1 Overview

Quiquet et al. present the results of a fully coupled ice sheet-climate model applied to the deglaciation of the Northern Hemisphere ice sheets from the Last Glacial Maximum. They use a climate model of intermediate complexity to couple in a bi-directional manner. Impressively, this is done at a very high frequency, with 5 years of ice sheet model for ever one year of simulated climate. The coupled model run produces a satisfactory representation of the deglaciation. One of the major findings is that the gradual input of freshwater in the ocean causes a shutdown of the AMOC. In some of their additional tests, they find that the AMOC does not shut down without this freshwater input. Other experiments showed only weak dependence on parameters related to surface and subglacial processes.

Overall, I enjoyed reading this paper, and found that it was well written. The experimental setup was well described (though note some of my comments below), and the implications for the ice sheet collapse on the overall climate was thorough. My comments below are mainly related to some parts of the modelling that should be elaborated a bit, and I do not think additional experiments are necessary. Therefore, I recommend relatively minor corrections.

Thank you for your review and your valuable comments.

2 Comments

2.1 Computational overhead

I am particularly impressed by 5/1 ratio of ice sheet to climate model years in this coupled setup. My question, though, is what is the computational costs of this model setup? If there is relatively low cost, then perhaps it could easily be adopted by other groups to investigate other problems, so this would be a good way to sell it.

In fact, we performed five experiments with synchronous, yearly, coupling (1/1 ratio): the reference experiments (DGL), three experiments with reduced freshwater flux (DGL_FWF/2, DGL_FWF/3 and DGL_noFWF) and finally one experiment with sinking brines parametrisation (DGL_brines). These experiments took, individually, about 31 days to complete.

The asynchronous experiments using a 5 to 1 ratio were roughly 5 times faster (since the ice sheet model is much less expensive than the rest of the climate model). They can be run in less than a week.

These numerical performance are probably not as good as the work performed with the LOVECLIM climate model (e.g. Heinemann et al., 2014 ; Choudhury et al., 2020) with which we share the main components (atmosphere, ocean and vegetation). This is due to the fact that the interactive downscaling used in our set-up to compute the surface mass balance decreases the performance of the standard model by about 40%.

We now provide this information in the text of the manuscript:
“The climate model computes about 850 years in 24 hours on a single core of an Intel® Xeon® CPU@3.70 GHz. The computational cost of the ice sheet model is negligible with respect to the rest of the climate model, while the interactive atmospheric downscaling decreases the performance by about 40% compared to the standard climate model. The coupled synchronous experiments took roughly one month to complete while the asynchronous experiments were approximatively five times faster.”

Of course, we encourage other groups interested by our tool to contact us to initiate a collaboration.

2.2 Ice sheet model resolution

Perhaps as part of the last point, I am curious as to why an ice sheet model resolution of 40 km is used. Is this a computational limitation? In experiments done by our group here at the Alfred Wegener Institute, we found that the trajectory of the ice sheet evolution can be radically different just by going from 40 km to 20 km resolution. In general, the computational expense of ice sheet models tends to be pretty low compared to climate models, so I think some discussion on this choice needs to be made.

It is true that the ice sheet model is relatively inexpensive compared to the rest of the climate system. In fact in our case, it is the atmospheric downscaling at the ice sheet model resolution that considerably increases the computational time (loss of about 40% in performance). Currently, using an higher ice sheet model resolution would results in a higher cost due to the downscaling. We could eventually imagine to downscale the atmospheric variables at a fixed spatial resolution (for example 40 km as here) and using simple spatial interpolation (e.g. bilinear) to transfer these variables to the ice sheet model at a higher resolution. However, we do not plan to implement this in the short term for mostly two reasons. First, given the non-linear nature of surface mass balance simple bilinear interpolation is not really adapted at the ice sheet margin. Second, we performed deglaciation GRISLI stand-alone experiments using two different spatial resolutions (40 km and 16 km) and the results were very similar. The evolutions of the ice sheet topography and velocity for selected snapshots, showing typical sheet deglacial geometry changes, are shown in Fig. R1 and Fig. R2, respectively.

