Response to Referee#1

General comments

- From l. 175 to 237, the authors selected three combinations that cover the spectrum in terms of insolation / CO2 to achieve inceptions and they discuss the temporal and spatial evolutions of the ice sheets. What we mostly see is that the AMOC is very different for these three simulations. Although I appreciate this section, I found it hard to compare directly these simulations because of the differences in terms of AMOC. I have the feeling that we are not really looking at the impact of insolation/CO2 on ice sheet evolution here but instead we are looking at the impact of inso/CO2 on the AMOC (and ultimately yes, the ice sheet evolution). Perhaps what could have been done is alternative experiments using larger freshwater flux than the ones in Sec. 3.2, in order to have a shutdown state for these three simulations. In doing so, we would have quantified the impact of insolation/CO2 disregarding the state of the AMOC.

We appreciate this reviewer's comment and agree that the role of individual factors should be discussed. To this end we performed a set of additional experiments with prescribed present-day ice sheets. Namely, we performed three sets of quasi-equilibrium experiments where 1) only orbital forcing was set to Hsmx65 and Lsmx65 values with constant 280 ppm CO$_2$ concentration; 2) CO$_2$ was set to LCO2 and HCO2 values with the present-day orbital forcing; 3) Water hosing experiment under present-day boundary conditions in which AMOC was weakening by ca. 10 Sv which corresponds to the difference between Hsmx65_LCO2 and Lsmx65_HCO2 experiments. Weaker AMOC state was obtained by applying of a constant freshwater hosing of 0.1 Sv in the northern North Atlantic. The results are shown in the figure below. The main conclusion is that summer temperatures at the locations where the ice sheets start to growth during glacial inceptions (i.e. Scandinavia and northeastern Canada), are much more sensitive to CO2 and orbital forcing than to changes in the AMOC. It has to be noted that changes in CO$_2$ and insolation also affect AMOC. In particular, AMOC is stronger by 9 Sv in experiments with high CO$_2$ compared to low CO2 and by 3 Sv in experiments with high insolation compared to low insolation. However, as it is seen from the right panel, these changes can contribute only a little to the direct effect of CO$_2$ (left panel) and insolation (middle panel). Thus, in both potential locations of glacial inception, AMOC changes provides positive, but not very strong, feedback to both primary forcings. We will discuss this issue in the revised manuscript.

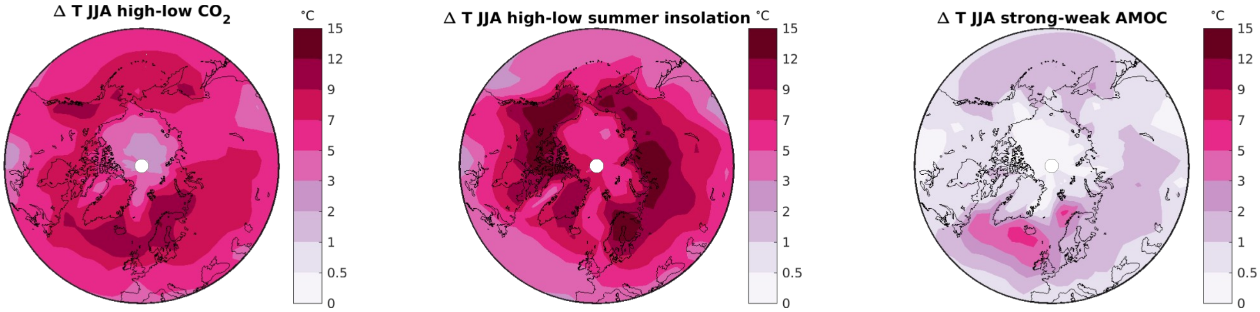

Fig. 1. Summer temperature difference between experiments with high (460 ppm) and low (190 pp) CO$_2$ concentration (left panel); high (496 W/m^2) and low (430.8 W/m^2) maximum summer insolation at 65°N (middle panel); two states in which AMOC strength differs by 10 Sv (right panel).

- Sensitivity of the AMOC to CO2. The simulations presented here show a general weaker AMOC for lower CO2. The authors suggest that it was also the case in CLIMBER-2 and that some GCMs also display this feature. However, in fact, the majority of GCMs show the opposite: higher CO2 levels are associated with weaker AMOC (e.g. Swingedow et al., 2007; Ma et al. 2021; Fortin et al., 2023), due to several processes (high latitude water cycle intensification, energy dissipation by eddies, etc.). Since the results presented here show a very strong dependence of glacial inception to the AMOC state, I think that a more thorough discussion on how robust are the changes in AMOC can be considered in models (not specifically yours). You might have ideas why lower CO2 leads to weaker AMOC?

