

Interactive comment on “A hybrid GCM paleo ice-sheet model, ANICE2.1 – HadCM3@Bristolv1.0: set up and benchmark experiments” by Constantijn J. Berends et al.

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Author comment replying to the referee comments by L. Tarasov and T. Bahadory

We'd like to thank the reviewers for their comments on the manuscript and would hereby like to address the concerns they raised.

The reviewers raised several questions about technical aspects of the model set-up presented in the manuscript, especially regarding the rationale behind the choices of different parameterisations. We agree that the manuscript could be improved at this

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point and we clarified this in the new version, details are listed below. We'd like in particular to express our gratitude to the reviewers for pointing out an important detail – the fact that the stated dimensions of some of the parameters in the precipitation model were erroneous had indeed escaped our attention.

In *Italics* the comments, below our rebuttal. Page and line number refer to the new manuscript version.

The inclusion of "GCM" in the title is misleading.

We agree that the current title could be interpreted as meaning we constructed a coupled GCM-ISM. We will change the title to more accurately reflect our model set-up, where an ISM is forced with output from a GCM.

P1, L1: changed the manuscript title.

with pre-calculated output from several steady-state simulations with the HadCM3 general circulation model Inaccurate and misleading. Two simulations is not "several".

Although the method presented in this manuscript can be used to force the ice-sheet model with output from any number of GCM snapshots, the results presented here were indeed produced with only two snapshots. We agree that the statement is inaccurate, and will correct this.

P1, L11: changed “several” to “two”. P1, L16: clarified that the presented method can be applied to a matrix containing any number of GCM snapshots.

The simulated ice-sheets at LGM agree well with the ICE-5G reconstruction and the

more recent DATED-1 reconstruction in terms of total volume and geographical 20 location of the ice sheets. Since ICE-5G use DATED-1 precursors for margin constraint and since the GCM was forced with ICE-5G boundary conditions, this is a weak result pg 10 comparison to ICE5-G GCM fields generated with say ICE5-G boundary conditions will have a strong imprint of the ice sheet margin on the resultant climate. So recreating ICE5-G ice extent with this interpolated climate forcing offers little validation as to the utility of the approach. For me, the challenge is to get a range of climates without the imprint of assumed ice sheet boundary conditions used by the GCM.

As is shown in the referenced study by Niu et al. (2017), forcing a GCM with a certain prescribed ice-sheet and then using the resulting climate to force an ice-sheet model is by no means a guarantee that that ice-sheet model will produce the same ice-sheet that was initially prescribed to the GCM. Indeed, by doing exactly this, with different GCM's, Niu et al. (2017) produced ice-sheets at LGM ranging from 50 to 150 m SLE and in many cases exceeding the initially prescribed ice-sheet's extent by hundreds of kilometers. We will add a few lines to the manuscript discussing these results in order to clarify this point. While we agree with the reviewer that the "ultimate" model would require only orbital forcing in order to produce glacial cycles, accurately simulating all the feedbacks between ice-sheets and the atmosphere, the ocean, the carbon cycle and the biosphere, without being in any way "limited" by observations of the past, such a model is beyond the scope of this study.

P14, L6: Added more context to the Conclusions section discussing the results presented by Niu et al. (2017).

Both types of studies share the shortcoming of having no clear physical cause for the prescribed climatological variations, I would argue that the approach presented herein also has no clear physical cause given the adhoc choice of weights and ignorance of

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all the other feedbacks from ice sheets to climate..

Although we do not claim that our model captures all existing feedback processes between the climate and the cryosphere, we do believe our model contains several important processes that are not represented in the “glacial index”-type models described in the statement. In these models, a temperature or climate forcing is prescribed based on an external forcing record, regardless of how the ice-sheets inside the model evolve. In our approach, we decouple the contributions to climate change caused by changes in pCO₂ and changes in ice-sheet size plus insolation, calculating the latter based on the internal model state through the matrix method. We therefore believe our model set-up to be, though still not as comprehensive as a fully coupled GCM-ISM, at least more physically realistic than the glacial index model described in the statement, especially in the way spatial patterns of climate change are treated.

We will clarify the advantages of our approach in the manuscript.

We will address the reviewer’s concerns regarding the “ad hoc choice of weights” later on, when he specifies exactly which choices of weights he finds to be insufficiently motivated.

