Comments on “Spatiotemporal ITCZ dynamics during the last three millennia in Northeastern Brazil and related impacts in modern human history.”

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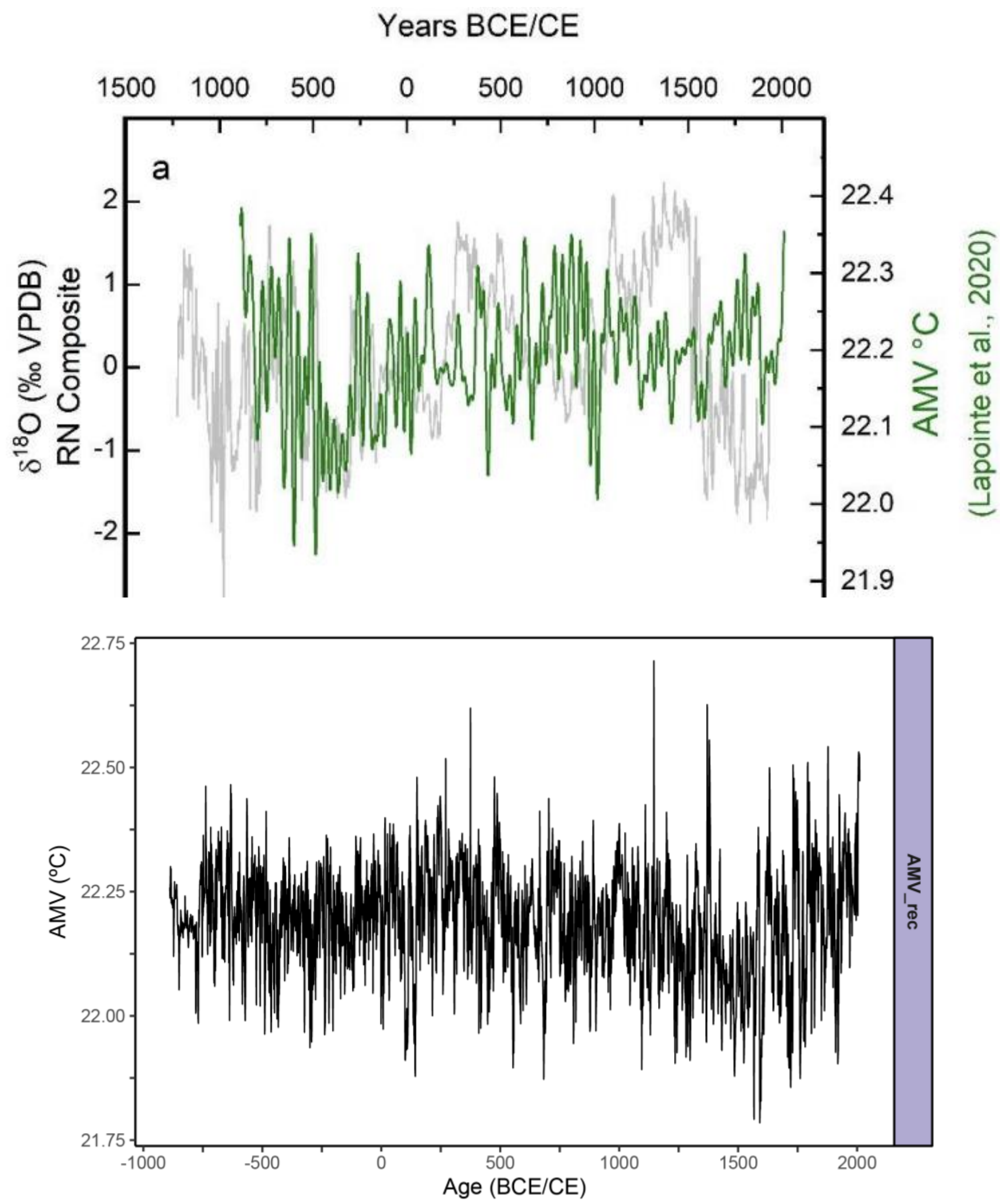
This is an interesting study that uses speleothem $\delta^{18}O$ and $\delta^{13}C$ records to characterize the nuanced behavior of the ITCZ/tropical rain belt and its impact on the regional hydroclimate (i.e., precipitation variability) of Nordeste and eastern Amazona during the late Holocene. The main objective of this study is to improve the interpretation of late Holocene ITCZ dynamics in the South American tropics, which may help to better our understanding of past SASM variability. Additionally, their interpretation of RN $\delta^{18}O$ as a recorder of extreme dry events during the last 500 years has archeological and societal implications. This manuscript presents several thought-provoking and novel ideas pertaining to Atlantic and Pacific impacts on ITCZ-related precipitation during the late Holocene, which have the potential to reconcile paleoclimate records from Nordeste and Amazonia. Overall, this study also has the potential to be an excellent contribution to the field of South American paleoclimatology. However, I find that the manuscript (in its present state) has several major issues, which require further consideration, detail, and development before it should be accepted for publication. As such, I would recommend major revisions of the manuscript before final acceptance.