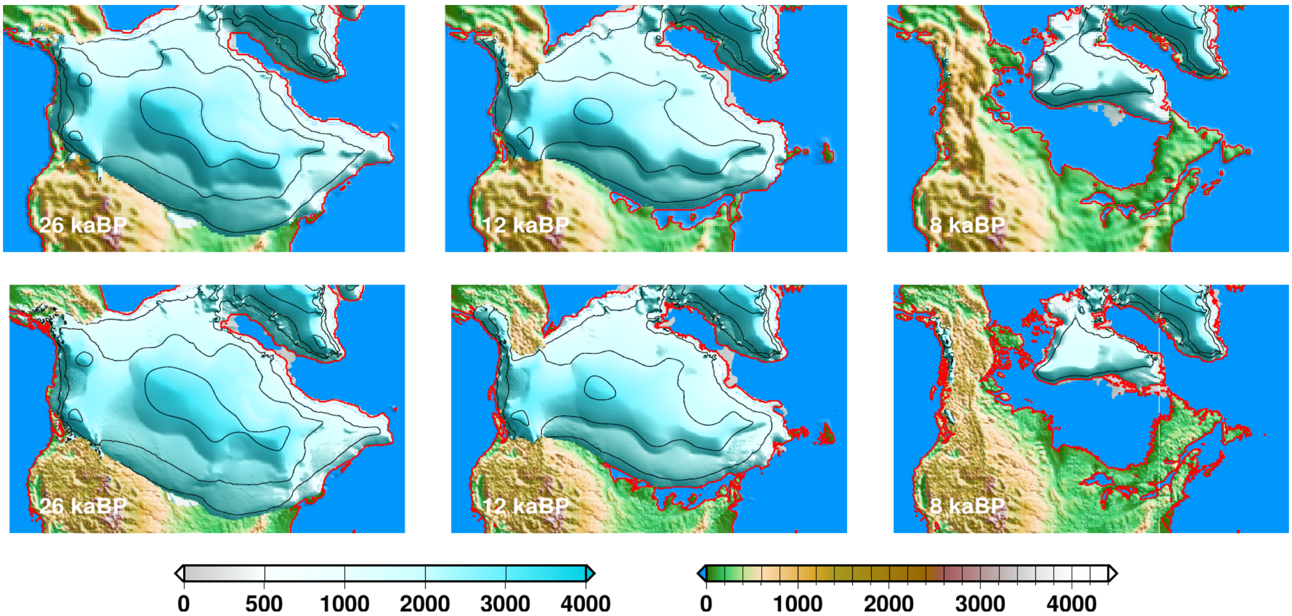

Figure R1. Simulated topography of the North American ice sheet for three selected snapshots (26, 12 and 8 kaBP) during the deglaciation. The top panels use a 40km grid resolution while the lower panels use a 16km grid resolution. The forcing methodology is the same for both resolutions and follows the standalone forcing methodology of Quiquet et al. (2021). Here the IPSL-CM5A-LR model outputs from the PMIP3 database is used with a weighing factor for the millenial variability of 0.25 (see Quiquet et al., 2021). 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).

