Reply to the reviewers' comments: Investigating oxygen and carbon isotopic relationships in speleothem records over the last millennium using multiple isotope-enabled climate models (cp-2021-152)

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Summary of changes

We thank the reviewer for their constructive comments and detailed reading. In response to the suggestions by the reviewer we plan to

- revise the method section to motivate and highlight the influence of different weighting procedures to the results more thoroughly,
- clarify the use of the word "offset" between different simulations or between simulation and record where necessary,
- revise the data section in conjunction with the discussions to clarify the differences between model simulation setups and boundary conditions,
- revise the text throughout the manuscript to clarify statements,
- fix formatting where necessary.

A detailed response to the helpful remarks of the referee is given below.

1 Reply to the first reviewer

(Original report cited in italics)

This paper takes results from five prominent isotope enabled GCMs and compares their simulation of isotopic values with speleothem records from SISALv2 over the Last Millennium. From these results, we see large-scale similarities and differences between *the five models and can draw the conclusion that temperature and precipitation are first-order controls on the simulation of isotopic values. The authors recommend a multi-model approach when comparing iGCM and speleothem reconstructions of paleoclimate as individual models have regional biases that can impact the comparison. The findings of this paper represent important advances in our understanding of isotope enabled modeling and comparison to paleoclimate proxies, especially in the cross-comparison of iGCMs.*

The paper is well-motivated and will be an important contribution to the paleoclimate field. I recommend it for publication after some minor revisions. My main comments on improvements that will strengthen the paper are listed here below, and several more specific and minor corrections are listed afterwards.

We thank the reviewer for this positive assessment.

Major comments:

1) ***Infiltration adjusted precipitation weighting:*** *d18Oiw is an interesting method and I think it be beneficial to discuss it a bit more thoroughly. 1) A stronger justification for its use in this paper would be useful, such as more clearly stating why the results are more realistic for comparing to speleothem data. 2) A more detailed description of this method regarding how it differs from d18Op would be useful (highlighting the strong role played by evaporation). 3) Is this method justifiable over marine environments? I understand that it is preferable for understanding infiltrating water into a cave system, but I wonder if it artificially elevates the importance of evaporation over marine environments where there is always available water to evaporate? Since a key finding in this paper is that temperature drives speleothem values even at lower latitudes, I wonder if this takeaway is at least somewhat attributable to an artificially heightened dependence on temperature (via evaporation) at lower latitudes?*

Thank you for this interesting comment especially with regard to evaporation.

1+2) We will add a stronger justification in the methods section as to why we use $\delta^{18}O_{iw}$ instead of annual-mean $\delta^{18}O_{sim}$ and we will highlight the role of evaporation in the method section as follows:

"... δ^{18}**O**$_{speleo}$ **forms from drip water that reaches the cave, which is the precipitation water minus all water that evaporates. When comparing modelled to speleothem isotopes it is more realistic to weight the modelled** δ^{18}**O**$_{sim}$ **by** precipitation minus evaporation amount (infiltration adjusted precipitation weighting, *iw*) to obtain annual values. **Simply using the annual mean** δ^{18}**O**$_{sim}$**would overemphasize the isotopic composition of seasons where little to no precipitated water reaches the cave as drip water due to strong evaporation above the cave. The weighting therefore automatically focuses on the local seasonal composition of SWI in precipitation that will theoretically reach the cave and form a speleothem**. [...] As isotopic fractionation **also** occurs during evaporation from the soil, models where $\delta^{18}O_{sim}$ is also available for soil layers, would be more realistic to compare to speleothem data. However, these were only available for a few simulations. Using infiltration-weighted $\delta^{18}O_{sim}$, therefore, offered a more equal handling of the data **while maintaining the large ensemble** and enabled a better comparison of results. ..."

3) In this study, we use $\delta^{18}O_{sim}$ in precipitation. When studying marine environments infiltration weighting of $\delta^{18}O_{sim}$ is not the right variable to look at, but instead one should focus on the $\delta^{18}O$ in seawater. Nonetheless, your thoughts on the artificially highlighted dependence on temperature through evaporation are justified. To this end, we added figure A1. The figure shows that the infiltration weighting of the SWI artificially lowers the dependence of $\delta^{18}O$ on temperature, as it puts a weight on months with high precipitation and little evaporation instead of months with high evaporation. The correlation estimates are smaller globally for infiltration weighted data. When looking at correlation maps for precipitation (not shown) the correlation estimates are increased through infiltration weighting in regions where high precipitation falls in months with lower temperature, and decreased in regions where high precipitation falls in months with high temperature.

