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Climate of the Past Discussions

Interactive comment on "A model-data comparison of the Last Glacial Maximum surface temperature changes" *by* Akil Hossain et al.

Anonymous Referee #1

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Review of Hosain et al. 'A model-data comparison of the Last Glacial Maximum surface temperature changes'. The authors show a comparison between reconstructions of LGM temperature on land and in the ocean and several climate model simulations. They use two different compilations of temperature data and pay special attention to potential recording biases in the marine proxy data. The authors also use several configurations of the COSMOS model with different ice sheets and the PMIP3 models. Hosain et al show that particularly in the marine realm there is considerable mismatch between the data and the models and they suggest that these are due to seasonal and depth recording biases in the proxies.

The paper has an ambitious two-fold goal: i) to assess the different ice sheet reconstructions and ii) to assess biases in the recording of (marine) proxies. Both are impor-

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Discussion paper

tant questions. However, after reading the manuscript it is still unclear to me why and how the ice sheet reconstruction by Tarasov is better and what we have learned about marine temperature proxies apart from the known fact that they might be biased to variable seasons and depths. A paper that explicitly states 'comparison [of proxies] with outputs from climate model will help to understand the recording system itself' (L73) should deliver more and provide new insights, or directions, into how we can overcome the known recording biases. The approach taken by the authors is to simply look at what depth or season the marine proxy system correlates best. This implies that the recording bias may vary randomly from site to site. While there is nothing wrong with that approach as a starting point, we know that the ecology of the proxy carriers is not random (see e.g. the discussion section on alkenones or Leduc et al. [2010] or Jonkers and Kucera [2015]). The offsets between the annual mean SST and the reconstructed SST are thus likely to follow a systematic trend, likely with temperature. Rather than showing that ecology leaves an imprint on proxies (which is old news) the authors should investigate whether they see such trends in their comparison. A model that shows a pattern in the offset that is consistent with our understanding of the ecology of the recorder could arguably be considered to have more skill than one that doesn't. The opposite (no pattern, or random deviations) are more likely to be related to simple noise in the reconstructions or models. In this way models and data can be more meaningfully compared and new insights about the recording systems might be obtained. Related to this, it remains unclear how depth and season in the recording bias are separated? The same temperature can often be found at different times of the year or at different depths. How is this dealt with in paragraph 4.4? And what season is assumed in paragraph 4.3? In addition, why is seasonal recording not considered for the terrestrial proxies? And is it right that the evaluation of the different ice sheet topographies in based solely on the terrestrial data? I couldn't find a figure or table with summary statistics.

Equally importantly, the comparison between the reconstructions and the models could be improved. A simple correlation can be very misleading and the RMSE (deviation from the 1:1 line, why in per mille?) is a much more useful measure of the difference. Moreover, there is no statistical treatment of the uncertainties in the data or the model (at the minimum interannual variability in the model and the reported errors on the reconstructions should be taken into account). None of the statements about significance are accompanied by an explanation how this was determined and at what confidence level. This leaves the reader wondering whether the differences between the different ice sheet configurations or the different season/depth biases are real or meaningful. This is crucial as many differences between the models are very small.

At some places in the manuscript the authors mention uncertainty in the models too. It would be good if they discuss this more upfront. With so many models and different configurations of the same model (in this case the ice sheet topography) there are many degrees of freedom and there is a large chance of being right for the wrong reasons, not only because the proxies are biased (L163). How do the authors deal with that? Related to this, what have we learned about the model (configuration)? If some of the observed differences between the model runs are real/significant, then why? Where? Can the authors go deeper into the mechanisms or the physics that explain the differences?

In addition, the manuscript lacks a clear separation between results and discussion and the discussion section itself does hardly discuss the results, but rather summarises what others have said about potential recording biases in marine proxies. A lot of this could be placed in the introduction instead. Finally, there are numerous spelling and style errors. I have indicated some in the line-by-line comments below, but I recommend that the authors thoroughly proofread a revised version. I am sorry that I am unable to provide better news at this time, but I hope that my comments will help to improve the manuscript.

Line by line comments

L8: ‘abrupt’. Reconsider wording What is meant here?

L11-12: reword ‘ . . .pollen and plant macrofossil based. . .’

L16: it is the simulation using the Tarasov reconstruction that shows the highest correlation, not the reconstruction.

L33: Project instead of Projection

L40: please be more specific, uncertainty of what?

L54: please add a sentence or two to explain the link between the beginning and end of this paragraph. Importantly, Jonkers and Kucera [Jonkers and Kučera, 2017] –and before them several others [e.g. Mix, 1987; Schmidt, 1999; Schmidt and Mulitza, 2002; Skinner and Elderfield, 2005] – showed that there is predictability in the recording bias. This is an important point as it may help to distinguish between different models and or estimates of recording depth/season.