The reviewer is perfectly right: in all models, including all CLIMBER models, AMOC is weakening in response to abrupt or gradual CO_2 rise. However, this is a transient, centennial to millennial time scale response. In our paper, we performed experiments where the boundary conditions were kept constant for a sufficiently long time to reach an equilibrium state. Unfortunately, there are not many studies with GCMs where equilibrium AMOC response to different CO_2 levels was investigated. The first was by Stouffer and Manabe (2003), who found AMOC strengthening under doubling and quadrupling CO_2 and significant AMOC weakening under CO_2 halving in multimillennial simulations. Recently, Bonnan et al. (2022) described AMOC response to instantaneous CO_2 quadrupling. Mots of runs were not long enough, and AMOC continues to evolve through the runs, but at least in one model (CESM1), AMOC is appreciably stronger under CO_2 quadrupling. For lower CO_2 concentration, there are two systematic studies (Galbraith and Lavergne, 2019; Oka et al., 2012) where it has been shown that global cooling causes a weakening of the AMOC. It was also shown in these studies that large ice sheets tend to stabilize AMOC, but this is not applicable to our study of glacial inception, where simulated ice sheets are rather small. Thus, the AMOC-CO_2 relationship simulated in CLIMBER-X is consistent with previous modeling results. There are likely several reasons for AMOC weakening under low CO_2, and one of them is related to a larger increase in AABW density compared to NADW. This, in turn, can be explained by enhanced sea ice formation in the Southern Ocean and salinity increase of AABW, the effect which has already been demonstrated in Ganopolski et al. (1998). As far as the reviewer's assumption about the "very strong dependence of glacial inception to the AMOC state" is concerned, as shown above, it is not correct – AMOC plays some, but definitely not decisive, role in glacial inception.

Specific comments

- l. 53. By construction, in the radiative scheme of CLIMBER-X there is a logarithm structure with respect to CO2, right? So there is no surprise here.

Logarithmic dependence of radiative forcing on CO2 is not surprising, but it is not prescribed in the model. In CLIMBER-X the effect of CO2 (as well as water vapor) on radiation fluxes, is computed using the integral transmission function (see Appendix A8 in Willeit et al., 2022).

- l. 64. "(iv)": be more specific please.

Agreed. Will be explained in more details.

- l. 72. CLIMBER-X is really expected to reproduce the observed decadal variability? This is quite surprising given the model assumptions I guess.

No, CLIMBER-X does not simulate **internal** decadal-scale variability by its design, but it can simulate the forced response at this time scale, such as response to volcanic eruptions and CO_2 changes (see Fig. 19 in Willeit et al., 2022).

- l. 74. What is the vertical resolution of the oceanic model?

Vertical ocean model resolution: 23 unequally spaced vertical layers, with a 10m top layer and layer thickness increasing with depth and reaching 500m at the ocean bottom (Willeit et al., 2022).

- l. 78. Is there any reference for SEMIX? I know that it has been used before but it could be useful to have a reference here.

SEMIX model description is given in accompanying paper by Willeit et al., 2023b which is currently under review in CPD. We hope, it will be published soon as CP paper.

- l. 84-85. Reference?

Willeit et al. (2022)

- l. 87. It would have been nice to have a map of the major biases (temperature / precip at least) or a reference here.

Temperature biases are shown in Willeit et al. (2023b) (Fig. B1). We will also include precipitation biases in this paper and will give an appropriate reference here.

- l. 96-97. How do you conserve the water budget while using an acceleration factor? How do you compute your freshwater flux? This is important since it will impact the AMOC in all your simulations.

1) The conservation of the water budget in an Earth system models implies that the sum of water contents in the ocean, atmosphere, land and ice sheets remains constant. In CLIMBER-X, atmosphere-land and ice sheet components conserve water, but the ocean model (GOLDSTEIN) is based on the rigid-lid approximation, i.e. it does not include a prognostic equation for the ocean volume. Instead, the ocean volume and global sea level are determined by the global ice volume and the volume of the ocean is regularly adjusted by scaling the thicknesses of the ocean layers below a depth of 1000m to match the actual ocean volume derived from sea level change (Willeit et al. 2022). This ensures that the global change in salinity and other tracers, which is important in the case of the interactive carbon cycle, are consistent with the simulated ice sheet volume. This procedure works the same way irrespectively of whether acceleration of climate components is applied or not.

