We are grateful for the reviewer's comments on manuscript cp-2023-2. We addressed the reviewer's comments below in italicized text.

RC2: 'Comment on cp-2023-2', Anonymous Referee #2, 21 Mar 2023

Review „Spatiotemporal ITCZ dynamics during the last three millennia in Northeastern Brazil and related impacts in modern human history" by Utida et al.

I have read with great interest the discussion paper by Utida et al. The authors analyze spatiotemporal ITCZ dynamics during the last three millennia in NE Brazil (NEB), and claim to relate their inferences to modern human history. The study presents partially replicated speleothem proxy records from two caves in NE Brazil, and provide an overview of past (hydro)climate trends and variability in the greater region of NEB of the southern margin of the ITCZ.

This new data set is sound and I have no doubt about the quality of the applied methods and presented data. In principle, the scientific significance is valid, since this dataset complements the northern South American speleothem record in high resolution. However, I have some concerns about the structure and clarity of the manuscript, which I feel needs some improvements before final publication.

Main comments:

- The structure:

I find most of the conclusions concerning ITCZ dynamics intriguing and interesting. However, I found the manuscript sometimes hard to read and some parts of the discussion are not easy to follow. For example, the section in L388ff has a rather unclear structure. The first half seems to be organized chronologically along the time of record, and describes the observed trends. But before this discussion is finished, the discussion jumps to comparing relationships between proxies, and refers to sections of the record which have not been described yet. Later on, the discussion also jumps from describing potential processes back to certain events and forth to other aspects again. I feel like the whole discussion should be carefully restructured and streamlined to build the arguments better on each other, and to provide the reader a common thread throughout the manuscript to prepare and justify your conclusions properly. I suggest to choose a consistent, logic structure, such as building up the discussion more strictly chronologically along your record, and also discuss trends first and events later separately?! Another possibility would be to bring the proxy interpretation first, and then compare to other records and discuss the forcings and consequences… There are several possibilities, but please do not mix it all up…

Thank you for drawing attention to the structure of the manuscript. We will restructure the manuscript according to all reviewer suggestions. Most of the above comments related to structure are addressed in the comments below and we will do our best to improve the general structure of the manuscript after a final revision including all suggestions. As far as the paragraph in L38ff is concerned: we rewrote it and it is presented as part of the comments below.

I also strongly suggest to put special effort in elaborating how the two parts of the discussion (i.e., the paleo-record description, and the discussion of historical droughts) actually build on each other, and better justify why both aspects need to be discussed in one paper. In the current version of the manuscript these appear more as two separated stories.

Thank you for drawing attention to the connection between these paleo-records and historical records. The paleo-precipitation record from Northeast Brazil is important to understand the modern climate and to put it in a long-term historical context. Hence, there is really no separation between the two, just a continuing precipitation history over time, indeed. No observed or reconstructed precipitation record exists for the period prior to and 1850 CE in NE Brazil. The only available information is the historical record of droughts. Hence the speleothem record allows us to put these droughts in a longer-term context and provide a broader spatiotemporal assessment. As far as the possibility to discuss the historical droughts in *another paper is concerned, we believe that we still lack sufficient data for a second paper. The current analysis should really be viewed as a first attempt to compare paleo-precipitation and historical records in this region.*

- U/Th Results description

I miss a proper description of the U/Th results in the main text. This should e.g., comprise U and Th concentrations, uncertainties, Th contamination, description of inversions, etc, … (check Dutton et al. 2017 as a guideline to report U-series data). This is also important due to the presence of both calcite and aragonite, where we would expect an influence on the ages if recrystallization occurred! In addition, a statement concerning the final uncertainties of the age-depth model is essential, also regarding the several outliers. This is particularly relevant when reporting absolute ages for extreme events! From the so presented age models, it is not at all clear if the dating supports an annual precision of a single drought event, or the unequivocal allocation to an event reported in the historic record.

