In the following, the reviewer comment are in blue and our response in black.

Summary:

The study by Quiquet and Roche analyzes various aspects of the climate and ice-sheet evolution in the last two glacial terminations using the intermediate complexity model iLOVECLIM with an interactive ice sheet component. Experiments are presented in which the model is integrated forward from the glacial maximum state (LGM and PGM) through the deglaciation and the interglacial periods. Sensitivity experiments that isolate the influence of individual forcings (e.g., meltwater fluxes, insolation changes, greenhouse gas variations, etc.) are also conducted. The main conclusions are: (i) the Last Interglacial was warmer and had a higher sea-level than the Holocene; (ii) insolation variations is the main driver of glacial retreat during both interglacial periods; (iii) the Atlantic overturning circulation is found to be more sensitive to collapse under Last Interglacial forcing.

The main novelty of the manuscript is the side-by-side comparison of the last two deglaciations in a coupled model setting. However, it is not clear from the presentation what the truly new results are and in what way this study is advancing our understanding of the last two deglaciations. There are several reasons for this, but most importantly because (i) the manuscript does not include a dedicated discussion section where the results are contrasted with the established literature; (ii) the model is quite simplistic and may not be the most appropriate choice for this type of study; (iii) some of the results are undoubtedly model dependent as they contradict previously published results using other models.

I recommend major revisions before this manuscript can be accepted for publication.

Thank you for your thorough review of our manuscript. We have revised the text according to your suggestions. Notably we have added a dedicated discussion section where we discuss simulated atmospheric circulation changes between the LGM and the PGM, using different ice sheet geometries. In this new section, we also discuss our results with respect to the existing literature.

Major comments:

No discussion section:

The lack of a dedicated discussion section makes it hard to get a sense for how the results compare to the established literature and what the potential shortcomings of the study are. You do cite several papers in the results section, but these are primarily used to quantify (and to a certain extent justify) your results. A dedicated discussion section is essential for any study, and this manuscript would certainly benefit from having one as well.

We have added such a section in the revised version of our manuscript.

QGPV model at low resolution:

I wonder how appropriate the model choice is for this study. From reading the model description in Quiquet et al. (2021), the atmospheric component of iLOVECLIM is a spectral, quasi-geostrophic potential-vorticity (QGPV) model that was run at a nominal 5.6-degrees (T21) horizontal resolution.

It seems to me that this model choice is potentially problematic for at least two reasons:

(i) Several studies have shown conclusive evidence that the numerical convergence of both dry and moist dynamical cores breaks down somewhere between the T31 and T21 resolutions (e.g., Polvani et al. 2004; Lofverstrom and Liakka, 2018), and that resolution can have a substantial influence on the simulated climate (Lohman et al., 2021). The reason for this breakdown is (most likely) that the grid spacing becomes comparable to, or even exceeding the Rossby deformation radius in midlatitudes on sufficiently coarse model grids. This means that baroclinic waves are not appropriately resolved, which are one of the main drivers of the large-scale atmospheric circulation, including the distribution of temperature, precipitation, and wind in mid and high latitudes. While I recognize that it may not be feasible to run the simulations at a different resolution, this potential shortcoming should at least be acknowledged and discussed in the manuscript.

Following your other comment, we have added a discussion section in the revised manuscript, in which we confront our results to existing literature more thoroughly and we present the limitations related to model resolution and physical approximation. We also show the simulated atmospheric circulation differences between the LGM and the PGM.

(ii) I would like to see a thorough discussion on the appropriateness of using a QGPV model as the atmospheric component in a coupled, global model configuration. QGPV is a decent first-order approximation of the synoptic and planetary scale circulation in mid and high latitudes, but it is not an appropriate description of tropical and subtropical circulation where ageostrophic processes dominate because of the smallness of the Coriolis parameter near the equator. Can we really trust a coupled atmosphere-ocean model that is largely incapable of representing the low-latitude atmospheric circulation with even first order accuracy?

The reviewer raises indeed an important limitation of quasi-geostrophic models. However this problem has been identified during the initial development of the model. ECBilt includes ageostrophic terms in the vorticity equation (Opsteegh et al., 1998) that are neglected in the traditional QG approximation. These terms are computed diagnostically from the wind divergence and the tendency of the streamfunction, using an iterative method. We have added this precision of the revised version of the manuscript.

More generally, even with such limitations, this class of models has been proven useful in the past to study global climate dynamics on millenial timescale. An example of such study on the East-Asian Monsoon system (Caley et al., 2014, Nature Comm.) has shown that the model can reproduce some aspects of the multimillenial precipitation evolution in such regions favourably when compared with water isotopologues proxy records. A few additional examples of studies that have used iLOVECLIM or LOVECLIM model and published in highly-cited journals could further include Roche et al. (2004), Renssen et al. (2015), Menviel et al. (2018), Golledge et al. (2019), Menviel et al. (2020), Yin et al. (2021), Park et al. (2023), and many more. Therefore, it is fair to write that such models have been evaluated and confronted to palaeo-data on a range of diverse applications and that their versatility in computing sensitivity experiments renders them somehow more robust than GCMs that have mostly only run time-slices experiments for dedicated time period.