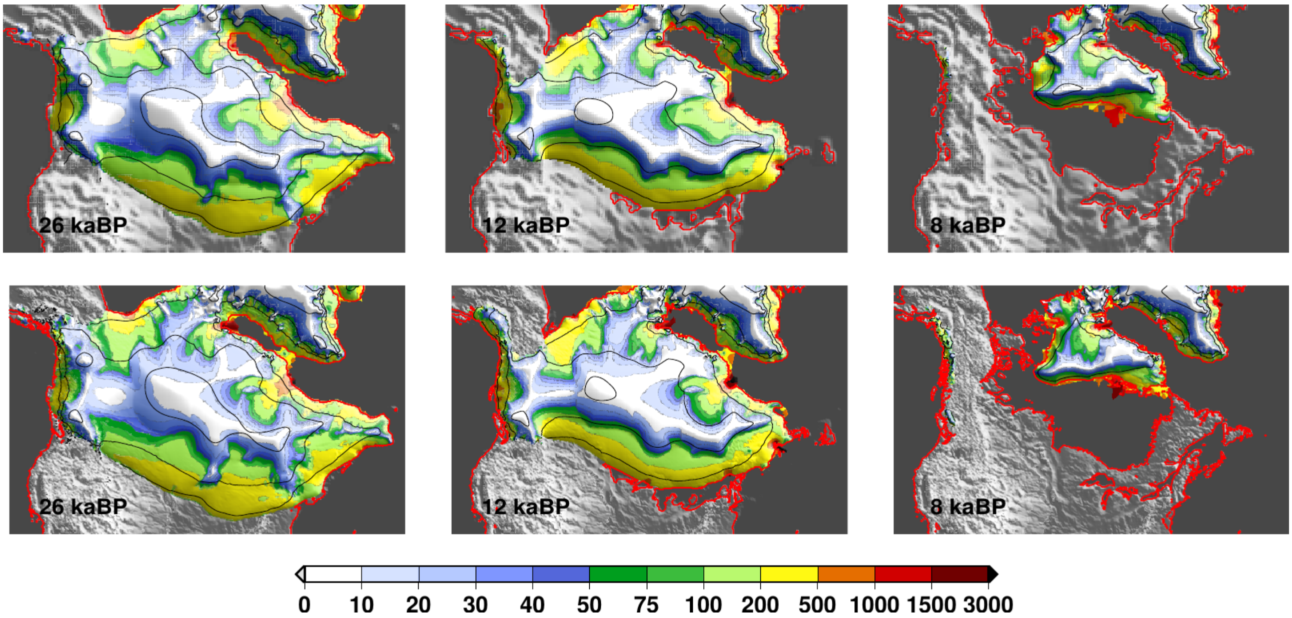

Figure R2. Same as Fig. R1 but for the simulated vertically integrated velocity (m/yr) draped over the simulated topography.

2.3 Glacial isostatic adjustment

The GIA model used for the experiments is not described, so I would ask that this be added. Looking at Figure 9, the topography is depressed far more than in reality, which causes broad glacial lakes to form along the southern margin that are much bigger than during the actual deglaciation. This is one of the possible reasons (or even the main reason) that deglaciation was faster than expected.

We added a description of GIA:
“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.”

The parameter of this simple GIA model are the same as in Quiquet et al. (2018). The fact that the bedrock topography is probably more depressed than in reality does not come from the GIA model but from the fact that the ice thickness is overestimated. The ice thickness overestimation can be due to an overestimation of the precipitation in the climate model and/or an underestimation of the ice sheet velocity (too low enhancement factor or a too high basal drag).

2.4 Sediment thickness for basal sliding

I don’t understand why the Laske and Masters (1997) dataset was used to parameterize sediment distribution. That dataset is a map of Phanerozoic, undeformed sedimentary rock thickness for use in global seismology problems, and is vastly different to the distribution of unconsolidated sediments that would be important for ice sheet sliding. There are places with sedimentary bedrock where there are no unconsolidated sediments (for instance in Ontario and central Manitoba), and there are places on the Precambrian Shield where there are thick unconsolidated sediments (for instance the Thelon Basin in Kivalliq). As a first order approximation, I guess you could assume that areas where the bedrock is Phanerozoic sedimentary rocks are more likely to be covered by unconsolidated sediment, so I don’t think it would radically alter the results. However, I suggest in the future that a different dataset be used. Full disclosure, I have created such a dataset for North America (Gowan et al., 2019).

Thank you for pointing this out. We fully agree, sub-glacial sediments are probably key for ice dragging. However, it is unclear yet how they should be implemented in ice sheet model. Most of the time it is simply a local change of a friction parameter (as in here) but this question is also pretty much linked to the way basal dragging is implemented. Here for example we use a linear Coulomb friction law but it is possible that a strongly non-linear law should be used in some places (Gillet-Chaulet et al., 2016; Brondex et al., 2020). The degree of non-linearity of the friction law is probably related to the nature of the sediments below the ice sheet. However, we think that these are still open questions and we plan to explore them with our model in the future.

Related to this, thank you for pointing us to your paper. Historically, we use the map of Laske and Masters (1997) for its coverage of the whole globe but we agree that this could be updated with more recent / appropriate dataset. In Fig. R3 we show the sediment data as they are used in the model (a simple threshold value indicating an absence or a presence). We also show your dataset for the sediment distribution. Although your dataset displays a much higher spatial variability, they display an overall similar pattern. However, in some places key for deglaciation (e.g. present-day Hudson Bay) they have important differences.

