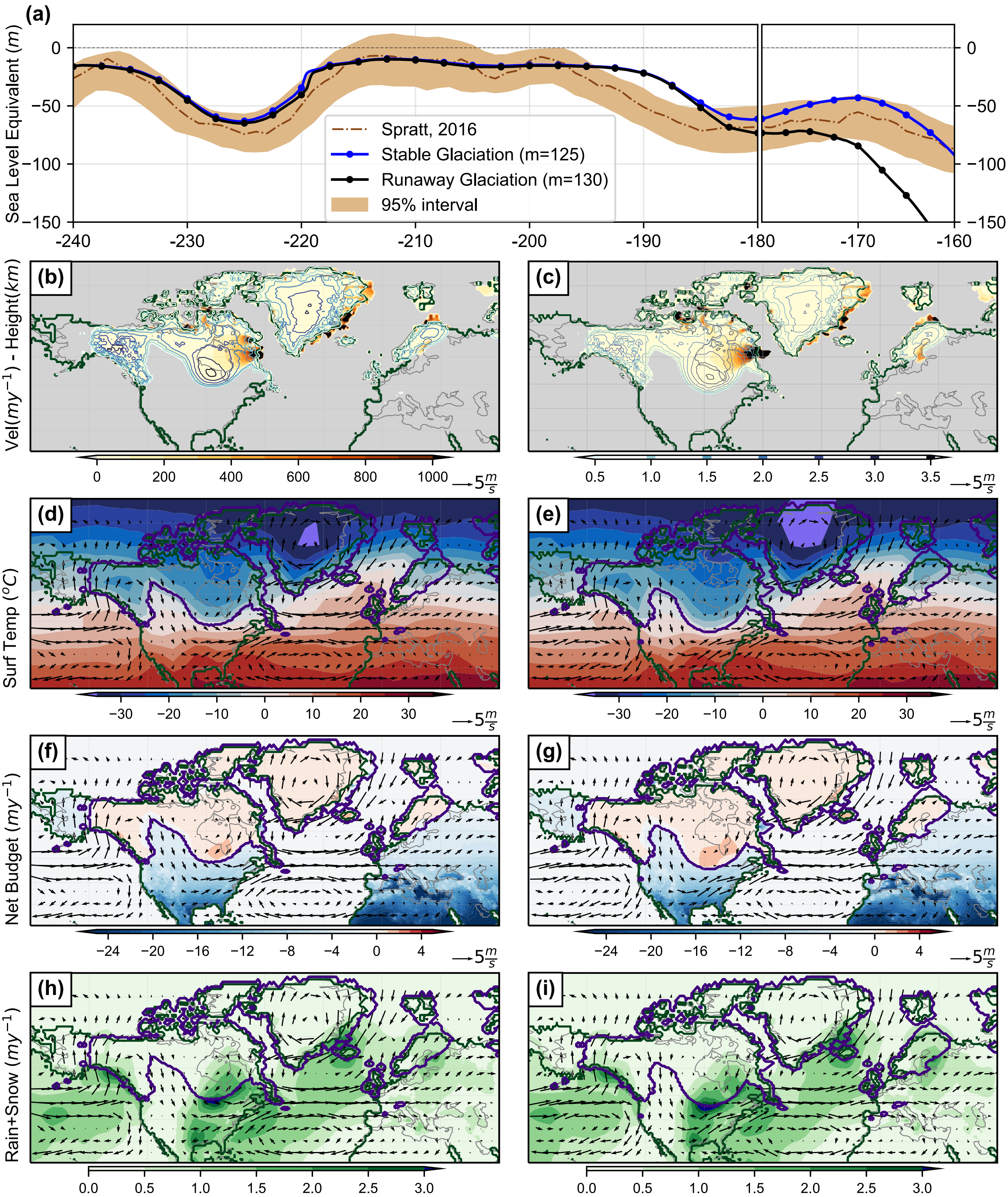


**Figure R1: Bifurcation of the system at 180ka while transitioning into MIS 6 over Laurentide. (a) Sea level reconstruction (m) and 95% confidence interval of Spratt and Lisiecki (2016) (brown). Total ice volume (in terms of SLE, m) from two ensemble members of LOVECLIP, one that leads to a stable glacial inception (blue; $\alpha$=2, $m$=125 Wm$^{-2}$) and another into a runaway glaciation (black; $\alpha$=2, $m$=130 Wm$^{-2}$). Climate and ice sheet variables at 180ka from the stable glaciation on the left column (b, d, f and h) and runaway glaciation on the right (c, e, g and i). (b,c) Basal ice velocity (solid colors, my$^{-1}$) overlaid with ice thickness (colored contours, km) and the grounding line (solid green lines). (d,e) Surface temperature (°C) overlaid with wind vectors at 800hPa (ms$^{-1}$). (f,g) Net mass balance (my$^{-1}$) overlaid with winds (ms$^{-1}$). (h,i) Net accumulation (my$^{-1}$) overlaid with winds (ms$^{-1}$). The purple contours in (d) to (i) mark the boundaries of the ice sheets from each run (stable for left and runaway for right).**

*this behavior is reminiscent of a saddle node bifurcation*

*We find that small changes in the Laurentide's ice distribution for similar total ice volumes can lead to a saddle node 400 bifurcation of the system # which is correct? Have you shown this to be a saddle node bifurcation # or is this reminiscent of a saddle node bifurcation?*

A. We apologize for this confusion. We see small differences in ice sheet distributions reminiscent of a saddle node bifurcation. We have now updated this in the text in *Lines 536-538* in *Section 4*:

> We find that small changes in the Laurentide's ice distribution for similar total ice volumes reminiscent of a saddle node bifurcation, which in turn determines whether the coupled trajectory will follow a deglaciation or a runaway glaciation pathway in response to the combination of forcings.