Major issues:

1. **I am concerned that the AMV reconstruction presented in figure 5 (also referenced in the main text) is misleading. Specifically:**

Figure 5 (and lines 451–454): It is true that the presented AMV time series and the RN composite $\delta^{18}O$ time series look similar, but it is unclear what the authors are plotting. The green time series in figure 5 (shown below, top figure) does not look like the AMV reconstruction from Lapointe et al. (2020) (shown below, bottom figure)—raw data from https://www.ncei.noaa.gov/access/paleo-search/study/31353. The full range of values from the Lapointe dataset is 21.7–22.7, while the reconstruction shown in figure 5 only appears to be from 21.95 to 22.40.



Perhaps the authors plotted a different reconstruction of the AMV and used the wrong citation? Or perhaps it is the reconstruction from Lapointe et al. (2020) but downsampled (if so, the authors need to make this clear in the methods or supplementary information)?

2. **The authors do not sufficiently explain the mechanisms driving the anti-phased behavior observed between the RN composite and Paraíso Cave $\delta^{18}O$ records. Specifically:**

Lines 436–440: It is unclear what is meant by "a zonal behavior of precipitation shifts in the ITCZ domain." Are the authors proposing that RN and Paraíso are in-phase from 250–1100 CE, anti-phased at ~1100 CE, back in-phase from 1100–1500 CE, and then anti-phased again from 1500–1750 CE? The authors should provide more explanation for this behavior.

Additionally, the authors state that "even though the Paraíso and Cariaco sites are located in different hemispheres, the observed in-phase climate relationship during the LIA suggests that their isotopic signatures were both sensitive to the same rainfall changes over northern South America." The Cariaco record is not an isotope-based record. Rather, it is a bulk titanium % record. The wording of this sentence should be changed accordingly.

Lines 446–451: Here, the authors discuss the AMV and ITCZ displacement during a warm AMV. However, the authors have not defined what a warm AMV is, albeit the reader could find out in the cited studies. I recommend the authors specifically define the AMV in detail, and make clear what is meant by a warm vs cold AMV.

Lines 461–463: The authors state, "Our analysis corroborates with this and points to increasing precipitation over N-NEB and decreasing precipitation over eastern Amazon, between 1500–1750 CE, when both AMV and PDV are in cold phase (Fig 4)." There is no reference to the PDV in figure 4, nor has the PDV been described/defined yet at this point in the text. No PDV reconstructions are provided in any of the figures, and the provided AMV reconstruction is in figure 5, not figure 4. Last millennium SST gradients from Steinman et al. (2022) are provided in figure 5, but they are not PDV or AMV reconstructions. I recommend either including a PDV reconstruction in one of the figures, or to remove this text from the manuscript.