Sub-glacial processes, including sediments, are certainly a very important direction for future research although it might not necessarily require a fully coupled climatic setup to be studied.

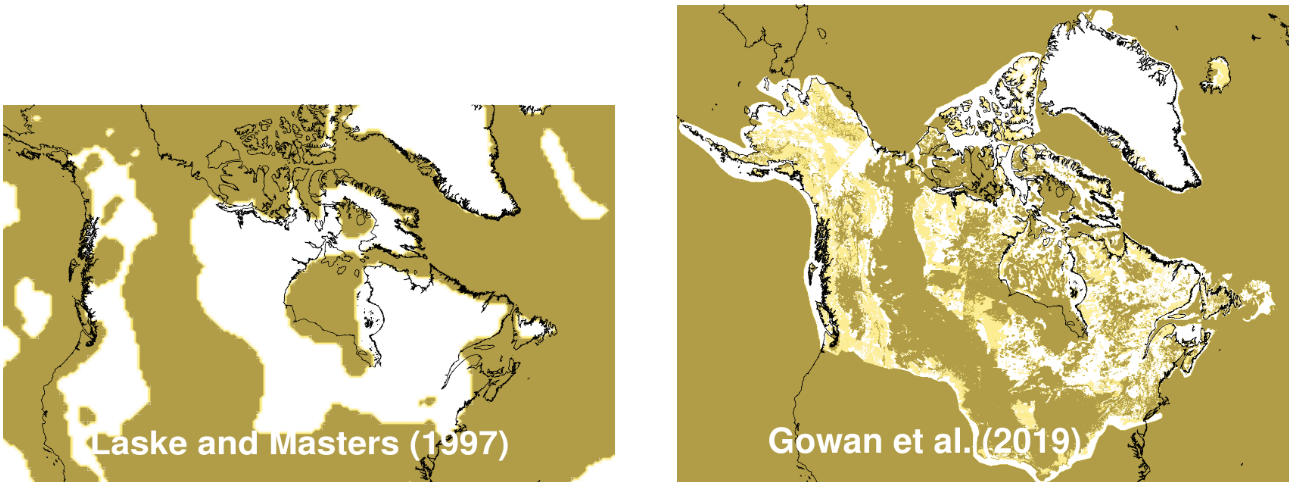

Figure R3. Left: sediment mask as it is used in the current version of the model, i.e. a 200 m threshold on Laske and Masters (1997). Brown areas indicate the presence of thick sediment leading to a smaller basal drag coefficient. Right: sediment distribution in Gowan et al. (2019) where the brown area shows the presence of thick sediment, yellow is discontinuous and white is an absence of sediment.

2.5 Spinup time

A 200 kyr spinup is used to initialize the ice sheet to the LGM state. I'm wondering why such a long spinup was necessary, considering that during the last interglacial (about 100 kyr before the LGM), there were essentially no ice sheets in the Northern Hemisphere except for perhaps part of the Greenland Ice Sheet. Even the Eurasian Ice Sheets were probably almost non-existent just 15 kyr before the LGM (Hughes et al., 2016). Would such a long spinup affect the results?

Our spinup methodology cannot be compared with a real glacial inceptions. In particular, for the whole duration of the spinup, the climate forcing remains constant (glacial conditions simulated by *i*LOVECLIM using prescribed ice sheet). In this climate forcing, there is a very low precipitation rate, in particular over the domes of the ice sheets. Ice sheet build-up with a more realistic transient climate evolution might be faster.

An other aspect is that we wanted to start our experiments with equilibrated ice sheets to avoid drifts in our ice sheet model. In doing so, it is easier to quantify the impact of climate change. Of course this is an approximation of the reality since the ice sheets were probably never "equilibrated" with the climate.

Some variables in the model requires long integrations to reach an equilibrium under constant climate forcing. The internal temperature needs a few tens of thousand of years, starting from a linear vertical profile in Greenland for example. An other variable that needs long integrations is the hydraulic head since we only rely on a simple advection/diffusion scheme, and this variable is coupled with the velocity field: a larger hydraulic head is associated with a larger velocity and the heat due to friction produces melt water.

