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Climate of the Past Discussions

Interactive comment on "Rapid waxing and waning of Beringian ice sheet reconcile glacial climate records from around North Pacific" *by* Zhongshi Zhang et al.

Anonymous Referee #1

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Review of "Rapid waxing and waning of Beringian ice sheet reconcile glacial climate records from around North Pacific"

This manuscript by Zhang et al. 2020 addresses an interesting and important issue, namely the spatiotemporal distribution of glacial ice-sheets in the northern hemisphere and the timing of presumed glacial evidence off the East Siberian Shelf. I thank the editor for the opportunity to review it, as well as the authors for this stimulating contribution to the discourse.

The analysis by Zhang et al. is novel and contributes to our understanding of a complex topic. I complement them on taking on a difficult task and gleaning useful insights from

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their results. It is a good fit for Climate of the Past and I hope to see it published once they have addressed my concerns.

I recommend major revisions to this manuscript. Before publication, I would like to see a revised manuscript which focuses more on the mechanisms and feedbacks which can be studied from their experiments, and less on reconciling complex palaeoclimatic paradoxes which I do not think their experiments are particularly well-suited to address. I have provided feedback on both of these elements below.

I have a few main concerns that I would like the authors to address, followed by more specific recommendations based on line numbers below.

1) The main point of the paper seems to be that including the Beringian ice sheet in an asynchronously, two-way coupled climate/ice-sheet/vegetation modelling protocol "reconciles glacial climate records from around (the) North Pacific." However I do not see this conclusion clearly supported in the main figures. The most direct evidence to support this claim would be something like Supplementary Figure 3. However, I cannot tell by eye which of these two modelled scenarios (the orange and magenta bars) are more similar to the proxy records presented (black and green lines). No quantitative measures of how similar the timeseries are is presented (even as simple as cross-correlation of the records in question) to support the assertion that the orange bars provide a better fit to the data than the magenta bars. Furthermore this is difficult to assess because the y-axis limits for the two subplots are different. I would like to see this figure presented in the main text, with a full discussion of why subplot (b) indicates a better fit to the data than does subplot (a). In such a discussion, presenting the cross correlation (or other quantitative indices) to support the main claim of the paper is critical. Figure 1c is also very central to your argument – that the offset between Western NH surface temperatures and the deep-ocean d18O is not reconcilable with Laurentide-Eurasian only ice sheets. Please provide a figure that more directly compares your results with these proxy records to show how the inclusion of the Beringian ice sheet supports this interpretation. Suppl. Fig 3 has the last glacial cycle but why

not show the last 4 since you set up the paper with this in mind?

2) It is not immediately clear to me from the paper, figures, or supplement the extent to which the NorESM-BIOME4-PISM modelling approach actually reproduces realistic ice sheet extents, namely the extent of the LGM Laurentide ice sheet. This is central to the main argument of the paper, because I am concerned that the authors interpret changes in atmospheric circulation as being diagnostic of the presence of a Beringian ice sheet, when such features may equally be due to the lack of a proper Laurentide. I believe that Figure 4 is somewhat geared towards addressing this, where you show the results of the climate model for the same time slice with and without certain ice sheets, to demonstrate their impact alone. However, the argument about reconciling glacial climate records depends to large part on what happened during the LGM, and the ice sheet extents used to force the climate model in Figure4E-H are not representative of the last glacial period. Why not use the modelled extent of the Beringian extent within the region you define as Beringia, and then Ice6G outside of that, to isolate the effect of the Beringian ice sheet on its own? I understand that this is sort of what you are trying to get away from – this assumption that all glacial periods look like the LGM, which I agree is a bad assumption. But your analysis has also focused on identifying mechanisms that can be used to fingerprint the presence of the Beringian ice sheet and I feel that the mechanisms you present would be more credible if the modelling set up allowed for a realistic expression of the Laurentide ice sheet at LGM. This is relevant to, for example, lines 264–268. Are these changes expressed in the same way when a Laurentide is included? The mechanistic explanation of strengthening the trough-ridge system is true of the Laurentide as well (see Figure 4C&G); the magnitude of temperature change is less relevant, in my opinion, because the temperature along the US west coast is not solely a function of the atmospheric circulation over the Pacific but will also be influenced by the presence/absence of a large Laurentide Ice Sheet. I recognize that these kinds of modelling approaches are computationally intensive and you are intentionally not fixing the ice sheet positions in order to investigate the novel, previously unappreciated consequences of capturing a fluctuating Beringian ice sheet.