2) Freshwater into the ocean from ice sheets in the acceleration experiments is computed the same way as in nonaccelerated: annual freshwater flux into the oceans is computed as a sum of surface runoff routed in the ocean according to the ice sheet/land topography and calving (solid ice discharge), which is delivered to the nearest ocean grid cell.

3) As far as the impact of freshwater flux associated with the growth of ice sheet on AMOC is concerned, it is negligible. Indeed, in the simulations which is used to determine the critical insolation-CO2 relationship, growth of less than 10 meters in sea level equivalent occurs during at least several tenths of thousand years, which gives a net evaporation from the North Atlantic of less than 0.01 Sv. Such freshwater flux has a negligible effect on AMOC. The main difference in AMOC strength between experiments with low and high CO_2 is explained by the effect of temperature on AMOC.

- l. 98-101. Do you need such a large acceleration factor since CLIMBER-X can perform 10 kyr a day right? Maybe it could have been nice to present a few additional experiments with no acceleration since it can affect your inception threshold.

Yes, we need a significant acceleration of the climate component to perform the stability analysis of the climate-cryosphere system in CO2-insolation phase space. In fact, 10 kyr per day is the performance of the climate component only (Willeit et al. 2022) and, in the case of interactive ice sheets (spatial resolution 30 km), CLIMBER-X is twice slower. Even though it is still much faster than GCM-based ESMs, one should not underestimate the amount of computations performed for this paper. To trace the stability diagram (Fig. 2) we performed experiments for 19 different orbital configurations. For each orbital configuration, we performed, on average, about 25 simulations with different CO2 to trace the critical CO2 value with an accuracy of 5ppm. Since each run was 100,000 years long, the total simulation length is 50 million years (!). In the case of the acceleration factor=10, this is only the ice sheet model years, but without acceleration, it would also be climate model years. Taking into account the model performance of 5 hr per 1000 years and the fact that each model run uses 16 processors, such simulations would require 4 million CPU hours. This is too expensive to produce just a single curve. Moreover, we have confidence that this can be done at least ten times cheaper, as demonstrated by Willeit et al. (2023b).

- l. 101. Why not using a properly spun-up ice sheet instead of using an ad-hoc vertical structure?

The meaning of “proper spin-up” is unclear in the context of our ensemble of quasi-equilibrium experiments aimed at tracing the stability diagram of the climate-cryosphere system. For present-day Greenland, spin-up is usually performed by running the model through the previous glacial cycle, but in the case of our study, there is nothing “previous” since our simulations are not related to real-time. Since the only ice sheet which is present in initial conditions is the Greenland Ice Sheet, we do not expect that the choice of initial temperature distribution in the Greenland ice sheet can affect in any way glacial inception, which is typically simulated 20 to 60 kyr after the beginning of the experiments and the GrIS has therefore enough time to adjust to the fixed climate forcings (orbital and CO2).

- Figure 1. It could have been useful to have indications for the different marine isotope stages in this graph (MIS 5,7,9 and 11).

In this study (unlike Willeit et al., 2023b), we did not model any specific (real) glacial inceptions. Instead, we just permutate different combinations of orbital parameters and CO_2 concentrations. Most of such combinations never materialized during the past 800 kyr. This is why referring in the paper to the real MISs can be misinterpreted.

- l. 156-158. You should perhaps temper this result since CLIMBER-X and CLIMBER-2 are not completely independent. In particular, they share a quite similar atmospheric model (although at a different spatial resolution).

In fact, CLIMBER-2 and CLIMBER-X are completely different models. Apart from very different spatial resolutions, all components of CLIMBER-X differ significantly from those in CLIMBER-2 (even if they have the same names as, for example, SICOPOLIS). However, we fully agree with the reviewer that all modelling results, irrespective of model complexity, are model-dependent. This is why we will change the sentence “that values for α and β might not be strongly model dependent” to “that values for α and β might not be as uncertain as it was assumed in Talento and Ganopolski (2021)”. Note that in this publication, we considered as “acceptable” all values α in the range from -150 to 0 (W m^{-2}).

- l. 261-269. Do these experiments display the same AMOC states as when using interactive ice sheets?

Since AMOC is primarily controlled by CO_2 in our model, in these experiments, AMOC is similar, but not identical, to the corresponding experiments with interactive ice sheets.

- l. 292-293. What about the role of shortwave radiation? The high CO2 simulation have a larger surface temperature, higher precipitation but also smaller shortwave radiation. I am a bit surprised to read here that the positive SMB is explained by larger precipitation rate. In my experience the extent of the accumulation area is primarily driven by temperature/shortwave (i.e. melt).

