Reply to Reviewer 1 Comments: South American Monsoon variability over the last millennium in paleoclimate records and isotope-enabled climate models.

We are grateful for the reviewer's comments on manuscript cp-2022-6. In the revised manuscript, we will include a broader discussion of the first mode of variability, separate more clearly the results and discussion sections, and improve the clarity of the methods used in the record composites and statistical analysis.

As a general response to the comments regarding unclear statements, we agree that clear communication of the science is important. We will make a thorough effort in the revised manuscript to use accessible language and appreciate RC1 for highlighting sections that could be clarified. However, in some instances we have made the choice to reject comments calling for clarity as we feel the processes described are sufficiently well explained, albeit with discipline-specific language.

We addressed the plain text reviewer comments below in **bold text**.

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.

We will include more detail regarding the links between the Bolivian High – Nordeste Low (BH-NL) system and the variability of the oxygen isotope record network. We will further expand upon the potential for PC1 to be a useful index of large-scale SASM variability: as the strength of the monsoon convection changes, so too will both the response of the BH-NL system and the variability of isotopic signal that is linked to the SASM. Released latent heat of condensation during the strong convection that characterizes the monsoon rainfall generates a Rossby wave response (Lenters and Cook, 1997). This establishes a strong correspondence between activity of the BH-NL system and the isotopic fraction that results from monsoon rainfall across the continent.

Unfortunately, the lack of low-frequency variability in the isotope-enabled climate models precludes a close examination of the causes of variability responsible for the observed variability of the first mode over time. However, our interpretation is consistent with the well-documented relationship between interhemispheric temperature gradients, latitudinal ITCZ displacements

and SASM intensity changes on decadal to centennial timescales (Bird et al., 2011; Vuille et al 2012; Lechleitner et al., 2017; Roldán-Gómez et al., 2022; Steinman et al., 2022).

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 would be greatly aided if a clear summary can be presented in the methods section.

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

Specific concerns regarding clarity of the methods are addressed in detail in the line item comments below.

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.

Thank you for this suggestion. We will revise the results section to more clearly describing only the results of the analysis, while the interpretation of these results will be reserved for the discussion section. Please see more detailed discussion of this aspect in the line item comments below.

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.

We will clarify this conclusion to more clearly state that the results highlighting the role of upstream processes in the depletion of the isotopic records are derived from analysis of the Little Ice Age (LIA) and Medieval Climate Anomaly (MCA) period. We do not elaborate on the importance of the upstream rainout process as a mechanism driving the fractionation of oxygen isotope proxies as recorded in archives from within the SASM domain; rather we rely on the existing published literature that highlights this mechanism across the monsoon domain in both observations and isotope-enabled climate models (Grootes et al., 1989; Vimeux et al., 2005; Vuille and Werner, 2005; Vuille et al., 2012; Ampuero et al., 2020).

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?

This ambiguity will be clarified in the revised manuscript. Both the model analysis and proxy data show an intensification of the SASM during the LIA. The proxy data show a notable negative $\delta^{18}O$ excursion during the LIA as identified by PC1 (Figure 2). The model analysis of the LIA period (Figure 5) reveals anomalous rising motion upstream of the monsoon domain, as well as more negative $\delta^{18}O$ values and enhanced rainfall.

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.

We wish to emphasize that the SASM is a highly seasonal phenomenon that is non-existent during the boreal fall and winter. A seasonal analysis therefore makes little sense given that the proxy records used in this study are driven to a first order by the climate climate signal within the monsoon wet season. This is precisely why this analysis, as is common practice, focuses exclusively on the mature monsoon season (DJF) as shown in Figure 1. Furthermore, both the original publications of the proxy records used in the network analysis (cited in Table 1) and multi-proxy synthesis studies (Vuille et al., 2012; Deininger et al., 2019; Campos et al., 2019) document that all records within this domain do indeed reflect variability of the SASM.

Line 49: This first sentence is confusing.