Lines 463–465: The authors state, "This sign reversal is assigned to perturbations of the regional Walker cell's produced by teleconnection between the Atlantic and Pacific (Kayano et al., 2022, He et al., 2021)." I find this explanation to be vague, and recommend that the authors provide a clearer and more detailed explanation for the sign reversal. What does "perturbations of the regional Walker cell's" mean exactly? What teleconnections are the authors referring to, and what are the mechanisms driving the aforementioned perturbations?

3. **The conclusion and abstract both discuss ITCZ dynamics forced by the AMV and PDV, including position, intensity, and width. However, in the main text, the authors do not sufficiently explain which dynamical aspect of the ITCZ responds to different AMV/PDV phases, nor do they explain any mechanism(s) behind the AMV/PDV forcing. Specifically:**

Lines 570–577: In this paragraph, the authors suggest that during the last millennia, ITCZ dynamics cannot be explained solely by north-south ITCZ migrations or one single forcing mechanism. They propose a zonally non-uniform behavior of the ITCZ during times when the RN record is anti-phased with the Paraíso cave record—forced by the interactions between the AMV and PDV modes that changed the regional Walker cell position and ITCZ intensity/width.

However, the authors never really attributed the anti-phased behavior between N-NEB and eastern Amazonia to the differential AMV/PDV phases. They discussed observed precipitation anomalies during overlapping periods of AMV and PDV phases in the modern, and suggested that it could be responsible for the observed anti-phased behavior. However, they never directly compared the speleothem time series with AMV and PDV reconstructions. Nor did the authors propose a detailed mechanism for how different AMV/PDV phases impact ITCZ width/intensity, despite changes in ITCZ width/intensity also being mentioned in the abstract (lines 46–50). In addition, the authors did not really describe when the ITCZ may have expanded/contracted or became weaker/stronger (aside from stating that this may have happened when the RN composite record and Paraíso are anti-phased). Ultimately, they never describe mechanism(s) for 1) how different AMV/PDV phases impact ITCZ dynamics, 2) how changes in ITCZ width/intensity may cause the observed anti-phased behavior, and 3) how the regional Walker cell position is forced by different AMV/PDV phases. I recommend that the authors provide more detail to this part of the Conclusions and Discussion sections overall, and propose/explain specific mechanisms that can reconcile the observed hydroclimate variability in N-NEB and eastern Amazonia.

Additional note: The authors should be extremely clear when generally discussing ITCZ width/intensity. What exactly do the authors mean by ITCZ width? Is it the width of the actual band of deep convection? Width of the seasonal range of the ITCZ? These terms should be explicitly defined early in the manuscript. Some papers that may be useful to reference include Donohoe et al. (2013), Atwood et al. (2020), Byrne and Schneider (2016), and Roberts et al. (2017).

Additional comments and concerns:

Lines 89–92: The authors cite Lechleitner et al. (2019), but I believe the correct citation is Lechleitner et al. (2017). Additionally, another relevant citation that may be relevant and could be included here is Asmerom et al. (2020) published in Science.

Lines 95–102: The authors call out the SASM and the ITCZ here as focus points of recent studies on tropical South American precipitation, but have not mentioned the South Atlantic Convergence Zone (SACZ). While not explicitly relevant to their findings, the SACZ should at least be mentioned here because of its important relationship with the SASM and ITCZ, and because it has been the topic of several recent paleoclimate and modern precipitation studies (Novello et al., 2018; Nielsen et al., 2019; Zilli et al., 2019; Wong et al., 2021).

Figure 1: It may help the reader to include annotations in the figure, including labeling the core SASM domain, ITCZ location, SACZ, etc. Additionally, while I understand the choice to include austral autumn precipitation climatology (when N-NEB receives most of its precipitation), it may be worthwhile to include panels with precipitation climatology for the austral winter and spring (either added to figure 1 or included in the supplement). This would allow for the reader to visually assess the spatiotemporal dynamics of the ITCZ, SASM, and SACZ, and how precipitation varies at sites 1–4 during the different seasons.

Lines 165–174: Figure S1 receives a lot of attention in this paragraph, and should probably be included as a main text figure. Alternatively, it could be incorporated into an existing main figure.