The description of the methods and U/Th results has been revised and will be included in the manuscript according to the text below. The methods were revised to be in accordance with Dutton et al. (2017) suggestions for U/Th series publications.

Section 3: Materials and Methods

Chronological studies on speleothems were based on U-Th geochronology performed at the Laboratories of the Department of Earth and Environmental Sciences, College of Science and Engineering, University of Minnesota (USA), and at the Isotope Laboratory of the Institute of Global Environmental Change, Xi'an Jiaotong University (China), according to Cheng et al. (2013). Subsamples of ~100 mg were obtained in clear layers, close to the growth axis trying to keep a maximum thickness of 1.5 mm, 10 mm wide and no more than 3 mm depth. The powder samples were dissolved in 14 N HNO_3 and spiked with a mixed solution of known ^{233}U (0.78646 ± 0.0002 pmol/g) and ^{229}Th (0.21686 ± 0.0001 pmol/g) concentration. Th and U were co-precipitated with FeCl and separated with Spectra/Gel® Ion Exchange 1x8 resin column with 6N HCl and super clear water, respectively. Th and U were counted in an inductively coupled plasma-mass spectrometer (MC-ICP-MS Thermo-Finnigan NEPTUNE PLUS) and the results were calculated in a standard spreadsheet based on Edwards et al. (1987) and Richards and Dorale (2003) using the isotopic ratios measured, machine parameters and corrections factors to eliminate effects of contamination by detrital Th to finally obtain the age of each sample. The decay constants used are: λ_{238} 1.55125×10^{-10} (Jaffey et al., 1971), λ_{234} 2.82206×10^{-6} and $\lambda_{230} = 9.1705 \times 10^{-6}$ (Cheng et al., 2013). Corrected ^{230}Th ages assume the initial $^{230}Th/^{232}Th$ atomic ratio of $4.4 \pm 2.2 \times 10^{-6}$. Those are the values for a material at secular equilibrium, with the bulk earth $^{232}Th/^{238}U$ value of 3.8 (McDonough and Sun, 1995). The ages are reported in BP (Before Present defined as the year 1950 A.D.) and converted to Common Years (CE). Age uncertainties are 2 σ.

Results and discussions

The results and discussions below regarding ^{232}Th contamination and calcite x aragonite crystallization will be included in the appropriate section of the manuscript, according to the reviewer's suggestion.

The high values of ^{232}Th and low $^{230}Th/^{232}Th$ ratio suggest incorporation of detrital Th transported by the seepage solution to the speleothems, which lead to a higher uncertainty of the age values. Recrystallization of aragonite into calcite might also reduce the U content and given older age for carbonates (Lachniet et al., 2012). These are the main reasons for age inversions along speleothems from Northeast Brazil. Therefore, we analyzed a large number of U/Th ages to improve the age model and reduce the errors associated with detrital Th and recrystallization.

FN1 is partially composed of calcite between the depths of 83 and 128 mm (Table S3), and top and base are composed of aragonite. Overall this stalagmite presents low U concentration and high ^{232}Th amounts. We considered the association of low $^{230}Th/^{232}Th$ and low U content the most important factor *affecting the age errors and inversions in the FN1 stalagmite. In contrast the FN2 stalagmite has a more precise chronology due to the predominant aragonite composition, with high ^{238}U content and higher $^{230}Th/^{232}Th$ ratio than FN1. The ages from the FN1 stalagmite are all in chronological order and contain low errors and were therefore all kept in the age model.*

The TRA5 stalagmite is entirely composed of calcite, but the ^{238}U content is relatively high compared to other stalagmites, which improves the confidence in its age results. However, the high ^{232}Th content of samples from the top of TRA5 affects the age results over the last 200 years. The other two inversions in TRA5 (71 and 104 mm, Table S2) might also be a result of ^{232}Th contamination resulting in increased errors.