We believe this sentence is fairly straightforward and maintains fidelity to discipline-specific language. Here we describe how the latent heat released during the phase transition from water vapor to cloud droplet leads to an anomalous heating of the mid/upper troposphere. This thermodynamic change leads to a dynamic response via the establishment of the Bolivian High – Nordeste Low system. We choose not to elaborate in too much detail on the fundamental atmospheric dynamics, but instead favor a focus on the isotopic variability and related processes in South America. The reader is also referred to key literature on this topic, such as Lenters and Cook (1997) and Chen et al. (1999). We therefore do not plan to change this sentence in the revised manuscript.

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.

We appreciate being pointed to this paper, but after reviewing it, we find its focus is on the variability of the SACZ during the mid-late Holocene and thus concerns a different timescale than the decadal-to-centennial variability that is the focus of our manuscript. We also wish to

reiterate that our analysis is entirely consistent with Novello et al. (2018), who explore a transect of $\delta^{18}O$ records across the core of the SACZ and unequivocally show past variations in the location of the SACZ axis during the last two millennia.

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.

We will remove the word "therefore" from the sentence.

Our interpretation of Rayleigh distillation as the primary process driving isotopic fractionation of paleorecords from across the SASM domain draws on a wide range of literature as we have discussed in our previous responses. We nevertheless do appreciate that, in addition to the influence of upstream rainout and Rayleigh distillation, records are influenced by other processes, such as the amount effect in southeastern Brazil (Moquet et al., 2016; Ampuero et al., 2020) and microphysical processes involved in convective activity (Samuels-Crow et al., 2014). The various fractionation processes that influence South American oxygen isotopic variability are discussed in the submitted manuscript (Lines 74-83) and will be expanded upon in the revised manuscript.

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

We understand the reviewers' concern about this phrase but fear it may be misplaced. The history of this expression is often misinterpreted as having an offensive history, while in reality no such connection exists (see: https://en.wikipedia.org/wiki/Rule_of_thumb). We nevertheless acknowledge that the misunderstanding around the etymology of this phrase seems to be common and therefore wish to avoid any unintentional association with the history some believe it invokes. We thus will modify the language to use the phrase "North's rule."

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, than readers need to be able to follow how these proxy records were reconfigured, as it could impact the results.

We have made the code for this procedure publicly available for any reader wishing to understand the granularity of the process and/or replicate the process. Please note that section 2.1 is intended only to provide a brief overview of the statistical procedure, which is then

described in much more detail in the later sections (2.2-2.4) for those readers interested in the specific details of the method.

We have revised the methods section to be clearer in response to the reviewer's comment. The relevant section of the manuscript is elaborated upon below for the specific questions that are raised by the reviewer.

We select fourteen oxygen isotope records, which all capture variability of the SASM. Using the age models and age model uncertainty of each of the fourteen records, we calculate ensembles of age models that contain 1,000 members using a Monte Carlo resampling. A unique oxygen isotope record is calculated for each age model ensemble member, generating a subsequent 1,000-member ensemble of oxygen isotope networks. To generate the modes of variability, an Empirical Orthogonal Function decomposition is calculated for each ensemble member of the oxygen isotope network and the median of the spatial and temporal modes are presented for analysis.

Due to the fact that not all records cover the entire period of study, we follow the methodology discussed in the published literature when merging discrete samples into composite records, referred to in our manuscript as "spliced together" records. We will clarify this language and more specifically refer to procedures from the published literature (Campos et al., 2019; Novello et al., 2021). Compositing discrete samples into continuous records is necessary in order to meet the criteria of continuous data coverage required for the MCEOF analysis. In all cases (save for MV1 and MV30 which are unpublished) the compositing performed in our analysis is based on samples that were composited when first published.

'Resampling' and 'truncation,' refer to small processing steps we conducted for the oxygen isotope time series. The 'resampling' of samples to annual resolution is a statistical interpolation of the oxygen isotope data to a common resolution of one data point per year. This was done for the approximately annual sections of the Pumacocha record and the Pau D'Alho (ALHO6) sample that contain irregularly dated samples not corresponding to rounded calendar years. This procedure was also applied to those records which had a sampling resolution between two and seven years (PAR, PAL, P00-H1, MV, PIM4, DV2, SBE3+SMT5, TM0, Boto, JAR, and CR1). Thef expression 'truncation' indicates that we are only using data during the common period 850 – 1850 CE. We will clarify these points in the revised manuscript. This truncation was necessary, as the MCEOF analysis requires that all records share a common time period.