L74: replace ‘will help’ with ‘might help’

L76: ‘can force’ – consider rewriting. Also, rewrite statement about all models in the next sentence. The PMIP3 ensemble does not contain all models of LGM climate.

L78: Strictly speaking there is no ecological effect on the proxy interpretation, there is an ecological effect on the recording of the climate sensor (proxy) [see for instance Evans et al., 2013].

L95: is Zhang et al. 2013 appropriate for the PMIP3 protocol?

L109-134: what exactly is compared, the gridded products of the reconstructions or the individual sites? If the latter, why is the gridding explained and how were the data compared precisely?

L148: positions of brackets is incorrect.

L166-174: this is discussion. No references in results section.

L195: Change to ‘Proxy-specific comparison’ or equivalent.

L211: add uncertainties in the transfer functions. Or non-temperature effects on the assemblages?

L231-240: discussion. It is also unclear to me what the main message of this paragraph is.

L252: $R = 0.01$ means no correlation, not a positive one.

L256: the data is not composed of planktonic organisms, it's based on measurements of their fossil remains. Also reword 'shift in the different water columns'.

L260: Coccolithophores (the alkenone-producing organisms) are phytoplankton and require light for photosynthesis. The same holds for other phytoplankton and symbiont-bearing planktonic foraminifera. 183 m seems rather deep for phytoplankton. I assume that light availability is not modelled, but the authors should look into this and assess whether the inferred recording depths (e.g. L269) are consistent with the ecology of the proxy carriers. There is also a lot of discussion in these sections.

L270-274: this sentence begins and ends with different statements about the habitat depth of planktonic foraminifera. Please explain the difference, or discuss it. See also Rebotim et al. [2017] for a discussion on the variability of depth habitat.

L289-295: I disagree, if the data and the model disagree, and consistently disagree the reason is unlikely to be due to uncertainty in the data alone. Uncertainty in the data would lead to random variations around the mean value, not indicate consistent (temporal/spatial) changes. It is more likely that the mismatch is due to uncertainties/unknowns in both the data and the models. It would be good if the authors acknowledge that more.

L327-329: this section on sediment traps needs referencing. It is also well known that there is no uniform seasonality of planktonic foraminifera, rather seasonality varies spatially [Jonkers and Kučera, 2015; Tolderlund and Bé, 1971] and has hence likely varied in the past.

L336-337: please be specific: uncertainty for planktonic foraminifera proxies, not the foraminifera themselves. Moreover, this not only holds for planktonic foraminifera, but for all proxy carriers with a short (< 1 year) life span [e.g. for coccolithophores that produce the alkenones Rosell-Melé and Prahl, 2013].

L344-357: so it seems that there is a pattern in the season that is preferably reflected in the UK37 ratio. Is this resolved in the model-data mismatch? Does any model yield data more consistent with such a pattern? It is this kind of analysis that is lacking from the present manuscript.

L364: proxies are not exposed to nutrient conditions, the organisms are.

L377: Deuser and Ross and Anand et al used the same sediment trap time series for their analysis, so this is only regionally constrained information. Crucially, one cannot infer living depth from sediment traps (perhaps the authors mean calcification depth).

L380-384: this idea is hardly new, Emiliani [Emiliani, 1954; 1955] already touched on this. Please include.

L391: it is unclear what is meant with ‘in such a way’.

L395: There is also observational data that shows the dampening effect of changing habitat of the proxy carrier [Ganssen and Kroon, 2000; Jonkers and Kučera, 2017].

L400: why on the contrary, I don’t understand the difference. And please explain why it is important to model foraminifera growth, rather than abundance. Note also that Fraile et al used many more variables than temperature alone [Fraile et al., 2008] (in fact, more than Lombard) and see Kretschmer et al [Kretschmer et al., 2017] for an update of this model.

L406-412: I think a more upfront discussion of inherent uncertainties in the model is essential and should be placed not at the end of the discussion and include more than just model resolution.

L420-421: Sentence incomplete or wrong.

L423-427: this fundamental mismatch between the models and the data is mentioned here for the first time. It deserves mentioning in the results and discussion. As to the question whether it is the models or the data that cause this discrepancy, it is important to note that our current understanding of proxy carriers (in particular planktonic foraminifera) is that they tend to underestimate the environmental change (see suggested references and studies cited in the manuscript). Such homeostatic behaviour only exacerbates the mismatch.

Fig. S1 is directly copied from the MARGO paper, I don't know if this is appropriate with regards to copy rights etc.

Table 1: why is there no RMSE for the Tarasov reconstruction? Also, none of the errors have units. Similarly, the legends in the figures often lack units.

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