P3, L9: Added a short paragraph clarifying the difference between the glacial index approach and the matrix method.

Others used dynamically coupled ice-sheet models to Earth System Models.. Since you’ve started a list of alternatives, you should make it complete. IE Should also consider asynchronous and accelerated coupling with GCMS, eg Gregory et al, 2012, and Herrington and Poulsen, 2012. on this same note, should also mention the option of using results from a range of climate models, Eg Tarasov and Peltier, QSR 2004.

We agree that these are valuable references. We will include them in the manuscript.

P2, L17: Added a reference to Tarasov Peltier (2004) to the list of studies using a glacial-index method to force an ice-sheet model with output from different steady-state GCM simulations. **P2, L25:** Added a few lines discussing the work by Herrington Poulsen and Gregory et al. **P18-22:** Added these studies to the list of references.

Difficulties in bridging the differences in model resolution, as well as other inconsistencies between model states, are addressed and solved This is a vague arguable claim. Be more precise and accurate as to what you do and do not "solve".

We agree that this statement should be more precise. We will change this in the manuscript.

P3, L20: changed this line to more accurately describe which differences in GCM state and ice-sheet model state need to be accounted for.

the model, we simulate ice-sheets at LGM that agree very well with geomorphology-based reconstructions This is not true for North America.

We agree that the phrase “very well” might be too optimistic for our simulation of North America. We will change this in the manuscript.

P3, L25: differentiated between Eurasia and North America in our assessment of model performance.

This ensures the constructed climate history is in agreement with the observed 15 pCO₂ record and the modelled ice-sheet configuration, thereby capturing the major

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feedback process between global climate and the cryosphere, where any change in ice-sheet configuration has an immediate impact on local climate through changes in albedo and orographic forcing of precipitation. This statement is not justified, especially with the use of only two GCM climate snapshots. Atmospheric circulation and therefore climate will depend non-locally on ice sheet geometry, a dependence that is not captured by two or even a handful of GCM snapshots.

P2-L14 Still I'm not convinced how "This ensures the constructed climate history is in agreement with the observed pCO₂ record and the modelled ice-sheet configuration". All the climate states other than PI and LGM are interpolations based on some weights, so why should they be in agreement with the actual climatic history? For instance if the jet-stream pattern variation would be a function of a threshold in ice altitude, how would that be captured by interpolation?

We did not intend to claim that the climate history constructed using our two-state climate matrix is a perfect representation of reality. The statement intends to illustrate how the changes in climate simulated by our model are all attributed directly to physical causes (changes in CO₂, ice geometry and insolation) This is in contrast to the inverse forward models discussed earlier in the paragraph, which prescribe changes in temperature based on observations regardless of a clear physical cause. Neither do we claim that either method is better than the other; in the Conclusions section of the manuscript (p12-13) we discuss how the mismatch between data on sea-level and benthic oxygen isotopes on the one hand, both showing a rapid increase in ice volume during the early phase of the glacial cycle, and CO₂ and temperature records on the other hand, both indicating a much less rapid cooling, is not solved by either type of model.

We agree that the statement might be interpreted as evidence of overconfidence in our methodology. We will correct this in the manuscript. We will also elaborate further on the various shortcomings and (over)simplifications of our method in the discussion section.

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P3, L31: Replaced the discussed statement with a new one that more accurately describes the differences between the inverse modelling approach and ours.
P14, L27: Expanded the discussion of the various shortcomings of our climate parametrization (also in light of the concerns raised by the other reviewer)

It combines the shallow ice approximation (SIA) for grounded ice with the shallow shelf approximation (SSA) for floating ice shelves to solve the mechanical how are fluxes at the grounding-line handled? How are sub-shelf melt and ice calving treated in this model?

In the transition zone near the grounding line, SIA and SSA ice velocities are combined using the approach by Winkelmann (2011), as explained by de Boer et al. (2013). Sub-shelf melt is calculated based on a combination of the temperature-based formulation by Martin et al. (2011) and the glacial-interglacial parameterization by Pollard deConto (2009), tuned by de Boer et al. (2013) to produce realistic present-day Antarctic shelves and grounding lines. A more detailed explanation is provided by de Boer et al. (2013) and references therein. Ice calving is treated by simple threshold thickness of 200 m, where any shelf ice below this thickness is removed. We will add more information to the manuscript to clarify this.