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.

We will modify this section to clarify that the analysis of raw samples is consistent with the analysis in the original publication of the data and instead refer the reader to the published literature. While we use the same procedure as in the original publications, we nonetheless do not use the published isotopic time series. We instead use the raw dates and raw isotopic data in order to establish new age models and corresponding ensembles of isotopic time series based on the Monte Carlo resampling techniques.

Our use of parenthesized sample names serves to specify which data sets have been modified by the stated procedure as was noted by R1. We find this to be consistent with currently published literature and to be grammatically correct.

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 transparancy 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.

The application of the MCEOF analysis requires proxy time series data to be continuous and regularly spaced (Björnsson and Venegas, 1997; Anchukaitis and Tierney, 2013). We will clarify this further in the revised manuscript.

The gap filling was performed for three of the records. For PIM, the hiatus is approximately 100 years, or 10% of the data, across all the ensemble members for this record. This gap is reported in Della Libera et al. (2022).

For SBE, the gap is not reported in the original publication, but is evident from the discontinuity in the published data and as a gap in Figure 3 in the original publication (Novello et al., 2018). This gap is approximately 25 years, or 2.5% of the data, across all the ensemble members for this record.

For MV, the gap is very small, approximately 4 years, or 0.4% of data, averaging across all the ensemble members for this record. Upon further review of the bias-correction, we find this step does not impact our result. It has been removed from the methods and both the code and manuscript will be updated to reflect this change.

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?

The records listed are composites. They are composited in the original publications and the raw samples are also composited for the purposes of this study. We name the records used in this analysis MV, JAR, PAR, SBE3+SMT5, PAL, and Boto, and the samples used in each of these composites is named in lines 124-129.

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.

Thank you for this suggestion. The model output used in this research is from previously published research. We will make this more explicitly clear at the beginning of this section.

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

These forcing reconstructions are part of a suite of climate forcings developed for the Last Millennium simulations used in the Paleoclimate Modeling Intercomparison Project – Phase 3 (PMIP3) (Schmidt et al., 2011). The PMIP3 experimental setup includes a number of alternative climate forcing histories from which modeling groups could select particular forcings to be used in their own simulations. Removing the constraint that all modeling groups use the same climate forcings but imposing strict constraints on model configuration allowed PMIP3 simulations to test reconstruction uncertainty in a coherent modeling environment.

Because the discussion and comparison of different reconstructions in a coherent model environment is not the focus of this study, we will remove this phrase from the manuscript. We will retain the reference to the list of forcing datasets, which includes a justification for how the climate forcing database was constructed.

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.

Our use of the pseudoproxy experiment approach to evaluate the signal-to-noise statistical profiles of proxy data is based on examples used in past literature (Smerdon et al., 2016; Yun et al., 2021). We apply the pseudoproxy experiment (PPE) approach as an independent way to evaluate the signal to noise ratios characteristic of the paleoclimate network in South America. The basis of the PPE is drawn from latitude-longitude points within the grid space of the climate model simulation and by adding white noise to the climate model data and generating PPE-based results, we establish the basis for an apples-to-apples comparison that translates between proxy-space and model-space. The PPE is a filter for analyzing the variability of the climate signal in both the models and paleorecord network as well as the noise inherent to both the real-world climate and the model simulations of the LM.

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

In the model experiments used in this analysis, the horizontal resolution of the grid cells is ~ 2°, or ~ 222 km. Sub-grid scale processes refer to those processes operating on the spatial scale of less than one model grid cell. Because only a single averaged value is provided in the model output for a single grid cell, this averaged value obscures local conditions that function on smaller spatial scales than ~222 km. Some of these features are discussed in the introduction and include thermodynamic and microphysical processes such as condensation or dry air entrainment into clouds.

There are certainly sub-grid scale processes that are not well resolved in climate models. 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.

Line 248: define PPE

We will introduce the 'PPE' abbreviation when the concept is first mentioned in section 2.5.

