Response to Reviewer #1

We would like to thank the reviewer for the positive appraisal of our work and for the constructive comments on our paper. In blue below is our response to the reviewer comments and suggestions (in black italic).

I found this submission to be an interesting contribution to the field of coupled paleo-climate modeling. The study utilizes CLIMBER-X to effectively simulate the rapid ice growth over Eurasia and North America during this period and the ice decay after MIS 5d. The authors quantify the relative importance vegetation, ice sheet, and carbon cycle feedbacks play in the CLIMBER-X model for default parameters on ice growth and decay during the last glacial inception. They confirm the significant role dynamic vegetation plays in facilitating rapid ice growth, which is the most important of the tested feedbacks in their model simulations. Moreover, they confirm the importance of a temperature bias correction over North America for successful inception simulations with CLIMBER. However, their application of a summer bias correction throughout the year and constant through time must be explained. The bias correction enhances the agreement between simulated ice sheet configurations and geological records. The study's exploration of small temperature (+/- 1 degree C) and albedo (0.025) perturbations and their substantial influence on ice sheet volume and area adds further depth to the findings.

The content of this manuscript is relevant, shedding light on the complex dynamics of the last glacial inception and the factors influencing ice sheet growth and decay. The insights gained from this research with CLIMBER-X can contribute to the refinement of other paleo-climate models. However, the authors should comment on model and initialization uncertainty that can't be addressed using a single simulation/model realization per experiment.

I recommend this manuscript for publication with moderate edits. The authors have effectively addressed essential aspects of coupled paleo-climate modeling. With some adjustments, especially in explaining the assumptions made, this study will make a valuable addition to the body of literature in the field.

Major comments:

The introduction doesn't adequately prepare the reader for the results. What is actually novel in your study? While you present a literature review, you don't show clearly enough where this manuscript fits in, which previous issues it addresses and what new knowledge it will contribute. The only time the present work is addressed is in the last sentence ("In this study we employ the Earth System model CLIMBER-X (Willeit et al., 2022, 2023) with interactive ice sheets, viscoelastic solid Earth response and dynamic vegetation to simulate the last glacial inception from 125 ka to 100 ka.") which is not enough to guide the reader (who might not want to read the full paper but look for specific subsections) and stir interest. For example, lines 44-52 are unclear. Are you listing issues that previous studies had? Or important feedbacks that other studies have found that must be included to simulate the last *glacial inception successfully? Here would be a good time to mention how your work will include/improve/explore said feedbacks and findings*

We agree with the reviewer that the contribution of this study to our general understanding of the last glacial inception needs to be more clearly explained and emphasized in the introduction. We will add an extra paragraph explaining what has been achieved so far and what the advancements of the current work are in terms of understanding mechanisms and important processes at play during the glacial inception. In the revised paper we will also give an outline of the paper at the end of the introduction to guide the reader.

Limitations of this non-ensemble approach, uncertainties in parametric values and model initialization should be clearly stated

We assume that "ensemble" in this context means "perturbed physics ensemble" since in climate modelling, "simulation ensemble" or "ensemble of model simulations" have different meanings (see IPCC glossary). As far as the perturbed physics approach is concerned, we were among the first to apply it in paleoclimatology (e.g. Von Deimling et al. (2006)). While this approach has some merit, for example, for assessing uncertainties of future climate projections, it also has serious problems. The main problem is that in such an ensemble, only one member, the standard model version, is properly calibrated and tested; some members are less realistic, and some are completely unrealistic. In addition, it is known that a perturbed physics ensemble does not mimic a multi-model ensemble since the perturbation of model parameters does not reproduce the structural uncertainties. This is why, in most of our publications, we used another approach: we first carefully selected the baseline model version that had the most realistic performance and then performed a dedicated set of sensitivity experiments following the principle: one experiment, one model parameter change. We believe this approach is better suited for understanding the mechanisms of past climate variability.

We do not believe that there is a meaningful way to assess "uncertainties in parametric values", and this is why we never tried to do that. Instead, we study the sensitivity of model results to key model parameters.

Concerning model initialisation, there is no indication that climate was not in quasi-equilibrium at 125 ka. Therefore, except for carbon cycle processes, which are not relevant in this study because we use prescribed CO_2 concentrations, only the Greenland ice sheet would likely not be in quasi-equilibrium with 125 ka climate conditions. However, Greenland is clearly not the focus of our study and plays a negligible role in the glacial inception over the other NH continents. In the revised paper we will discuss this issue in some more detail.

Application of the temperature bias correction (around line 118). Why would you apply the summer bias correction throughout the year? I don't see any reasonable explanation for that. What does the winter bias look like? And how can you assume the present-day bias is constant over time?

The reasoning behind the choice of applying summer temperature bias correction throughout the year is guided by the fact that, following Milankovitch theory, ice sheet surface mass balance is largely determined by ablation during summer, which is highly sensitive to temperature. The winter temperature biases over North America are similar in pattern but larger than the summer biases (see Fig. 5 in Willeit et al. (2022)), but since temperatures are anyway below freezing during this time of the year, this bias has only a very limited effect on surface mass balance. We have actually also tested the use of bias correction applying

monthly mean temperature anomalies, but the model results indicated absolutely negligible differences compared to using the mean summer bias. We therefore prefer the simpler and physically based choice of using the mean JJA temperature bias throughout the year. In the revised paper we will add some sentences explaining the reasoning behind the use of mean summer temperature bias correction.
Since the focus of our study is on the last glacial inception and the boundary conditions at ~120ka (just before the expected onset of ice sheet growth) are similar to pre-industrial in terms of GHGs concentrations and orbital parameters, we do not expect significant differences in temperature biases compared to the present-day. The assumption on stationarity of the temperature biases is therefore, at least during the initial ice growth phase, well justified. This obviously changes when substantial ice cover starts to develop over northern North America, but for this time period we have neither good paleoclimate data nor GCM model simulations available to test the assumption.
In the revised manuscript we will add a few sentences discussing the assumption of stationarity of temperature biases along these lines.