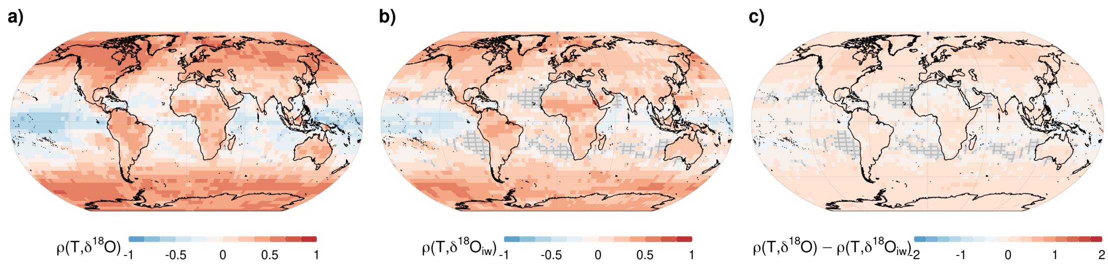

Figure A1: Correlation map between simulated $\delta^{18}O$ (a) or $\delta^{18}O_{iw}$ (b) to temperature. c) shows the difference between the two correlation maps.

2) ***"Offset"****: Throughout the paper, the term "offset" is used, but is generally loosely defined. It will help the readers to be explicit in the definition of this word. I was confused at times and wondered if this term referred to a) the difference of an individual model's values from the multi-model mean or b) the difference between model values (either individual or multi-model means) and speleothem values.*

Thank you for pointing this out. In the manuscript, we have used the term "offset" in both circumstances a) and b) mentioned by the reviewer. However, we agree with the reviewer that this can cause confusion and hence decrease readability. When revising the manuscript, we plan to keep the term "offset" when referring to the differences of individual models to multi-model mean, and use the terms "differ" and "deviate" when referring to the difference between model and speleothem values.

*3) **Temporal and spatial averaging in the models**: Please include more discussion on the uncertainty related to your choices regarding model averaging at speleothem locations. Annual mean model results are taken from a single gridbox that most closely corresponds spatially with the speleothem record – did I interpret this averaging method correctly? This paper will be strengthened if it includes some more discussion on the ways in which the choices in averaging impact the results – 1) How might the results change if instead of annual averages, seasonal averages (i.e., wet season, summer season, etc.) are used? Or if instead of a single gridbox, a larger spatial averaging region (i.e., also including all adjacent gridboxes) was used?*

Thank you for your interesting thoughts. 1) In this study, simulation data was available at monthly resolution. This allowed us to do infiltration weighting on the time series and calculate an annual value that emphasizes the season with the highest amount of precipitation that is not evaporated. In a global analysis, this results in different months dominating the annual isotopic value at each gridbox depending on local climate conditions. The same however is achieved, when taking the annual mean of monthly mean $\delta^{18}O_{sim}$. This averaging would over-represent specific months with only little precipitation. As all averaging processes include such seasonal biases, we chose the weighting since this theoretically correspond best to cave systems.
2) The averaging method is not choosing the gridbox that most closely corresponds spatially, but we extract simulated values by bi-linear interpolation, already taking into account neighbouring gridboxes. This is described in Sec. 3.2 Data processing on page 10 line 25. However, other extracting methods such as kriging interpolation have already been tested in other studies (Latombe et al., 2018). They show that bilinear or bicubic interpolation techniques distort either the temporal variability or the values of the response variables. We will add a short paragraph of the impact of our interpolation method to our results in the limitations-section.

Detailed Comments

Page 4 lines 17-26: The objectives of this paper are currently in the form of somewhat run-on sentences. Readers may understand them more clearly if they are organized more effectively. For example, one possible way to reorganize could be: "With this study, we aim to contribute to the understanding of both model and data: 1) How do different simulations model oxygen isotopes in the hydrological cycle and how do they compare to archived speleothem data? 2) What processes influence speleothem isotopic composition and what effects of variability can be captured and later analyzed?"