But statements like line 282-284 really distract from that overall project by making a direct comparison with the LGM, which your simulations are not well-suited to address because they do not reproduce a Laurentide ice sheet. Why not instead focus on the interesting elements which do not depend on being able to perfectly reconstruct the LGM ice sheets – for example, the importance of vegetation feedbacks 5.1 which has not received as much attention in the literature but which I find very compelling. You can also describe the dynamical consequences of the fluctuating Beringian ice sheet (e.g. Supplementary Figure 6) without saying that you propose this precise configuration for the last glacial cycle, but rather want to study what the impacts and effects of such an ice sheet are in general. To me any mention of the LGM in the context of your results is not very compelling, because your reconstructed LGM Northern Hemisphere ice cover does not compare favorably with what we know from physical evidence (e.g. Peltier et al. 2015).

3) Regarding the ice sheet model set-up, the "IDL" parameters produce much larger ice sheets during glacial periods than the "FAV" parameters. I would suggest you include a table of the parameter choices which could go in the supplement for interested readers. I was genuinely confused by "IDL" and "FAV" because I kept reading them as "ideal" and "favorite" which are basically the same thing. To me these seem like high mass balance and low mass balance end members, with "IDL" having less mass loss due to melting (lower PDD factors), and calving (both from thickness calving and eigen calving). Could you call them something else – high-MB and low-MB for example? (Anything else is fine too, just something that makes it easier to differentiate between them for the reader). And to be clear, the IDL parameters produce northern hemisphere ice cover with a sea level equivalent of $\sim$130m (Supplementary Figure 8E) but with an extent restricted to approximately what is shown in Supplementary Figure 5L?

I really appreciate that these authors have taken on such an interesting and rich topic, on which little modelling work has previously been done. However I think that the current framing "reconciles glacial climate records" is not supported by the figures/text.

I would advocate for describing and analyzing the simulations while being more forthcoming about what questions these simulations can, and cannot, directly address. This has been done to some extent in the “uncertainties” sections, but these to me seem very focused on small details rather than the big picture.

Some finer details below, organized by line number:

âĂć Line 2 “the” North Pacific âĂć Line 36 replace “demonstrate” with “to evaluate the climatic consequences of” or similar âĂć Line 36-40 This is a very strong claim and I would like to see it better supported in the text and figures. âĂć Line 95 The two important “pieces” of evidence âĂć Line 101 interpolated replace with “Interpreted” âĂć Line 105 hardly replace with “hard” âĂć Line 180-181 The IDL parameters were tuned to the volume of the Laurentide, right? Rather than the extent? That’s important to clarify here. Saying “close to reconstructions” to me implies that the measured extent of the Laurentide (e.g. Ice-6G) was used as a constraint. âĂć Line 236 “Consistent with” âĂć Line 274-275 I would be cautious with the assertion that your modelled Beringian ice sheet is really indicative of the last four glacial cycles. I think the mechanism is robust and its important to show there could be an ice sheet, but the precise configuration is probably not captured by your model (for example, it is my understanding that Lake El’gygytgyn has never had an ice sheet over it, as there has been continuous sedimentation throughout the last 3.6 Ma and even during glacial periods (e.g. Melles et al. 2012, Science)). How can this be reconciled with <2 kms of ice (your figure 5) over this site? In my opinion the strength of your approach is understanding the mechanisms and feedbacks related to a Beringian ice sheet, rather than in identifying its actual extent/thickness. âĂć Lines 293-296 I do not see what you are talking about regarding the diachronous retreat of the Beringian vs. NH ice sheets. They seem to have max ice volume at the same time (Figure 6B&C). âĂć Lines 303-304. This is really central to your argument.. please point to a figure here âĂć Lines 449-451 This interpretation is not supported by Supplemental Figure6Q–S where a Beringian ice sheet is present âĂć Lines 456–458 “The ice sheet grows. . .” check this sentence – it does not make

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sense to me as written currently.