We acknowledge the fact that the assumption that the ice sheets are in equilibrium with the glacial climate has consequences. However we do not see how this could be properly quantified except by running a full glacial-interglacial cycle, which remains currently a numerical challenge for us.

We added the following 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."

2.6 Comparison with geological data

There is a section that compares the modeled results with some ice sheet reconstructions. I think this is fine, but don't feel too bad that you don't match things exactly, since the margin chronology in North America is in the process of being revised (Dalton et al., 2020). In some places the timing of advance and retreat is being revised by thousands of years. In particular, I would say that the 20.5 ka timing of your maximum ice extent is actually closer to observations than what is presented in these reconstructions (for instance, the maximum of the western half of the Laurentide ice sheet was achieved around that time Jackson et al. (2011); Lacelle et al. (2013)).

One thing that might be interesting to look at more is the causes of more major discrepancies in the model from geological observations. There are three main things that I would like to see comments on. I am guessing that these discrepancies are likely the result of biases in the climate model, but it would be interesting to know more.

1) The Northwestern part of the Laurentide Ice Sheet, which covered Banks and Victoria islands, was one of the first places to deglaciate, but in your model it remains ice covered until after 8 ka.

2) An ice cap persists on the outer parts of the Grand Banks at the end of your simulation, a place that probably wasn't even glaciated during the MIS 2 glaciation. This seems like an odd place for an ice cap, considering it is below sea level and surrounded at all sides by the ocean.

3) Iceland remains ice covered through to the end of the simulation.

Your point 1 & 2 can be largely explained by the biases in the climate model.

We present in Fig. R4a a map of the absolute annual near-surface temperature for a reference pre-industrial experiment (with fixed ice sheets). This pre-industrial simulation is run in a similar way than the deglaciation experiment. For example it uses a LGM oceanic bathymetry and a closed Bering Strait as this are fixed climate model features for the deglaciation experiments. The Northern Hemisphere topography and ice mask are nonetheless here at their present-day reference value for GRISLI, i.e. ETOPO1 (Amante and Eakins, 2009) when ice free and Bamber et al. (2013) when ice-covered. Fig. R4b is the absolute annual near-surface temperature for ERA5 climatological mean over 1979-2008. Fig. R4c is the temperature difference (b-a).

We also show the annual mean total precipitation simulated by the model (Fig. R4d), in the CRU-CL-v2 dataset (New et al., 2002, Fig. R4e) and the ration between the two (d/e, Fig. R4f).

The regions you mention in your point 1 & 2 show a cold bias associated with an overestimation of the precipitation.

In addition, there is an other factor that can explain your point 2 & 3. The ice caps over the Grand Banks and Iceland represent an important topographic anomaly with respect to the standard pre-industrial climate. This topographic anomaly drastically increases the annual precipitation rate (greater than 3 m/yr). Also, there is a strong albedo feedback than leads to little melt in this areas even at the end of our simulations at 0 kaBP. We acknowledge that is counter intuitive to retain an ice cap over the Grand Banks. However, the simulated ice thickness there is large enough (>1000 m) to maintain a grounded ice sheet over the ocean that is relatively shallow (no deeper than 200 metres).

We have added the following in the manuscript:
"The chronology and pattern of the deglaciation is largely affected by the biases in the climate model. We present these bi- ases in term of mean annual temperature and total precipitation rate in Fig. 10. To construct this figure we use a reference pre-industrial experiment (with fixed ice sheets), performed with a similar setup to the deglaciation experiments. Notably, this pre-industrial experiment uses the same last glacial oceanic bathymetry with a closed Bering Strait. The Northern Hemisphere topography and ice mask are nonetheless at their present-day reference value for GRISLI (Amante and Eakins, 2009; Bamber et al., 2013). The model presents a cold bias associated with an overestimation of the precipitation in the northwestern part of the North American continent. This explains why this region of the North American ice sheet deglaciates much later than its eastern sector where a warm bias is present. Also, Grand Banks and Iceland remain ice covered at the end of the simulation where the model is generally too cold and too wet. More generally, the climate model tends to overestimate the precipitation over mountainous areas which can induce a positive feedback over some ice caps such as Iceland, Grand Banks, the Ellesmere Islands and the Scandinavian mountains."

