**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 #2

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I am grateful for the opportunity to review the article entitled "A model-data comparison of the Last Glacial Maximum surface temperature changes" by A. Hossain and co-authors. This study simulates the temperature changes during the Last Glacial Maximum with the GCM model COSMOS and compare the outputs with the temperature reconstructed from both marine and terrestrial data (Bartlein et al., 2011 and MARGO datasets). They use six different Laurentide Ice Sheet reconstructions in the COSMOS model. Simulations based on PMIP3 and IPSL models are also compared with the proxies-inferred SSTs. They evidenced some mismatches between simulated and reconstructed SSTs and investigate the role of the seasonality and the depth as potential bias in the proxies to explain such discrepancies.

I think that the paper of A. Hossain et al. presents interesting findings in terms of results

to be published in C. Past but I also think that it cannot be published in its current version. My first point concerns the structure of the paper which is not appropriate as it is. In draft I have, a lot of things are confusing (for ex., figures 6 et 7 are not mentioned and discussed in the text; it's quite the same for the figure with PMIP and IPSL models even if I have downloaded the corrected version). I have the feeling that the paper has been written fast and a considerable effort of rewriting is needed to improve the quality of the draft before to be accepted. The structure of the results and discussion parts is not clear and not easy to follow. I strongly recommend to group the results and the discussion in the same part. I propose to restructure it as follow: 1. model comparison 1.1 LIS simulations 1.2 COSMOS, PMIP3, IPSL comparison 2 data-model comparison: 2.1 terrestrial temperatures changes 2.2 SSTs changes (including seasonality and depth) The comparison with the terrestrial temperature changes is an important point given that if I remember well, PMIP models often underestimated the LGM cooling. SO I would like to add this figure not in the supplementary material as it is in the current form but in the text to support the discussion. May be you can also compare the seasonal parameters (temperature of the coldest and warmest month) simulated by COSMOS with MTCO and MTWA inferred from pollen.

My second point concerns the originality of this paper. The objective and the questions of your paper need to be better justified. There is a lot of simulations on the LGM as this period has been chosen by the PMIP modelers; therefore we need to better understand what are your questions, and what is new compared to previous studies. I have the same feeling for data-model comparisons: they are a lot of, and the Bartlein and Margo datasets are not new: could you better clarify the originality of your approach and of your results?

Moreover, several points of the discussion must be discussed more in depth or are lacking (see below).

Other points:

-Title: should be more informative - Abstract : must be more precise (key results, conclusions) - Introduction : -line 32 : correct "Projection" with Project"; more refs are needed for PMIP (Joussaume et al. . .); -Line 36: You state that "Previous studies proposed that these northern hemisphere ice sheets, especially the North American Laurentide ice sheet (LIS), are of crucial importance on modulating glacial climate": could you briefly precise its role on glacial climate? -Line 51 and 110 : Please avoid to use the term "plant macrofossils": the annual temperature is mainly based on pollen data through transfer function (sites with macrofossils data are very few for the LGM and its complex to provide robust quantitative temperature estimates from macrofossils alone). -Line 63: I don't think the ref Davis and Brewer (2009) is appropriate here, they don't talk about the bloom season; -Line 76: a ref is needed for the model COSMOS

- Material and methods - a table with the different LIS reconstructions will be welcome -line 111: you mention the different climate parameters reconstructed from pollen data in the Bartlein et al synthesis (MTWA, GDD5 MTCO and MAT). As you only use in your paper the annual temperature reconstruction, it's not necessary to mention the others climate parameters. In contrast, you can write few words on the method used (Mat, transfer function or inverse modelling). I strongly recommend to also compare the seasonal parameters (temperature of the coldest and warmest month) simulated by COSMOS with MTCO and MTWA inferred from pollen. -line 136: avoid words as Âń proxy-derived observational data Âż; the temperature is reconstructed from proxies, it's not observational data. Use instead proxy-inferred temperature; there is a lot of such approximations in the text, please correct it everywhere.

- Results: not appropriate as it is. I also strongly recommend to group the results and the discussion in the same part to avoid to be lost. -line 225: you state that In the North Atlantic Ocean, the best agreement of planktonic foraminifera, dinoflagellates, and alkenones is found for local summer. I don't agree with you, I don't see it on the figure. -line 259: how did you define the different layers? Arbitrary or statistical threshold? -line 264: why 37m? not 27 or 47as in the caption? -line 305: what do

you mean by "Instead, the ensemble median (Fig. 2a) typically displays the common signal. In this case, it is the mean value of the fourth and fifth ensemble member out of eight models which are ordered according to ranked values"? Really not clear!

- Discussion is too short, it must be clarified according to the objectives of the paper and the results (which also need to be more precise). A comparison between these results and previous LGM simulations is required and must be discussed. A comparison with the CLIMAP values and other studies will also be welcome. A more in depth discussion of the agreement between terrestrial pollen-based temperature and model output is also needed as I have in mind that usually the models underestimates the values inferred from the data as it was the case in the previous PIMP simulations. -in the 4.3 part, you discuss foraminifers, alkenones and MGCa ratio, but nothing is written about the dinos. I'm sure that a lot of papers are available. - Data model discrepancies can also been explained by the proxy itself or by the method (transfer function. . .); this point is important and need to be discussed - Line 410: You state that the proxy records used in most of the studies are more often located in coastal areas. I don't agree with you: Dino and forams records are not only located in coastal areas.

- Figures : the order of each figure must be carefully checked in the text. The colors of the figures 5 a and 7a must be changed for more clarity

I think that there is potential in this paper in terms of results; however a considerable effort of rewriting is needed to improve the manuscript before acceptation.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-9/cp-2018-9-RC2-supplement.pdf