Quiquet and colleagues investigate the last deglaciation in the Northern Hemisphere using a coupled ice sheet - climate model. They use a climate model of intermediate complexity and a hybrid ice-sheet-shelf model. Overall, they simulate a deglaciation in good agreement with reconstructions. If they consider all the amplitude of the freshwater flux from the melted ice sheets, then the AMOC shuts down and is not able to recover. However, if they reduce these freshwater fluxes or consider additional mechanisms, such as brine rejection, then the AMOC can recover. Additional experiments show the sensitivity of their model to key parameters.

This is a very valuable effort and well suited for the scope of Climate of the Past.
The manuscript is well written and easy to follow and I don't think that additional simulations are needed, but I have some comments and questions.

Thank you for your time revising our manuscript. We answer your comments in the following and we changed the manuscript accordingly.

General comments:

Reference experiment:

I am curious about the selected parameters of the reference experiment. Were they chosen to simulate a realistic last glacial maximum (LGM) state? Have you tried to tune your present-day (PD) state? If so, what type of LGM state do you obtain/expect?

In fact, this work is a result of a few years of development and calibration, for both the climate and ice sheet models. From the climate model side, over the years, we made a few modifications from the Goosse et al. (2010) original core of the climate model (ice mask, surface energy budget, parameters related to the downscaling of precipitation, etc.) with the aim of reducing known important model biases under pre-industrial conditions (notably: warm bias in North America, cold bias in the Arctic, overestimation of precipitation over mountainous regions). If some of the biases were sometimes reduced we were nonetheless not able to suppress them all. A map of the temperature and precipitation biases is shown in this response (Fig. R1) and now included in the manuscript. We did not specifically try to tune the LGM climate state, we simply checked that the LGM vs. PI temperature change was in a relatively good agreement with published literature (e.g. Kageyama et al., 2020).

However, for the coupling parameters and coupling strategy (melt coefficient in the surface and sub-shelf melt models, the sub-grid albedo, ageing of the snow albedo, etc.), we tuned both the PI and the LGM in parallel. These model choices were first tested under a PI climate and assessed with the simulated Greenland ice sheet volume. However, the Greenland ice sheet offers only a relatively weak constraint for these parameters given its extension with respect to the atmospheric model grid size. Melt models that produced a closer agreement with the present-day ice sheet volume were producing largely too small Northern Hemisphere ice sheets at the LGM.

Finally we did not consider the ice sheet model parameters as tuning parameters for the coupled model. These were calibrated independently with offline ice sheet model simulations of four glacial-interglacial cycles of the Antarctic ice sheet, as in Quiquet et al. (2018). Even if some ice sheet parameters could influence the coupled response (e.g. a more dynamic ice sheet could in principle have a larger extent) the climate model biases seem in our case much more influential on the simulated ice sheet state at the first order.

