Response to the reviewers

We again thank the reviewer, Andrey Ganopolski, for taking time for this final? review.

Reviewer 1, Andrey Ganopolski

Reviewer Point P 1.1 —

I appreciate the efforts which the authors made to respond to my criticism and suggestions and in general, I am satisfied with their response. However, I still have two remarks on the revised manuscript.

1) In response to my criticism that in the introduction the authors do not cite previous works on modelling of glacial inception (nothing personal – all my relevant papers have been cited in the manuscript by Bahadory et al.), the authors responded " that most of the "25 modeling papers" cited in Calov et al. (2005) used what we judge to be "poor model/experimental configuration/designs and obtained poor results in large discord with paleo proxy constraints ..." Since many more papers on this subject have been published after Calov et al. (2005), the authors obviously consider all of them also to be "poor". I do not believe that such attitude (unfortunately, not unusual) when previous publications considered to be obsolete and not worth mentioning just because they were based on the "wrong" models. However, only thanks to these earlier studies, we now can do some things better than it was possible 15 or 20 years ago and this is why the earlier efforts deserve at least to be acknowledged. After all, do the authors believe that their own study is problems-free and their results are in perfect agreement with paleo proxy constraints?

Reply: By Andrey's logic one might argue that every published say GCM study should cite every every previous GCM study for the given context, which would make the papers unreadable and the majority of text plain citation. Blind citation does not help the reader in our opinion. Yes, we acknowledge citing early ground-breaking work, but only to a point. Furthermore, a number of the early papers that Andrey cites in his 2010 paper are not directly relevant to this study, ie exploring the possible to likely geographic evolution of the last glacial inception ice sheets. We have, though, done one more near exhaustive litterature source on the topic, including all papers that cite Calov et al. (2005) (according to web of science). And have added the following 3 citations: Bonelli et al., 2009, Herrington and Poulsen (2011), Gregory et al. (2012. We've also added a whole paragraph about coupled GCM/ice sheet modelling of LGI that references the latter two.

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There are not many that have modelled northern hemispheric last glacial inception with fully coupled models that offer some probability of encapsulating the geographic evolution of the ice sheets (as opposed to earlier studies that just focused on explaining ice volume changes inferred from sea level proxies). And we see no point in citing coupled modelling studies that examined different glacial intervals.

Reviewer Point P 1.2 — 2) In my first review, I suggested that the modelled east-west asymmetry in the North American ice sheet distribution is not consistent with paleoclimate reconstructions and is caused by temperature biases of the LOVECLIM model. Following my recommendation, the authors now show (Fig. 14) modelled summer temperature biases and discuss their potential

implications for modelling of glacial inception in section 3.1.2 ("Labrador and eastern NA remain ice-free, likely due to warm model biases in this region") and 4.1 ("... temperature biases ... may have inhibited glaciation over Hudson Bay and northern Quebec"). However, in section 4.5, the authors continue to insist that ("except for Alaska") their modelling results are consistent with paleoclimate reconstructions.

Reply: The last statement is incorrect. We state "Except for Alaska (and certain adjacent parts of the Yukon), our results are, within (large) age uncertainties, consistent with the till stratigraphies presented in Clark et al. (1993) and their summarizing figure 19", and that statement is correct given the large uncertainties in ages and incomplete statigraphic sequences.

Reviewer Point P 1.3 — To this end, the authors dismiss MIS5d reconstruction by Batchelor et al. (2018) as unreliable and claim that what Clark et al. (1993) wrote in their paper about the initialization of glaciation in Keewatin and Quebec contradicts to their own figure (Fig. 19) which shows the opposite. Although the authors are much closer to Quebec than me, I would insist that their reading of Fig. 19 in Clark et al. (1993) is mistaken. Clark et al. (1993) wrote "Following the last interglaciation, the Laurentide Ice Sheet first developed during Oxygen-Isotope Stage 5 over Keewatin, Quebec and Baffin Island" on the same page where they discuss Fig. 19. This is why I doubt that the text and Fig. 19 contradict each other. In fact, Fig. 19 only suggests that ice sheet was absent in the most southern part of Quebec during MIS5 which does not contract, for example, Batchelor's MIS5d reconstruction.

Reply: As was indicated in the revised submission, Batchelor et al, in their supplement list available empirical data points for each of their time-slices. There are none for MIS5d Laurentide. The sole constraint (apart from one small region) is the empirical reconstruction from Kleman et al, 2010, based on glacial flow indicators (flow sets). Chronology is again a major challenge, and the latter are unable to rule out that the indicators used were for pre-Eemian flow: " If older than the Wisconsinan, such restricted ice volumes are only compatible with a stage preceding the Illinoian maximum, an alternative we consider less probable due to the preservation of the Atlantic morphology." For which I would counter, how was this morphology preserved through MIS3:1?

We also now bring up the contradiction between Batchelor's MIS5d LIS and Clark et al. (1993), with one having complete Hudson Bay glaciation and the other having it ice free:

"Their complete lack of MIS5d glaciation of Hudson Bay contradicts the favoured inference of Clark et al. (1993), 470 pointing to the challenge of inferences from sparse geological data with very poor age control. It should be noted however that Batchelor et al. (2019) barely reach the inferred MIS5d sea level minimum of ?, using an ice volume to area scaling relationship derived for a circular mono-dome ice sheet with plastic rheology (Cuffey and Paterson, 2010). We find that this relationship over-estimates ice volume during last glacial inception by at least 50% when compared to the base GLAC1-D nn9927 ice sheet chronology from the calibrated glaciological modelling of Tarasov et al. (2012)."

