

Clim. Past Discuss., referee comment RC1
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Comment on cp-2022-6

Anonymous Referee #1

Referee 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-RC1>, 2022

The authors use a Monte Carlo technique, published and new stalagmite records, and isotope enable modeling to evaluate variability in the South American Summer Monsoon over the last millenium. In general, I think that papers like this, which take a holistic approach to evaluating all available data are very valuable. The findings of this paper are quite interesting. However, I see a few major things that should be included/revised.

1. The connection between the SASM and the SACZ is overemphasized at a cost to explaining the variability in the Bolivian High/Nordeste Low structure, its influence over the records that are used in this paper, and the causes of the variability in this circulation system over the LM.
2. There is a lot more discussion and explanation needed on what exactly the authors did in the methods section. I found myself unable to follow the development of the statistical tests and the development of the pseudoproxy network. I read the papers the authors refer to here and am still confused as to what the authors in this paper did. I think the reader woud be greatly aided if a clear summary can be presented in the methods section.
3. The results and the discussion should be more clearly separated. In the results section, there is a lot of qualifying. Clear separation would make it easier to read.

The rest of my concerns are laid out in line item format:

Line 25: The authors state: 'Model analyses suggests that the local isotopic composition is primarily a reflection of an upstream rainout processes.' I missed the full explanation of

how the authors came to this conclusion based on the analysis. (It's possible I just misinterpreted something). However, a clearer explanation of this would be interesting.

Line 28: "The monsoon was intensified during the LIA over the central and western parts of tropical South America and the South Atlantic Convergence Zone (SACZ)" based on the analysis of the model output? The proxy compilation? Both?

Line 34: Later in the text the authors mention that this is not true everywhere in the SAMS domain. It might be helpful if the authors show precipitation data from their sites in a way that the readers can see the seasonal cycle and if that is true everywhere.

Line 49: This first sentence is confusing.

Introduction: I'm surprised that the authors didn't cite the work of Wong et al., 2021. This work argues for another driver on the SACZ and may be a good reason to do the kind of work that the authors outline here, good motivation for this study.

Line 74: "The current interpretation of isotopic signatures in South American paleorecords..." The 'therefore' is used incorrectly. More importantly, I think it is delicate, but the current interpretations of the records from this region don't necessarily allow for the Rayleigh model, the records are interpreted assuming that model is true.

Line 123: The phrase "rule-of-thumb" has a difficult and potentially offensive history. I would encourage the authors to consider a different phrase.

Lines 130-135: It is difficult to understand what the authors did with the proxy records in order to complete the statistical assessments. What exactly does it mean for records to be "spliced together" What does it mean for the samples to be resampled to annual resolution (using what? how? which samples?). What does it mean for samples to be truncated (again, which ones?). If the results from this work are interpreted, then readers need to be able to follow how these proxy records were reconfigured, as it could impact the results.

Line 190: "Isotopic measurements excluded from previously published analyses were likewise excluded here (SBE3, PIM4), as were individual data points exceeding 3 standard deviations (MV30). Multiple isotopic measurements from a single depth were averaged to establish one value per depth (PAL3)." Do the parentheses in this sentence mean that these things were only done to the records in that parentheses? This is very difficult to follow. Furthermore, I'm concerned that these modifications weren't in the original publications of these records. If they weren't, then the authors should be very explicit

about what and why they did these things.

Section 2.3: Gap interpolation. It would be good for the reader if you said why you need to do gap interpolation. However, I would encourage some transparency from the authors regarding how long the gaps were in the records, how many datapoints in the end (maybe as a percentage of overall datapoints) were derived from the gap interpolation... etc. On line 200, the authors mention bias-corrected. I am not sure what that means in this context.

Line 205: Do the authors mean that each of these speleothem records listed are composites? Or are all of these listed speleothem records combined for this analysis?

Section 2.5: I'm assuming (possibly naively) that the authors are using outputs from previous modeling efforts. If that's true, clarity on the point that these are previously published outputs would be useful. If I'm wrong (apologies) then greater detail on how these models were spun up and what biases may be included in them would be useful.

Line 224: "they rely on somewhat different forcing reconstructions." Please explain.

Line 231 (on pseudoproxy experiment approach): Further information on what and why this reconstruction technique was used is imperative. My reading of Smerdon 2012 is for the ultimate goal of trying to understand how far away from an input we get through our reconstruction techniques. Not as a way to compare climate models to proxy measurements.

Line 243: "sub-grid scale processes" Like what? And how much does it impact the results? If it doesn't, why not?

Line 248: define PPE

Line 259: "a commonly employed proxy..." needs citation.

The figure captions throughout the text need much more explanation. All of the dots are correlations, between what and what? All of the "same as in" phrases in figure captions make it difficult on the reader to reconstruct what is happening.

Line 280-282: An example of the needed separation between results and discussion: "This mode is interpreted as representative of the isotopic variability in the core monsoon region and is shown to vary on centennial timescales." Additionally, I would like to see more explanation on why the result is interpreted that way. Just not in the results.

Line 296: There are two thoughts in this first sentence. Separate into what the results show and then the caveats.

Fig. 3: so the graphs on the far left, (f), (g) those are the pseudoproxy values?

Section 3.2.2 I don't understand why these are being compared to each other. In my reading of the methods - the pseudoproxies are just the climate models with some white noise.. .so of course they agree well?

Line 384: I think it would help the reader if the authors explained the connection between OLR and rainfall. Then rainfall and depleted d18O, then depleted d18O and the stalagmite records.

Line 394: I don't quite see how the authors got to this conclusion: "SACZ activity within the dipole structure suggested by MCEOF2, and underscored by the precipitation dependence on OLR, is a function of the SAMS strength" based on the authors discussion of the data here. I see it in the results, but it's hard to follow how that connection is made in the text.

Line 410: "This was therefore a period when the SAMS was enhanced overall and both the ITCZ and the SACZ were displaced to the south of their mean locations" Again, I don't quite understand how this conclusion was derived following the in-text discussion.

The paragraph starting on Line 414: OK... does this play a role? does it add support to your hypothesis? refute it? why is this paragraph summarizing these things here?

Line 430: "Those records are more sensitive to large-scale circulation changes and related non-monsoonal influences outside the mature monsoon season (DJF)." What is this based on?

Discussion starting on line 477: Scientifically, it's important that the authors keep in mind that the anomalies aren't significant, according to Fig. 5, pretty much everywhere there is a proxy record. I bring this up because it seems like the authors interpret the results as if there is a significant anomaly.

Line 478: strike "surprisingly"

Line 481: I agree that topography presents a challenge to models, but I don't understand why this site (Boto) has trouble and the other sites along the Andes do not. Or should we question the correlations at any of the sites a long the Andes?