Figure 2: Readers who are green-red colorblind will not be able to see the small green dots (that denote the location of the GNIP stations) in any of the panels. I recommend changing the color to black and potentially increasing the size of the dots.

Lines 362–363: It gets confusing when the authors use both before present (BP) dates and before common era/common era (BCE/CE) dates. Additionally, ky has not been defined before this point, so the authors should spell it out before using the abbreviation.

Figure 3: Same red–green issue as mentioned in Figure 2.

Figure 4: It would be extremely helpful for the authors to include vertical bars when referencing specific time periods in the text. Such periods include the LIA, MCA, Bond 2 event, etc. Additionally, the authors reference trends resulting from insolation forcing in the paragraph starting at line 417. The authors should consider including a time series of solar insolation.

Also, the δD record from Boqueirão Lake is relative to VSMOW, not VPDB (Utida et al., 2019). This appears to be a typo and should be changed accordingly.

Lines 389–392: The authors state that from 1060 to 480 BCE, there was increased precipitation in N-NEB as suggested by negative $\delta^{18}O$ anomalies. But it is unclear what the authors mean by ‘increased precipitation’. During this time, there is multidecadal variability in the RN composite $\delta^{18}O$ record, but no clear/obvious trend between 1060 and 480 BCE. Perhaps the authors meant that there was increased precipitation *relative* to another part of the record. I would recommend clearing this up.

Lines 408–409: The authors reference the δD record from Boqueirão Lake, and the same record is shown in figure 4. However, the authors describe the record as a “δD lipids” record. Lipids are a broad group of molecules which include waxes, glycerides, terpenoids, tetrapyrrole pigments, etc. The authors should be more specific, and should reference the record as a leaf wax δD record of *n*-C_{28} alkanoic acids from Boqueirão Lake sediments (hereinafter referred to as δD lipids).

Lines 495–497: The authors focus their discussion of extreme dry events recorded in the TRA5 $\delta^{18}O$ record between 1500 and 1850 CE. However, it is unclear why the authors do not discuss dry events/distinct $\delta^{18}O$ peaks after 1850 CE, despite their record extending into the 21st century. Is it because the TRA5 speleothem chronology is not as precise during this time?

Lines 518–523 and Figure 6: The authors reference several historical droughts that had severe societal/socioeconomic consequences. It may be helpful to annotate figure 6 to highlight the most severe droughts referenced in the text. The number/letter labeling in figure 6 makes it hard to discern the severity of the droughts by looking at the figure alone.

Lines 533: The authors should provide more detail here. Which Governor are the authors referring to? Governor of what/where?

Figure 6: Why focus on just TRA5? TRA7 and FN1 appear to cover the same period as TRA5. Is TRA5 the only speleothem that records the extreme drought events? Do TRA7 or FN1 record any of the same drought events? If they do not, why would only one speleothem record these drought events and not the others?

It may be helpful to include the age uncertainty in the right panel of the figure under the heading "TRA5". For example, 1546 ± XX. Especially because this figure focuses on only the last 500 years, it would allow the reader to critically compare the speleothem dates to the historical drought dates listed in the column labeled "Historical."

Additionally, I am curious if there is an available archeological record(s) or something similar that could be plotted with the TRA5 $\delta^{18}O$ record. Especially since the authors discuss the societal implications of the extreme droughts in relation to human population and welfare, it would be useful for the reader to visualize the impact through comparison with the speleothem record.

Line 565–567: The authors state, "The N-NEB record presents a trend toward drier conditions as is also being observed in the Diva de Maura Cave in S-NEB, interpreted as an ITCZ withdrawal and SASM weakening, respectively." It is unclear what the authors mean by "ITCZ withdrawal," especially since the authors highlighted the dynamical behavior of the ITCZ earlier in the paper. Is it a withdrawal via mean ITCZ displacement? Contraction or weakening of the ITCZ? More detail here would be helpful for the reader.

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