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

We disagree that this statement on the relationship between outgoing longwave radiation and tropical convection requires an additional citation. The citation provided for the dataset (Liebmann and Smith, 1996) discusses this product based on the premise that estimates of outgoing longwave radiation are useful for identifying areas of deep tropical convection. The citation for the dataset itself has been sufficient in a number of other publications (Jones and Carvalho, 2002; Espinoza et al., 2021) and this relationship has furthermore been referred to in numerous publications without further citations (Coelho et al., 2021; Grimm et al., 2021; Zhang et al., 2021; Zili et al., 2021).

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.

The magnitude of the dots for a given mode in Figure 2 represent the correlations between local proxy records and the Principle Component time series of that mode. This will be clarified when the figure is introduced in the text and in the caption of Figure 2. After some consideration, however, we prefer to retain the "same as in" construction for the sake of brevity. This is standard practice, significantly reduces the amount of text in the caption and the other text modifications will help clarify the figure content to the reader.

We will streamline the Figure 3 caption.

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.

We will rephrase this dynamic interpretation as a description of the results and expand further on the interpretation of the first mode in the discussion in section 4.1.1. Our interpretation of the first mode as representing the isotopic variability in the core monsoon region is because the highest loadings in EOF1 are associated with proxy records located in this core region. The centennial variability of the mode is apparent from PC1.

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

The sentence will be modified to move the caveats to the Discussion, section 4.2.1.

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

Yes, Figure 3f,g are the pseudoproxy values. We will articulate this more clearly in the figure caption.

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?

The pseudoproxy modes are derived from a discretely sampled number of locations corresponding to paleorecord sites that are also perturbed with noise. The agreement between the pseudoproxy modes and the spatially continuous climate model modes is not obvious a priori. We do not presume any coherency of the time series sampled from discrete points in the climate model space during the construction of the pseudoproxy time series. It is only after the EOF analysis of the network of these discrete points is performed that the modes emerge and are found to share similarities with both the proxy network and the spatially continuous EOF approach from the model data. The shared similarity of the modes of these three data sets

confirms that the heterogeneous spatial distribution of the proxy network is able to capture a coherent signal across the domain that is shared by a heterogeneous (and continuous) dataset.

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.

We will provide a more detailed interpretation of the dynamic relationships in this analysis.

The anti-correlation between OLR and rainfall in the tropics is well established: as noted in the earlier comments, OLR is a commonly employed metric for deep tropical convection – more negative values of OLR indicate colder cloud-top temperatures resulting from stronger convection. Stronger convection derives from enhanced vertical motion, which forces condensation of water vapor and thus, enhances rainfall.

The rainfall is related to changes in $\delta^{18}O$ in the ways that have been previously described, primarily depletion via Rayleigh distillation along the moisture transport pathway in the upstream region of the monsoon (in our analysis, particularly upstream of the tropical Andes) and a mixture of path-dependent distillation and amount-effect driven distillation within the SACZ region. A variety of microphysical processes linked to tropical convective activity influence the downstream delta values of precipitation, however, these processes modify the fractionation along the moisture trajectory and rainout which primarily corresponds to the Rayleigh distillation model.

The isotopic composition of rainfall is recorded in stalagmite records as it infiltrates karstic caves, dripwater degasses and calcite precipitates in the cave environment. Multi-year monitoring at most cave sites indicates that the isotopic composition of rainfall outside the caves is identical with the one of cave drip water and fresh calcite deposition (Moquet et al., 2016). Calibration and modeling work also shows that the isotopic composition of lake calcite at Laguna Pumacocha and of ice on the Quelccaya ice cap are faithful recorders of the isotopic composition of precipitation falling at these sites (Bird et al., 2011; Hurley et al., 2016).

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.

This sentence will be clarified in the revised text.

We also agree that the link between SACZ activity and monsoon strength is strongly supported by the results. We interpret the proxy-derived MCEOF2 as a dipole structure based on the anti-correlation between records to the northeast of the SACZ core (TM0, SBE3+SMT5, DV2, MV) and records to the southwest of the SACZ core (ALHO6 and CR1). We see this as clear evidence of a dipole within the decomposition of the isotopic records in the monsoon domain

on the time-scales of this study. In the below comment we describe in greater detail the links between the monsoon strength and dipole activity.

