

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

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This study examines outputs of a regional climate model (RCM) enabled to compute fractionation of water isotopes over the Greenland ice sheet. The COSMO_iso RCM is forced at the lateral boundaries with isotope enabled GCM simulations with atmospheric nudging. Outputs of COSMO_iso simulations for the present day and the mid-holocene (at a 50 km spatial resolution) are compared against ice core isotopic measurements. For the present-day simulations the RCM simulations generally improved the agreement with observations compared to the GCM results, with the improvements generally occurring in regions with coarser GCM resolution. Higher-resolution RCM simulations at 7 km did not further improve the agreement, producing a worse agreement in some instances. For the mid-Holocene simulations, there was not a large improvement resulting from the RCM simulations (although data were available only from four ice cores). The authors note that the higher-resolution simulations provide a range of spatial variability for the coarse resolution grid that can be used to generate a distribution for comparison against ice core measurements. They also examine gradients of isotope ratio relative to temperature, finding higher variability in temperature and isotope ratios along the ice sheet margins.

General Comments

In general, the study appears to be scientifically sound, and well-organized. The work represents an important step in developing an improved understanding of the relationship between measured isotopic ratios and historical climate. The presentation, particularly the language, needs improvement, with many grammatical errors. The figures are somewhat difficult to read at first glance and also require improvements.

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

I also have some concerns about the manuscript, in particular:

1. The in situ measurements are all located within the high-elevation center of the ice sheet, with one exception. It is therefore difficult to evaluate the degree to which the model simulations capture the spatial variability. While the RCM simulation improves the agreement with the southern-most observations, it introduces a positive bias in the north. It seems this could be due to differences in the dynamical simulation in the RCM relative to the ESM rather than increased variability in the higher resolution RCM as the authors argue.

- we agree and changed the argumentation according to your suggestion. Thank you very much for this helpful comment (Lines 231 – 235):

“As visible in Figure 3, these systematic differences are rather caused by a southward shift of the area of low yearly mean $\delta^{18}\text{O}$ values in central Northern Greenland in COSMO_iso_50km relative to ECHAM5-wiso. As a result, the simulated $\delta^{18}\text{O}$ values in central Northern Greenland in COSMO_iso_50km are higher than in ECHAM5-wiso. Since there, ECHAM5-wiso has already a high agreement with the observed $\delta^{18}\text{O}$ values, a model bias is introduced in COSMO_iso_50km, causing the deviations relative to the observations in Northern Greenland.”

2. Given the above points, the added value of the RCM simulation is not entirely clear, even in the present-day simulation, although the plots seem to suggest that it does provide some improvement in

the mean value. The authors should provide quantitative estimates as to the improvement associated with the RCM.

- we agree and mention now quantitative estimates of the RCM improvements in the present-day simulations (Lines 217 and 246). The average bias reduction of COSMO_iso_50km over all snow pit samples is 0.7‰, the average reduction of COSMO_iso_7km is 0.6‰.

3. The method of averaging observational data (which may contain missing values) is not entirely clear. The authors have not discussed potential errors in the observations.

- of course, observations are also associated with uncertainties. The impacts of firm diffusion, post-depositional erosion of surface snow by wind and the spatial uncertainties related to micrometeorological effects are now discussed in the revised paper (e.g. Lines 167-173). Since the used observational datasets do not contain missing data, no special averaging method is applied (See comments 16 & 17).

“Since all snow pit samples cover different time periods, the present-day $\delta^{18}\text{O}$ values (black numbers in Table 2) are calculated as an average of all available $\delta^{18}\text{O}$ values measured between 1940 and 2007. With this procedure uncertainties in snow pit samples and top ice core samples, associated with post depositional diffusion and the resulting constraints in analysing annual and interannual top ice core data (e.g. Johnson et al., 2000), can be neglected. However, further uncertainties in snow pit samples and ice core data remain, regarding the timescale assignment (Steig et al., 2005) and the spatial variability (Weißbach et al., 2016b).”

4. I think the authors’ approach of using the high-resolution variability as an indicator of the potential spatial variability within a coarse resolution grid cell, that can then inform the point observation to model grid cell comparison, is interesting. If the authors can find any literature supporting this argument, I think this would strengthen the manuscript.

- With the publication of Shi et al., (2020), an additional reference substantiating our argumentation is now cited in the revised manuscript. In this study, the importance of small-scale processes to understand the measured water isotope variability is highlighted. According to this, GCM deficiencies in simulating this isotope variability are therefore caused by the missing representation of such small-scale processes in GCM simulations (this statement is now included in Lines 429-431). However, the authors are not aware of any further supporting literature.