Thank you for this clarification. We will add an abc-enumeration, to not confuse the reader with the following text that starts with "We first..." and "In a second step...", as follows:

"...With this study, we aim to contribute to the understanding of both model and data: **a) H**ow do different simulations model oxygen isotopes in the hydrological cycle and how do they compare to archived speleothem data? **b) What** processes influence speleothem isotope composition and what effects of variability can be captured and later analyzed? ..."

Table 1: Definitions (can be brief) of GTOPO and ETOPO are missing from either the table caption or manuscript text.

Relevant references and definitions to GTOPO and ETOPO will be added to table 1 in the revised manuscript (i.e. Gesch et al. (1999); NOAA National Geophysical Data Center (2009); Amante and Eakins (2009); National Geophysical Data Center (1993)).

Page 4 Data section: There are many differences in the boundary conditions used between the five models and their setups. It would be helpful to add text on the impacts that these differences may have on the resulting simulations. This will be important in understanding how much (or how little) we can attribute the variations in each simulation to their underlying boundary conditions or if other factors play a more dominant role in their simulated differences.

Thank you for this interesting thought. We will add a short paragraph on the impacts of the different forcings in the data section 2.1 as follows:
"... Their basic characteristics and boundary conditions are listed in Tab. 1. They are both used individually in the analysis, as well as by the ensemble mean of all models. Fig. 1 shows the climate as represented by the different models and external forcings used in the simulations. **Since SWING2, there has not been a consistent protocol for paleoclimate simulations with isotope enabled models. Hence, the simulations used in this study largely follow the PMIP3 Last millennium experiment protocol (Schmidt et al., 2011, 2012) with its proposed climate forcing reconstructions, with some variations in vegetation and orography. Of the external forcings used, differences in volcanic forcing may have the largest influence on differences between the simulations (Colose et al., 2016; Schmidt et al., 2011), as different responses on larger eruptions may have a long term impact. Large eruptions can cause local anomalies to the mean state δ^{18}O of up to $\pm 1.5‰$ (Colose et al., 2016), hinting at the magitude of change that can be caused by different forcings. These volcanic eruptions are among the most prominent drivers of natural climate variability (Jungclaus et al., 2017). Compared to volcanic forcing, the choice in solar or orbital forcing has a less strong effect over time in the last millennium. Although the simulations do use different forcings based on different reconstructions which then act on different timescales, differences in response may not only**

arise from the forcings, but from the implementation in the models Jungclaus et al. (2017). ..."

We will also change "vegetation" in the table to "land cover" as it describes the forcing more precisely.

Figure 1: For Figure 1a, please state what the anomalies are relative to (i.e., what is signified by 0°C? It appears to be 1900 CE).

Thank you for pointing this out. The anomalies are relative to the period of the last millennium (850-1850CE). We will add this in the caption.

Figure 1: Please describe more clearly what the difference is between the noisy background lines and the less variable darker colored lines in Figure 1a.

Thank you for spotting this. The noisy background are the down-sampled values at cave location while the bold line are the down-sampled values with a 100 yr Gaussian kernel bandpass and smoothing from the R-package nest (`https://github.com/krehfeld/nest` Rehfeld et al. (2011); Rehfeld and Kurths (2014)).

Page 9 line 20: Are speleothem record values of d18Oc from the Last Millennium being converted into d18Odweq? If so, please describe how the past temperatures are calculated or inferred.

As explained in the text on page 6 line 26-28, we use the annual mean modelled surface temperatures as a surrogate for measured cave temperatures, as these are often not available especially in paleoclimate.

Page 12 lines 16-18: The text states that iCESM and iHadCM3 show stronger depletion towards the poles compared to other models. From my view of Figure 3, I do not see this stronger depletion because I see that GISS-E2-R shows stronger polar depletion than either iCESM or iHadCM3.