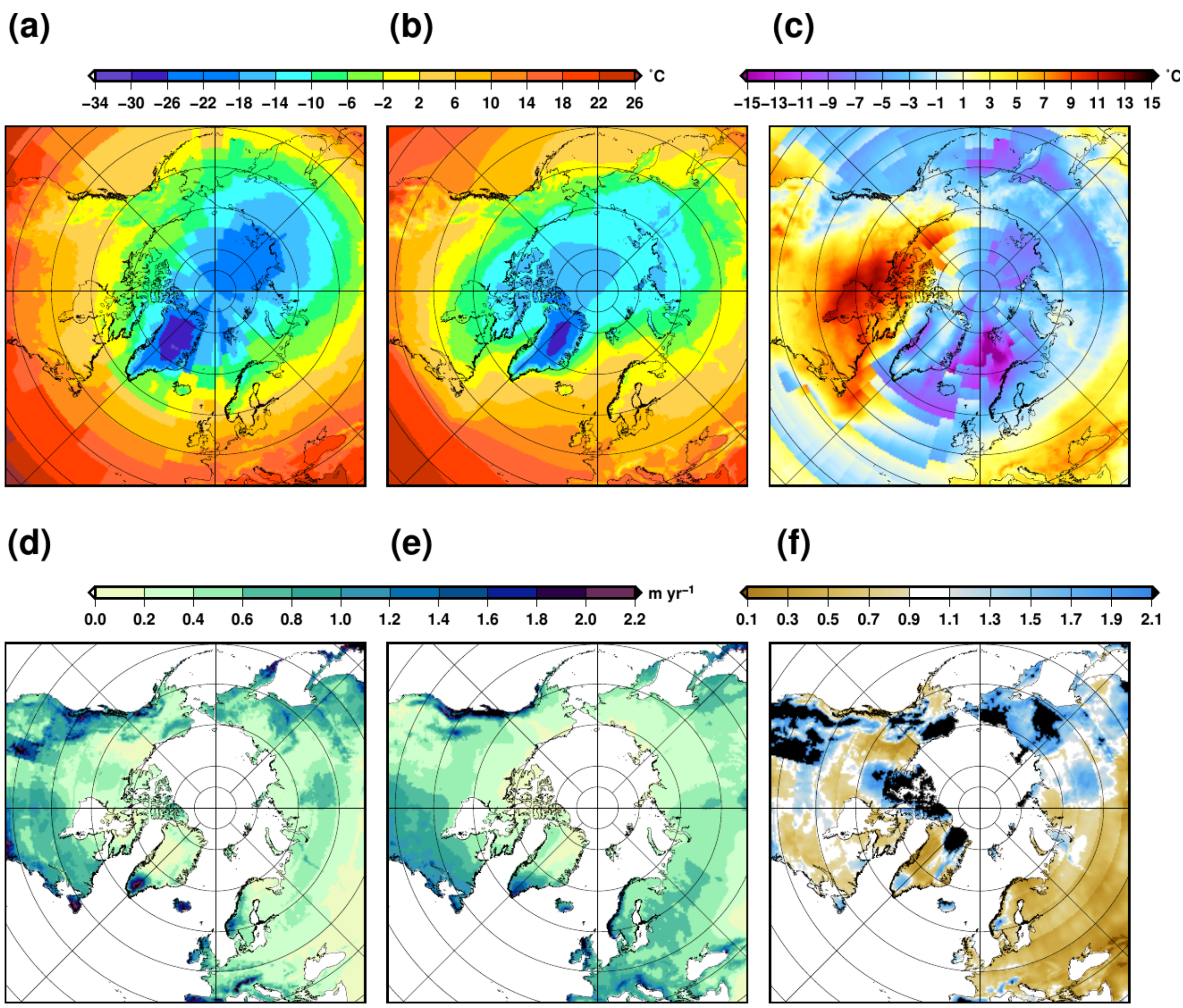

Figure R1. (a) Simulated annual near-surface air temperature for a pre-industrial climate experiment using the model configuration used for the deglaciation experiment (i.e. with a LGM ocean bathymetry) but with a present-day topography and ice mask for the Northern Hemisphere. (b) Annual near-surface air temperature for the ERA5 climatological mean over 1979-2008. (c) Temperature difference a-b. (d) Simulated annual total precipitation rate for the same pre-industrial experiment. (e) CRU-CL-v2 annual total precipitation rate. (f) Precipitation ratio d/e.

Spin up

You simulate separately the LGM state for the ice-sheet-shelf model and for the climate model. Then, your DGL experiment starts at 26 kyrBP, I guess to reach a sort of LGM equilibrium state for the coupled experiments. Do you obtain an equilibrated state? Have you tried to run an equilibrated LGM state with both models coupled from the start?

With our methodology we do not claim to reach an equilibrium at 21 kaBP since we use transient climate forcing from 26 to 21 kaBP. However, we expect to reach some consistency between the simulated climate and the simulated topography. We agree with what you suggest: the consistency would have been even better with an equilibrium simulation coupled from the start. In fact, we perform such an experiment more recently using a coupling frequency of 1:10 yr and a duration of 1 kyr for the climate model (10 kyr for the ice sheet model). The simulated ice sheets at the end of the equilibrium is shown in Fig. R2. They do show some differences but they are generally similar.