No discussion about atmospheric circulation changes:

Previous studies have shown that the large-scale atmospheric circulation is strongly influenced by both the height and spatial distribution of the Northern Hemisphere ice sheets (e.g., Lofverstrom and Lora, 2017; Kageyama et al. 2021). Importantly, it has been shown that the North American ice sheet affects the temperature and precipitation distribution (i.e., the surface mass balance) over the Eurasian Ice Sheet (e.g, Liakka et al., 2016).

I think this study would be more convincing if the authors also included figures showing changes in the atmospheric circulation. Not least since the ice-sheet mass balance (i.e., the deglaciation) is to first order driven by changes in the temperature and precipitation distribution, and the QGPV atmospheric model is quite simplistic and may not capture some of the main circulation changes identified in numerous other studies using more comprehensive models.

Atmospheric circulation difference between the LGM and PGM is now shown and discussed with respect to the existing literature.

With respect to the quality of SMB with more comprehensive models, it has been shown on several occasions GCM model outputs are not necessarily appropriate to drive ice sheet models. Using outputs from PMIP3 and PMIP4 model ensemble, both Niu et al. (2019) and van Aalderen et al. (2023) have shown that only a subset of these models were able to maintain reasonable Northern Hemisphere ice sheets at the LGM. The simulated ice sheets at the LGM with iLOVECLIM are not too far from the reconstructions which is an indication that the model is not drastically misrepresenting the LGM climate.

Model dependence:

It is compulsory to discuss potential model dependence on results and conclusions in any modeling study. You mention model dependence in a few places in the text, but it would be good to consolidate this in a dedicated discussion section. One of your main conclusions is that insolation is more important for deglaciation than vegetation changes. I agree that this is what your results shows, but it appears to be contradicting the results in, e.g., Sommers et al (2021), who argued that vegetation changes are at least equally important, if not more important than insolation changes for the deglaciation of Greenland in the Last Interglacial. This is just one example of potential model dependence of your results that should be acknowledged and properly discussed in the manuscript.

We agree that we only use one climate model and that our results are representative of this specific model. We do think that inter-model comparison exercises are really useful with this respect so that we can compare model-specific behaviour to more general responses to forcing changes. This is why we have participated to PMIP4 LGM (Kageyama et al., 2021) and deglaciation experiments (in preparation) with our version of the iLOVECLIM model. We follow the same strategy with the GRISLI ice sheet model, participating to the recent ISMIP6, ABUMIP and LarMIP experiments. Outcome of such participations is that our models are not particularly standing out with respect to other participating models.

Unfortunately, there is not yet any intercomparison exercise of coupled ice sheet – climate model simulations of glacial terminations. One reason is that it is far out of reach from most GCM modelling groups at present, but this might change in the future thanks to better computational facilities and improved numerical scaling.

The paper of Sommers et al. (2021) is indeed very relevant since they used a coupled ice sheet – climate model to simulate the last interglacial period. However a direct comparison of this study with our work is not obvious. While we simulate the entire glacial termination, starting from the PGM, Sommers et al. (2021) start their simulation close to the peak insolation of the LIG, at 127 kaBP. Thus they focus on Greenland ice sheet change, not Northern Hemisphere ice sheets. That being said, if our results show that the vegetation change is not the major driver for Northern Hemisphere ice sheet retreat during TII, we nonetheless simulate a larger Greenland ice sheet volume during the LIG when vegetation change is discarded (+20% in ice volume). This is consistent with the work of Sommers et al. (2021). We have added the comparison with this work in the new discussion section of our revised manuscript.

General experiment design:

I am confused by the experiment design. The introduction states that the Northern Hemisphere ice sheet distribution in the PGM and the LGM were quite different, where the former had comparatively more ice in Eurasia relative to the LGM, and vice versa in North America.
However, the experiments presented here use the same ice sheets as initial conditions for both the LGM and the PGM. What is the reason for this choice since this appears to be a substantial deviation from reality? Would a different ice sheet initial condition alter the results in any way, for example through differences in the large-scale atmospheric circulation?
I recognize that spinning up the ice sheets for the PGM is a major task that may be computationally unfeasible. Therefore, I am not necessarily recommending that you re-run the simulations with more appropriate ice sheets for the PGM, but a discussion of the potential influence of these types of deviations from reality should at least be recognized and appropriately discussed in a dedicated discussion section.

Sensitivity experiments with different ice sheets at the LGM (smaller North American ice sheet and larger Eurasian ice sheet) were already part of the initial manuscript. However, it is true that we did not expand too much on the outcomes of these experiments. We have now added more description of the results of these sensitivity experiments. We also discuss the impact of different topographies on the simulated atmospheric circulation.

The motivation for having identical ice sheets as initial conditions for our transient experiments is twofold:
- The extent and size of the PGM ice sheets is a scientific question in itself. There are currently large field-data uncertainties which make these ice sheets a relatively weak target from a modelling point of view.
- Starting from the same ice sheets is a way to more directly quantify the impact of the different external forcings on climate and ice sheets during the last two terminations.

To make it clearer that our model experiments are a simplification of actual past changes we have also added this in the introduction section:
"Using a relatively simplified setup, we do not aim to precisely match the available proxy data but instead we aim at better understanding the role of external forcings (orbital configuration and greenhouse gas concentration) on glacial terminations."

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