Thank you for spotting this. We double checked with latitudinal averages as shown in Fig. A2. We will change the section as follows:
"... The global mean $\delta^{18}O_{iw}$ values are fairly similar in area-weighted global mean of 8.48‰ (90% CI: −8.61, −8.36) and −8.41‰ (−8.62, −8.2) for isoGSM and GISS-E2-R, respectively. The ECHAM5-wiso run is less depleted with a global $\delta^{18}O_{sim}$ mean of −7.27‰ (−7.46, −7.09), but with clearly visible more strongly depleted mid-latitude oceans than in the other simulations. iCESM and iHadCM3 show a stronger depletion of −9.39‰ (−9.51, −9.28) and −9.15‰ (−9.29, −9.01) respectively, with **iCESM showing** stronger depletion **in the mid-latitudes and iHadCM3** towards the **Antarctic**

compared to the other simulations. **Although GISS-E2-R shows strong depletion especially in the arctic region, the less depleted mid-latitudes dominate the global mean.** ..."

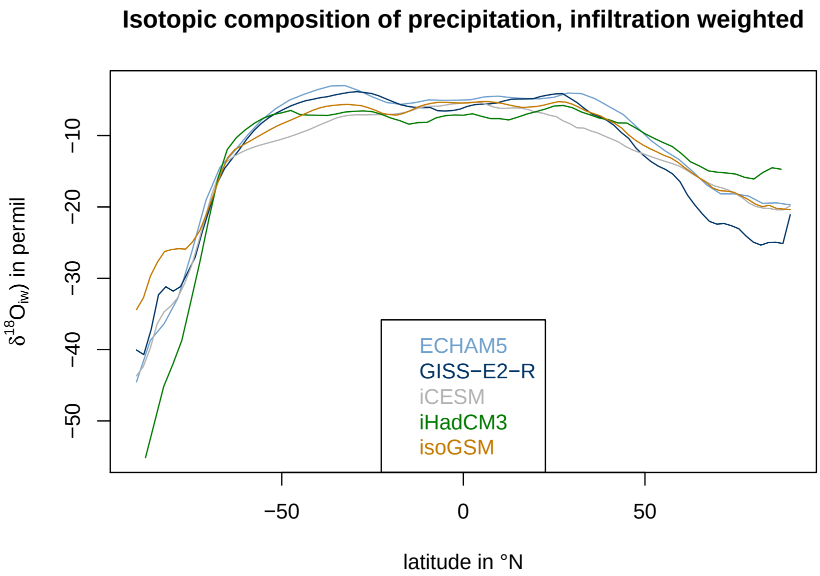

Figure A2: Mean simulated $\delta^{18}O_{iw}$ across all latitudes for all simulations.

Page 12 line 16: When interpreting d18Oiw over the ocean, is ECHAM5-wiso being more depleted than other models in the mid-latitude oceans potentially due to how much evaporation takes place here since the P – E weighting will likely assign a heavy role to E in determining amount weighting? Inclusion of a figure for global evaporation in the supplement, like SFigs 3 & 4 for temperature and precipitation, may help in answering this question.

Thank you for the suggestion. In fig A3), we find, that ECHAM5-wiso is not exceptionally different in its evaporation. However, after more evaluation, we do find that ECHAM5-wiso simulates less precipitation in the mid-latitudes than the other simulations. ECHAM5-wiso is least depleted in heavy oxygen isotopes in the mid-latitudes in Fig A2, but deviates not too much from the model ensemble range. We add an additional figure of precipitation minus evaporation (Fig A4) to the supplement of the manuscript, to see differences between the simulations, that are affecting our analysis.

Page 13 lines 2-4: I disagree with the statement that iHadCM3 deviates in its simulation of northern Africa from the other models, but that all other models agree with each other. From my view, Figure 3 shows very different results in northern Africa across all

