Reply Reviewer 2:

First of all, we would like to thank the reviewer 2 for his/her insightful comments as well as providing additional literature suggestions. In this document, replies are listed with regular font type, while the reviewers' comments are listed in bold.

General comments:

(i) The discussion section is relatively weak in its current form and could benefit from some revisions. For example, you mention that both the index and the matrix methods are missing certain processes and cannot realistically represent abrupt circulation changes. I agree with this statement, and I would like to see a more thorough discussion on how this shortcoming is (potentially) influencing your results. Here are a few papers that have examined and at least partially explained abrupt changes in the large-scale atmospheric circulation in the last glacial period:

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL074274
https://journals.ametsoc.org/view/journals/atsc/73/8/jas-d-15-0295.1.xml

https://cp.copernicus.org/articles/12/1225/2016/

We agree with the reviewer and we will add a more thorough discussion in the manuscript on limitations regarding modelling climate feedbacks. In our current discussion we stated that we are unable to simulate threshold behaviour in the climate system with our current set-up, such as the effect of ocean circulation on the closure of the Arctic Archipelago gateway. However, the discussion should also include our method's limitation regarding feedbacks between ice and atmospheric /oceanic circulation.

The papers suggested by the reviewer will be included in the manuscript. With these changes, the discussion will contain remarks on the effects of atmospheric circulation on the North American and Eurasian ice sheet (Liakka et al., 2016), abrupt changes in atmospheric circulation in the North Atlantic during deglaciation (Löfverström et al., 2017), ocean circulation changes due to fresh water influx into the ocean (Otto-Bliesner et al., 2010). The manuscript will also benefit from explicitly mentioning that the climate matrix method should not be viewed as a replacement or improvement for GCM models or intermediate complexity models. Instead, the climate matrix method should be viewed as an alternative to the glacial index method which has the effect that the ice sheet evolution influences the dynamics rather than acting as a passive response to a climate forcing as in a glacial index method.

(ii) The manuscript could benefit from including a supplementary document that shows the simulated model climates (pre-industrial and LGM), and at least a few snapshots of the ice sheets prior to the LGM. I would suggest showing the ice sheets every 30 kyrs or so. It could also be good to compare these fields with some proxy data to better understand the quality of the simulation and what errors the different methods introduce.

The manuscript currently shows maps of the last glacial maximum (21 ka) ice thickness. It also includes maps of the evolution of ice extent. These maps show the time at which ice accumulates for the first time in a region. However, we can add maps to the supplementary information showing ice thickness between the glacial index and climate matrix method for every 20 thousand years. This should help to intercompare the ice dome shape and sizes, which differ substantially between the climate matrix and glacial index method. Similarly, we can add GCM LGM and PI temperatures in the supplementary information.

Regarding the temporal evolution of the ice sheets, our current model shows considerable discrepancies in ice volume during MIS5. This can most likely be attributed to a too weak effect of insolation on ice growth. During MIS5, summer insolation in the Arctic reaches a minimum, which currently has a minimal effect on ice growth. Insolation is included in our SMB model as it increases ablation. The temperature forcing in the climate matrix method depends slightly on insolation. Temperature is interpolated with respect to CO_2 and annually averaged absorbed insolation. Absorbed insolation is calculated using insolation and albedo. However, the net effect of insolation on ice evolution is limited. This may explain the largest discrepancy in ice evolution, including the slow inception and the relatively small volume at 60 ka. This limitation will be elaborated in the discussion section of the manuscript.

In this work we focused on obtaining a realistic LGM ice sheet, rather than having an optimal transient simulation. The model is tuned to obtain good ice volumes at the LGM and at the end of the deglaciation. However, we did not tune it to obtain a good ice volume throughout the LGC. We will emphasis this point better in the manuscript.

(iii) What is the reason for using PMIP3/CMIP5 models instead of the updated PMIP4/CMIP6 models (documented here: https://cp.copernicus.org/articles/17/1065/2021/)? Do you have any reasons to assume that the results are robust/not robust across PMIP generations? The paper by Kageyama et al (2021) should be cited no matter what as it documents similarities and differences between the LGM simulations in the older PMIP3 (used here) and the newer PMIP4 models.

In our manuscript, we have used climate forcing from the paleoclimate modelling intercomparison project phase III (PMIP3). The successor of PMIP3, PMIP4, currently has several LGM and PI simulations.

