

Clim. Past Discuss., referee comment RC1
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Comment on cp-2021-152

Anonymous Referee #1

Referee comment on "Investigating stable oxygen and carbon isotopic variability in speleothem records over the last millennium using multiple isotope-enabled climate models" by Janica C. Bühler et al., Clim. Past Discuss.,
<https://doi.org/10.5194/cp-2021-152-RC1>, 2021

- An initial paragraph or section evaluating the overall quality of the discussion paper ("general comments")

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.

1. Infiltration adjusted precipitation weighting: d_{18Oiw} 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 d_{18Op} 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?

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.

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?

- Followed by a section addressing individual scientific questions/issues ("specific 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 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.

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.

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).

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.

Page 9 line 20: Are speleothem record values of d18O_c from the Last Millennium being converted into d18O_{dweq}? If so, please describe how the past temperatures are calculated or inferred.

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.

Page 12 line 16: When interpreting d18O_{iw} 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 S Figs 3 & 4 for temperature and precipitation, may help in answering this question.

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 models.

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.

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.

Page 15 line 15: The text states that there is a decreasing spread in $\delta^{13}\text{C}$ 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.

Page 16 line 5: The results indicating that $\delta^{13}\text{C}$ is more enriched with altitude are described as "results not shown". It would be great if these results were shown in the supplemental.

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.

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.

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 "*d18O_{sim} 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: "*d18O_{sim} showed that modeled isotopic values can generally detect climatic changes...*"

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.

- By a compact listing of purely technical corrections at the very end ("technical corrections": typing errors, etc.)

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

Page 12 line 14: Missing a minus sign?

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

Page 20 line 24: Missing a parenthesis?