Response to Reviewer #2

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

The manuscript presented by Willeit and co-workers is very much in line with other studies that have been conducted with similar complexity models at long time scales.

We are only aware of a few (apart from our own) modelling attempts to simulate the last glacial inception with coupled climate-ice sheet models, and even fewer realistic ones.

As such, the aim is fine and the research presented is sound. Adequate attention is given to model parametrisation and process analysis, which is the strong point of the presented research. As presented however, the manuscript is an hybrid between a development paper and a research paper. It clearly ***lacks a research focus and is incremental on previous research with the same group****. The latter point is not an issue and should not prevent publication.*

We are grateful to the referee for not ruling out the possibility of publication of our article, but we respectfully disagree with the highlighted statement:

1. We think that the research question is clearly formulated: understanding the mechanism of glacial inception. In particular, why and how ice volume rises so rapidly immediately after the onset of glaciation. Based on other publications related to this topic, we believe that this is still a valid research question. Following also the comments from Reviewer#1 we will add some more discussion in the introduction to make it clearer how our study contributes to the advancement in the understanding of the processes and feedbacks at play during glacial inception.

2. As we discuss below, for this study we use a new and superior Earth system model compared to CLIMBER-2, which we used for a similar study 20 years ago. It is quite common in climate modelling to revisit similar problems time after time as new and better tools are developed and become available. In the paleoclimate context, this is for example the case for numerous simulations of LGM climate performed during four rounds of PMIPs by more or less the same groups, with more or less similar models and with similar boundary conditions.

Main concerns

1-/ Target journal and article format. The manuscript, as mentionned above is an hybrid between a development paper and a first research application paper. I find very surprising that the authors have chosen to pack all this in one manuscript, while other model developments manuscript are already published or in the way with the same first author in Geoscientific Model Development. Personally, I recommend that the current manuscript is *splitted in two parts : a model development manuscript that would cover the themes of ice-sheet coupling, sea-level prediction model coupling and snow mass balance computation (including comparison to present-day fields since this is crucial for ice-sheet evolution). This would remove the large appendixes in the current manuscript and allow proper discussion of the modeling choices made by the expert community. A second, much lighter manuscript, would target the inception question with the model in a Climate of the Past research artcile. I see no good reason to proceed the way that the author did, yielding a manuscript that is complicated to evaluate correctly since other discussion are missing in an already too long manuscript.*

CLIMBER-X is a very well documented Earth system model. There are already three published GMD papers describing different components of CLIMBER-X and their performance: Willeit and Ganopolski (2016), Willeit et al. (2022) and Willeit et al. (2023). In addition, individual components of CLIMBER-X, which were developed outside of PIK – GOLDSTEIN, HAMOCC, SICOPOLIS and VILMA - are described in separate papers. The purpose of a description paper is not only to present model equations but also to demonstrate model performance versus observational data and results of other models. Since in this study we are interested only in ice sheets of the Northern Hemisphere, the only ice sheet for which there are observational and good modelling data at present is Greenland. However, the performance of our model for Greenland has already been presented in Calov et al. (2018) (stand-alone SICOPOLIS model) and Höning et al. (2023) (CLIMBER-X in full configuration). We will add these two references to the revised paper to make it clearer that the model evaluation for Greenland has already been performed elsewhere. For past climates, good modelling results exist only for the LGM, but we have already discussed the CLIMBER-X performance for the LGM in Willeit et al. (2022). Moreover, a more detailed comparison of ice sheet surface mass balance with e.g. PMIP models for the LGM would be of very limited use, as it was shown that the different GCMs produce very different LGM climates that would lead to simulated ice sheets varying widely among models (Niu et al., 2019). Thus, it is unclear what else we could present in a fourth CLIMBER-X description paper proposed by the Referee, except for the set of equations shown in the Appendix.

Two examples of this issue :

line 114-120, page 5. Discussion of the constant bias correction over time in SEMIX is one example where this should be much more discussed and potentially be evaluated : we have many simulations in the different PMIP phases with GCMs that are performed for interglacials where you could test the validity of your stationarity of the bias (using other model anomalies and your own).

We do not understand how simulations of other interglacials can be used to test the stationarity of the temperature bias. The bias is the difference between model simulations and observational (reanalysis) data. Obviously, there are no real observations from the past and paleoclimate reconstructions are extremely uncertain. Thus, model biases for paleoclimates cannot be evaluated in principle. GCMs have their own climate biases, which are comparable in magnitude with the biases of CLIMBER-X, and can therefore not be used as a reference. However, 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 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.