However, we did not use PMIP4 for several reasons. First of all, when we started conducting preliminary experiments for this paper, PMIP4 was not yet finished, so we used PMIP3 instead. An additional benefit to this was that we could make comparisons to earlier conducted research such as the ice sheet simulations by Niu et al., 2019 and Adler & Hostetler 2019. In addition, Kageyama et al., 2021 stated that PMIP3 is not fundamentally different from PMIP4. A wide range in LGM temperature and precipitation is both found in PMIP3 and PMIP4, so we would expect to find a large range in ice sheets as well. Hence, we preferred the possibility of a more direct comparison to Niu et al and Adler and Hostetler and sticked to PMIP3. We will explain this in the manuscript with reference to the Kageyama et al. 2021 paper.

(iv) This study is suggesting that the Eurasian Ice Sheet was at maximum extent/volume around 60 ka. This is not captured in your results at all. Is this a result of the lack of "realistic" circulation changes?

https://cp.copernicus.org/articles/9/2365/2013/

In our model, the Eurasian ice sheet reaches maximum volume at LGM, instead of the 60 ka suggested by reconstructions. We believe that this discrepancy, and slow ice inception during MIS5, is partly due to a too weak influence of insolation on temperature and SMB.

In both our glacial index and climate matrix methods, temperature change is mostly driven by CO_2 changes. While insolation is used both in the SMB model as well as the albedo feedback, it's overall influence on ice evolution is limited.

During MIS5, summer insolation in the Artic reaches a minimum, while CO_2 is still relatively high. Since temperature is mostly driven by changes in CO_2 we are unable to capture the fast growth in ice during MIS5. Faster inception, as well as a stronger dependence on insolation may help to reach a maximum Eurasian volume at 60 ka.

As stated by the reviewer, another reason for the discrepancy could involve atmospheric circulation changes. The topography of the North American ice sheet may affect the size of the Eurasian ice sheet, as stated by e.g., Liakka et al., 2016. This interaction is not simulated using our method.

These discrepancies, as well as these aforementioned reasons will be added in the method and discussion section.

Line comments:

Line 1: The title is a bit misleading since you didn't really study coupled interactions between ice sheets and climate. Consider changing the title to be a bit better suited for your study

The title of our manuscript: *Interactions between the Northern-Hemisphere ice sheets and climate during the last glacial cycle* may imply that we used a transient climate model or reconstructions / observations.

Instead, we will change the title to: *Modelling feedbacks between the Northern-Hemisphere ice sheet and climate during the last glacial cycle*. This title should reflect that we have conducted a modelling study and investigated some feedback processes between ice sheet in the Northern Hemisphere and the climate system.

Line 11: pre-industrial should not be capitalized

Line 12: computationally unfeasible

Line 22: exceeds --> exceed

Line 23: Specify that this is referring to ice sheet volume

Line 40: Rearrange the sentence to increase readability

The five comments listed above state some small grammar and spelling mistakes, each of which will be fixed.

Line 42: There are newer references that are more appropriate here:

https://www.nature.com/articles/s41586-020-2617-x

https://cp.copernicus.org/articles/18/1883/2022/

The references suggested by the reviewer refer to global temperature reconstructions at LGM. In the manuscript, we used a relatively old reference (Annan & Hargreaves et al., 2013). The reviewer suggested some references to research that has been conducted more recently, which will replace the 2013 reference. We will follow this suggestion.

Lines 50-54: These types of sentence constructions are difficult to read. Please consider reformulating in a more general way that is not including both cases.

This comment refers to a line that can be read in two different ways: e.g., the albedo increases (decreases) with decreasing (increasing) temperature.

These types of sentences, while concise, can be difficult to read. We will change it accordingly to improve readability. A sentence employing a similar technique in the discussion section will be changed as well.

Line 84: A similar technique was employed in:

https://gmd.copernicus.org/articles/7/1183/2014/

In this line we state different methods that have been used in the past to create climate forcing without a transient GCM model.

The paper mentioned by the reviewer refers to a paper by Fyke et al., 2014. They have interpolated LGM, mid-Holocene and PI surface mass balances to simulate the last glacial cycle. This research will be added to this sentence.

Lines 110-113: This preamble is not necessary and should be deleted

This preamble for the method section is going to be deleted

Lines 115-125: There are several abbreviations here that are not defined: IMAU-ICE; SIA/SSA; PISM; CISM

This part of the method sections contained some abbreviations that were not defined. We have added definitions for SIA/SSA, PISM and CISM. IMAU-ICE is not technically an abbreviation and cannot be defined. This ice sheet model was developed at our institute (IMAU), hence the name IMAU-ICE.