Shi et al., (2020): <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD031751>

5. This is not essential but the presentation of the manuscript could be improved if the authors use a different projection that doesn’t distort the Greenland ice sheet, and if they label figures with brief headings that summarize each sub-figure without necessitating a thorough reading of the caption.

- as you already mentioned in your first comment, most of the point measurements are located in Central and Northern Greenland very close to each other. The chosen projection is therefore beneficial to better distinguish between these observations (especially in Figure 3). For that reason, we would like to stick to this projection.

In the revised manuscript, all figures are labeled with headings.

Specific Comments

1. Title: The title could be improved to better describe the study. The title should include mention of Greenland and types of models that are used. Possible revision: “Applying an isotope-enabled regional climate model over the Greenland Ice Sheet: effect of spatial resolution on model bias”

- the title is changed as you suggested. Thank you very much for your suggestion

2. Lines 7-9: The authors should mention here the motivation and purpose of the study, which is described well in the introduction section.

- the motivation of the study is now mentioned at the beginning of the abstract (lines 8-10):

“In order to investigate the impact of spatial resolution on the discrepancy between simulated $\delta^{18}\text{O}$ and observed $\delta^{18}\text{O}$ in Greenland ice cores, regional climate simulations are performed with the isotope-enabled Regional Climate Model (RCM) COSMO_iso.”

3. Line 9: Change “isotopic ratios in Greenland” to “isotopic ratios in Greenland ice cores”.

- is changed.

4. Line 10: Explain that ECHAM5-wiso and MPI-ESM-wiso are GCM simulations and spell out acronyms.

- in the revised manuscript, it is now mentioned that ECHAM5-wiso and MPI-ESM-wiso are isotope-enabled GCMs. The acronym MPI-ESM-wiso is now spelled out in the model description section 2.1.2. Since the GCM ECHAM is well-known in the modelling community and its acronym is very complex (a combination of ECMWF, which is already an acronym, and **H**amburg, the location of the Max-Planck-Institute), we decided to not spell out ECHAM.

5. Lines 15-16: This sentence is confusing. Suggest revising to something like: “...the COSMO_iso estimates provide a distribution of values representing spatial uncertainty that give context to comparison with observed isotopic ratios.”

- the abstract is rephrased in consideration of your suggestions (see comment 6).

6. Lines 20-23: These sentences are confusing. I think the authors can simply say something like: “Despite the lack of improvement in model biases, the RCM simulations provide a distribution that allow the effects of spatial uncertainty to be taken into account in the comparison between point measurements and model outputs.”

- the abstract is rephrased in consideration of your suggestions (Lines 21-26):

“Despite this lack of improvements in model biases, the study shows that in both periods, observed $\delta^{18}\text{O}$ values at measurement sites constitute isotope ratios which are mainly within the subgrid-scale variability of the global ECHAM5-wiso and MPI-ESM-wiso simulation results. The correct $\delta^{18}\text{O}$ ratios are consequently already included but hidden in the GCM simulation results, which just need to be extracted by a refinement with an RCM. In this context, the RCM simulations provide a spatial $\delta^{18}\text{O}$ distribution by which the effects of local uncertainties can be taken into account in the comparison between point measurements and model outputs.”

7. Line 60: The authors mention temporal resolution here, but this is not discussed in the rest of the manuscript. I suggest providing further details here about temporal downscaling and noting that the focus of the present study is on spatial downscaling.

- in the revised manuscript, the text is adjusted as follows (Lines 66-71):

“Therefore, in the presented study, isotope-enabled GCM simulation results for the Arctic region are dynamically downscaled with an isotope-enabled RCM to a higher temporal and spatial resolution. By means of such regional simulations, the spatial and temporal variability of the isotopic ratios in the Arctic is potentially increased, accounting for the heterogeneity of local conditions at the different ice core locations and the associated uncertainties. In this way, the impact of highly resolved local conditions on the spatial and temporal variability of isotopic ratios is investigated, and the impact of such small-scale variability on the discrepancy between simulated and observed paleo-climate conditions in the Arctic region is examined.”

According to this, an analysis of the temporal variability is additionally included in the paper (Figure 7d and 9d).

8. Lines 70-75: The text here repeats some information that was mentioned earlier. Suggest revising to avoid repetition.

- this information was only mentioned in the abstract. Therefore we would like to keep it in the text.

9. Line 92: It should be first noted here that snow surface albedo is fixed and is not spatially and temporally variable.