P5, L7: Added a few lines detailing the way grounding line fluxes, sub-shelf melt and calving are treated in the model.

Horizontal resolution is 20 km for Greenland and 40 km for the other three regions For future work, I would recommend 20 km or finer grid resolution for non- ensemble best runs

We thank the reviewer for this recommendation.

fig 4: please include present-day continental outlines even under ice using a different colour than the black/grey contours for ice to aid geolocation

We will add these outlines to the figure.

Fig. 1, 4, 7: Added blue lines showing present-day shorelines to the relevant figures.

strongly parameterized -> highly parameterized

We will correct this in the manuscript.

P5, L25: Change “strongly” to “highly”.

should reference earlier work, eg EBM climate model coupling to ISMs

We will add more relevant references to the introduction section of the manuscript.

P2, L19: Added references to work by Stap et al. with their EBM-ISM set-up.

eq 1, linear co2 weighting factor given the near logarithmic depending of radiative forcing on pCO2, justify why a linear dependence is imposed

Several preliminary experiments, which we chose not to include in the manuscript, were dedicated to trying out various ways to translate changes in CO₂, ice sheet geometry, surface albedo and other model variables into the weighting factors for the

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climate matrix. We ran experiments with both a linear and a logarithmic dependence of the weighting factor w_{CO_2} on p_{CO_2} and concluded that the difference in outcome was negligible. We will add a line to the manuscript to describe these preliminary experiments and their influence on our choice of parameters.

P6, L9: Added a line to describe these preliminary experiments.

eq 5: justify the equal weight contribution for W_{co2} and W_{ice} . Given the large variation in insolation changes from the South to North of eg the North American ice sheet complex over a glacial cycle, I don't see how this constant weight mix makes sense.

We feel the reviewer might have misunderstood the way W_{ice} is calculated in the model. This spatially variable weighting factor is calculated based on “absorbed insolation”, the product of $(1 - \text{albedo})$ and insolation. This links the changes in climate to the two components of this process: changes in insolation (an external forcing) and changes in albedo caused by advancing or retreating ice (a modelled variable) and thereby ensures that the large variations in absorbed insolation caused by the changes in the geometry of the ice-sheet complex are reflected in the calculations.

As with the previous comment, some preliminary experiments, which we chose not to include in the manuscript, were dedicated to finding proper values for the contributions of the two weighting factors. We clarified this in the text.

Sensitivity to the distribution was found to be relatively low; as can be seen from the results in the paper, the temporal evolution of ice volume and CO_2 are very similar. This means the values of the two separate weighting factors are usually very close to each other, implying that assigning more weight to one or the other doesn't change the outcome much. Of course, when more weight is given to W_{ice} , at some point the drop in CO_2 during the inception doesn't decrease temperatures enough to trigger the inception any more. We will add a few lines to the manuscript to clarify this.

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P7, L9: Added more context to justify this choice of weights.

Gaussian smoothing filter F with a radius of 200 km, and Why 200 km?

This value is based on earlier work with ANICE by de Boer et al., who used a similar smoothing algorithm to calculate changes in precipitation over the ice-sheet. As before, preliminary experiments not described in the manuscript investigated this parameter and found that results were not very sensitive to its value, so we chose not to change it. We will add a line to the manuscript to clarify this.

P7, L1: Added a few lines to justify the choice of a 200 km smoothing radius.

Since the relative changes in ice-sheet size for Greenland and Antarctica are much smaller than those for North America and Eurasia, the changes in absorbed insolation in those regions should have less impact on local climate. This is reflected in the model by giving more weight to the pCO_2 parameter. So why not use this same weighting for the part of Canada covered by the same latitudinal range as Greenland, especially given the proximity of NorthWestern Laurentide/Innuitian ice sheets to Greenland? Why not rely on the 200 km Gaussian radius to take care of the ice sheet scale? I highly suspect that the need for this adhoc change is weighting is due to the lack of accounting for larger scale (eg atmospheric dynamical) effects of ice sheet on climate.