Line 123: The abbreviation ELRA is only used here and should be omitted

An abbreviation for ELRA is indeed not necessary and will be removed.

Lines 160-161: Did you test the sensitivity of this assumption?

This line refers to a correction that we apply transiently to precipitation. In Greenland we use the Clausius Clapeyron relation to correct precipitation for changes in temperature. We believe this is justified because the shape and elevation of Greenland only experiences relatively small changes throughout the last glacial cycle. However, the topography of the North American and Eurasian ice sheets changes substantially throughout the last glacial cycle. Therefore, we need to apply a correction to account for changes in topography. As precipitation is enhanced up slopes and is decreased down slope. Therefore, we use the Roe and Lindzen (2001) model. This model uses wind and surface slope to correct precipitation changes for topography change. The reason behind this choice will be added to the method section.

However, we did not test the sensitivity between the Clausius Clapeyron and the Roe and Lindzen model.

Lines 174-174: How is the planetary albedo calculated? Clouds will affect the amount of insolation at the surface…

A surface albedo is calculated in the ice sheet model. This albedo model first applies a background albedo (land, sea or bare ice) and adds a snow layer on top. This is sufficient for our set-up. The SMB scheme does not include cloudiness explicitly.

We will include a sentence in the manuscript stating that we specifically use surface albedo and did not take cloud coverage into account.

Lines 180-181: This preamble is not necessary and should be deleted

The results section contained a one sentence preamble to state what will be discussed in the section. This preamble will be deleted.

Lines 196-197: I assume that the PI simulations included the observed ice caps on these islands. Thus, the climate is already primed (through albedo effects) to grow ice there. Perhaps a small point, but potentially important to comment on here and in the discussion section.

During pre-industrial some regions in North America and Eurasia have ice caps. This includes the islands surrounding the Barents Sea (e.g., Nova Zembla, Svalbard) as well as Iceland and the Canadian Arctic Archipelago. We start our simulations with no ice in the North American and Eurasian domains. As a result, these artic regions are already favourable for ice growth and near pre-industrial temperatures are enough to incept ice sheets. So, we agree with the reviewer.

However, the main goal of this sentence is to help explain the accompanying figure; figure 4. Though we will add a small remark that these regions were close to full glaciation during the pre-industrial period. Therefore, it is reasonable that these regions are the first to incept ice.

Table 1: The LGM simulation with CNRM-CM5 didn't include the ice sheets! Therefore, you may wanna exclude that model from the study. See point 19 here:

https://www.umr-cnrm.fr/cmip5/spip.php?article24

CNRM-CM5 is one of the climate models that is part of the PMIP3. In our simulations it was one of climate forcings that lead to very small ice sheets. The Eurasian and North American ice sheets had no ice beyond the present-day ice coverage. Greenland partly melted when using CNRM-CM5 forcing which could be attributed to summer temperatures well above freezing. The temperature above Antarctica also shows strange patterns with warm regions on the ice sheet. Obviously, these results have a very large discrepancy with reconstructions. The website shown here shows a number of bugs found in CNRM-CM5. Point 19 states that the topography field (orog) is wrong. While we cannot find an issue in the CNRM-CM5 orog fields downloaded directly from the PMIP3 database, we cannot deny that there is discrepancy in LGM temperatures.

Despite these large discrepancies, we did decide on including the CNRM forced simulation in the manuscript. First of all, leaving out CNRM brings the question why we should not leave out MRI and GISS or any of the other GCM simulations. MRI has relatively high temperatures in Eurasia and North America, while GISS has low temperatures in most of Asia. Clearly also not a very good result. We believe, it is quite arbitrary to determine which GCM to omit fully from the manuscript. At the same time, it is also important to show the ice / paleo community that these long-timescale ice sheets simulations are very sensitive to climate forcings by the GCMs. There are large differences in the extent of the ice sheet that can be attributed directly to the climate forcing. By showing them all we hope to convey the message that the quality of the modelled ice sheet depends strongly on the quality of the climate forcing.

This is why we opted on using a subs-selection of GCM models for the main part of the analysis. After running the model using all nine PMIP3 GCMs, we found a large range of ice volumes. Some of these ice sheets had large discrepancies compared to reconstruction, so we made a selection of the climate models that were able to produce reasonable LGM ice sheets. Therefore, CNRM has in the end a minimal impact to the main analysis of the paper.