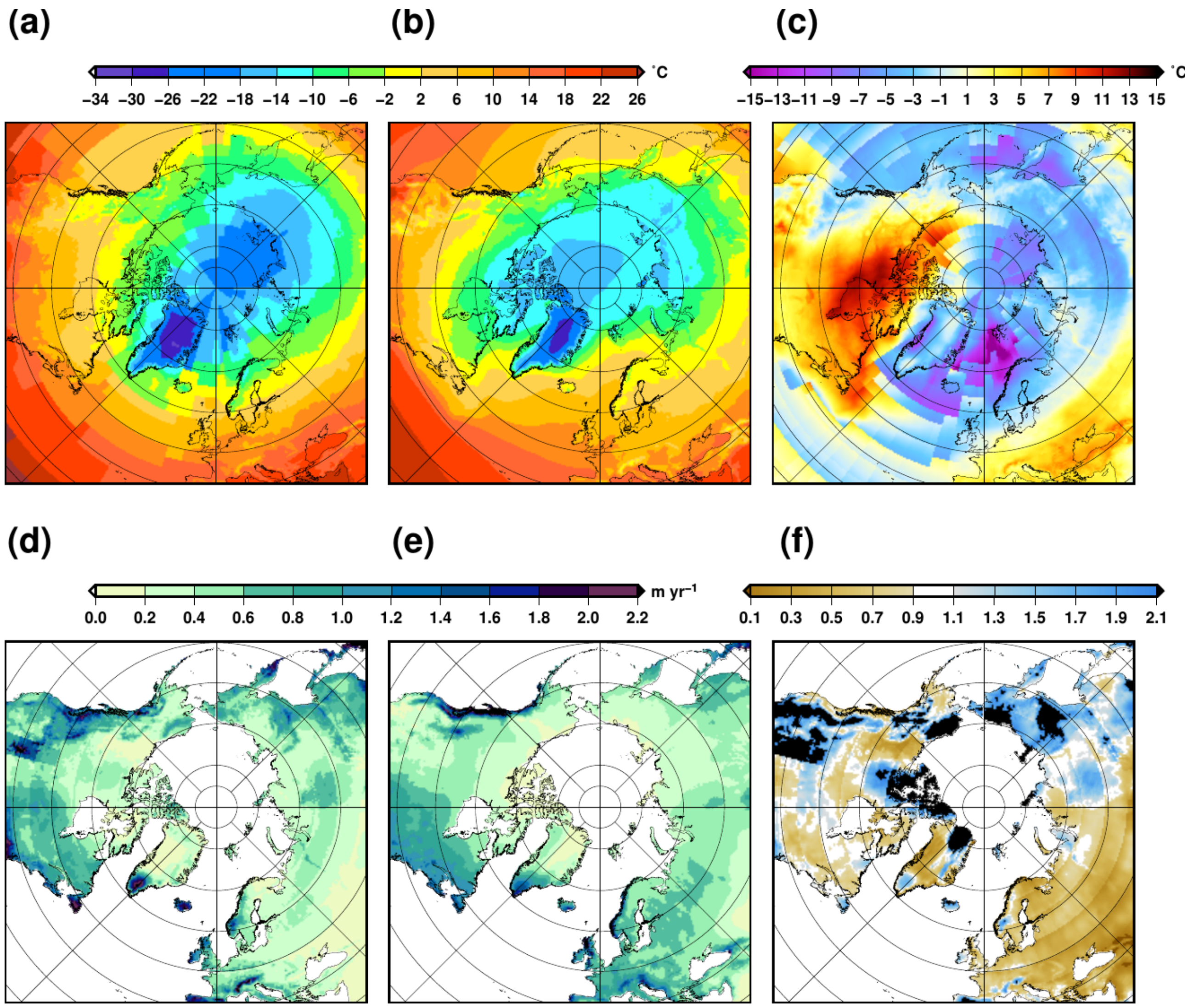

Figure R4. (a) Simulated annual near-surface air temperature for a pre-industrial climate experiment using the model configuration used for the deglaciation experiment (i.e. with a LGM ocean bathymetry) but with a present-day topography and ice mask for the Northern Hemisphere. (b) Annual near-surface 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.

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