*Also, the stationary wave feedback reported here 410 could be a model dependent feature of LOVECLIM, given it has only three atmospheric levels # and LOVECLIM is run at a relatively coarse T21, while the # litterature indicates that at least T42 is needed to avoid major # resolution sensitivity of the eddy driven jet (eg Lofverstrom and # Liakka, 2018).*

A. We thank Lev for this suggestion. We have now discussed and expanded on this in *Lines 358-368* in *Section 3.2*:

> Our model's difficulty in simulating the Eurasian ice sheet can be attributed to the competition between Laurentide and Eurasian ice sheet growth, which makes it arduous to realistically simulate them simultaneously alongside generating the right atmospheric patterns. Some previous studies have suggested that teleconnections from stationary wave patterns induced by a large Laurentide ice sheet could lead to warming over Europe and influence Eurasian ice sheet evolution (Roe and Lindzen, 2001;Ullman et al., 2014). The Laurentide building up first in our simulations could have changed the storm tracks and dried out Eurasia. It is also worth reiterating that LOVECLIM has a coarse T21 grid with a simple 3-layered atmosphere. While the circulation changes reported here maybe model dependent, Lofverstrom and Liakka (2018) reported that at least a T42 grid was needed in their atmospheric model (CAM3) to generate a Eurasian ice sheet using SICOPOLIS, albeit for the LGM. They attribute this discrepancy to lapse rate induced warming due to reduced and smoother topography and higher cloudiness leading to increased re-emitted longwave radiation towards the surface. These teleconnection patterns are further discussed in Sect. 3.6.

And in *Lines 520-522* in *Section 3.6*:

> As mentioned earlier in Sect. 3.2, it is important to acknowledge the low horizontal and vertical resolutions of LOVECLIM's atmosphere, which could mean the circulation changes reported here to be model dependent.

*Results also suggest that our coupled simulations are realistic over a narrow range of parameters # what does "realistic" mean? Again, be precise*

A. We meant 'realistic' simulations to have good agreement with the reconstructions. We have clarified this in *Lines 543-545* in *Section 4*:

> The simulated ice sheet volume is well within the range of reconstructions for a rather narrow range of parameters. Small changes in parameter values can produce strongly diverging trajectories, and the emergence of multiple equilibrium states may also suggest the model's dependence on initial conditions.

*is more difficult than conducting timeslice experiments # I would say much more difficult and therefore offers much more # self-constraint*

A. Agreed. We have added in *Lines 574-577* in *Section 4*:

> Nevertheless, we would like to reiterate that simulating a trajectory is more difficult than conducting timeslice experiments, as climate and ice sheet components work on totally different timescales and a fine interplay of parameters can add up to

very different equilibrium states. And such coupled climate-ice sheet paleo-simulations offer great opportunities for constraining parameter sets for future simulations.

*Fig S1 # summer (JJA for NH and DJF for SH) temperature is much more critical # for ice sheet growth than mean annual temperature, given surface mass-balance # dependencies, so please add these plots.*

A. We have now added the summer temperature biases in Fig. S2 in the supplementary.

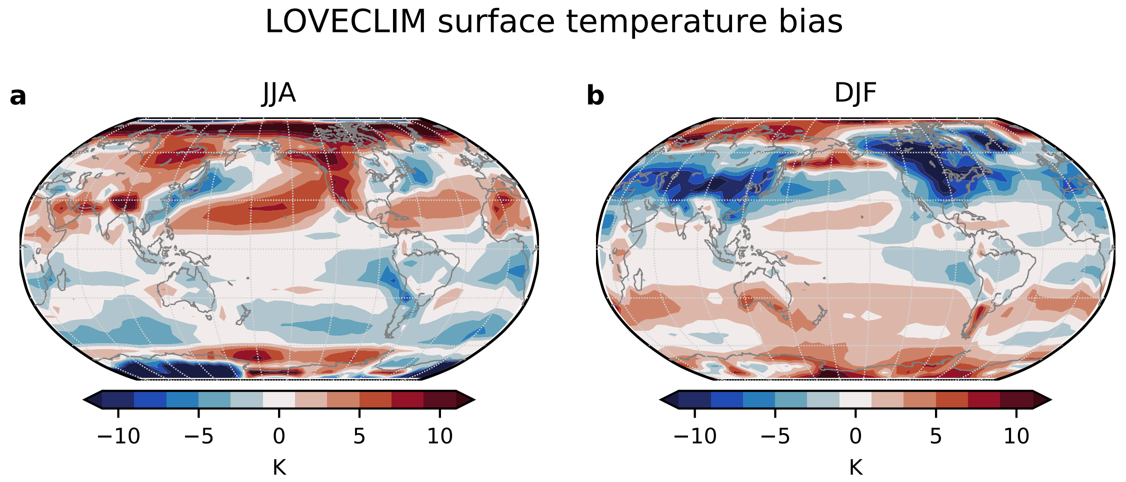


**Figure S2: Seasonal biases in surface temperature (K) from LOVECLIM for (a) JJA, and (b) DJF.**

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