Review: The dependency of the d18O discrepancy between ice cores and model simulations on the spatial model resolution

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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.
- Dear Reviewer. Thank you very much for your constructive and very helpful comments. We think that you addressed some important issues. We hope that we are able to respond satisfactorily to your comments and clear the open issues you raised.

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.
- the root mean squared error for COSMO_50km is reduced by 0.98 ‰, for COSMO_7km by 0.83 ‰. These improvements are statistically significant at the 95% level. This is now mentioned in the text (line 232-238):

"In COSMO_iso_50km the root mean squared error (RMSE) is reduced by 0.98 ‰ over all snow pit samples (the RMSE of ECHAM5-wiso is 2.42 ‰ and the RMSE of COSMO_iso_50km is 1.44 ‰). This RMSE reduction is significant at the 95 % level, assessed by performing a t-test. Especially for the snow pit samples for which ECHAM5-wiso exhibits strong deviations from the observed $\delta^{18}O$ values (1,3,4,6,7,8,9,10,16,17,19; see Table 2), a regional downscaling with COSMO_iso_50km reduces the bias considerably (Figure 4). For these stations, the RMSE of ECHAM5-wiso of 3.09 ‰ is reduced by 1.65 ‰ to 1.44 ‰. But for snow pit samples at which ECHAM5-wiso has already a high agreement with the observations (2,5,11,13,14), COSMO_iso_50km increases the RMSE from 0.34 ‰ to 1.51 ‰."

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 d18O 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 d18O and temperature are linked than the slope. An analysis of the coefficient of determination (R2value) for d18O vs. temperature would be more appropriate in supporting the authors' points in this section.
- we calculated the temporal (left) as well as the spatial (right) correlations (R^2 value) for d18O vs. temperature. The results are shown in the figure below and the figure is additionally included in Supplementary Material (S1).

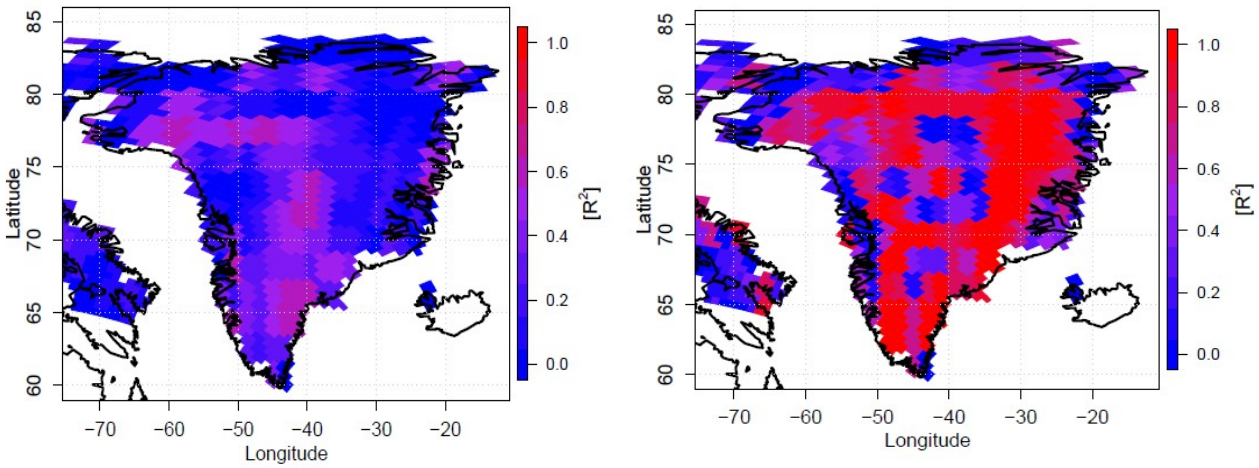

Figure: temporal (left) and spatial (right) correlations (R^2 value) for d18O vs. temperature for the COSMO_iso_50km present-day simulation.

(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.

- you are right, a comparatively low (or better moderate) temporal d18O-temperature slope could also be associated with a large variability for both, d18O and temperature and thus a high correlation, as seen for the spatial slopes in coastal areas (figure 7c and figure above (right)). But the temporal slope shown in figure 7d is very low, while the spatial slope is moderate at the coastline (this is now explicitly mentioned in the manuscript). This fact already indicates that the temperature variability is larger than the d18O variability and that the correlation is reduced. This is also demonstrated by the analysis of the R^2 values in the figure above (left). But with respect to your detailed explanations, the text is adapted as follows (line 352-357):

"That means that the interannual $\delta^{18}O$ variability is less pronounced than the interannual surface temperature variability and thus, also the correlation between both quantities is not as strong as for the spatial $\delta^{18}O$ variability and the spatial temperature variability (Figure S1b in Supplementary Material). The mean spatial correlation over Greenland is 0.61 compared to 0.25 for the mean temporal correlation. The impact of interannual surface temperature variations on the temporal $\delta^{18}O$ variability in Greenland is therefore not as dominant as for the spatial $\delta^{18}O$ variability."