- snow surface albedo is not fixed. An alteration of the snow albedo with growing age is considered in the model. The increase in the snow albedo value from 0.7 to 0.8 refers to the albedo value of fresh snow. This is now specified in the manuscript (Line 97).

10. Lines 120-144: How are the ocean boundary conditions specified? Are these from reanalysis data?

- in the ECHAM5-wiso simulations, sea surface temperatures and sea ice cover are varying monthly based on ERA data. In MPI-ESM-wiso, the ocean component is calculated dynamically with the ocean model MPIOM. This is now mentioned in the text (Lines 131-132 and 151-152).

11. Line 111: What is meant by “the models”? Please clarify.

- we mean state-of-the-art isotope-enabled models. This is now clarified.

12. Lines 114-119: Are the authors referring to work they have performed comparing COSMO_iso to observations, or is this referring to the Christner et al. (2017) study? Please clarify. Also, please clarify how the processes are treated in the COSMO_iso model.

- these processes are not yet included in state-of-the-art isotope-enabled models. This is, for instance, discussed in Christner et al., (2017). The paragraph is rephrased to avoid confusion and to clarify how fractionation at snow covered surfaces is treated in COSMO_iso (Lines 114-122):

“Isotope fractionation during sublimation from a surface snow layer is poorly understood. Several different processes are suggested to be involved, which are not yet taken into account in state-of-the-art isotope enabled models (see e.g. discussion in Christner et al., 2017), such as non-fractionating layer-by-layer sublimation (e.g. Ambach et al. 1968), kinetic fractionation during sublimation into sub-saturated air, a diurnal cycle of sublimation combined with fractionating vapor deposition on the snow (e.g. Steen-Larsen et al., 2014), and fractionating melt water evaporation combined with recrystallization of residual melt water have been suggested (Gurney and Lawrence, 2004). To approximate this complex interplay of different influencing factors, in this study, an equilibrium fractionation during sublimation from surface layer snow and sea ice is assumed. However, the authors are aware that this is just a simplified description of isotope fractionation during sublimation.”

13. Lines 123-124: Note the domain boundaries for the Arctic simulation.

- an additional figure showing the model domains (50 km and 7 km) is now included in the manuscript (Figure 1)

14. Lines 128-130: Is this an additional simulation forced by the coarse resolution run, or a nested domain within the larger domain?

- this simulation is nested in the 50 km simulation with COSMO_iso. This is now clarified in the text (Line 134-135).

15. Line 130: What is meant by “technical reasons”? Please clarify.

- this statement is removed from the text.

16. Lines 152-153: How are the authors dealing with missing data? If there are large temporal gaps in some of the datasets this could influence the average values.

- In the selected time periods, no missing data occurred in the yearly d18O values of the snow pit samples.

17. Table 1: Are all the datasets available for the specified period? What is the effect of missing data on the estimates? Does the depth of the cores/snow pits affect the average? Please comment and perhaps perform calculations to assess these affects.

- No, not all samples cover the whole period. But the individual datasets are consistent in themselves and do not contain missing data. In addition, the averaging periods of the respective snow pit samples are long enough to rule out statistical outliers.

18. Line 183: What is the average reduction in the bias?

- The average bias reduction of COSMO_iso_50km is 0.7%, the average reduction of COSMO_iso_7km is 0.6%. This is now mentioned in the text (Lines 217 and 246).

19. Lines 199-205: I don't quite understand the logic here. I think what the authors are saying is that the high-resolution simulation leads to a higher degree of variability in locally simulated values. Due to the uncertainty in the model simulation, this may lead to a larger bias with respect to in situ point measurements, which may actually be closer to the average value on the coarse resolution grid. However, running the high resolution simulation allows for computation of a range of local variability, which can be used to compare model to observed values, accounting for the inherent uncertainty of the in situ measurement associated with local variability. This is an interesting and reasonable argument. I think the authors need to articulate it better here. Also if the authors can find any literature showing similar results this would be helpful in supporting this argument.

- we rephrased the paragraph (Lines 264-267 and 271-276). Thank you very much for your helpful suggestions.

“As a consequence, an additional spatial variability is introduced in the RCM simulations in comparison to the GCM results. Due to uncertainties accompanied by model simulations, this can potentially increase the RCM bias with respect to in situ point measurements, which may actually be closer to the spatially averaged values simulated by the coarse GCM model.”