The reason why we did not use this approach from the start is that it is still much more computationally expensive than the one we followed. In our approach, we use only one long (3000 yr) climate equilibrium under glacial conditions to perform plenty of short (100 yr) experiments with various formulations of the melt model. The different climatological surface mass balance are then used to force offline the ice sheet model (inexpensive).

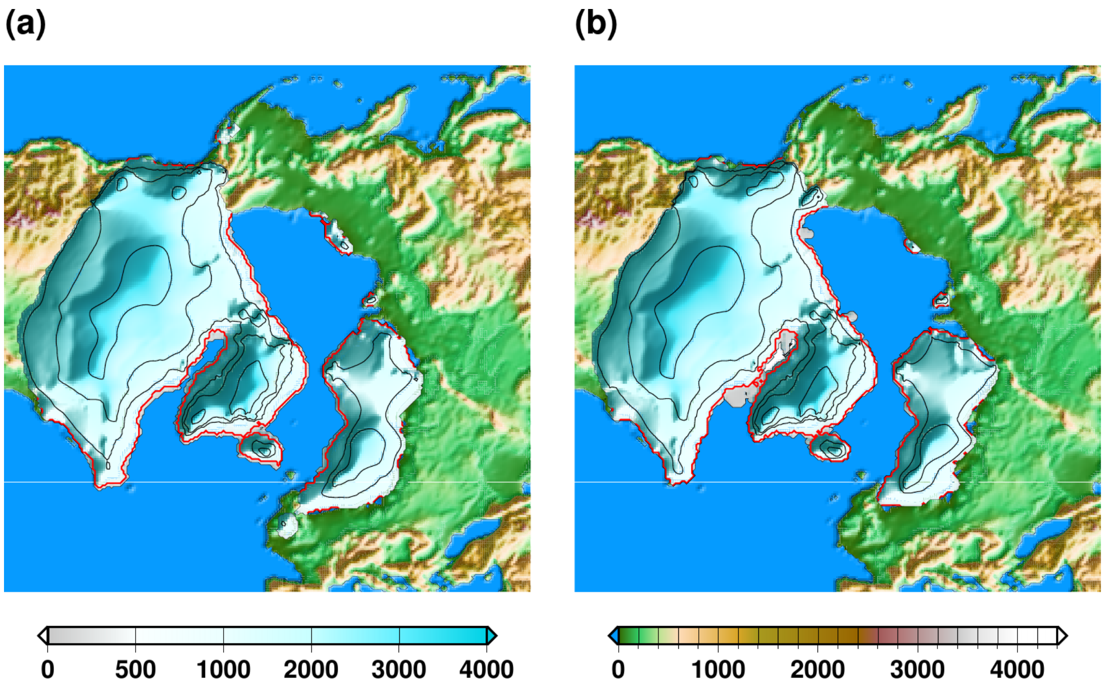

Figure R2. (a) Simulated ice sheet topography used as initial condition for the start of the coupled experiments, i.e. after the 200 kyr offline ice sheet spin-up. **(b)** Simulated ice sheet topography simulated after a 1000 yr coupled experiment under perpetual glacial conditions starting from (a), using an acceleration factor of 10 (10 kyr are simulated by the ice sheet model). The colour scale is different for ice-free and ice-covered regions. The simulated ice sheet grounding line is represented by the red line while the black lines represent isocontours of ice sheet surface elevation (separated by 1000 metres).

We added few elements in the discussion section:
"Lastly, we run deglaciation experiments starting from 26 kaBP assuming that the Northern Hemisphere ice sheets were in equilibrium with the simulated glacial climate. However, the last glacial maximum ice sheets were the results of the long previous glacial period starting from the last glacial inception. Ideally, it would have been best to perform a transient coupled experiment covering this period of time in order to have a more realistic ice sheet states. Notably, slow evolving ice sheet variables such as glacial isostasy or internal temperatures are expected to be affected by a transient spin-up instead of a constant glacial spin-up. However, this remains currently a numerical challenge to perform such a transient spin-up."

Glacial isostatic adjustment:

In P7 L204 it says: " We use a recent implementation of the last glacial maximum bathymetry at 21 kaBP (Lhardy et al., 2020), which is left unchanged for the duration of the experiments."