Line 100 : « fields are downscaled onto the high-resolution topography ». This is totally insufficient since most of the results of this manuscript and all the forthcoming with CLIMBER-X are dependent on the details of this downscaling. A detailed evaluation of the downscaling for rough topography should be given in a development manuscript. The current description of the equations in the appendix B1 is clear, but the absence of evaluation is unacceptable.

The term "downscaled" is misleading in this context. As described in Appendix B1, we only use horizontal and vertical interpolation of climatic fields and use the standard temperature correction for elevation with a constant lapse rate. We will rephrase the sentence and replace the term 'downscaled' to make it clearer that we are not performing any dynamic downscaling, but a simple bilinear interpolation with elevation correction.

2-/ The main message of the manuscript is the relationship between surface albedo change (snow cover extent) and vegetation feedbacks that are at the source of the rapid ice-sheet expansion simulated, not the ice-volume. This is a fine conclusion, but is also one that has been largely promoted already by the same group (Ganopolski et al., 2010, doi :10.5194/cp-6-229-2010) with the previous generation of their model. In the current version of the manuscript, little is said about the comparison to this previous results.

Actually, not in Ganopolski et al. (2010) but in Calov et al. (2005a) and Calov et al. (2005b), i.e. nearly 20 years ago. Since then, this conclusion has neither been confirmed nor disproved. The new paper revisits this problem with a new modelling tool. The new results confirm our earlier finding that a rapid expansion of the ice area at the beginning of glacial inception is crucial for simulating realistic ice volume during MIS5d. The importance of this finding crucially depends on the degree of novelty of our modelling tool. The referee wrote:

***Given the amount of components that are shared with the CLIMBER-2 model**, it is in my view a requirement that such a comparison is made to assess what is really new in the study presented or at least what is mostly the same model response and what is not.*

CLIMBER-X is a new Earth system model which does not share any modelling components with CLIMBER-2. The only things they have in common are the acronym and the fact that both these models (as well as CLIMBER-3) were developed in the group led by the last author of the manuscript. Although CLIMBER-X does employ a simplified (compared to modern AGCMs) atmospheric model, it is clear from Willeit et al. (2022), that this model, SESAM, is much more advanced than the atmospheric model in CLIMBER-2 (Petoukhov et al., 2000); the ocean in CLIMBER-2 is a zonally averaged model (Stocker et al., 1992) while in CLIMBER-X we use the 3-D ocean model GOLDSTEIN (Edwards et al., 1998; Edwards and Marsh, 2005; Edwards and Shepherd, 2002). Similarly, the sea ice component in CLIMBER-2 is 1-D and in CLIMBER-X it is 2D with an explicit treatment of sea ice transport; the integrated land-vegetation model PALADYN (Willeit and Ganopolski, 2016) employed in CLIMBER-X is by complexity and degree of realism absolutely incomparable with the simplistic land and vegetation components of CLIMBER-2 (Brovkin et al., 1997;

Petoukhov et al., 2000). The situation is similar with the ice sheet component, SICOPOLIS. Ralf Greve used the same generic name SICOPOLIS for the ice sheet models which he developed during the past ~30 years. The SICOPOLIS version that was used in CLIMBER-2 is a shallow ice approximation model typical for the 1990s, with the simplest local bedrock relaxation parameterization. SICOPOLIS v5.3 (https://doi.org/10.5281/zenodo.6872648), which is used in CLIMBER-X, is a modern hybrid model with a proper treatment of ice streams and ice shelves. Furthermore, CLIMBER-X is coupled with the state-of-the-art 3D solid Earth model VILMA (Klemann et al., 2008; Martinec et al., 2018). Last but not least, CLIMBER-2 has a climate component resolution of 51°x10° and an ice sheet resolution of about 100 km, while CLIMBER-X has a horizontal resolution of 5°x5° and (in this study) 32 km, respectively. In terms of grid cells by unit of area this means an increase by an order of magnitude for both climate and ice sheet components. Higher spatial resolution not only improves model performance, but also allows us to introduce time-dependent land/sea mask and river routing as well as evolution of proglacial lakes, features which for obvious reasons were absent in CLIMBER-2. Thus, CLIMBER-X is new and superior in all respects compared to the 25 years old CLIMBER-2. This is why we think that applying this model to scientific problems which we already studied with CLIMBER-2 is fully justified.
To make it clearer to the reader that CLIMBER-2 and CLIMBER-X are fundamentally different models, we will add some more detailed discussion of the CLIMBER-X model in the methods, in addition to the references to the GMD papers describing CLIMBER-X.