The weighting factor W_{ice} scales the absorbed insolation between two extremes: its maximum value at present-day and its minimum value at LGM. An LGM-sized ice-sheet will therefore always yield a weighting factor of 1 (meaning the GCM LGM simulation is used as forcing), regardless of the absolute change in absorbed insolation. For North America and Eurasia, where the continent changes from virtually ice-free to covered by vast ice-sheets, these changes are very large (a relative change of about 32

While we agree that a more elaborate approach, especially taking into account changes in North American ice sheet size into the calculations of Wice for Greenland and Eurasia, along the lines of Abe-Ouchi et al. (2013), would be more realistic, we chose to limit this first model set-up to only first-order effects.

We will add a few lines to the manuscript to clarify this.

P7, L25: Added a few lines to justify the altered wice-wCO2 distribution for Greenland and Antarctica.

eq 10 Novel lapse rate approach that addresses a common problem especially for those modellers who rely on a constant lapse rate value.

We fully agree with the reviewer.

eq 10 Need to show equation for $T(x,y,t)$ given $T_{ref,GCM}(x,y)$ and $lapseLGM(x,y)$ As I understand, eq A1 is for de Boer et al 2014, not this paper (since a constant lapse rate is used)

We will add an equation to demonstrate how the new variable lapse-rate is used to calculate surface temperature.

P9, L1: added this extra equation. P9, L3: fixed several typing errors.

For Greenland and Antarctica, where the changes in ice cover are relatively small even during glacial cycles, the constant lapse-rate is still applied. justify 8K/km choice

P8-L12 Did you do the same calculation for lapse-rate over Greenland and Antarctica

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to check how small the difference would be?

We did not do the same calculation for Greenland and Antarctica. We only applied the variable lapse-rate to North America and Eurasia to solve an observed problem, i.e. mean annual surface temperatures increasing instead of decreasing towards LGM over ice-free areas, thus inhibiting ice growth. For Greenland and Antarctica such problems were never observed, so we never applied this method there. The choice of 8 K/km is based on earlier work with ANICE by de Boer et al. (2014) and with the regional climate model RACMO by Helsen et al. (2013). We will add a few lines to the manuscript to clarify this.

P9, L18: added references to de Boer et al. (2014) and Helsen et al. (2013) to justify the 8 K/km choice.

and that the drop in precipitation caused by the ice-plateau-desert effect scales appropriately with ice-sheet size and that the drop in precipitation caused by the ice-plateau-desert effect scales appropriately with ice-sheet size what does "scales appropriately" mean? By what criteria?

This statement only attempts to give a qualitative description. Preliminary experiments not described in the manuscript showed that if only the local ice thickness is used to calculate the weighting factor, precipitation decreases too fast over the main dome, because ice thickness reaches its peak value long before ice extent does. The resulting decrease in mass balance results in modelled ice-sheets that are far too small. By adding ice volume into the weighting factor calculation, precipitation decreases less quickly when the ice grows, allowing the ice-sheet to grow faster and reach its LGM size.

We agree that this is not clear in the manuscript right now. We will add a few lines to the manuscript to clarify this.

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P10, L9: Added a few lines explaining the rationale behind including total ice volume in the calculation of the weighting factor.

Similarly, for North America and Eurasia, precipitation is adjusted using the Roe and Lindzen parameterization for wind orography- based correction of precipitation as described in Eq. A3 - A6, but now by using the GCM-generated precipitation and orography as reference fields instead of their ERA-40 equivalents Why are no orography effects imposed on Greenland? Observed PD fields show such effects

The Roe and Lindzen parameterization described in the manuscript is included in the model to account for changes in orography. For North America and Eurasia this is important, because the flanks of the ice-sheet, where orographic forcing of precipitation occurs, move around over the continent as the ice sheets expand and retreat. For Greenland, the orographic changes are important for present-day but the changes throughout the glacial cycle are much smaller, as the ice flanks hardly migrates. The orographic forcing is already captured in the two GCM snapshots and it is therefore sufficient to use the interpolated states without requiring this correction.

We will add a few lines to the manuscript to clarify this.

P10, L20-29: Clarified the explanation of the precipitation calculations and fixed Equation reference numbers.

Although the main dome of the ice-sheets is not as thick as in the ICE-5G reconstruction this is a good thing

We agree with the reviewer.

Although the main dome of the ice-sheets is not as thick as in the ICE-5G reconstruction, it now lies more westward than in the simulation with the 5 default ANICE model, which is in better agreement with the reconstruction. Not clear where your main dome is given the 1000 m contour interval

We agree that this is not clear from the figure. We will add a few extra contour lines to clarify this.