(3)I think the interpretation of the data here needs to be revised. If the authors look more carefully at the correlation between d18O and temperature, they may find that temperature explains most of the variability in d18O, while other processes explain additional variability. The high d18O 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 d18O variability is small. Therefore, overall, temperature still may be the dominant factor in d18O variability. The authors should investigate this more carefully through a correlation analysis.

- in general, we agree. Temperature could still be the dominant factor for d18O variability. This is partly shown in the analysis of the spatial R^2 values (figure above (right)). The correlation is highest at the coast and less pronounced in Central Greenland, but still on a moderate level. Therefore, the respective statement in the manuscript is adapted as follows (line 341-348):

"The moderate spatial $\delta^{18}O$-temperature slope at the coastline (Figure 7c) is therefore a result of a high surface temperature variability in this region counteracting the high $\delta^{18}O$ variability in the slope calculation. There, the correlation between both quantities is consequently high (Figure S1a in

Supplementary Material, mainly between 0.7 and 0.99). In Central Greenland, the spatial $\delta^{18}O$-temperature slope is further increased due to the relatively high $\delta^{18}O$ variability compared with the surface temperature variability. Therefore, the spatial distribution of $\delta^{18}O$ cannot be solely explained by land surface processes and the associated spatial temperature variability. The additional spatial $\delta^{18}O$ variability consequently must be caused by dynamic atmospheric processes. In this way, isotopic ratios based on atmospheric fractionation processes along the trajectory of an air mass are transported to Central Greenland and increase the isotopic variability there."

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.
- according to your suggestions, we performed a t-test on the anomalies of the Mid-Holocene conditions relative to the pre-industrial conditions. The results of this statistical test showed that the anomalies are not significant at the 95 % level. This is now mentioned in the text (line 397-399):

"But overall, the biases of the MPI-ESM-wiso mid-Holocene-PI model anomalies to the observed mid-Holocene-PI anomalies are for all ice cores small and statistically not significant at the 95 % level (assessed by performing a t-test)."

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.
- is changed according to your suggestion.

2.Line 14: Define GNIP.
- is now defined

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.
- is now noted.

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.
- is changed according to your suggestion.

5.Lines 23-25: The statement "The correct d18O 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: "d18O ratios are consequently not resolved in the GCM simulations .....
- is changed according to your suggestion.

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.

- a detailed documentation about the impact of the snow albedo change is provided by Karremann & Schädler (2021). This is now mentioned in the manuscript.

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?
- in order to guarantee at the boundaries of the regional model domain a physically consistent transition between the coarse model resolution of the GCM and the fine model resolution of the RCM, the model resolution is step-wise increased. This procedure is called nesting. This is now mentioned in the manuscript (line 131-137):

"In order to guarantee at the boundaries of the regional model domain a physically consistent transition between the coarse model resolution of the GCM and the fine model resolution of the RCM, the model resolution is step-wise increased. This procedure is called nesting. In a first nesting step, the spatial resolution of COSMO_iso is set to 0.44° x 0.44°, corresponding to 50 km x 50 km in rotated coordinates (COSMO_iso_50km). In a second nesting step, an additional COSMO_iso simulation with a spatial resolution of 0.0625° x 0.0625° (corresponding to about 7 km × 7 km) for Greenland (COSMO_iso_7km) is nested in the COSMO_iso_50km simulation."

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.
- The standard deviations of the observed yearly d18O values at the snow pit samples is now included in table 2.

9.Line 174: When the authors say the "observed isotope ratios are compared with simulated yearly mean d18O 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 d18O values and compared the multi-year 2008-2014 model mean to the observed values."
- is changed according to your suggestion.

10.Line 175:Change "calculation of this yearly mean" to "calculation of the yearly modeled mean" for clarity.
- is changed according to your suggestion.

11.Table 2 caption: Suggest revising to "...blue numbers in parentheses indicate mid-Holocene values." for clarity.
- is changed according to your suggestion.

12.Figure 2caption: Note that the solid black line is the 1:1 line in (a) and (b).
- is changed according to your suggestion.