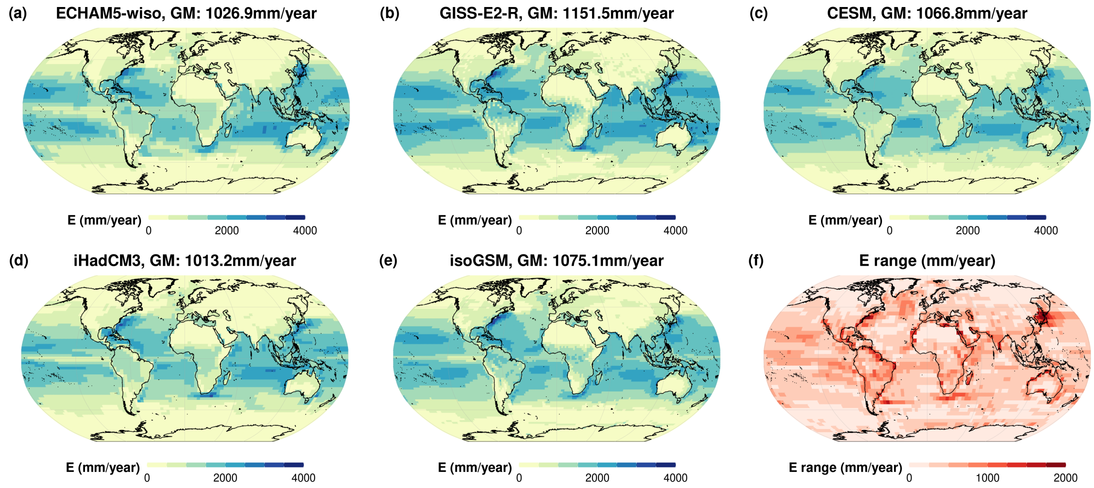

Figure A3: Simulated evaporation climatology (a-e) of the respective simulation: a) ECHAM5-wiso, b) GISS-E2-R, c) iCESM, d) iHadCM3, e) isoGSM).

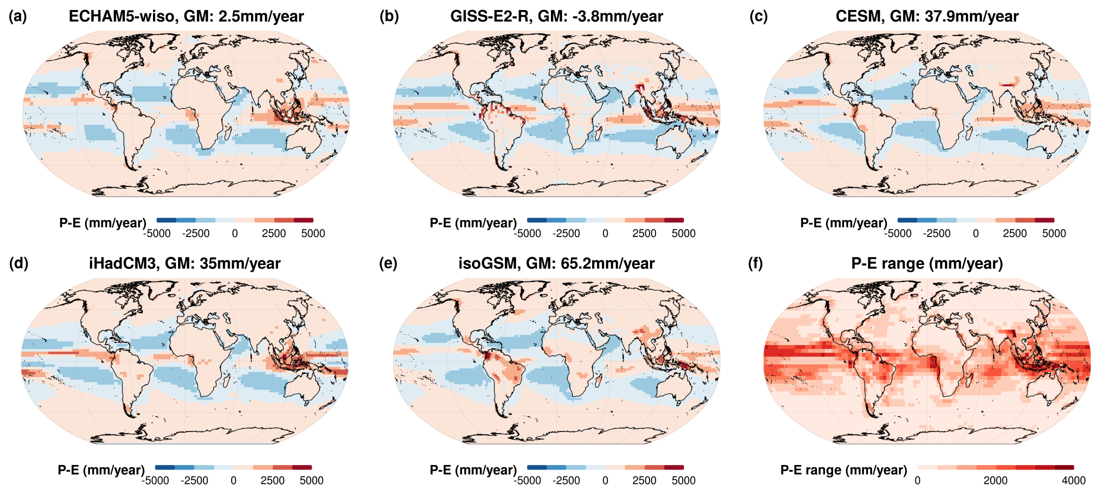

Figure A4: Simulated precipitation minus evaporation climatology (a-e) of the respective simulation: a) ECHAM5-wiso, b) GISS-E2-R, c) iCESM, d) iHadCM3, e) isoGSM).

models.

Thank you. The statement is indeed wrong and we will change the text as follows: "...Restricting the view to low- to mid- latitudes, the largest model data difference is in the area of the Sahara desert, the Arabian peninsula, the Indian peninsula, **and Siberia, where low humidity, high precipitation amount or high continentality are the driving local forces of δ^{18}O**.

Page 13 lines 23-24: The text states that ECHAM5-wiso is the only model with a positive offset mean, but based on Fig. 4b it appears that isoGSM also has a positive offset mean? Please address this.

The dashed lines in Fig. 4b represent the medians (0.28‰), however the simulation mean of isoGSM is negative in relation to the speleothem dataset (-0.17‰). Both mean and median are presented for the simulations and in their differences to the speleothem dataset. We do this to both include the full data (through the mean) and to have less impact of extreme values and skewed distributions (through the median). To clarify this better, we will change the text from the third sentence of the paragraph as follows: "The general distribution and **differences** between each model and speleothem data are shown as kernel density estimates (Fig. 4). **The full datasets are acknowledged through the mean value, whereas median values exclude skewed distributions and extremes.**".