Fig. 1, 4, 7: added an extra contour line at 3500m ice thickness to the relevant figures.

The Antarctic ice-sheet now shows a much stronger increase in ice volume around LGM, matching the 16 m of eustatic sea-level contribution postulated by ICE-5G (Peltier, 2004). Should reference more recent literature. The ICE-5G Antarctic ice sheet has little constraint.

However, since it was the ICE-5G reconstruction that was used as input for the HadCM3 simulation by Singarayer and Valdes (2004), we aim to maintain consistency and reproduce that particular ice-sheet with our model rather than the DATED-1 LGM ice sheet. By what logic? You are assuming that ICE5-G is in conformity with the GCM climate generated using ICE5-G boundary conditions. That is a big assumption. The ice mask leaves a strong climate footprint and so I would expect it not hard to match ICE5-G extent but matching I see no rationale to otherwise match ICE5-G topography

While we agree with the reviewer that ICE-5G is hardly perfect and that there is more recent data available for both volume and extent, we believe that a chain of model simulations such as the one performed here (ice-sheet -> GCM -> climate -> ice-sheet

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model -> ice-sheet) should aim for consistency first, i.e. the ice-sheet produced by the ice-sheet model should match the one that was prescribed to the GCM. Otherwise we'd be prescribing to the ice-sheet model a climate which was calculated based on a different ice-sheet, making it even harder to determine the cause of any observed model-data mismatches.

fig 4 and 7 add the ICE5-G ice margin extent as say a red contour to these plots to aid comparison also use 500 m ice thickness contours to show more detail (1 km is awfully coarse)

We agree that the requested elements would be of added value, and will add them to the figures.

Fig. 4, 7: Added the ice-5g ice margin and a 3500 m ice thickness contour line.

The southern margin lies a little too far to the north This is an understatement. Be precise

We agree that this statement is imprecise and overly optimistic. We will correct this in the manuscript.

P11, L18: Clarified how far the modelled ice margin and ICE-5G margin lie apart.

Fig 10 Please replace this with a sensitivity parameter range that captures say 90 between PMIP III results from 2 different GCMs will from my experience give a much larger spread in ice sheet volume

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We will adapt the figure to make estimating the uncertainty in modelled sea-level arising from the uncertainty in our model parameters more intuitive.

Fig. 10: Replaced lines of individual simulations with ± 2 -sigma interval.

regarding Greenland surface temperature anomalies when neglecting the strong negative excursions during Dansgaard-Oeschger events, which are not present in our model forcing or 10 climate reference runs and are also not included as feedback mechanisms in our model physics larger diffs than just missing D/O events in fg 12. Plot 4kyr running mean and you'll see significant diffs.

Modelled temperature anomalies over Greenland and Antarctica agree well with ice-core isotope-based reconstructions. When not for NGRIP

We agree that the current way the icecore data was plotted made interpreting model-data differences difficult. After subjecting the Greenland records to a 4 ky running mean, we find that modelled surface temperature anomalies fall within the high end of the ± 1 sigma range most of the time.

Fig. 12: Merged the GISP2 and NGRIP records into a single stack. Subjected both the EPICA and Greenland stack records to a 4 ky running mean filter and added 4 ky window standard deviation range. P13, L18-21: changed the manuscript text to reflect the changes in the figure.

Local monthly ablation Abl is parameterised as a function of the 2-m air temperature Tano, albedo a and incoming solar radiation at the top of the atmosphere QTOA, following the approach by Bintanja et al. (2002): ... with $c1 = 0.0788$, $c2 = 0.004$ and $c3$ a tuning parameter different for each individual ice-sheet. equations are dimensionally

inconsistent and need dimensional coefficients.

The reviewer is correct, there was a typo in the units of coefficient α . We will correct this, and modify Equations A3 to A6 to be more in line with the original publication by Roe et al. (the current version in the manuscript describes the rather complicated analytical solution of a much more elegant integral).

P15-17: Rewrote the equations described the Roe precipitation in their original, unintegrated form and expanded the explaining text.

These climate states span a two-dimensional climate matrix, with This is not what most modellers would take as a climate matrix

While we agree that a climate matrix consisting of only two GCM snapshots is indeed rather small, we believe our method of model forcing has more in common with the matrix method than the glacial index method – especially because all the algorithms presented in the Methodology section are readily applicable to matrices consisting of more snapshots. However, we agree with the reviewer that stating that two points span a two-dimensional space is mathematically incorrect. We will correct this in the manuscript.

