

Second round of review of *'Using a multi-layer snow model for transient paleo studies: surface mass balance evolution during the Last Interglacial'* by Hoang et al.

I would like to thank the authors for the revised paper, which substantially improves on the original manuscript. I really appreciate the effort made in making it a more scientific paper with a widely extended analysis of the SMB evolution of the Greenland and Antarctic ice sheets over the last interglacial. Also, the methodology and the modelling framework and setup are much clearer now. I only have a few minor comments which should be addressed before the paper is acceptable for publication.

The text would still greatly benefit from a read by a native English speaker.

The line numbers in the comments below refer to the revised paper version with tracked changes.

Minor comments

L. 19-20: why would this improvement only be applicable to 'paleo periods'?

L. 108: 'in exponential relationship with' -> 'exponentially after'

L. 110: 'resulted' -> 'results'

Fig. 2: the caption should be extended to explain the different grids and what they represent

Equation 1: what are the units of m_{runoff} ?

L. 241: 'during' -> 'for'

L. 242: Specify that sublimation is ignored. Maybe also consider writing out the SMB equation explicitly again.

Table 1: Specify which years are considered in the present-day case. Specify that Paleo study data are from iLOVECLIM. Why is the mean summer temperature over Antarctica so different in iLOVECLIM for present-day and PI?

L. 292: 'is' -> 'are'

L- 296-297: To quantify the impact of biases, you need to run simulations both with and without bias correction, no?

L. 524: in terms of timing yes, but can anything be said about the magnitude?

L. 699: 'satisfied' -> 'satisfying'

L. 729: check this sentence, sounds weird

L. 729: Possibly add some information on how much of the computation time would be consumed by BESSi in a fully coupled iLOVECLIM with GRISLI setup. Would it really be the bottleneck?

L. 752-753: Do you mean stronger sensitivity to model biases? It seems to me that ITM is responding less to LIG forcings than BESSl.

L. 875: why is the range different for RH? It seems to contradict the values larger than one in Fig. C2. Also, in the text it is mentioned that humidity is strongly underestimated in iLOVECLIM, but from Fig. C1 and C2 it seems to be overestimated? Is it because in one case you are referring to specific and in the other to relative humidity?

Generally, a comment on the large SW radiation biases in iLOVECLIM would help to interpret the results. How can SW radiation be almost a factor 10 too low over the ice sheets in the model? I presume this cannot be solely explained by differences in surface albedo, but is somehow related to clouds...? Is the bias larger than a factor 10 (which is the maximum used in the bias correction procedure) during the LIG? Could this (partly) explain the underestimation of melt in the ITM compared to BESSl?