Figure 5 caption: It is slightly unclear what you mean here by the correlation. Is this the correlation of time-mean values in speleothems vs. models? Is it the time-varying mean? Clarifying this in the text will be beneficial.

We will change the caption to enhance readability as follows:
"Correlations between **SWI** and **modelled** temperature (a) and precipitation (b) **time series** in each gridbox. The background shows the average over all 5 simulation **correlation estimates** between annual $\delta^{18}\mathbf{O}_{iw}$ and simulated annual temperature per gridbox (a), and for precipitation (b). Crosses indicate gridboxes, where correlation **estimates** for four or more models **have** the same sign as the **averaged estimate over all** simulations. Symbols indicate the mean correlation of the simulated temperature (precipitation) to the recorded $\delta^{18}O_{speleo}$ at record resolution. Crossed circles mark those, where more than four models agree in the mean sign of the correlation to $\delta^{18}O_{speleo}$. Black circles indicate the location of those speleothems in the last millennium subset that show no significant correlation to any model."

Page 15 line 15: The text states that there is a decreasing spread in d13C with increasing altitude. Is this result robust? It looks to me like there is instead decreasing data density with increasing altitude, which would suggest that this result is not robust.

We agree, and we also discuss this in the Discussion chapter, however you are right, that we should already point this our earlier. We will adjust the sentence as follows:
"...However, the spread in $\delta^{13}C_c$ appears to decrease with increasing altitude (Fig. 6d), **although under decreasing data density**. ..."

Page 16 line 5: The results indicating that d13C is more enriched with altitude are described as "results not shown". It would be great if these results were shown in the supplemental.

A figure of scatter plots between the two isotopes and altitudes for the seperate latitudinal bands will be added to the revised supplement file (Fig. A5). In the text "results not shown" will be replaced by "SFig. X".

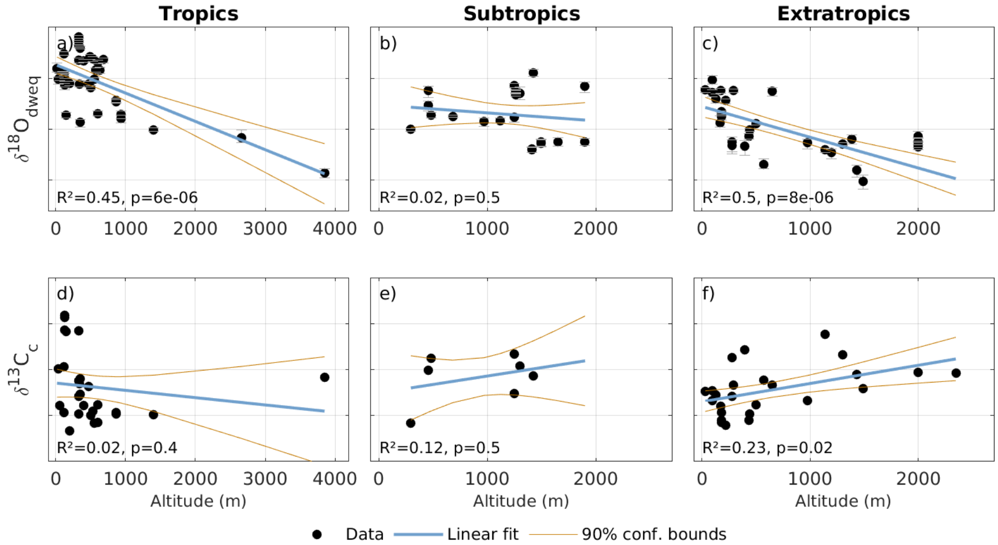

Figure A5: Speleothem $\delta^{18}O_{dweq}$ and $\delta^{13}C_c$ against altitude as provided by the database.

Page 15 lines 4-5 and Pages 16 lines 7-8 & 17 lines 1-2: With these summary statements, please acknowledge existing literature to claim that, as expected or not as expected, you see these specific literature-established relationships (i.e., strong relationship with temperature) in your analysis.