P6, L1: Changed the relevant sentence to more accurately describe the climate matrix.

calculated temperature between the LGM and PI fields over the ice-free area in the region at LGM. specify region

The region alluded to in this statement is the geographic area covered by the model grid. We will clarify this in the manuscript.

P9, L5: Changed the relevant sentence to clarify which region is meant.

When accounting for uncertainty in the applied forcing and model parameters, the simulated volume of the four major continental ice-sheets (excluding contributions from smaller ice caps, glaciers, thermal expansion and ocean area changes) at LGM amounted to 97 ± 6 m sea-level equivalent. This shows that uncertainties are not adequately addressed. The uncertainties in this modelled system (ie compared to "reality") are going to be much larger than 6 m SLE.

We did not intend to claim that the uncertainties in the applied CO₂ forcing and in our ice-sheet model parameters are the only sources of uncertainty in our sea-level reconstruction – merely that those are the only uncertainties that can be meaningfully investigated with this model set-up. We will clarify this difference in the manuscript.

P14, L16: Clarified whence the uncertainty in the quoted number arises.

At least 3 of the references to equations in the text have the wrong equation number.

We thank the reviewer for his attention to detail. We will correct the erroneous references.

Entire manuscript: correct all erroneous equation and figure references.

P4-L21 What does "some external forcing" mean?

The full statement, "Surface temperature is calculated from present-day monthly values, including a global temperature offset calculated based on some external forcing, and a constant lapse-rate orographic correction", refers to the way ANICE was used

by de Boer et al., Bintanja van de Wal. In these studies, ANICE was forced using the inverse coupling method, where a global temperature offset is calculated from a benthic oxygen isotope record, icecore isotope record or sea-level record. We agree that this is not clear. Since we do not use this method of forcing and do not allude to it any further, we will remove this statement from the manuscript.

P5, L12: removed this statement.

P4-L28 What is the "existing independent literature"?

de Boer et al. (2013) compared their results to other modelling studies (Huybrechts 2002, Pollard deConto 2009, Bintanja et al. 2005, Bintanja van de Wal 2008), geomorphological evidence (Ehlers Gibbard 2007), sea-level records (Rohling et al 2009, Thompson Goldstein 2006) and the contribution of ice-sheets to sea-water heavy isotope enrichment (Duplessy et al 2002, Lhomme Clarke 2005). We will add more references to the manuscript to support our confidence in the ice-sheet model.

P5, L18-22: included the references listed above in the manuscript.

P8-Eq. 11 Why don't you use local altitude instead of the ice-thickness? The difference at LGM could reach 1 km and it is surface elevation that physically matters.

Since the ice thickness is scaled between two extremes (LGM value and zero) to calculate the weighting factor (which scales between 0 and 1), using surface elevation instead of ice thickness will yield the same result as long as the two variables change at the same rate. During the build-up phase of the glacial cycle this is generally true (the ice rarely grows faster than the lithosphere can adjust). During the deglaciation it is not, but since that process is dominated by ablation rather than accumulation, we

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believe changing the parameterization from ice thickness to surface elevation will not significantly impact our results.

P8-L16 "Whereas a continental-sized ice-sheet influences temperature mainly through albedo"; is this true? What about changes in atmospheric circulation, runoff and therefore changes in ocean circulation, and the elevation itself, hence the lapse-rate effect?

We agree that this statement is incorrect – the changes in temperature are caused not only by changes in albedo but indeed also other processes, not all of which are captured by our model set-up. We will change this in the manuscript.

P9, L20-23: Changed the statement so that it only illustrates why the choice was made to use a different parametrization for precipitation than the one for temperature without seeming to suggest that the current temperature parameterization captures all possible processes.

Fig. 6 The total ice volume evolution, specially during the inception phase, doesn't follow the records; eg the 110 ka max volume.

The mismatch between our own modelled ice volume evolution and available records during the early part of the glacial cycle is discussed in the Conclusions section of the manuscript (page 12 – 13). We will add the ICE-5G pre-LGM eustatic sea-level record to Figure 6 to illustrate this mismatch.

Fig. 6: Added the ICE-5G pre-LGM eustatic sea-level contribution record to the figure.

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