When I first read this, I understood that the bathymetry was set constant for the whole experiment, including the deglaciation. However, in P14 L422 it is written: "At this time, the bedrock is still depressed below sea level over the northern most part of America but slowly returns to its present-day value."

Indicating that the bedrock responds to changes in the load. I agree with the other reviewers opinion, that the GIA model needs to be described. Also, its potential implications in the retreat of part of the Eurasian and the Laurentide Ice Sheet should be discussed.

GIA is accounted for in the ice sheet model with a simple Elastic Lithosphere – Relaxed Asthenophere (ELRA) model (LeMeur and Huybrechts, 1996). The ice sheet model is also forced by transient eustatic sea level rise (Waelbroeck et al., 2002).

This has been clarified in the text, in the model description section:
"Glacial isostatic adjustment is accounted for in GRISLI using a elastic lithosphere - relaxed asthenosphere model (LeMeur and Huybrechts, 1996), with a relaxation time of the astherosphere of 3000 years."
And later, in the experimental setup section:
"On the ice sheet model side, in addition to the climate forcings, an other forcing is the transient eustatic sea level reconstruction from Waelbroeck et al. (2002)."

Glacial isostasy largely explains the grounding line instability that occurs at the southern margin of the North American ice sheet. This is discussed in Quiquet et al. (2021), now referenced in the manuscript. Glacial isostasy can also play a role for the Eurasian ice sheet since the bedrock in the Barents-Kara region is more depressed than today with retrograde slopes from the grounding line. This favours the marine ice sheet instability that occurs at 14.5 kaBP in our experiments. We added this in the manuscript:
"Such instability is favoured by the depressed bedrock, with a ~300 m deepening in the Kara sea with respect to the present-day bathymetry, resulting in steeper retrograde slopes."

However, the bathymetry of the oceanic model is left unchanged in the course of the deglaciation. We have clarified this, first in the method section:
"For the experiments presented here, changes in the ice sheet size do not affect the global ocean volume. The bathymetry in the oceanic model remains thus constant."
And later for the description of the experimental setup:
"For the oceanic model, we use a recent implementation of the last glacial maximum bathymetry at 21 kaBP (Lhardy et al., 2021), which is left unchanged for the duration of the experiments."

Oceanic forcing

You use in your ice-sheet model a linear melting law and you double the value for floating points in contact with the grounding line. I'm not very familiar with the most suited melting laws for the Greenland Ice Sheet, but I guess that in order to be more realistic, more complex processes should be taken into account, such as the plume formation or frontal ablation (Slater et al., 2019, 2020).

As I am more familiar with the Antarctic Ice Sheet, I know that a linear law is the least appropriate as it doesn't account for the positive feedback between the sub-shelf melting and the circulation in the ice-shelf cavity (Favier et al., 2019). Also, applying higher melting rates close to the grounding line for coarse resolution, as it is here, can overestimate the rates of grounding-line retreat (Seroussi and Morlighem, 2018). Perhaps, you may add one or two sentences on this point.

We fully agree with this comment although the Greenland ice sheet glacier frontal melt is certainly a too fine scale process to be correctly represented in a 40 km grid resolution using oceanic fields at a 3° resolution.

We have added the following in the manuscript when we discuss the mass loss partitioning:
"The lesser importance of the sub-shelf melt rate for the first phase of the deglaciation could arise from the simple model we use to represent this process. Notably, we use a linear melting rate dependency on temperature change, while a quadratic dependency could best reproduce this process (Favier et al., 2019). A quadratic dependency would result in more sensitive melt rate changes to temperature changes."

We plan to implement an alternative sub-shelf melt model at the interface between GRISLI and iLOVECLIM. However, the main driver for ice sheet retreat in our experiment is surface mass balance, at least until 12.8 kaBP. After this date, sub-shelf melt rate becomes important only becauzse grounding line instabilities have been triggered. These instabilities are not triggered by the artificially high grounding line melting rate since the experiment with higher sub-shelf melt displays a similar ice sheet evolution.