“However, by performing higher resolved RCM simulations, the subgrid-scale variability of $\delta^{18}\text{O}$ within GCM grid boxes can be simulated and compared to observed $\delta^{18}\text{O}$ values. In this way, the inherent uncertainty of in situ measurements, associated with a local micrometeorological variability, can be considered. Thus, in the following, snow pit samples are not anymore solely compared to the

model grid boxes covering the samples location. Instead, it is investigated whether the $\delta^{18}\text{O}$ range of all adjacent RCM grid boxes to a snow pit location is consistent with the observed $\delta^{18}\text{O}$ value of the same site. For this, all RCM grid boxes located within the corresponding GCM grid box are included in the comparison with the observations.”

20. Figure 2: Why are sites 17 and 18 missing here? Are data from these locations missing for this year? Please clarify in the caption and in the main text.

- this is corrected in the revised manuscript. Now the corresponding figures show all data points (now 19).

21. Lines 223-228: This argument does not make sense to me. Looking at the box plots in Figure 3, the variability for these particular stations does not seem to be larger here than at other locations. Rather, there appears to simply be a model bias at this location. One can also see from Figure 1, that COSMO_iso seems to shift the low isotope values in central northern Greenland further south relative to the ECHAM5-wiso, thereby increasing the bias in these areas somewhat. The authors should clarify or revise their arguments here.

- We agree with you and adapted our argumentation according to your suggestions (see general comment 1). Thank you very much for this helpful comment.

22. Lines 251-257: This paragraph would more appropriately follow the first paragraph of the section, detailing the mid-Holocene results.

- this paragraph is relocated according to your suggestions.

23. Figure 4: The y-axis label is confusing. Suggest changing to $\delta^{18}\text{O}$ difference. In the caption labels, suggest replacing with MPI_ESM_wiso –obs. and COSMO_iso_50km –obs.

- We changed the labeling of Figure 4 (now Figure 8) according to your suggestions.

24. Line 261: Is the green point for the 50 km grid cell closest to the measurement location? Please clarify.

- Yes it is. This is now clarified (Line 364).

25. Line 263: Spell out PI.

- is corrected.

26. Lines 266–294: I suggest making this a new section, discussing sub-ESM-grid variability.

- sections are now arranged in the revised manuscript according to your suggestions. Now, we discuss for both, present-day and mid-Holocene, first the simulated $\delta^{18}\text{O}$ data in comparison to the point measurements and then the GCM $\delta^{18}\text{O}$ subgrid-scale variability in sub-sections, respectively.

27. Line 286: Calling this a temperature gradient suggest that it is a change in temperature with elevation. Is this indeed a gradient, established through a linear fit of isotope ratio vs. temperature for the sub-grid results for each grid cell, or is it simply a ratio of the standard deviation? Please clarify by revising the text here.

- It is an isotope-temperature slope which constitutes a linear fit between the simulated $\delta^{18}\text{O}$ ratios and the surface temperatures at all COSMO_iso_50km grid boxes within the respective GCM grid box. The isotope-temperature slope is a measure that is frequently used to analyze how strong isotope ratios and surface temperatures are interrelated. This is now clarified in the text (Lines 303-304):

“The spatial isotope-temperature slope constitutes a linear fit between the simulated $\delta^{18}\text{O}$ ratios and the surface temperatures at all COSMO_iso_50km grid boxes within the respective ECHAM5-wiso grid box.”

28. Line 294: Change “the same mechanisms” to “similar mechanisms”.

- the sentence is rephrased in the revised manuscript.

29. Figure 5: Site 1 is very difficult to see here and in other figures. Is there a way to improve visibility, perhaps by changing colors? Also label the color axis “ $\delta^{18}\text{O}$ standard deviation” and “temperature standard deviation[K]” for clarity.

- We changed the color of the markers to green in the corresponding figures. The labeling is changed according to your suggestions.

30. Line 301: Change “Simulated variability” to “simulated sub-grid-scale variability”.

- is adapted in the revised manuscript.

31. Figure 6: This color map is likely not suitable for red-green colorblind readers. Suggest using a different color map.

- we changed the color map to blue-red.

32. Lines 330-331: As noted earlier, in some cases this may be a result of increased variability, but it could also be a bias introduced in the RCM simulation.

- This was actually a statement meant about the Renland station. This is corrected in the revised paper. Sorry for this mistake (Line 418-420):

“This in turn can lead to additional noise and thus, a deviating RCM behaviour with even an increase in the absolute model bias, as seen for the Renland station.”

33. Line 343: Suggest changing “The same” to “Similar”.

- is corrected

34. Line 358: Change “prove” to “test”.

- is corrected

Technical Corrections

1. Line 7: spell out RCM at the beginning of the line: “isotope-enabled Regional Climate Model (RCM) for Greenland. The capability of the applied RCM COSMO_iso,...”