Most of the TRA7 stalagmite used in our composite is composed of calcite (from top to 130 BCE). According to age results produced by Utida et al. (2020), most of the ages are in chronological order and the inversions seem to not have a direct relationship with ^{238}U, and the high ^{232}Th content is similar to other ages from TRA7.

The age uncertainties caused by high ^{232}Th concentration and calcite recrystallization in stalagmites might affect the age model. However the strong coherence between the $\delta^{18}O$ curves from different stalagmites argues in favor of the good quality of our chronology. This is evident when FN2, which is composed 100% of aragonite, is compared with other samples. There is a different amplitude range in its $\delta^{18}O$ values, but when the curve is superposed on other $\delta^{18}O$ records the variability is similar. This amplitude range is corrected when the $\delta^{18}O$ results are submitted to the ISCAM composite construction, since it normalizes the results.

Historical records and age model uncertainties

The errors of our age model for TRA5 are around ± 30 years (95% confidence interval) and we are thus aware that this uncertainty complicates the attribution to a single three-year long event. There exist no precipitation reconstructions or observations from this region between 1500 and 1850 CE, aside from these historical drought records. We thus consider our speleothem-based record as a first attempt to reconstruct precipitation in Northeast Brazil that would allow a comparison with historical droughts. If our speleothem records regional hydroclimate, it should retain a signal of the most intense droughts over NEB that are known to have struck the region based on the available historical literature of Brazil. The historical droughts we discuss in the paper, and we identify in our record, are the longest drought events in Northeast Brazil that occurred within the zone of influence of the ITCZ, and are thus probably the most likely to be recorded by stalagmites. Note that despite dating uncertainties of our record, the $\delta^{18}O$ peak of each drought event recorded, is consistent with the historical record of Lima and Magalhães (2018). Furthermore, the period between 1620 and 1717 CE is devoid of any abrupt drought events in the TRA5 stalagmite, which is again consistent with the historical records. Lima and Magalhães (2018) registered only 3 short drought events within this period of almost 100 years. It is also important to mention that Lima and Magalhães (2018) report all drought events in NEB and do not indicate their location. As discussed above northern and southern NEB are influenced by different climatic systems, the ITCZ and SASM, respectively, and this can explain, in part, the differences between historical and stalagmite records of Rio Grande do Norte.

I have some more general comments to the style of the writing and presentation, which I summarize here. Please find specific locations related to the following points in my minor comments along the text:

- Across the manuscript I found repetitive statements, but also rather irrelevant information. This makes the reader lose focus, so I suggest to try to shorten/streamline the text in general.
- In many figures, some aspects are hardly visible. Please improve accessibility, e.g., text sizes, increase size of markers of locations, use colors that are better visible.
- Sometimes past and present tense is mixed, please check language style.

We are in the process of performing a complete revision of the text in order to improve the language quality and the conciseness of the text. The figures are updated and can be seen below. They were updated *for text and markers sizes, as well as adapted for color blind readers. Certainly, the points mentioned will help us produce a higher quality manuscript.*

Minor comments:

L49 weakening

The word spelling will be corrected.

L62-63: Is there a reference for this statement?

The references are the same as those mentioned after this statement. We will add the references Marengo and Bernasconi (2015) and Lima and Magalhães (2018) to this sentence.

L91: I think the Lechleitner Paper is from 2017.

The year of publication will be corrected to 2017.

L129-131 is this relevant?

We believe so. But we combined the two sentences into one, and we clarified the meaning of the text.

"The caves were developed in the Cretaceous carbonate rocks of the Jandaíra Formation, Potiguar Basin, close to the Apodi River valley in a region of exposed karst pavements (Pessoa-Neto, 2003; Melo et al., 2016; Silva et al., 2017)."

L138: Any idea why the cave temperature is considerably lower than the annual mean temperature? Is this relevant for your data?