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.
- we agree, we should have provided further statistical details. This is now done in the revised manuscript (line 232-238):

"In COSMO_iso_50km the root mean squared error (RMSE) is reduced by 0.98 ‰ over all snow pit samples (the RMSE of ECHAM5-wiso is 2.42 ‰ and the RMSE of COSMO_iso_50km is 1.44 ‰). This RMSE reduction is significant at the 95 % level, assessed by performing a t-test. Especially for the snow pit samples for which ECHAM5-wiso exhibits strong deviations from the observed $\delta^{18}O$ values (1,3,4,6,7,8,9,10,16,17,19; see Table 2), a regional downscaling with COSMO_iso_50km reduces the bias considerably (Figure 4). For these stations, the RMSE of ECHAM5-wiso of 3.09 ‰ is reduced by 1.65 ‰ to 1.44 ‰. But for snow pit samples at which ECHAM5-wiso has already a high agreement with the observations (2,5,11,13,14), COSMO_iso_50km increases the RMSE from 0.34 ‰ to 1.51 ‰."

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.
- the improvements of the regional climate simulations are statistically significant (see comment above and the response to the major comment regarding Section 3.1.2). In the revised manuscript we mention now the overall improvements of the RCM simulation (line 252-253):

"But in all, COSMO_iso_50km yields an overall improvement in simulating the yearly mean $\delta^{18}O$ values compared to ECHAM5-wiso"

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..."
- is changed according to your suggestion.

16.Line 287: Suggest changing "not covered within" to "fall outside of the range of".
- is changed according to your suggestion.

17.Line 288: "increase the accuracy" is unclear. Suggest changing to read "further downscaling... does not substantially change the simulated isotopic ratio spread..."
- is changed according to your suggestion.

18.Line313: Remove "the increase in" before "the spatial isotopic ratio variability".
- is changed according to your suggestion.

19.Line 317: Suggest changing to "In central Greenland, surface temperature variability is very low (Figure 7b)."
- is changed according to your suggestion.

20.Line 321: There is not increased d18O variability in central Greenland. It is lower than along the coast but is relatively high compared with the temperature variability. Please revise.
- is revised as follows (line 344-345):

"In Central Greenland, the spatial $\delta^{18}O$-temperature slope is further increased due to the relatively high $\delta^{18}O$ variability compared with the surface temperature variability."

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 R2 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 d18O variability is correct. To the contrary, temperature may be found to be the dominant factor in d18O variability both spatially and temporally if a more complete analysis is conducted.
- again we agree with you that the interrelation between d18O and temperature variability is not necessarily as described in the manuscript (see response to major comments regarding section 3.1.3). But for the temporal correlations only low R^2 values between d18O and temperature are calculated. But with respect to your detailed explanations, we adapted the statement as follows (line 352-357):

"That means that the interannual $\delta^{18}O$ variability is less pronounced than the interannual surface temperature variability and thus, also the correlation between both quantities is not as strong as for the spatial $\delta^{18}O$ variability and the spatial temperature variability. The impact of interannual surface temperature variations on the temporal $\delta^{18}O$ variability in Greenland is therefore not as dominant as for the spatial $\delta^{18}O$ variability."

22.Lines 339-340: Suggest revising to read "simulated mid-Holocene d18O ratios with comparison to observed mid-Holocene d18O values."
- is changed according to your suggestion.

23.Lines 348-349: Suggest changing to: "For COSMO_iso_50km, the deviation of d18O values relative to observations are opposite in sign compared with MPI-ESM-wiso at all locations except Renland."
- is changed according to your suggestion.

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."
- is changed according to your suggestion.

25.Line 371: Please quantify "very small".
- this statement is removed from the manuscript and replaced by a statement of the statistical significance (line 397-399). Additionally, we added at several places in the text concrete numbers to substantiate and quantify statements like this (see lines 324, 343-344, 355, 415-417).

26.Line 377: Add "for the mid-Holocene" after "COSMO_iso_50km simulation" for clarity.
- is added.

27.Line 389-390: Again, this conclusion is problematic because the authors are examining the slope, but not considering the correlation between d18O and temperature.
- see the correlation analysis performed for the response to the major comments. The text is adapted as follows (line 418-419):

"But in principle, the influence of interannual surface temperature variations on the temporal $\delta^{18}O$ variability in the mid-Holocene is not as dominant as for the spatial variability."

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 d18O seems to follow a similar pattern, being somewhat temperature dependent. They do point to a strong local influence on the spatial variability in d18O.

- the statement is removed in the revised manuscript.

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."
- is changed according to your suggestion.

30.Line 415: Suggest changing "already leads to" to "produces".
- is changed according to your suggestion.