Is this not a major feature that raises questions about which of these are accurate? :

I (Lev Tarasov) have reviewed Clark et al 2003 one more time. The core issue is that no data is provided for Quebec and Labrador aside from the chronologies in fig 11 and 12 (St. Lawrence Lowlands and Appalachian uplands), which indicate an absence of glacial tills over the 75 ka (option A) or 95 ka (option B) to 130 ka interval.

The Clark et al 1993 reasoning for the statements that Andrey is referring to relate to (as far as I can tell) the following quote "The Rocksand and Amery tills underlie the Fawn River and Nelson River

sediments, respectively (Fig. 17). The Rocksand Till contains reworked marine shells with alle/Ile values similar to those obtained from Bell Sea sediments (Wyatt, 1989). If the TL chronology is correct, these tills record west-northwestward flow of ice from Quebec during the middle of Oxygen-Isotope Stage 5" (Clark et al 1993). However the TL chronologies are quite uncertainty.

Anyway, to ensure my interpretation is accurate, I contacted two of the co-authors of Batchelor et al, 2016. Both are glacial geologists who have worked extensively on last glacial cycle records for North America. They indicated:

(Chris Stokes, Durham U., pers. comm.) "I don't really see Kleman et al.'s work as much of a constraint. It's a sketchy bit of evidence that is undated. The problem is that it is pretty much all we have." It should be noted, that this cited Kleman et al, 2010 work was virtually the sole input for the Batchelor et al, minimum and best guess Laurentide ice sheet configurations for MIS5d.

(April Dalton, Durham U., pers. comm.) "As for the Hudson Bay Lowlands, the stratigraphic record there is complex and unfortunately its not currently possible to determine whether this area was ice-free or glaciated during MIS 5 stadials." As noted above, it is these records that form the basis for the Clark et al (1993) interpretation of early formation of ice over Quebec and Labrador.

Given all this, we've rewritten the NA comparison against past geological inferences, to better emphasize the uncertainties. We've also removed that statement comparing to Clark et al 1993 that Andrey finds so problematic and now refer to a more recent (though shorter review, but at least which presents/cites more useful data about Quebec LGI):

"Stokes et al. (2012a) provides the most recent review of geological inferences and modelling results for NA LGI. The main discrepancies in our results are the already noted issue of excessive Alaskan glaciation and likely inadequate ice extent over Quebec and Labrador."

Reviewer Point P 1.4 —

In any case, whatever the reliability of paleoclimate reconstructions of MIS5 North America ice sheet is, the Fig. 14 in the revised Bahadory et al. manuscript shows that their modelling results in the north-eastern part of North America are not trustworthy. Indeed, the figure shows summer temperature biases over Keewatin and Quebec of more than +10C (!). Since, according to model simulations, climate response to changes in orbital forcing at 116ka was only half of that, this area in the model remains much warmer during glacial inception than it is in reality even at present. This, of course, completely preclude ice sheet formation in eastern Canada. As I made it clear in my previous review, I have no intention to give advice to the authors which modelling approach they should use. However, I would now add to the list of publications which applied temperature biases corrections, the recent paper by Choudhury et al. (2020) and I believe this paper reinforces my concern about the impact of strong summer temperature biases in LOVECLIM on simulation of glacial inception.

Reply: We already state "Model temperature biases under present-day conditions are larger over NA than over EA and may have inhibited glaciation over Hudson Bay and northern Quebec". And it should be noted that Loveclim has a stronger warm bias over Hudson Bay then the adjacent sector (ie same latitude) of Quebec/Labrador, but that doesn't inhibit full glaciation of Hudson Bay (though not James Bay) in LCice by 112 ka. However, to further assuage Andrey's concerns, and ensure that the readers are clear on simulation limitations, we have somewhat rewritten that subsection

"Figure 14 shows the present-day mean June/July/August temperature bias of the 55-member ensemble for both NA and EA. Model temperature biases under present-day conditions are larger over NA than over EA and the 9:5oC to 14oC mean regional summer temperate biases would have hindered

at least initial glaciation and likely LGI maximum extent (to a more uncertain degree) over Hudson Bay, Quebec, and Labrador. However, though the ensemble mean summer temperature bias is stronger 410 over Hudson Bay than the adjacent sector of Quebec/Labrador, this doesn't preclude complete glaciation over Hudson Bay (but not James Bay) by 112 ka (figure 6) in ensemble members. Increased LGI stadial ice over these latter regions would also improve fits to global mean sea-level proxies (cf. Figure 1). Thicker stadial ice could also enable a stronger and faster post-stadial retreat."

and now also make explicit reference to the issue in the conclusions:

"Discrepancies are likely due to the absence of a modelled (and probably out of phase) Antarctic ice sheet contribution in LCice 1.0, model limitations (as evidenced by the present-day warm summer temperature bias over Hudson Bay and Quebec), and dating uncertainties in the proxy-based reconstructions"