- is corrected.

2. Line 13: Change “a downscaling” to “dynamical downscaling” for clarity.

- is corrected.

3. Lines 14-15: Revise to “yields improvements only for coastal areas with complex terrain.”

- is corrected.

4. Line 19: Change “already on a high level” to “already agrees well with observations”

- is corrected.

5. Line 26: Change “deviations to” to “deviations relative to”
- is corrected.
6. Line 32: Change “like past changes of temperature, out of” to “such as past temperature changes using”
- is corrected.
7. Line 37: Change “was steadily rising” to “steadily rose”
- is corrected.
8. Line 39: Change “were steadily decreasing” to “steadily decreased”.
- is corrected.
9. Line 40: Change “took place” to “had taken place”.
- is corrected.
10. Lines 41-42: Suggest revising to read “period of particular interest, given recent Arctic warming, as it was characterized by Arctic warming resulting from orbital forcing...”
- we keep the current phrasing
11. Line 43: Change “processes, leading to this warming,” to “processes leading to this warming...”
- is corrected.
12. Line 44: Suggest changing “reflect” to “reproduce”.
- is corrected.
13. Line 46: Remove “which are” before “documented in”.
- is corrected.
14. Line 51: Suggest changing “does not meet” to “does not reproduce” or “does not adequately represent”
- is corrected.
15. Line 54: Change “also often not entirely resolved” to “not well resolved” and “coarsely resolved GCMs” to “coarse resolution GCMs”
- is corrected.
16. Line 56: Change “deviations to” to “deviations relative to”
- is corrected.
17. Lines 63-64: Suggest changing to “investigated, and the impact of such small-scale spatial variability on the discrepancy between simulated and observed paleo-climate conditions in the Arctic region is examined.”
- is corrected.
18. Line 67: Change “separated” to “separate”.
- this sentence is removed in the revised manuscript.
19. Line 82: Spell out “COSMO”.

- is corrected.

20. Line 87: Change “presented” to “present”.

- is corrected

21. Line 100: Change “2 m temperature” to “2 m air temperature” for clarity.

- is corrected

22. Line 114: Add “the” before “best agreement”

- this sentence is removed in the revised manuscript.

23. Line 121: Change “reflect” to “reproduce”.

- is corrected

24. Line 134: Change “simulation has been” to “simulation is”

- we keep the current phrasing

25. Line 138: Is the improvement to surface albedo for all surface types or one particular surface type?

- For ECHAM6, a new land-albedo has been developed (Brovkin et al., 2013, JAMES, <https://doi.org/10.1029/2012MS000169>). But since we are focusing on Greenland in this study, different surface types are not so relevant. However, the albedo over sea ice area is also considered (treatment of melt ponds on sea ice). For land ice surface, the snow age is taken into account.

26. Line 147: Perhaps remove “different” from before “different observational data”.

- is corrected

27. Line 151: Remove “used” before “d18O values”.

- is corrected

28. Line 172: Change “models capability” to “models’ capability”.

- is corrected

29. Line 175: Change “decline stronger” to “decline more rapidly”.

- is corrected

30. Line 179: Change “stronger pronounced” to “more pronounced”.

- is corrected

31. Line 181: Change “at which” to “for which”.

- is corrected

32. Line 182: Change “deviations to” to “deviations from”.

- is corrected

33. Line 185: Change “results anymore” to “results further”.

- is corrected

34. Line 188: Change “a complex terrain” to “complex terrain”

- is corrected

35. Line 194: Change “a higher agreement” to “an improved agreement”.
- is corrected
36. Line 196: Change “an enlarged heterogeneity” to “an increased heterogeneity”.
- is corrected
37. Line 236: Change “differences for” to “differences between” and “grid box results to the” to “grid box results and the”
- the sentence is rephrased in the revised manuscript.
38. Line 238: Change “shown as Box-Whiskers” to “shown as a Box-Whiskers”.
- is corrected
39. Lines 277-278: Change “the three regions...”to “in three regions of Greenland with substantially different sub-pixel isotopic ratio variabilities.”
- is corrected
40. Line 281: Change “exhibiting also regional variations” to “which also exhibits regional variations...”
- the sentence is rephrased in the revised manuscript.
41. Line 283: Change “does consequently not only depend” to “consequently not only depends”
- is corrected
42. Line 313: Change “agreement to climate” to “agreement with climate”
- is corrected
43. Lines 322-324: Revise to “But for northern Greenland, regional climate simulations with COSMO_iso increase the bias with respect to observations and
- is corrected