3-/ The discussion of the Figure 7 is not at an appropriate scientific level. Line 208 mentions that the model and data « compares reasonably well » which falls short of the mark. There is then a few consideration on the different places where the model is glaciated and not. However, there is in my view a fundamental problem in the ice evolution presented at 117 and 115 ka. Cited reconstructions seem to indicate that there is a double semi-independent ice-sheet build up, on one side on the Cordilleran ice-sheet and second over the northern part of Canada with more expansion over land in the Nunavut and Quebec areas. The model to the contrary indicates a very zonal expansion over the Hudson Bay, which is not what is indicated in the reconstructions (that are uncertain, but clearly indicate this more extensive expansion over Canada). The model also have a tendency to merge the two ice-sheets (Cordilleran and Nunavut/Northern Territories). What is the impact of all this on the results ?

As acknowledged by the reviewer, the general introductory sentence "compares reasonably well…" is followed by a whole paragraph discussing agreement and disagreement between model and reconstructions over the different regions. We believe that a more quantitative ('scientific'?) comparison of model and reconstructions is clearly not possible, given the poor quality of the ice sheet reconstructions. Any pre-LGM ice sheet reconstruction should be treated as a rough representation of sparse empirical data supported by some modelling exercises. This is for instance the case for the Batchelor et al. (2019) reconstruction, where the table in the Supplementary Information describes the sources of information used to reconstruct ice sheets at MIS 5d. For North America they are: (1) single empirical outline (sketch) from (Kleman et al., 2010), the dating of which is uncertain (5b or 5d); (2) results of several model simulations, including our own simulations with CLIMBER-2 (Ganopolski and Calov, 2011) and another CLIMBER-2 simulation performed at LSCE (Bonelli et al., 2009); (3) a single empirical data point which is used to constrain the ice sheet extent in the eastern part of North America (Figure 6c Supplementary Information, Batchelor et al. 2019). This large uncertainty is also reflected in the "min", "best" and "max" reconstructions in Batchelor et al. (2019). We performed a set of quasi-equilibrium experiments using CLIMBER-X constrained by the ice extent of all Batchelor's reconstructions (S. Talento pers. communication) and found that the "best" 5d reconstruction significantly underestimates global ice sheet volume at that time while "max" significantly overestimates it. Whether one can conclude from this fact that the truth is somewhere in-between of "best" and "max" – we do not know.

Likewise, very little is said about the clear expansion of the ice-sheet in Alaska (obvious tendency in figure 14 that is only partially corrected) and which was already a persistent feature of ice-sheet simulations in CLIMBER-2.

'Excessive' glaciation of Alaska at the LGM isn't a persistent feature only of CLIMBER-2 but also of other models of the same class and complex models. As far as the glaciation of western North America during MIS 5d is concerned, it is not constrained by paleodata, and one can only guess how much ice was in Alaska during this time.

4-/ There is no discussion of the potential impact of a fixed Antarctic ice-sheet. It is mentionned at the beginning but then totally ignored. This is very much worrying since the authors are simulating sea-level. At the very least, a discussion of the potential impact, limitations etc. should be included.

We agree that such discussion is needed. However, it is unclear why the Referee considers not modelling the Antarctic ice sheet to be "very much worrying". Most studies of glacial inception are limited to the Northern Hemisphere. The assumption is that the AIS contribution to global ice volume during glacial inception is small and it is relevant mainly for comparison of simulated and reconstructed sea level. In the publications where the Antarctic ice sheet was included, from the classical Huybrechts (2002) and 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#1 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.

5-/ In many places in the manuscript, the term « carbon cycle feedback » is used, but for me there is no carbon cycle feedback simulation in this research. The pCO2 of the atmosphere is fixed to reconstructions and vegetation is simulated on land (so probably the carbon as well) but not discussed. If the authors means « vegetation feedbacks » which is my guess there, then it should be corrected accordingly.

