

## **Review: The dependency of the $\delta^{18}\text{O}$ discrepancy between ice cores and model simulations on the spatial model resolution**

Marcus Breil, Emanuel Christner, Alexandre Cauquoin, Martin Werner, Gerd Schädler

### **General Comments**

I feel the authors have addressed some of the concerns from both reviewers, but I still have some concerns about the manuscript. In particular, although some additional quantitative evidence is now provided, I feel the authors have not supported their claims sufficiently with quantitative evidence, and additional quantitative analysis should be performed before the manuscript is published. If the authors find that the analysis does not support their claims, they should modify their conclusions to reflect this analysis. I think these changes will mostly be minor but since they have the potential to change the conclusions of the study I have selected major revisions. Some remaining general comments are:

In **Section 3.1.2** the authors have computed the average reduction in bias for the RCM simulation. But it is still unclear how much of an improvement this represents. Is this a statistically significant difference? Has the root mean squared error also been reduced in the RCM simulation. I feel some further analysis is necessary here. Additional comments are provided in the specific comments below.

**Section 3.1.3** still requires improvement. I feel additional statistical analysis is necessary and I feel the authors' arguments at the end of the section need to be revised. (These points are also relevant to **Section 3.2.2**.) In particular:

- (1) There is no assessment of the goodness of fit for the regression of  $\delta^{18}\text{O}$  against temperature for the analysis shown in Figures 7 (c) and (d). I feel this information is perhaps more important to the authors' arguments about the degree to which  $\delta^{18}\text{O}$  and temperature are linked than the slope. An analysis of the coefficient of determination ( $R^2$  value) for  $\delta^{18}\text{O}$  vs. temperature would be more appropriate in supporting the authors' points in this section.
- (2) The lower slope values for the temporal analysis shown in Figure 7 (d) are consistent with the lower coastal values shown in Figure 7(c), in that the seasonal analysis includes a larger range of variability in the high elevation areas. Thus, a higher degree of temporal variability produces a lower slope.
- (3) I think the interpretation of the data here needs to be revised. If the authors look more carefully at the correlation between  $\delta^{18}\text{O}$  and temperature, they may find that temperature explains most of the variability in  $\delta^{18}\text{O}$ , while other processes explain additional variability. The high  $\delta^{18}\text{O}$  vs. temperature slope at higher elevations may be indicative that in these locations, temperature variability plays a lesser role than other factors, but here the degree of  $\delta^{18}\text{O}$  variability is small. Therefore, overall, temperature still may

be the dominant factor in  $\delta^{18}\text{O}$  variability. The authors should investigate this more carefully through a correlation analysis.

In **Section 3.2.1**, I am a bit concerned about the comparison shown in Figure 8(b) as the anomalies for the mid-Holocene relative to the pre-industrial are smaller than the difference between model and observations and the range of sub-grid-scale variability. I suppose the degree of temporal variability may be different from the degree of spatial variability. I would suggest performing a statistical test (e.g. t-test) to determine whether the anomaly is statistically different from 0 in both the case of observation and model results. This could be performed considering the interannual variability in both the Mid-Holocene and Pre-Industrial for both the model and observations.

### Specific Comments

1. **Lines 10-11:** Suggest revising to “For this purpose, isotope-enabled simulations with the ECHAM5-wiso General Circulation Model (GCM) under present-day and the MPI-ESM-wiso GCM under mid-Holocene...” for clarity.
2. **Line 14:** Define GNIP.
3. **Lines 16-19:** Note here the spatial resolution of the GCM simulation (roughly). Again, provide some numbers to quantify the improvement in the agreement, and to support the statement.
4. **Lines 17-18:** This is a bit misleading. It should be noted that the 7 km simulation does not yield a substantial improvement overall, except in one area with complex terrain.
5. **Lines 23-25:** The statement “The correct  $\delta^{18}\text{O}$  ratios are consequently already included but not resolved in the GCM simulations results...” is misleading. The correct values are not “included” in the GCM simulation. Rather the conclusion is that the discrepancies between the point measurements and GCM values are likely due sub-grid-scale variability not captured by the GCM. Suggest revising this to read: “ $\delta^{18}\text{O}$  ratios are consequently not resolved in the GCM simulations .....
6. **Lines 97-99:** I think the authors should provide some documentation of the effect of this albedo change. Perhaps they can briefly document the improvements (through a set of tables for example) in a supplementary section or appendix.
7. **Line 139:** Can the authors briefly comment in the text on the choice of model domains? Why run the large domain RCM simulation over the Arctic and not simply run the RCM at high resolution over Greenland?
8. **Lines 166-168:** It is still unclear what the percentage of missing values is between 1940 and 2014 at each station and how this might affect the results. It would be helpful if the authors could estimate the uncertainty in the average value, which would place the model simulation in context.
9. **Line 174:** When the authors say the “observed isotope ratios are compared with simulated yearly mean  $\delta^{18}\text{O}$  values in precipitation”, it sounds as if the

annual modeled values are being compared to observed annual values, whereas only interannual averages are compared. Suggest changing “the observed isotope ratios...” to read “we compute modeled annual mean  $\delta^{18}\text{O}$  values and compared the multi-year 2008-2014 model mean to the observed values.”

