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Climate of the Past Discussions
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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 #2

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Malmierca-Vallet and colleagues present simulations with an isotope-enabled climate model for the last interglacial (LIG), approximately 125,000 years before present. They analyze and discuss two ensembles of sensitivity simulations with artificially changed topography over the Greenland ice sheet (GIS) and different sea ice perturbations. Based on these simulations, the elevation gradients of the precipitation-weighted oxygen isotopic ratio d18Op are determined, quantified, and some feedbacks and nonlinearities are discussed.

The manuscript is written well and the figures adequately support the text. The topic and findings are a very good match for Climate of the Past and the underlying questions are highly relevant for the interpretation of paleo proxy records. There are, however, a number of shortcomings that have to be addressed before publication. My most important criticism concerns the incomplete and sometimes confusing analysis of the simulations and, related to that, the incomplete discussion of the results as they compare to existing literature. A technical issue that needs to be addressed is that only insufficient details are included about the second ensemble of simulations. Most of the necessary information appears to be included in a paper under review (Domingo et al.). The full information on how this ensemble was constructed should be available to the readers of the present manuscript.

I was confused over how the two ensembles were used in the analysis. Ensemble 1 comprises 16 simulations where the only direct perturbation is the GIS topography. Other anomalies, including sea ice thickness and extent, are consequences thereof, mediated by the coupled model. Ensemble 2 perturbs the sea ice directly. However, since in the coupled model this is mostly a quantitative difference, not a qualitative one both ensembles could in principle be analyzed simultaneously throughout the paper and maybe this means that they should. For example, figure 7 shows results from all 48 simulations, but figure 1 only of the first ensemble.

Related to this, I believe the analysis falls short of answering the key question about which one of the perturbations, sea ice or elevation, has the stronger impact. Are there regional differences in the relative importance? Given the limited knowledge we have about sea ice and elevation anomalies during the LIG, how do the typical ranges of uncertainty in both variables translate into an unaccounted part of the reconstructed d18Op signal in ice cores? Are there regions that have a particularly large impact on d18Op and from which improved reconstructions would be especially insightful? I think the simulations presented here have the potential to make good progress on these important questions and answering some of them would greatly increase the interest and impact of this manuscript. I do not expect all of them answered, but a revised manuscript should at least clarify the relative importance of sea ice and elevation perturbations more clearly.

The manuscript would greatly benefit from a more careful discussion of the existing literature on the subject, in particular the three papers by Niklaus Merz from 2014 and 2016. The provide a very detailed analysis of how elevation changes impact temperature over the GIS (2014a), precipitation (2014b), and on the role of sea ice (2016). All three are referenced in Malmierca-Vallet et al. (2018), but only Merz et al. (2014b) here. The dynamical consequences of an altered GIS topography are discussed in detail in Merz et al. (2014a) and I think that several of the findings apply here, too. For example, the strengthening of the Greenland anticyclone with higher elevations (Fig. 4) and the peripheral warming (Fig. 2; p8 l12ff) were found there too and turbulent heat fluxes were the cause. This earlier paper also discusses changes in the seasonal temperature cycle with some surprising details such as a marked cooling in regions of lower topography in winter. I suspect that these effects are important for d18O and should therefore be discussed here. The dynamic explanation that is currently given in the manuscript is based on studies of glacial climate and how the Laurentide ice sheet interacts with the atmosphere. The main effect described in these studies is how the ice sheet barrier affects the jet stream and therefore the storm tracks. The Greenland ice sheet is too far north to affect the storm tracks and hence the dynamics are very different. Please remove these references as they are misleading (Felzer et al., 1996; Singarayer and Valdes, 2010; Pausata et al., 2011). The (local) barrier effect is however very important for precipitation (e.g., p8 l6ff), as previously shown by Langen et al. (2012), Hakuba et al (2012), and Merz et al. (2014b).

Lastly, HadCM3 is a model with a relatively low resolution, which is known to negatively impact the circulation and surface climate over Greenland (Vizcaino et al, 2014). A discussion of how this impacts the results should be included in the revised manuscript. It is clear that the main advance of the present study is the inclusion of oxygen isotopes, but the correct simulation of d18O depends on a good representation of the physical

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climate system.