The annual mean temperature was taken from a climate station in the city, kilometers from the cave. Temperatures in cities tend to be higher than in pristine environments (urban heat-island effect) such as those where the caves are located. This information is not directly relevant for the interpretation of our results, but nonetheless helpful for those who want to better understand the climatology of the region.

L148: I feel like most of this section is rather results than material/methods description?

We agree with the reviewer that this part of the text is better suited in the results than the methods or Regional settings sections. We will adjust this section accordingly.

L149ff: There is a lot of discussion of the different sectors within NEB, it may be helpful for the discussion and the readers to indicate those in a figure?

The spatial correlations of Figure 2 are used to define the northern and southern NEB climatologically. The new version of this Figure includes the labels "Northern NEB" and "Southern NEB" in graphs a) and b). Please, see the revised version of Figure 2.

L164: How is "most significant" defined?

The "most significant" years of El Niño in NEB are those that most drastically impacted the precipitation amount. We changed the text to clarify the statement. Please see the revised text below.

"…we excluded the 39 El Niño - Southern Oscillation (ENSO) years that most drastically changed the precipitation amount in NEB, following the methodology of Araújo et al. (2013)."

L174: "is primarily the result of a shorter rainy season". This is not quite what is described above. There you write that the rainy season has the same length but is weaker?

L175: "The anomalous length…" See previous comment, according to your own results, this is only for the wetter years.

We rewrote the paragraph mentioned in comments L174 and L175 to clarify these aspects. Please see below.

"The results (Fig. S1) reveal that in the majority of years (normal years - interquartile range) the rainy season persists from February to April, with precipitation varying from 100 to 180 mm/month, and minor contributions occurring in January and May (50-70 mm/month). During the drier years (lower quartile), February has a reduced precipitation amount, similar to the amount in January during normal years, as described above. The maximum precipitation of 90 mm/month occurs between March and April. For wetter years (upper quartile), the rainy season starts in January with more than 100 mm/month and lasts until May with almost 150 mm/month, reaching values higher than 250 mm around March. These data show that wetter years are characterized by increased precipitation amounts and a longer rainy season starting in January and ending in May, while the precipitation deficit during drought years is a result of decreased precipitation amount and a shorter rainy season, with a peak in precipitation between March and April. The anomalous length of the rainy season during dry and wet years is attributed to variations in the meridional SST gradient in the tropical Atlantic that results in a shift of the ITCZ to the north or south of its climatological position (e.g., Andreoli et al., 2011; Marengo and Bernasconi, 2015; Alvalá et al., 2019)."

L189ff: If this is relevant for the discussion later, I feel like the authors should clearly define the difference between ITCZ related rainfall in NEB, and SASM related rainfall in S-NEB. Some reader may not be able to recall the exact difference at once…

As mentioned in comment L149ff, the spatial correlations of Figure 2 are used to define the northern and southern NEB climatologically. The new version of this figure indicates the "Northern NEB" and "Southern NEB" in order to call attention to the differences. Please see the new version of Figure 2.

L229-230: This information is not relevant for this study.

The information will be removed.

L282 to significantly reduce

The error will be corrected.

L327: Avoid repetition of the method description.

We will remove the sentence of lines 327-329.

L343: Why is a composite record only constructed for $\delta^{18}O$? what about the early phase 2-3k BCE? It is shown in the figure, but I didn't find a statement why it is shown but not discussed

$\delta^{18}O$ is a proxy for regionally integrated climate and circulation upstream, with the main signal related to atmospheric processes, while $\delta^{13}C$ values are more diverse and more site-specific, related to the seepage solution pathway and spots of vegetation above the cave. Therefore, a composite for the $\delta^{13}C$ will give a more heterogenic mosaic that may not be related with the regional conditions. For the early part of

our record, we therefore decided to remove it from the main text. As it is not being discussed, as suggested by the first reviewer, we will move Figure 3, with this early part of our record, to the Supplemental Material as Figure S5.

