Climate of the Past Discussions

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Reply on RC2

Rebecca Orrison et al.

Author comment on "South American Summer Monsoon variability over the last millennium in paleoclimate records and isotope-enabled climate models" by Rebecca Orrison et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-6-AC2, 2022

We are grateful for the reviewer's comments on manuscript cp-2022-6. We addressed the *italicized reviewer comments* below in **bold text**.

The paper does a nice job of integrating longer-term proxy records and Earth system models, but links between recent observational datasets of modern water isotopes in precipitation (e.g., Aron et al., 2021; Fiorella et al., 2015; Guy et al., 2019; Vimeux et al., 2005, 2011) and water vapor (e.g., Galewsky & Samuels-Crow, 2015; Samuels-Crow et al., 2014) are not well established. Many of these authors came to similar conclusions regarding the relationship between water stable isotope ratios over South America and convection, and a few sentences to a few paragraphs incorporating these observations I think would help strengthen your conclusions. There are several areas in this manuscript where such a comparison would be relevant, but L. 486-488 stands out in particular.

Thank you for bringing these useful citations to our attention. We will reference some, but not all of them to further discuss how processes involved in convective activity might influence paleoclimate records of $\delta^{18}O$. Observations, albeit from a seasonal/intra-seasonal perspective, do show a greater depletion than predicted by Rayleigh distillation alone, highlighting the role of deep convection and the importance of convective processes at play in determining isotopic variability. However, we also note that the primary focus of our research is on the common signal in oxygen isotopes of precipitation as archived in a range of paleoclimate records across the continent on long timescales, and not on the specific processes involved with mixing water vapor and cloud condensate delta values on intraseasonal timescales.

On a related note, the authors indicate multiple times that precipitation isotope ratios are strongly related to rainout (e.g., L. 25, 368-370, 492). This may well be true, but the analysis presented doesn't establish this. Given the complexity in convective systems, there may well be additional factors that are also quite important. I would suggest revising these sections to be a bit more circumspect about the dominance of rainout in the region. Another approach would be to better establish links between rainout and precipitation isotope ratios using a more quantitative approach (e.g., Aron et al., 2021;

Fiorella et al., 2021; Konecky et al., 2019; Sodemann et al., 2008; Sodemann & Stohl, 2013).

We will provide a more nuanced discussion of the role of convective processes in controlling the isotopic composition of precipitation. We agree that the role of upstream processes is complex, and it is not merely condensation and rainout that determine isotopic variability.

We do not elaborate in great detail on upstream rainout processes as a main control on the oxygen isotopic composition recorded in archives from within the SASM domain. However, this control has been documented in a vast body of literature, identifying this mechanism across the South American monsoon domain in both observations and isotope-enabled climate models (e.g. Grootes et al., 1989; Hoffmann et al., 2003; Vuille et al., 2003; Vimeux et al., 2005; Vuille and Werner, 2005; Vuille et al., 2012; Ampuero et al., 2020). Hence, while interesting, a detailed quantitative investigation of these links in modern observations would result in a completely different paper and is therefore not feasible within the constraints of this research. While it is not possible for us to pursue such a research topic here, we agree that it would certainly be an interesting follow-up study!

This paper would also benefit for a more detailed explanation of the MCEOF approach, how the number of EOFs to present have been chosen, etc., as the description of these methods is quite brief. For example, the authors seemed to have selected the first two EOFs based on guidelines form North et al. (1982), but then note that these two EOFs only explain ~30-35% of the variance (and less in the pseudoproxy experiment), meaning there remains a lot of unexplained variations. This could also be discussed a bit more. (For example, one potential source of this unexplained variance is ENSO, but ENSO is treated extremely briefly in this manuscript – once in the introduction, and then re-enters unexpectedly in the conclusions) – of course there are other potential mechanisms of variation, but I think this manuscript would benefit from additional analysis/discussion regarding the explained vs. unexplained sources of variation.

We will clarify the summary of the MCEOF analysis in the opening paragraph of section 2 (Data and Methods) and better explain how the pseudoproxy network was developed. However, we are hesitant to add more excessive detail to the methods, given the length of the paper as currently written, and due to the general method being well documented in a number of publications which have been cited in the manuscript (Anchukaitis and Tierney, 2013; Deininger et al., 2017; Campos et al., 2019; Novello et al., 2021).

Regarding the selection of modes, we selected only the first two modes due to the rapid decrease of explained variance in subsequent modes, which indicates very little separation between modes three and beyond. Though the total explained variance of the first two modes equals less than 50%, we emphasize that this analysis is not attempting to maximize the explained variance among the proxy records, but rather to isolate the shared variability of records within the network.