Figures In general I feel the figures could be simplified by reducing the number of times you plot the same timeseries. For example in Figure 1 the DH speleothem record is plotted 4 times and in Figure 2 the same record appears 5 times. I think this makes comparisons more difficult and really implies we should be seeing the similarity of the records rather than using some kind of quantitative metric (cross correlation for example) to see where the records share variance. Where possible, please have each record only appear once (I know that sometimes plotting the record again is warranted, so I am just asking that you take it as a general principle to only plot each record once per plot, and I recognize there will be exceptions.) Please note that the locations of all the cores are shown in Figure 10. In general I do not feel like the core names are particularly illuminating in the actual axis labels, I would prefer geographic labels (i.e. Northwest Pacific SST, or simplified LAT/LON), with the core names listed in the caption.)

Figure 4 – Temperature is really Change in Temperature relative to the simulation with no ice sheet, right? Why are some of the areas white? No change, change not significant at some threshold. . .? (For example I'm thinking of the white areas where you have placed the "5" on top of the arrow. By the way, what are the units for those arrows?)

Figure 6 – Why are these timeseries shaded in below what seems to be an arbitrary value? Does this value mean something – i.e. it's 0.5km in A, $\sim$12m in B, 15m in C, etc. Why not just plot these as a time series as well – what benefit is filling it in? Again insolation is plotted twice. The pink and orange "bars" are confusing here as well, why are the plotted from 13.25 degrees in D and 4 degrees in E? Why not just use the black line (the time series) and color it? Perhaps with dots along it where you have actually run the model (which is what I assume the bars are trying to show)? It's confusing the way it is currently. Please use (m) rather than (10m) for the unit of subplot C.

Figure 7 – This is a really interesting figure, thanks for showing these model outputs.

Supplement

Figure 1 – the same timeseries is shown 5 times. Why? "Percentage" should range from 0-100 Figure 3 – see my comments above – this figure should be in the main manuscript, extended to 4 glacial cycles, and with timeseries instead of the vertical bars for the model outputs. Please do some analysis to show that Fig 3b better agrees with the proxy records. Please make the y-axis of both simulation outputs the same so that it is clear the difference in magnitude of the changes. Can you scale the d18O to a change in temperature using published relationships, or discuss in the text how you think the magnitude of the modelled changes in temperature would be expressed in terms of the DH d18O record? Figure 4 – again, should be change in temperature I think. Figure 5 – Which set of ice sheet model parameters is used here? FAV? Please include. "North American east coast" should be "Hudson Bay" or some other geographically relevant description (Northern Canada, etc.) – "east coast" is confusing. Figure 6 – quite generous to say the ice sheet is "largely gone" at 22ka, especially given current thinking that this ice sheet did not exist at the LGM. I would revise the caption to remove this. Still interesting to see how the ice sheet fluctuates along with the climate – thanks for including this. Figure 8 – if you are going to fill all of these in fill them in above 0 – it's confusing that they seem arbitrarily filled in. But since we cannot see the "IDL" scenarios behind the "fav" scenarios I would plot them just as time series, or find a way (partial transparency?) to make both sets of simulations visible throughout to see how the simulations differ during the early glacials. In E, seems like you meant to just show the purple, so that you are comparing ice thicknesses for two different regions from the same set of simulations. Throughout, the unit (10m) is confusing and I would advocate for (m).

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