L389: if the age axis is correct, the oldest period of the RN composite is around 1.5 to 1k BCE?

L390: I see rather positive anomalies between 1 and 0.5 BCE…

L391ff: Confusing section, please clarify. In the previous sentence you state, there is soil erosion, here you state that did not contribute much… Now what?

L397: more negative as compared to what? to me, the $\delta^{18}O$ values are rather higher than in the earlier part of the record… please clarify.

L399ff: This paragraph is hard to follow. Please don't start compare/discussing sections of the record that haven't been mentioned before… (here the LIA suddenly pops up)

Thank you for calling attention to this paragraph. We combine our reply to comments L389 - L397 below. We rewrote the paragraph as shown below, paying attention to all suggestions made by the reviewer.

"The oldest period covered by the RN Composite, from 1200 to 500 BCE, is characterized by successive dry and wet multidecadal periods, with increased precipitation in N-NEB from 1060 to 750 BCE and from 460 to 290 BCE, as suggested by the negative departures seen in the $\delta^{18}O$ values. During this last period, there is also a tendency from lower to higher $\delta^{13}C$ values, suggesting progressive surface soil erosion related to rainfall variability (Fig. 4), as interpreted by Utida et al. (2020). This period ends up in a stable interval from 300 BCE to 0 CE with $\delta^{13}C$ values close to the bedrock signature at about -1‰ to +1‰, indicating a lack of soil above the cave. After an abrupt reduction of $\delta^{13}C$, the values decrease to approximately -2‰ between 200 CE and 1500 CE. From 1500 CE to the present, negative values of $\delta^{13}C$ is responding to wet climatic conditions as indicated by lower $\delta^{18}O$ values. The more negative $\delta^{13}C$ during this period can be related to denser vegetation that favored both soil production and stability above the cave."

L408: On the millennial scale, yes… since you also mention shorter timescales earlier, I would clarify this here…

We adapted the text to clarify this statement. Please see below.

"During the last 2500 years, the RN Composite shows similar characteristics as the lower-resolution δD lipids record obtained in Boqueirão Lake sediments"

L413: Unclear, why is this?

L414 do you mean "latter"?

L415: Very vague statement, please specify. Also, how well are the lake sediments dated and is that comparable to your chronology?

The lake sediments were dated with the 14C method, which has larger errors than the U/Th method used for stalagmites. Furthermore, the age model of Boqueirão Lake was constructed with fewer ages compared to stalagmite chronology. We rewrote the sentence to simplify it and answer the comments from L413 to L415. Please, see below.

"This inconsistency might be related to different chronological controls between lake and stalagmite records and possibly also by the location of Boqueirão Lake that is more strongly affected by the ITCZ and as it is located in the eastern coastal sector of NEB (Zular et al., 2018; Utida et al., 2019)."

L420 Maybe indicate the insolation curve in the Figure?

We have included the insolation curve in Figure 3 and 4 of the manuscript. This Figure in question already contains a lot of information and adding even more would make it difficult to read. We will instead add the insolation curve to the Figure S5 in the Supplement as shown below.