We added the following in the discussion:
"Second, we have used a very simple parametrisation for sub-shelf melt when alternative parametrisations display a better agreement with complex sub-shelf cavity oceanic models (Favier et al., 2019). This process is key for the future of Antarctic ice sheet (Seroussi et al., 2020) and could be equally important for the deglaciation of marine based sectors of the Northern Hemisphere ice sheets (Petrini et al., 2018; Clark et al., 2020). For this reason, we plan to implement an alternative sub-shelf melt model at the interface between GRISLI and iLOVECLIM. However, in our experiments, the main driver for ice sheet retreat is surface mass balance, at least until 12.8 kaBP. After this date, sub-shelf melt rate becomes important only because grounding line instabilities have been triggered. These instabilities do not seem to be triggered by an artificially high grounding line melting rate since the experiment with higher sub-shelf melt displays a very similar ice sheet evolution. This results could be revisited with a more complex sub-shelf model."

Antarctic ice sheet

P5L129: "It is important to mention that only the Northern Hemisphere ice sheets are interactively simulated, while the Antarctic ice sheet topography and ice mask remains prescribed."
Prescribed to what? Present day? Last Glacial Maximum?

At the Last Glacial Maximum, following the PMIP protocol. This is now explicitly mentioned.

Also, if prescribed to LGM state, then you don't consider its potential sea-level rise which could accelerate grounding-line instabilities in your model.

The ice sheet model is forced with transient eustatic sea level reconstruction from Waelbroeck et al. (2002), which include the contribution from Antarctica, which is probably about 10 m (e.g. Whitehouse et al., 2012; Briggs et al., 2014). For earlier version of the coupled model, we performed sensitivity experiments with other eustatic sea level reconstructions (Lambeck et al., 2014) with no significant differences. The reason is that for our experiments the ice sheet evolution is primarily driven by climate forcing, not sea level forcing.

Brine rejection

I found very interesting your results when you consider brine rejection in your model. I like this finding, maybe you can add a sentence on this in the abstract.

We have done so:
“The inclusion of a parametrisation for the sinking of brines around Antarctica also produces an abrupt recovery of the Atlantic meridional overturning circulation, absent in the reference experiment.”

Sensitivity experiments

Do you run a new spin up for every sensitivity experiment? If so, how is it possible that all start at ~-100 msle in Figure 11?

This is an important point indeed, and no, we did not. This is a questionable modelling choice but we wanted to quantify the different climate trajectories starting from a common initial condition. We have added this precision in the manuscript, in the experimental setup section:
“All the experiments, including the sensitivity experiments with perturbed parameter values, use the same spun-up climate and ice sheet states.”

It is true that an other alternative would have been to run new spun-up ice sheets for the different sensitivity experiments. This would have resulted on sometimes important differences for the ice sheet geometry at the start of the coupled experiment. One reason why we did not choose this approach is that it would have lead to a poorer agreement with the geologically reconstructions during the glacial period.

Technical comments:

- You may cite here Simms et al., 2019.

We have added the reference.

- P8L227: “With have performed ...” Do you mean “We have performed...”?

Yes, thanks for noticing.

- Figure 4: Color scale is missing in (a)

I do not understand: (a) and (b) use the same colour scale. Both sub-panel show absolute annual near-surface air temperature, for the LGM (a) and for PI (b).

- Figure 5: If you draw temperature differences as in Figure 4 (b) then I would use the same color scale for consistency.

Fig. 4b is the simulated temperature for the pre-industrial, not a temperature difference. It might be best to not use the same colour scale for the absolute temperature field and a temperature difference. But maybe you meant Fig. 4d, which is the LGM temperature anomaly with respect to PI? Fig. 5 is the temperature difference from the two simulated PI, with and without the ice sheet melt freshwater feedback on the ocean. The range of Fig. 5, with positive and negative values, is largely different from the range of Fig. 4d (mostly negative).

- Figure 11: Same as before. I would use the same colour for DGL_noFWF as in Figure 3 for consistency.

We have done so.

- P10 Table1: Although you explain in the manuscript what every parameter means, I would repeat it again in the description of the table.

Information added.

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