Though ENSO constitutes a dominant mode of variability influencing South American climate on interannual timescales, it does not emerge as one of the leading modes of isotopic variability in our proxy network. Although the isotopic records used in our paleoclimate analysis show an approximate annual resolution, we still would need a much higher resolution for the U/Th chronology

to constrain interannual climate variability precisely. We have used a suite of precisely dated isotope records with age uncertainties of less than 1% at 2 sigma statistical confidence, but the number of U/Th dates usually produced in speleothem samples is not enough to retain an interannual ENSO signal. This a normal case for the studies using speleothem isotope records. Thus, our interpretation is focused primarily on the multi-decadal-/centennial-scale variability, which emerges from the two identifiable PCs. In observations and in annually resolved and precisely dated archives, however, the ENSO signal on the isotopic composition of precipitation can easily be identified (Vuille and Werner, 2005; Hurley et al., 2019). These studies show that the influence of ENSO is indirect, via modulation of the monsoon mean state, rather than directly affecting the precipitation or temperature at the proxy sites. We will reiterate the importance of ENSO in the revised manuscript.

Specific Comments

L. 74-76: Various processes in convection (e.g., raindrop evaporation, interaction with unsaturated downdrafts, entrainment of mid-troposphere vapor, etc.) can promote deviations in isotope ratios that would be expected from Rayleigh distillation. More details on these processes and their impact on isotope ratios are provided by: (Lee & Fung, 2008; Moore et al., 2014; Risi et al., 2008).

Thank you for this comment. As mentioned above, we will revise the manuscript to include a more nuanced understanding of the processes at play as moisture is transported into the monsoon region. However, we do emphasize that the evaluation of these small-scale processes is not the focus of this work. Our discussion of fractionation processes is focused on those mechanisms that influence coherent network variability, rather than locally relevant processes.

L. 144: was the annual resolution in these records determined by U/Th dating? Or by other methods? Not my area of expertise with respect to this paper, but I was surprised that U/Th dating could be so precise.

Three of our records are determined to have annual resolution. Two records (a lake sediment core and an ice core) were determined to have annual resolution based on layer counting. The third record with annual resolution is a speleothem (ALHO6) that was found to have annual resolution by layer counting and verification with U/Th dates along the entire record.

Speleothem resolution derives from the number of isotopic samples available in between two U/Th ages. Most of the isotope records used in our MCEOF analyses are characterized by close to annual resolution. The other records are sub-decadally resolved, as determined by U/Th dating. This dating provides a range of uncertainty around of 1% at 2 sigma statistical confidence, yielding errors on the order of a few years (less than 10 years in the vast majority of samples used in this study). U/Th dating accuracy is a function of the purity of the samples collected from speleothems and can have sub-decadal 2σ uncertainty.

L. 225-226: It would be worth specifying which version and configuration of the iCESM this refers to, since this bias is not constant throughout different versions of iCAM/iCESM. For example, compare simulations in Brady et al. (2019) (fully coupled iCESM1) to Nusbaumer et al. (2017) (iCAM5/iCLM4) and Fiorella et al. (2021)(iCAM6/land bucket scheme).

Our data is drawn from the fully coupled model (iCESM1), and we will include the citation for this model in the bias specification.

L. 439-441: It may be worth noting here the impact a 2° grid may have on the ability of a model to resolve complex topography.

Thank you for this suggestion; we will include grid resolution as being a source of uncertainty in the model-derived EOF calculations.

L. 451-453: Perhaps the models are incorrect here, but another possibility is that this is subgrid variability that cannot be resolved.

We agree that given the coarse resolution of the climate models, there are certainly subgrid-scale processes which may suffer from inadequate parameterizations. Indeed, Rojas et al. (2016) have documented that the PMIP ensemble for the LM simulates a circulation response consistent with the imposed LIA forcing over tropical South America, but that these circulation changes do not translate into precipitation changes, suggesting problems with implementation of feedbacks in the models or that the models may be too dependent on microphysics and convective parameterization schemes. We will provide more specific detail about these areas for model improvement in our revised manuscript.

However, it is worth noting that these sub-grid scale processes generally contribute to the noise that is unique to each paleorecord. Because the MCEOF extracts the fraction of the variance that is common to all records, synthesis of the coherent variability by the EOF analysis isolates the signal from the noise and we do not expect the key results to be impacted.

Technical Corrections

L. 142-3: I would suggest providing links here for both data sources for consistency.

Unfortunately, there is no direct link to the São Paolo Geosciences speleothem database. We will provide a contact in the data availability section.

L. 334 – "EOF" should be "EOF2" here I think?

Yes, we will make this correction. Thank you.

Figures:

- *Figure 3 is really hard to read. Could it be made significantly larger?*

We agree that this figure is small, but we feel it is important to maintain this panel structure in a single figure. In the online version of the published article, readers will be able to zoom into the figure and thus see the results in high-resolution. We hope this will provide for sufficient detail for the readers.

- *Consistency with labeling the proxy sites – these sites are labeled in Fig. 2 and 4, but not Fig. 3 and 5. These plots might be easier to read if these sites were labeled in Fig. 1 (since this allows you the most room), but perhaps omitted in Fig. 2 and 4.*

We agree that the labeling was not applied in a consistent way and that plots would be easier to read if only applied in Figure 1. We will apply this correction in the revised manuscript.

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