- 10.Line 175:** Change “calculation of this yearly mean” to “calculation of the yearly modeled mean” for clarity.
- 11.Table 2 caption:** Suggest revising to “...blue numbers in parentheses indicate mid-Holocene values.” for clarity.
- 12.Figure 2 caption:** Note that the solid black line is the 1:1 line in (a) and (b).
- 13.Lines 214-219:** It is not clear how much of an improvement the RCM provides. I suggest providing further details, for example what is the bias for the RCM and what is the bias for the GCM? What is the bias at the stations with poor agreement with the GCM and what is the bias with the RCM, and the same for the stations with a good agreement for the GCM initially? Can the authors evaluate whether the change in the bias is statistically significant? Also, the authors could compute the root mean squared error for the RCM and GCM simulations. From this information the reader can better understand the degree of improvement associated with the RCM.
- 14.Line 233:** If the authors can provide a bit more evidence that there is a significant improvement in agreement with observations when employing the RCM (as noted above), the authors could reiterate here at the end of the paragraph that despite the increased bias in northern Greenland, there is an overall improvement associated with the RCM simulation.
- 15.Lines 283-285:** This may be true at locations 11-13, where the bias is larger in COSMO\_iso than in ECHAM5-wiso, but at locations 9 and 10, COSMO\_iso is not very different from ECHAM5-wiso. Suggest revising to make clear that the southward shift explains part but not all of the differences; e.g. “this is likely partially associated with the southward shift...”
- 16.Line 287:** Suggest changing “not covered within” to “fall outside of the range of”.
- 17.Line 288:** “increase the accuracy” is unclear. Suggest changing to read “further downscaling... does not substantially change the simulated isotopic ratio spread...”
- 18.Line 313:** Remove “the increase in” before “the spatial isotopic ratio variability”.
- 19.Line 317:** Suggest changing to “In central Greenland, surface temperature variability is very low (Figure 7b).”
- 20.Line 321:** There is not increased  $\delta^{18}\text{O}$  variability in central Greenland. It is lower than along the coast but is relatively high compared with the temperature variability. Please revise.
- 21. Line 328-330:** The lower slope does not necessarily imply a poor correlation. Despite the higher slope in the spatial analysis, the correlation could be lower in these locations, while the correlation might actually be higher in the case of a lower slope, given the larger range of variability. I

suggest computing the  $R^2$  value for the linear regression for all grid cells as this will provide an indication of the degree of correlation. It is not clear whether the final statement that interannual temperature variations have a small impact on  $\delta^{18}\text{O}$  variability is correct. To the contrary, temperature may be found to be the dominant factor in  $\delta^{18}\text{O}$  variability both spatially and temporally if a more complete analysis is conducted.

- 22. Lines 339-340:** Suggest revising to read “simulated mid-Holocene  $\delta^{18}\text{O}$  ratios with comparison to observed mid-Holocene  $\delta^{18}\text{O}$  values.”
- 23. Lines 348-349:** Suggest changing to: “For COSMO\_iso\_50km, the deviation of  $\delta^{18}\text{O}$  values relative to observations are opposite in sign compared with MPI-ESM-wiso at all locations except Renland.”
- 24. Lines 352-354:** Suggest revising to “However, when the spatial isotopic ratio variability within MPI-ESM-wiso grid cells simulated by COSMO\_iso\_50km isotopic ratios is taken into account, the model results are in agreement with the isotopic ratios of the ice core samples.”
- 25. Line 371:** Please quantify “very small”.
- 26. Line 377:** Add “for the mid-Holocene” after “COSMO\_iso\_50km simulation” for clarity.
- 27. Line 389-390:** Again, this conclusion is problematic because the authors are examining the slope, but not considering the correlation between  $\delta^{18}\text{O}$  and temperature.
- 28. Line 399:** These results are interesting, but I’m not sure they are so remarkable, given that temperature is expected to vary with elevation, and  $\delta^{18}\text{O}$  seems to follow a similar pattern, being somewhat temperature dependent. They do point to a strong local influence on the spatial variability in  $\delta^{18}\text{O}$ .
- 29. Line 403:** This seems a bit exaggerated. Clearly there is some difference with respect to the present-day simulation, and therefore the results are not entirely independent of the boundary conditions. I would suggest revising to read “...not strongly dependent on the oceanic boundary conditions.”
- 30. Line 415:** Suggest changing “already leads to” to “produces”.
- 31. Line 417:** Again define “considerably reduced” by providing some quantification. This is also not always the case as the results show.
- 32. Lines 418-419:** I think the authors should note here the lack of improvement when increasing to 7 km.
- 33. Line 424:** Not sure what is meant by “as it was simulated by”. Possibly change to “as was the case in a similar study by Sjolte et al. (2011)”?
- 34. Lines 452-456:** I agree with this statement, but it seems to contradict the authors’ previous statements that there is not a strong relationship between temporal variations in temperature and  $\delta^{18}\text{O}$ , which was suggested based on the low  $\delta^{18}\text{O}$ -temperature slope. As discussed above, the authors should examine the correlation between temperature and  $\delta^{18}\text{O}$  in order to determine the strength of that relationship, as well as to confirm the spatial relationships discussed here.
- 35. Line 458-459:** Again, this contradicts the previous statement.