The Referee is right. In the case of the interactive carbon cycle, as it was in the studies by Ganopolski and Brovkin (2017) and Willeit et al. (2019), the climate-carbon cycle feedback was explicitly modelled and discussed. In the current study, since CO_2 concentration is prescribed, the term "the effect of CO_2" is more appropriate than "carbon cycle feedback" and it will be changed accordingly.

Minor concerns

1-/ line 22, page 2 « interglacial (no significant ice sheets over the northern continents) ». Please reformulate. I have a hard time not finding the current Greenland ice-sheet not significant.

We will reformulate this to account for Greenland.

2-/ line 236, page 14, « deciduos » → « deciduous »

Will be fixed, thanks.

3-/ line 172 : « continuosly » → « continuously »

Will be fixed, thanks.

4-/ line 181-182 : justification of such a starting point for the ice-sheet model is not justified. Why equilibrium at 125ka and not another condition ? How is this impacting the dynamics of the first part of your experiment, not having an transient evolution at the start ?

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 only a very limited role in the glacial inception over the other NH continents. We will add a few more sentences on the reasoning behind the model initialization procedure in the revised manuscript.

5-/ line 184 : another instance of « carbon cycle feedback », misleading

Agreed: "carbon cycle feedback" will be changed to "the role of CO_2 variations".

References

Albrecht, T., Winkelmann, R., and Levermann, A.: Glacial-cycle simulations of the Antarctic Ice Sheet with the Parallel Ice Sheet Model (PISM) – Part 2: Parameter ensemble analysis, Cryosph., 14, 633–656, https://doi.org/10.5194/tc-14-633-2020, 2020.

Batchelor, C. L., Margold, M., Krapp, M., Murton, D. K., Dalton, A. S., Gibbard, P. L., Stokes, C. R., Murton, J. B., and Manica, A.: The configuration of Northern Hemisphere ice sheets through the Quaternary, Nat. Commun., 10, 1–10, https://doi.org/10.1038/s41467-019-11601-2, 2019.

Bonelli, S., Charbit, S., Kageyama, M., Woillez, M.-N., Ramstein, G., Dumas, C., and Quiquet, a.: Investigating the evolution of major Northern Hemisphere ice sheets during the last glacial-interglacial cycle, Clim. Past Discuss., 5, 1013–1053, https://doi.org/10.5194/cpd-5-1013-2009, 2009.

Brovkin, V., Ganopolski, A., and Svirezhev, Y.: A continuous climate-vegetation classification for use in climate-biosphere studies, Ecol. Modell., 101, 251–261, 1997.

Calov, R., Ganopolski, A., Claussen, M., Petoukhov, V., and Greve, R.: Transient simulation of the last glacial inception. Part I: glacial inception as a bifurcation in the climate system, Clim. Dyn., 24, 545–561, https://doi.org/10.1007/s00382-005-0007-6, 2005a.

Calov, R., Ganopolski, A., Petoukhov, V., Claussen, M., Brovkin, V., and Kubatzki, C.: Transient simulation of the last glacial inception. Part II: Sensitivity and feedback analysis, Clim. Dyn., 24, 563–576, https://doi.org/10.1007/s00382-005-0008-5, 2005b.

Calov, R., Beyer, S., Greve, R., Beckmann, J., Willeit, M., Kleiner, T., Rückamp, M., Humbert, A., and Ganopolski, A.: Simulation of the future sea level contribution of Greenland with a new glacial system model, Cryosphere, 12, 3097–3121, https://doi.org/10.5194/tc-12-3097-2018, 2018.

Edwards, N. and Shepherd, J.: Bifurcations of the thermohaline circulation in a simplified three-dimensional model of the world ocean and the effects of inter-basin connectivity, Clim. Dyn., 19, 31–42, https://doi.org/10.1007/s00382-001-0207-7, 2002.

Edwards, N. R. and Marsh, R.: Uncertainties due to transport-parameter sensitivity in an efficient 3-D ocean-climate model, Clim. Dyn., 24, 415–433, https://doi.org/10.1007/s00382-004-0508-8, 2005.

Edwards, N. R., Willmott, A. J., and Killworth, P. D.: On the Role of Topography and Wind Stress on the Stability of the Thermohaline Circulation, J. Phys. Oceanogr., 28, 756–778, https://doi.org/10.1175/1520-0485(1998)028<0756:OTROTA>2.0.CO;2, 1998.

