2nd review of the article by Parker et al, CP

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The paper has been improved by the authors and is now clearer and more accessible. The additional figures in the supplementary material are helpful.

However, my major comment remains. I think that the wording could be modified to clarify the limitations of this study.

1 Major comment: correlations do not always indicate causality, even within the world of a GCM

Reading the response to reviewers, I understand that the authors want to document what climate variables are associated with changes in speleothem $\delta^{18}O$. Ideally, they would have used observations only, but they argue why they prefer to use a GCM: I think this an important argument that should be added in the introduction of the paper.

I agree that a GCM provides a physically consistent framework, where all climate variables are available for analysis. However, the world of he GCM is extremely complex, almost as complex as reality. Analyzing GCM outputs to identify drivers of $\delta^{18}O$ variations is thus extremely complex. This is why different authors in previous studies have developed decomposition methods to quantify the relative effects of different processes (e.g. [Botsyun et al., 2016, Tabor et al., 2018]). In absence of such decomposition methods, the drivers cannot be quantified. At best, you can look at how $\delta^{18}O$ variations correlate with climate variables. This is what you do. This identifies concomitant changes, but not drivers. Some concomitant variations may be fortuitous, mediated by other variables, or may contribute to a small fraction of $\delta^{18}O$ variations. I think this should be clarified in the paper. The wording "causes", "drivers", "explanations" should be avoided, for example:

- l 91: "provide plausible explanations for" -> "provide the changes in climate variables associated with"
- l 110: "main drivers of" -> "changes in climate variables associated with"
- l 112: "potential and plausible causes of" -> "trends in climate variables associated with"
- same l 232, 262, 467, 469, 475, ...

In the discussion and conclusion, the main limitations of the approach should be recalled: (1) limitations associated with the GCM-observation mismatches, emphasizing the need for a thorough evaluation of the GCM simulations; and (2) limitations related to the correlation analysis that does not allow to identify drivers: a decomposition method would be necessary.

2 Minor comments

- l 59: "reducing" -> "weakening" (because it's probably negative)
- l 345-347: "warmer and wetter": this would have opposite effects on $\delta^{18}O$. So what is $\delta^{18}O$ consistent with?
  Same for "cooler and drier"

- l 462: this number is a local recycling ratio. For $\delta^{18}O$, what matters is the total fraction of the precipitation that comes from continental recycling on any land grid box, and the number can be larger than 50% ([Yoshimura et al., 2004, Risi et al., 2013]). For the effect of continental recycling on paleo isotopic records, you may cite [Pierrehumbert, 1999].
- l 466: could the greater water-calcite fractionation at colder temperature also contribute to the observed change in speleothem $\delta^{18}O$? Could you do at least a simple back-of-the-envelope calculation to check this?
- Fig 3: I still find it very inconvenient to have a different axis for the model and observations. The figure allows us to see the sign of changes, but not the amplitudes. If the model capture the sign but not the amplitude, this is a very important information. So can you please use the same axis?
- Fig 3 caption: "shown" -> "show"

References

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