## Technical Corrections

1. **Line 46:** Change “warming, in more detail” to “warming in more detail”.
2. **Line 60:** Suggest changing to read “not able to quantitatively reproduce regional changes in isotope ratios”
3. **Line 63:** Change “ratios in precipitation, by a regional” to “ratios in precipitation through a regional”.
4. **Line 66:** Change “presented study” to “present study”.
5. **Line 81:** Change “Holocene conditions, is performed” to “Holocene conditions is performed”.
6. **Line 119:** Remove “have been suggested” before “(Gurney and Lawrence, 2004)”.
7. **Line 121:** Change “just a simplified” to “a simplified”.
8. **Line 173:** Change “Since both, snow pit” to “Since both snow pit...”
9. **Line 191:** Change “parameter” to “parameters”.
10. **Line 196:** Change “Both, simulated” to “Both simulated”
11. **Line 210:** Change “is able to reflect” to “is able to reproduce”.
12. **Lines 269-270:** This sentence could be worded more clearly. Suggest revising to: “Despite the lack of improvement in the point to grid-cell comparison, higher resolved RCM simulations allow the subgrid-scale variability of  $\delta^{18}\text{O}$  within GCM grid boxes to be simulated and compared to observed  $\delta^{18}\text{O}$  values.”
13. **Lines 271-272:** Suggest revising to read: “Thus, in the following sections, snow pit samples are no longer solely compared...”
14. **Line 300:** Change “how strong” to “how strongly”
15. **Line 324:** Remove comma after “air mass”. Change “increase there the isotopic variability” to “increase the isotopic variability there.”
16. **Line 328:** Change “in accordance” to “in agreement”.
17. **Line 330:** Change “lowly correlated” to “poorly correlated”.
18. **Line 334:** Suggest revising “COSMO\_iso\_50km is not anymore...” to “COSMO\_iso\_50km is driven by MPI-ESM-wiso rather than COSMO\_iso\_50km.”
19. **Line 341:** Change “differences of” to “differences between”.
20. **Line 342:** Change “to the observed” to “and the observed”.
21. **Line 345:** Change “deviates only about 1 ‰ to the observations” to “deviates only by about 1 ‰ relative to the observation”.
22. **Line 358:** Change to “ the observed ratios derived from ice cores are subtracted from the simulated  $\delta^{18}\text{O}$  ratios.”
23. **Line 361:** Change “differences to” to “differences with respect to”. Change “anomalies of the MPI-ESM-wiso simulation to the pre-industrial” to “anomalies of the MPI-ESM-wiso simulation relative to pre-industrial”

- 24.Line 362-363:** Change to “shown in red dots” to “shown as red points.” For clarity, change “the observed mid-Holocene-PI” to “the observed anomalies for the mid-Holocene relative to present-day are shown as orange points.”
- 25.Figure 8 (b):** I would suggest changing the title to “Mid-Holocene anomalies (relative to PI)”, and changing the caption to “MPI-ESM-wiso” and “observed”. I would also suggest changing one set of points to be a different style to make the figure more easily readable.
- 26.Line 368:** Add a comma after “especially during the summer”.
- 27.Line 370:** Add “and” after “slightly underestimated,”
- 28.Line 377:** Change “for whole Greenland” to “for all of Greenland”.
- 29.Line 381:** Add “and” before “the GRIP and GISP2”.
- 30.Line 406:** I think this should read “The locations of the ice core samples are shown in green.”
- 31.Line 410:** Change “deviations to” to “deviations from”.
- 32.Line 425:** Add a comma after “rather”.
- 33.Line 426:** Remove “But” before “all in all”
- 34.Line 431:** Change “with even an” to “and even an”
- 35.Line 435:** Remove “Now,” before “by analysing”.
- 36.Line 438:** Remove comma after “applies for both”.
- 37.Line 442:** Change “to reproduce” to “in reproducing”.
- 38.Lines 448-449:** Change “spatial variability pattern of” to “patterns of spatial variability in”.
- 39.Line 450:** Change “variability patters” to “patterns of variability”.
- 40.Line 457:** Change “structures” to “patterns”?
- 41.Line 466:** Change “and their deviations to” to “and understanding their deviations from”.
- 42.Line 467:** Remove comma after “regions”.