

Interactive comment on “Sea-ice feedbacks influence the isotopic signature of Greenland Ice Sheet elevation changes: Last Interglacial HadCM3 simulations” by Irene Malmierca-Vallet et al.

Anonymous Referee #1

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Review

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Sea-ice feedbacks influence the isotopic signature of Greenland Ice Sheet elevation changes: Last Interglacial HadCM3 simulations

This study investigates the changes of stable water isotopes in the precipitation over Greenland and their dependence on the height of the Greenland ice sheet. A set of climate simulations with the HadCM3 model is used for this purpose. The temporal

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focus of the work is the climate of the Last Interglacial. The simulation results indicate non-linear complex processes in the hydrological cycle, which lead to different delta 18O-height relationships for a past increase or decrease of the current Greenland ice sheet height. Further analyses show that differences in Arctic winter sea ice cover seems to be the main reason for a significant change in the delta 18O-height relationship.

The results of the analyses are presented in a comprehensible order and discussed in detail. The manuscript is clearly structured and very well written, and the methodological approaches are comprehensible and insightful. Overall, I can recommend a publication of this paper in CP without hesitation and suggest only minor corrections and additions to the text.

Comments/Questions:

Page 3, Line 7ff: What is the horizontal and vertical resolution of the HadCM3 simulation ensemble?

P3L21ff: The usage of the second simulation ensemble is less clearly described. No information of the change in GIS extent for the different simulations is found in the text or the appendix. It also remains unclear, how the different influence of GIS extent, GIS height and sea ice changes can be separated from this ensemble, as all three parameters are apparently changed at the same time.

P4L5ff: Some information about the overall (dis)agreement between modelled LIG-PI d18O anomalies as compared to ice core data should be added.

P4L5: A reference for the difference between NEEM drill site and deposition site should be given.

P4L9ff: References to some plots of Fig. 1 are wrong in this paragraph. E.g, NGRIP delta 18O changes are shown in Fig. 1e (not 1f), GISP2 values in Fig. 1m (not 1p), etc. Fig. 1z does not exist, at all.

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P5L2: Are the differences in temperature lapse rate (0.47C/100m to 0.44C/100m) statistically significant or within the model-intrinsic uncertainty?

P5L6: Figure 5 is discussed before Figure 3+4 have been mentioned. This figure order could be improved.

P5L32: Is there an explanation for the non-linear behavior of precipitation changes?

P6L4ff: At Camp Century, the same sign in precipitation changes for increased and decreased elevation is explained by winter sea ice conditions in the Baffin Bay. The changes show in Fig.3d, 3f are only subtle and it is hard to believe that these minor differences have any effect on precipitation formation. They will barely change the high mean winter sea ice concentration of the 125k control simulation shown in Fig. 3e.

P6L11: For a better assessment of the simulated PI mean sea ice extent (5.8 mil km²), it should be compared to observed/reconstructed values.

P6L31: The reference to Fig. C1 would be better placed after the next sentence ("Since the surface [...] center of basin.").

P7L8: The method of isolating the impacts of delta 18O due to sea ice variation should be explained in more detail. E.g., how is a potential effect of changed GIS extent treated in this analysis. And why is this analysis performed for winter sea ice retreat, only? Are summer SIC changes negligible?

P8L26ff: In this paragraph simulated temperature lapse rates are compared to observed mean lapse rates, but also to changes in temperature lapse rates for a warming climate. These two quantities (mean state and its temporal deviation) should not be mixed in this comparison.

P9L21ff: How large is the modelled modern spatially derived delta 18O-elevation gradient? How does it compare to the cited values of Dansgaard/Johnsen et al./Vinther et al.?

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Fig. 1: The last column of plots (Fig, 1d, 1h, etc.) lacks an explanation. Why is the plot of winter sea ice extent vs. GIS elevation changes different for the various ice core sites?

Figs. 2/3/5/6: Are all anomalies shown in the plots statistically significant? No non-significant values can be detected in these figures.

Fig. 3: The color bar values in the plots range from 0..1, but the figure caption states that sea ice concentration and its anomalies are given in percent (0..100%).

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