We fully understand the reviewer's bewilderment. Indeed, it is generally recognized that the mass balance of the ice sheet is primarily controlled by insolation and temperature, while precipitation plays a secondary role. However, in Fig. 8, we do not compare cold climates with warm climates. Here, we compare climate conditions along our critical insolation-CO_2 line. The question we address here is how similar/different are climate conditions under which glacial inception occurs in the high CO_2 and low CO_2 worlds. Not surprisingly, in terms of summer temperature, climate conditions are rather similar because they all correspond to "cold" summers. But there are some interesting nuances, namely, that in the wet (high-CO_2 world), glacial inception can occur under summer temperatures similar to PI (but with high precipitation) in the areas of ice sheet nucleation, while in the dry (low-CO_2) world, glacial inception requires temperatures lower than PI because precipitation is also lower. Thus, these results do not contradict the notion that temperature and insolation are the primary drivers of glacial inception. We will clarify this issue in the revised version of the manuscript.

- l. 336-338. Is this difference explained by AMOC differences, here again?

The primary reason for the scattering of the individual points around the logarithmic curve in Fig. 2 is that the metric for orbital forcing, which we use in this and previous studies - maximum summer insolation at 65N - is a good, but not a perfect one. This is not surprising in the view that the effects of precession and obliquity on insolation differ both in space and in temporal dynamics. The "outlier" mentioned in the manuscript, corresponds to orbital parameters at 209 ka when obliquity was 24.3°, which is close to its maximum value during the past 800 kyr. At the same time, seven other points with a similar smx65 and which are clustering around the logarithmic curve in Fig. 2 correspond to times when obliquity was close to its average value or lower. This is consistent with the notion that obliquity may be slightly undervalued in the smx65 metric. Thus, one would expect that triggering of glacial inception under higher obliquity for the same smx65 would require a lower CO2, which is precisely what is seen in our modelling results. It is also worth mentioning that all real glacial inceptions of the last 800 years occurred during periods of low or medium obliquity

- l. 410. Not necessarily, e.g. Harder et al. (2017).

Apparently, the reviewer disagreed with our statement that "surface latent heat flux can be neglected during the melt season." This assumption is justified by the fact that during the short melt season in the considered region, latent heat flux is typically smaller by an order of magnitude than short-wave and long-wave radiation fluxes (e.g. Ettema et al., 2010). In principle, the effect of latent heat flux can be incorporated by modifying the parameter γ in Eq. 5, but this parameter does not enter the final expression (Eq. 12) anyhow.

As far as the interesting results presented by Harder et al. (2017) are concerned, the situation with patchy snow cover at the end of the snow season for which their measurements were performed, obviously, is not applicable to large perennial snow fields, which are the precondition for glacial inception.

- l. 426. It is quite variable geographically and depends on continentality, seasonality of precipitation etc.

Our assumption that “the duration of the melt season at the location of glacial inception is about two months” is applicable only to the region where glacial inception is simulated by our model, namely, the Canadian Arctic. Since present (more precisely, PI) climate conditions are rather close to the condition of glacial inception, the continentality, seasonality, precipitation, and other characteristics of climate in this area at the time of glacial inception must be similar to the known present ones. Note that the duration of the melt season only affects (and not significantly) the values of k_S and k_T, which also do not enter the final expression (Eq. 12).

- l. 431. I guess it comes from the temporal integral between the beginning and the end of the melt season? Why these are different for insolation and temperature then, if in both case the integral is over two month around the peak value?

The reason is that although temperature and insolation have a similar seasonal evolution, they are described by different formulas. At 65°N, daily insolation S(t) can be described with good accuracy as

$$S(t) = 0.5 S_{\max} (1 + \cos \omega t),$$

while surface air temperature is given by the formula

$$T(\tau) = A \cos \omega \tau + B = T_{\max} + A(\cos \omega \tau - 1),$$

where $\omega=2\pi/365$, t is time counted from the summer solstice, τ is time counted from the date of temperature maximum which lags summer solstice by about month ($t-\tau \approx 30$ days), A is the amplitude of seasonal temperature variations and T_{max} maximum surface air temperature. This is why, when integrating these two characteristics during the melt season, the relationship between average insolation and maximum insolation, and between averaged temperature and maximum temperature, are different. However, the values k_S and k_T are not very sensitive to the choice of the duration of the melt season, and as it was noted above, these parameters do not enter the final formula (Eq. 12).

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