31.Line 417: Again define "considerably reduced" by providing some quantification. This is also not always the case as the results show.
- the text is adapted as follows (line 443-446):

"Especially in regions where the global ECHAM5-wiso model, which has been used to derive necessary forcing fields for the COSMO_iso simulations, deviates strongly from the observed $\delta^{18}O$ values, the RMSE is significantly reduced by 1.65 ‰ for regional climate simulation with COSMO_iso "

32.Lines 418-419: I think the authors should note here the lack of improvement when increasing to 7 km.
- the lack of improvement in highly resolved simulations is now mentioned in the manuscript (line 449-450):

"But for the rest of Greenland, highly resolved regional climate simulations do not yield further improvements"

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)"?
- is changed according to your suggestion.

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 d18O, which was suggested based on the low d18O-temperature slope. As discussed above, the authors should examine the correlation between temperature and d18O in order to determine the strength of that relationship, as well as to confirm the spatial relationships discussed here.
- the original statement is qualified in the revised manuscript. Now it is only stated that the temporal interrelations between temperature and d18O are not as dominant as the spatial ones (line 352-357). See response to the major comments.

35.Line 458-459: Again, this contradicts the previous statement.
- see previous comment.

Technical Corrections

1.Line 46: Change "warming, in more detail" to "warming in more detail".
- is changed.

2.Line 60: Suggest changing to read "not able to quantitatively reproduce regional changes in isotope ratios"
- is changed.

3.Line 63: Change “ratios in precipitation, by a regional” to “ratios in precipitation through a regional”.
- is changed.

4.Line 66: Change “presented study” to “present study”.
- is changed.

5.Line 81: Change “Holocene conditions, is performed” to “Holocene conditions is performed”.
- is changed.

6.Line 119: Remove “have been suggested” before “(Gurney and Lawrence, 2004)”.
- is removed.

7.Line 121: Change “just a simplified” to “a simplified”.
- is changed.

8.Line 173: Change “Since both, snow pit” to “Since both snow pit...”
- is changed.

9.Line 191: Change “parameter” to “parameters”.
- is changed.

10.Line 196: Change “Both, simulated” to “Both simulated”
- is changed.

11.Line 210: Change “is able to reflect” to “is able to reproduce”.
- is changed.

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 d18O within GCM grid boxes to be simulated and compared to observed d18O values.”
- is replaced.

13.Lines 271-272: Suggest revising to read: “Thus, in the following sections, snow pit samples are no longer solely compared...”
- is changed.

14.Line 300: Change “how strong” to “how strongly”
- is changed.

15.Line 324: Remove comma after “air mass”. Change “increase there the isotopic variability” to “increase the isotopic variability there.”
- is changed.

16.Line 328: Change “in accordance” to “in agreement”.
- is changed.

17.Line 330: Change “lowly correlated” to “poorly correlated”.
- is changed.

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.”
- is revised.

19.Line 341: Change “differences of” to “differences between”.
- is changed.

20.Line 342: Change “to the observed” to “and the observed”.
- is changed.

21.Line 345:Change “deviates only about 1 ‰ to the observations” to “deviates only by about 1 ‰ relative to the observation”.
- is changed.

22.Line 358: Change to “the observed ratios derived from ice cores are subtracted from the simulated d18O ratios.”
- is changed.

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”
- are changed.

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.”
- are changed

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.
- the figure is changed according to your suggestions.

26.Line 368: Add a comma after “especially during the summer”.
- is added.

27.Line 370: Add “and” after “slightly underestimated,”
- is added.

28.Line 377: Change “for whole Greenland” to “for all of Greenland”.
- is changed.

29.Line 381: Add “and” before “the GRIP and GISP2”.
- is added.

30.Line 406: I think this should read “The locations of the ice core samples are shown in green.”
- is changed.

31.Line 410: Change “deviations to” to “deviations from”.
- is changed.

32.Line 425: Add a comma after “rather”.

- is added.

33.Line 426: Remove “But” before “all in all”
- is removed.

34.Line 431: Change “with even an” to “and even an”
- is changed.

35.Line 435: Remove “Now,” before “by analysing”.
- is removed.

36.Line 438: Remove comma after “applies for both”.
- is removed.

37.Line 442: Change “to reproduce” to “in reproducing”.
- is changed.

38.Lines 448-449: Change “spatial variability pattern of” to “patterns of spatial variability in”.
- is changed.

39.Line 450: Change “variability patterns” to “patterns of variability”.
- is changed.

40.Line 457: Change “structures” to “patterns”?
- is changed.

41.Line 466: Change “and their deviations to” to “and understanding their deviations from”.
- is changed.

42.Line 467: Remove comma after “regions”.
- is removed.