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Climate of the Past Discussions

Interactive comment on "The dependency of the δ^{18}O discrepancy between ice cores and model simulations on the spatial model resolution" *by* Marcus Breil et al.

Anonymous Referee #1

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This manuscript presents first outputs of the COSMO-iso model for the Arctic regions over the present-day and mid-Holocene. The results are compared to measurements performed in snow and ice cores and the agreement is rather good, better than with a GCM, between model and data hence validating the use of a RCM equipped with isotopes to look at fine spatial scale the variability of water isotopes in this region.

Even if I am not very enthusiastic with this manuscript, this is a valuable contribution but I feel that the study could be developed a bit more following the comments given below. In general, I am a bit disappointed by the manuscript compared to the previous study on the same subject, Sjolte et al., 2011. This previous study using a regional model

with isotopes presented numerous applications especially on the temporal variability, an aspect which is fully absent here. Could perhaps the authors elaborate a bit more on the temporal variability (seasonal and interannual variability) and compare to available data or to this previous study ?

I understand that the authors like to focus their study on the mid-Holocene but it is not clear why. Also, the difference between mid-holocene and PST is not very large so that the comparison between the two periods is not the best to validate the temporal variability of the model. It is also complicated to perform such a comparison because COSMO-iso is associated with ECHAM-5 wiso for present-day and MPI-ESM-wiso for the mid Holocene. Without a comparison between ECHAM5-wiso and MPI-ESM-wiso which is not discussed here, it is quite complicated to perform comparison between mid-Holocene and PST. Was it really impossible to use the same GCM for both simulations ?

I am quite worried that the present study is submitted while the evaluation of the COSMO model (without isotopes) is not performed (cf sentences 66-67). Why then compared d18O values to observations if we have no validation of basic climatic parameters (temperature, etc. . .). At least some sentences for the most relevant parameters should be included here. I am quite surprised by the paragraph on fractionation at snow covered surfaces. For the work on the Arctic, you have a large number of paper co-authored by Hans Christian Steen Larsen which discuss the isotopic equilibrium or disequilibrium between surface snow, precipitation and water vapor in Greenland. It is quite strange to use a dataseries from Karlsruhe to calibrate fractionation between snow and water vapor in Greenland when data are available there.

Similarly, I am surprised that you do not have more observations gathered in part 2.2. Why only concentrating on core top while you have some series of observations (Bonne et al., ACP, 2014; papers co-authored by Steen-Larsen). You may also want to include the core studied by Furukawa et al., JGR, 2017).

I am not so convinced by figure 4b and the associated discussion stating that the bias are very small. First, the scale is much to large, it would be enough to draw the y-axis between -2 and +2 permil. And then, you obtain opposite variations between the red (model, negative d18O anomaly) and orange (observation, positive d18O anomaly) so that the comparison of the results is actually not convincing even if the changes are small in both cases but this is expected sine Mid Holocene is not very different from PI. I see this point as a strong weakness.

It would have been nice to discuss the temporal d18O vs Temperature gradient and not only the local spatial one. Also, we are awaiting some discussions / perspectives on the implications of these calculated spatial gradients for ice core interpretation. It would be nice to elaborate on this.

Other comments to consider: - I do not understand the following sentence in the abstract: "Furthermore, by investigating the δ18O ratios in all COSMO_iso grid boxes located within the corresponding ECHAM5-wiso grid box, the observed isotopic ratios can be classified as a possible local δ18O ratio within the spatial uncertainties, derived by the regional downscaling approach."

- This sentence in the abstract is not very concrete "But again, the range of the COSMO_iso_50km δ18O variability in the corresponding MPI-ESM-wiso grid boxes around each station is consistent with the observed δ18O values"

- I am surprised in the introduction by the discussion about mid-holocene. In Greenland, the temperature better seems on a plateau between the beginning of the Holocene (optimum) and the mid-Holocene.

- L. 46: why do you discuss the ability of a GCM to reproduce the regional changes – why not discuss better the (dis)ability of a GCM equipped with isotopes to reproduce the regional changes of water isotopic composition.

- Table 1: Please correct the date for the reference of Weissbach et al., 2016. . . ; also

give the units for d18O

- It is very difficult to compare data and measurements on figure 1

- How is the yearly mean d18O value calculated ? Is there any weighting by the precipitation amount ? Could this effect be discussed when compared to the observations ?

- L. 289: I do not understand this sentence "At the coastline, the δ18Otemperature

- gradient is low, reflecting the high surface temperature and δ18O variability in this region" – in general the whole paragraph needs to be rewritten since it is largely unclear (last sentence of the paragraph is particularly vague -> to what mechanisms do you refer ?)

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