We set the results into perspective of existing literature in the discussion section - specifically the summary of pages 16 (lines 7-8) & 17 (lines 1-2) are then later discussed on page 21 (line 1-23). For the summary of page 15 (line 4-5), which is discussed on age 21 (24-33), we will add existing literature for perspective as follows in the discussion section:

"...For all simulations, temperature variability was the dominant driver in $\delta^{18}O_{sim}$ at high latitudes and precipitation variability at low latitudes (Fig. 5). **However, local and regional climate dynamics, such as landward moisture transport and ice sheet changes can mask and alter these relationships, as found for simulated isotopes in GISS-E2-R in a global study by LeGrande and Schmidt (2009).** At the cave sites, model-internal regional variability as well as the records' age uncertainties substantially decreased correlation estimates. ..."

We will additionally add short summaries in the results section:

For Page 15 line 4-5: "...The data suggests that two main drivers for 18O can be distinguished in specific regions - temperature is dominant in the high latitudes, while precipitation appears to be the main driver in the low latitudes, which is what we expected following the principles established by Dansgaard (1964)."

For Pages 16 lines 7-8 & 17 lines 1-2 : "... The spatial testing shows globally strong relationships between $\delta^{18}O_{dweq}$ to environmental factors, in particular to altitude, temperature, precipitation, and evaporation, **which is in line with previous studies (for example Comas-Bru et al., 2019; Baker et al., 2019)**. The spatial relationships between speleothem entity mean $\delta^{13}C_{speleo}$ and meteorological variables from model ensemble mean (Fig 8) only show clear relationships in the extratropical region, but not on a global scale. **This indicates more local influences as by Fohlmeister et al. (2020).**

Page 20 lines 9-10: The tone of this sentence could be softened because as it stands the statement is probably too strong considering all of the other factors that could also be at play. I find that the word choice "likely" helps to soften the tone in statements like this.

Thank you. We will add this as follows:

"...We found that the mean $\delta^{18}O_{sim}$ fields show global differences of 2.12 ‰ between the models, that could mostly **likely** be attributed to the global mean temperature differences 1.8 K between the models. ..."

Page 23 lines 23-29: The present wording makes it seem like this paragraph contradicts itself, even though that is not the case. When stating "d18Osim showed that cave locations are in general suitable to detect climatic changes due to volcanic or solar forcing", this could easily be erroneously interpreted as saying "caves are generally suitable..." I recommend changing the language to something like the following: "d18Osim showed that modeled isotopic values can generally detect climatic changes..."

Thank you for this clarification. Your recommendation, however, does not emphasize enough, that we also mean the locations where the caves are set, which we think is important too. We will change the section as follows and hope to resolve possible erroneous interpretations with it:

"...Summarizing, the comparison to **modelled values** showed that cave locations in this study are in general suitable to detect $\boldsymbol{\delta^{18}O_{sim}}$ **variations due to modelled** climatic changes **as reactions on** changes in volcanic of solar forcing. ..."

Page 24 line 23: In the Conclusion, there is a statement that says, "This effect can be compensated by using the multi-model mean." In thinking about the recommendation for using a multi-model approach, I am left wondering if this recommendation is based on 1) that a multi-model mean is always a less extreme model value because it reduces local spatial biases from individual models, and thus generally provides a better matches to speleothem values as they are less extreme, or instead 2) that multi-model means mostly converge to the real speleothem value, regardless of whether it is an extreme value or not.

It may be useful to address this nuance during discussion of the multi-model approach recommendation.

Thank you for pointing out, that this can be understood in more than one way. We will clarify the section in the conclusion as follows:
"...We found that $\delta^{18}O_{sim}$ differed substantially between models on a regional scale as well as at speleothem cave sites. This could mostly be attributed to differences in modelled temperature between models. **Extreme model values that differ greatly from the rest can be compensated for** by using the multi-model mean **and thus reducing local spatial biases**. The isoGSM simulation showed the lowest absolute mean offset to the speleothems at cave locations, while all other simulations show only slightly higher offsets. ..."

Technical Comments/Corrections

Page 10 line 19: "Annually weighted" is crossed out Done.

Page 12 line 14: Missing a minus sign? Done.

Page 13 line 5: The first supplemental figure mentioned in the text is SFig 3. Should SFigs 1 & 2 be mentioned prior to this?

We will change the order of the supplementary figures according to their appearance in the text. Thank you for spotting this.

Page 20 line 24: Missing a parenthesis? Done.

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