Ganopolski, A. and Brovkin, V.: Simulation of climate, ice sheets and CO_2 evolution during the last four glacial cycles with an Earth system model of intermediate complexity, Clim. Past Discuss., 1–38, https://doi.org/10.5194/cp-2017-55, 2017.

Ganopolski, A. and Calov, R.: The role of orbital forcing, carbon dioxide and regolith in 100 kyr glacial cycles, Clim. Past, 7, 1415–1425, https://doi.org/10.5194/cp-7-1415-2011, 2011.

Ganopolski, A., Calov, R., and Claussen, M.: Simulation of the last glacial cycle with a coupled climate ice-sheet model of intermediate complexity, Clim. Past, 6, 229–244, https://doi.org/10.5194/cp-6-229-2010, 2010.

Höning, D., Willeit, M., Calov, R., Klemann, V., Bagge, M., and Ganopolski, A.: Multistability and Transient Response of the Greenland Ice Sheet to Anthropogenic CO_2 Emissions, Geophys. Res. Lett., 50, 1–11, https://doi.org/10.1029/2022GL101827, 2023.

Huybrechts, P.: Sea-level changes at the LGM from ice-dynamic reconstructions of the Greenland and Antarctic ice sheets during the glacial cycles, Quat. Sci. Rev., 21, 203–231, https://doi.org/10.1016/S0277-3791(01)00082-8, 2002.

Klemann, V., Martinec, Z., and Ivins, E. R.: Glacial isostasy and plate motion, J. Geodyn., 46, 95–103, https://doi.org/10.1016/j.jog.2008.04.005, 2008.

Martinec, Z., Klemann, V., van der Wal, W., Riva, R. E. M., Spada, G., Sun, Y., Melini, D., Kachuck, S. B., Barletta, V., Simon, K., A, G., and James, T. S.: A benchmark study of numerical implementations of the sea level equation in GIA modelling, Geophys. J. Int., 215, 389–414, https://doi.org/10.1093/gji/ggy280, 2018.

Niu, L., Lohmann, G., Hinck, S., Gowan, E. J., and Krebs-Kanzow, U.: The sensitivity of Northern Hemisphere ice sheets to atmospheric forcing during the last glacial cycle using PMIP3 models, J. Glaciol., 65, 645–661, https://doi.org/10.1017/jog.2019.42, 2019.

Petoukhov, V., Ganopolski, a., Brovkin, V., Claussen, M., Eliseev, a., Kubatzki, C., and Rahmstorf, S.: CLIMBER-2: a climate system model of intermediate complexity. Part I: model description and performance for present climate, Clim. Dyn., 16, 1–17, https://doi.org/10.1007/PL00007919, 2000.

Stocker, T. F., Mysak, L. A., and Wright, D. G.: A Zonally Averaged, Coupled Ocean-Atmosphere Model for Paleoclimate Studies, J. Clim., 5, 773–797, https://doi.org/10.1175/1520-0442(1992)005<0773:AZACOA>2.0.CO;2, 1992.

Willeit, M. and Ganopolski, A.: PALADYN v1.0, a comprehensive land surface–vegetation–carbon cycle model of intermediate complexity, Geosci. Model Dev., 9, 3817–3857, https://doi.org/10.5194/gmd-9-3817-2016, 2016.

Willeit, M., Ganopolski, A., Calov, R., and Brovkin, V.: Mid-Pleistocene transition in glacial cycles explained by declining CO 2 and regolith removal, Sci. Adv., 5, eaav7337, https://doi.org/10.1126/sciadv.aav7337, 2019.

Willeit, M., Ganopolski, A., Robinson, A., and Edwards, N. R.: The Earth system model CLIMBER-X v1.0 – Part 1: Climate model description and validation, Geosci. Model Dev., 15, 5905–5948, https://doi.org/10.5194/gmd-15-5905-2022, 2022.

Willeit, M., Ilyina, T., Liu, B., Heinze, C., Perrette, M., Heinemann, M., Dalmonech, D., Brovkin, V., Munhoven, G., Börker, J., Hartmann, J., Romero-Mujalli, G., and Ganopolski, A.: The Earth system model CLIMBER-X v1.0 – Part 2: The global carbon cycle, Geosci. Model Dev., 16, 3501–3534, https://doi.org/10.5194/gmd-16-3501-2023, 2023.