Minor comments:

Line 23: Typo: Milanlkovitch

Will be fixed, thanks.

Line 29: "relatively well covered by paleoclimate data": is it well covered? What is "relatively"? Aren't there significant uncertainties in any pre-LGM geological reconstructions?

Relative only to the previous glacial inceptions. The uncertainties of all pre-LGM reconstructions are large indeed. We will clarify this in the revised paper.

84: "while Antarctica is prescribed at its present-day state in this study": reasoning for this assumption?

This is not an assumption, this a model setup since we wanted to concentrate only on the Northern Hemisphere. The assumption is that the AIS contribution to global ice volume during glacial inception is small. This is only relevant for comparison of simulated and reconstructed sea level. Most studies of glacial inception are limited to the Northern Hemisphere. In the publications where the Antarctic ice sheet was included, from the classical work by Huybrechts (2002) to the recent Albrecht et al. (2020), the Antarctic contribution to global sea level rise during MIS 5 is only about 5 msl, which is 10% of global sea level variations reconstructed for this period.
Following also the comment of Reviewer#2 we will provide further justification of the fixed-Antarctica setup and discuss the potential role of Antarctic contribution to sea level change when discussing Fig. 4.

Line 99: "subsequently, temperature, humidity and radiation fields are downscaled onto the high-resolution topography.": can you account for orographically forced precipitation on the high-resolution topography?

The term “downscaled” is misleading. Climatological fields are horizontally and vertically interpolated. We do not account for the additional orographic effect on precipitation. Such an effect is crudely accounted for by the atmospheric component of CLIMBER-X, which has a much higher resolution (5°x5°) compared to CLIMBER-2, where a parameterization of the orographically forced precipitation was included (Calov et al., 2005).

Line 101: “concentration of dust in snow”: what dust? Is there dust forcing? Are dust sources and transport simulated? Where the dust is coming from should be explained here in short and in more detail in the supplements.

CLIMBER-X incorporates a fully interactive dust cycle, including atmospheric dust transport and dust deposition, as described in Willeit et al. (2022) and Fig. 11 and 12 in Willeit et al. (2023). Thus, the average concentration of dust in snow is computed. We will make this clear in the revised manuscript and also add dust to the schematic in Fig. 1.

Lines 117-118: “we implemented a temperature bias correction over northern North America that has a dipole structure”: while explained in the supplements, it is not clear here if this is constructed or simply the JJA summer temperature field of ERA5 minus CLIMBER

The temperature bias correction used in SEMIX is the difference in mean summer temperature between modern reanalysis data from ERA5 and CLIMBER-X near-surface air temperature fields. The “dipole structure” of this bias correction is mentioned mainly in relation to Ganopolski et al. (2010). Since we agree that it can be misleading, we will remove the term ‘dipole’ when not explicitly referring to Ganopolski et al. (2010).

Line 199: Cite/compare to snowfield glaciation versus spreading from high-elevation nucleation sites in Bahadory et al. 2021: Last glacial inception trajectories for the Northern Hemisphere from coupled ice and climate modelling

We will add that a similar behaviour was observed in Calov et al. (2005) and Bahadory et al. (2021).

Table 1: unclear from the table what T offset, geo, and snow albedo offset are

We will expand the caption of the table to include an explanation of all the terms and abbreviations used.

Figure 9 title: Zonal mean differences -> Northern hemisphere zonal mean differences

Will be changed.

Lines 232-236: structure: I’m missing a short experiment description before we dive into the results

We will add a short experiment description.

Line 254: “higher albedo of ice compared to ice-free land”: wouldn’t most of the now ice-free land be snow-covered?

This would not be the case during the ice-retreat phase, which is characterized by negative surface mass balance and snow-free conditions for at least part of the summer.

Figure 14: why not also include the fixgeo experiment here?

We will consider including the fixgeo experiment in the figure.

Figures 16, 17, 19: figure key consistently in the top panel like in other figures

We will move the figure legends to the top panel.

Line 321: "This result is fully consistent with the concept of glacial inception as a bifurcation in the climate system": You haven't really introduced the concept, and I don't quite see how this plays a role here…

The concept was introduced in Calov et al. (2005). We will clarify this issue in the discussion.

Line 334: "A climate acceleration factor of 10 would allow more complex Earth system models to run transient glacial inception simulations in a reasonable time using less computational resources.": Can we assume the finding still holds for more complex models? With the inclusion of more complex feedbacks and non-linearities, I would assume models can't be accelerated as much

Complex Earth system models currently used for modelling glacial inception use large acceleration because, at present, it is not possible to run high-resolution models for 20,000 years. Our results provide some justification for such an approach. The real problem with acceleration starts when strong millennial-scale variability arises, but this is not the case in our simulations of glacial inception.

Line 401: "A constant temperature lapse rate is used": is assuming a constant lapse rate reasonable?

This is a common assumption for such type of studies. We are not aware about any scientific basis for improving this approach.

Line 453: missing equation reference

Will be fixed, thanks.

All ice sheet maps: the grayscale color key offers a poor discernable resolution

We will replace the color map with a more discrete one, which will hopefully make it better readable.

Potentially unnecessary figures if the paper needs to be shortened: Figure 3, 18

We would like to keep Figure 3 as it shows that the model does not simulate glacial inception at present, which is an important constraint, but we will consider removing Figure 18, if asked for it.

References

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