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.

In this sentence we have combined our results with the discussion of current literature, which documents a southerly displacement of the ITCZ during the LIA (Lechleitner et al., 2017; Steinmann et al., 2022). We will separate our results from this discussion of published literature in the revised manuscript.

Our results show a clear departure of the proxy-derived modes from their mean state. Both the departure seen in PC1 during the LIA and the dominant loadings over the core monsoon region are a clear indication of the enhancement of the SASM. The enhancement of the SACZ mode is similarly documented by the negative excursion of PC2 during the LIA together with the corresponding dipole of the proxy loadings in the SACZ region.

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 summarzing these things here?

We highlighted these other factors influencing SACZ variability to provide a more complete perspective, but we agree that much of this discussion is beyond the scope of this study. We will remove this paragraph in the revised manuscript.

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?

Records located at sites distal from the core of the monsoon domain are more sensitive to influences from other systems that border the monsoon domain, thereby providing potential pathways for moisture from alternative sources other than the SASM to reach these proxy sites. For example, in the original publication of the Boto record (Apaéstegui et al., 2018 and references therein) the South Atlantic is mentioned as a possible source of moisture during the MCA period. Similarly, in the original publication of the PAR record (Wang et al., 2017), latitudinal shifts in the location of the ITCZ were discussed as having influenced this low-latitude record in response to insolation changes.

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.

Our interpretation of Figure 5 is primarily focused on the significance of upstream convection relative to the changes of the $\delta^{18}O$ values measured in the isotopic proxy records

during the MCA and LIA. The large-scale changes in upstream convective activity seen during the MCA and LIA in the models is consistent with our MCEOF1/EOF1 pattern representing the core monsoon circulation and the corresponding changes in strength present in PC1.

The lack of broad statistical significance in the modeled isotopic and precipitation changes is consistent with our discussion of the challenges models face in responding to external forcings. We also clearly state in our interpretation that isotopic variability is not primarily a response to an amount effect but rather a response to changes in upstream 500 hPa vertical motion. Thus we do not expect to see co-location of significance between $\delta^{18}O$ and precipitation anomalies.

Line 478: strike "surprisingly"

Done.

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 along the Andes?

We may not have correctly phrased this, but we wish to point out that the challenge in correctly simulating the climatic controls over the Boto site is not primarily a topographical argument. Modern climatological studies show that different modes of SASM precipitation can produce significant spatial variability between the northern and southern part of the central Andes at interannual timescales (Vuille and Keimig, 2004). The southwestern margins of the central Andes show considerable spatial heterogeneities in relation to records from the core of the Andes region. This feature has been observed in other reconstructions (e.g. Apaéstegui et al., 2018; Jara et al., 2020; Kock et al., 2020), which argues for the influence of the South Atlantic Ocean delivering moisture to the continent and influencing precipitation patterns over this region in South America. It has also been stated in Campos et al. (2019) that increased Atlantic sea surface temperatures will weaken the SACZ and promote the intrusion of extratropical cold fronts through the la Plata basin. In fact, both conditions represent two different, although not mutually exclusive, mechanisms that could explain some of the differences found between this and other Andean sites and determine the spatial configuration of SASM precipitation anomalies revealed by the paleoclimate records discussed here. More detailed discussion about the significance of the Boto record can be found in the original publication of this record (Apaéstegui et al., 2018).

Also, please note that this regional anomaly is limited to the MCA period only (Fig. 5), as proxy data and model simulations are in agreement during the LIA. Hence, we believe that the change in moisture transport to this distal site during the MCA constitutes the main challenge for the climate models. We do not call into question the model representation at the other Andean sites, given that the challenge is not primarily due to topography, but rather related to the interactions between tropical and extratropical climate that influence this transition region of the

SASM. Indeed, during both MCA and LIA the other Andean records show good agreement with the model data. This will be clarified in the revised manuscript.

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Reply to Reviewer 2 Comments: South American Monsoon variability over the last millennium in paleoclimate records and isotope-enabled climate models.

We are grateful for the reviewer's comments on manuscript cp-2022-6. We addressed the plain text 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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