References: Langen et al. (2012): Self-inhibiting growth of the Greenland Ice, GRL 39, L12502, doi:10.1029/2012GL051810 Hakuba et al. (2012): Impact of Greenland's topographic height on precipitation and snow accumulation in idealized simulations, JOURNAL OF GEOPHYSICAL RESEARCH 117, D09107, doi:10.1029/2011JD017052, 2012 Vizcaino (2014): Ice sheets as interactive components of Earth System Models: progress and challenges, WIREs Clim Change 5, 557–568. doi: 10.1002/wcc.285 Irvali et al. (2020): A low climate threshold for south Greenland Ice Sheet demise during the late Pleistocene, PNAS 117, 190-195, doi: 10.1073/pnas.1911902116

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Minor comments:

Reference list: The URL for Malmierca-Vallet et al., 2018 is incorrect. It contains "/doi.org" twice.

Methods: The idealized scaling for ensemble 1 strikes me as an odd choice and the naming convention is confusing. If I understood it correctly, the elevation of the entire ice sheet is scaled by a percentage. The naming is derived from the absolute anomaly in elevation at NEEM, which is not representative. Why not use the percentage as the name? Also, please include an explanation why this approach was chosen instead of the (arguably) more physical of Domingo et al. or Merz et al. (2014a,b).

p1 l20: Maybe Irvali et al. (PNAS, 2020) is a valuable addition?

p3 l4: prefer to use references to original work, not only IPCC AR4/5.

p3l25: A paper in review is not sufficient as a reference for a key method.

p4 l5ff: Since the increases and decreases in elevation are artificial, to what degree are the simulated d18O anomalies applicable to real-world reconstuctions? The

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precipitation-weighted d18O will greatly depend on precipitation and therefore barrier effects (see above).

figure 1: Why use a piecewise linear fit? Nonlinearity is referred to in the text.

figure 2: Is there any sign of inversion in winter or other local temperature effects like in Merz et al.? How would this impact d18Op?

figure 4: Why not show anomalies like in all other figures?

p5 l10: typo: aN increase

p5 l15: This part would benefit from looking at the findings of Hakuba et al. (2012) and Merz et al. (2014a). Discuss why local dynamic and temperature changes are not seen in HadCM3 as compared to CCSM4.

p6 l6: "likely linked to the reduced winter sea ice" Isn't the 2nd ensemble there to test this hypothesis?

p6 l21: Pausata reference is on glacial climate and a change in elevation of the Laurentide ice sheet. Not relevant here.

p7 l8: "to isolate the effect of sea ice" How? Greenland topography changes here too.

figure 7: To my eye, sea ice correction does not really improve the fit. Overall, the "GIS" ensemble does not show a clear correlation between d18Op and ice core elevation. The cloud of points are rather round. Does the fit stem from all simulations or only the 1st ensemble? What would a fit (and std.dev.) for only the GIS ensemble look like? Is the sea ice correction straightforward? Does it not relate to the rather patchy (Fig. 6) precip anomalies? Is it not important in which region the sea ice reduction takes place?

p7 l24: I might have missed the definition, but I do not understand what exactly "core-average" means.

p7 l25: changes in gradient are described, followed by an argument that sea ice influences the linearity of the d18Op-elevation relationship. Gradients only describe the linear trend, so I am very confused over what this statement means. I think a non-linear metric, e.g., the curvature or similar, is needed.

p7 l29: "The dependence of the δ 18 O variable on elevation variations occurs in response to variations in winter sea ice extent." This can be interpreted as if the authors want to claim that elevation changes are a result of sea ice anomalies, and that the latter are the ultimate reason for isotope changes. This touches on an important point: The relative importance of sea ice and elevation changes and feedbacks between the two effects, that should be discussed in much more details. I think the two ensembles are well suited for such an analysis.

p8 l22: The combined changes discussed above are on the GIS ensemble, but figure 1 is on the elevation ensemble only. I think this is an error in the methodology.

section 4.3: This section compares modelled and observed isotopic lapse rates. It seems to me that the modelled lapse rates are derived from the first ensemble, i.e., it describes a change in d18Op as a function of the lower elevation at the same elevation, while the observed gradients have a spatial component, and thus a lower elevation generally implies a closer proximity to the coast. These should not be mixed or the difference must at least be clearly stated.

There is an excessive use of appendix figures for a journal that does not have very strict length restrictions.

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