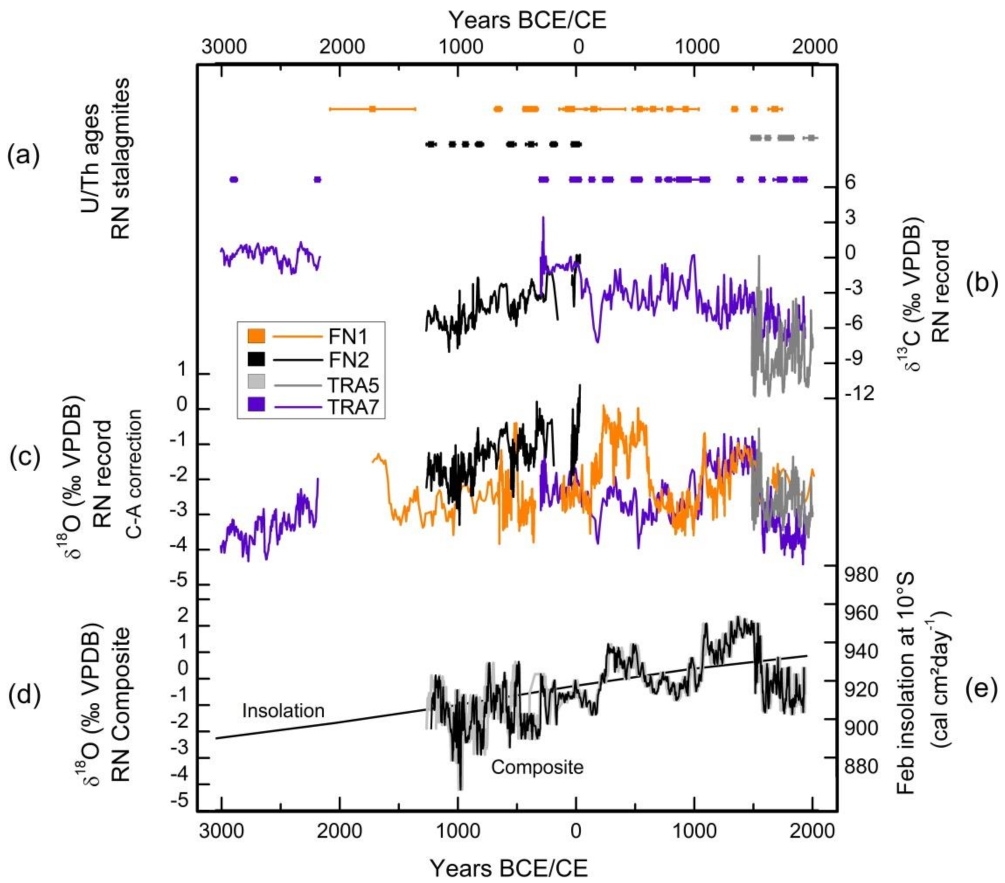


Figure S5 – Rio Grande do Norte stalagmite isotope record. (a) U/Th ages for RN stalagmites. (b) Raw data of $\delta^{13}C$. (c) Oxygen isotope results corrected for calcite-aragonite fractionation ($\delta^{18}O_{C-A}$), according to weight proportion of mineralogical results. (d) $\delta^{18}O$ RN Composite constructed using stalagmite records from NEB (black line). Grey lines denote the age model confidence interval of 99%. (e) February insolation curve at 10°S.

L421 persistently (?)

The word will be corrected.

L426ff: This is an interesting conclusion which is however barely discussed beforehand. The discussion here rather ends quite abruptly. I feel this could be more elaborated, because it seems to relate to the statement in the abstract, that you can make inferences on spatio-temporal ITCZ variability?

We rewrote the whole paragraph to call attention to the differences between N- and S-NEB and reinforce our conclusions. Please, check it below.

"It is important to note that the RN record exhibits a climatic signal that is distinctly different from the DV2 speleothem record from Diva de Maura Cave in S-NEB (Novello et al., 2012). The general trend toward more positive values, as a result of insolation forcing, occurs from 150 to 1500 CE in the RN Composite, but from 600 to 1900 CE in the DV2 sample (Cruz et al., 2009; Novello et al., 2012). This trend is a result of the persistent dry conditions in the entire NEB region following the 4.2 ky BP event. However, the DV2 record does not document the same multidecadal and centennial-scale climate variability as recorded

in the RN speleothem record, nor the less dry interval from 600 to 1060 CE seen in the RN Composite (Fig 3). As demonstrated by the spatial correlation maps between $\delta^{18}O$ values and regional precipitation (Fig. 2), the S-NEB and N-NEB regions are influenced by distinct rainfall regimes whose peaks of precipitation arise during the summer monsoon season and the autumn ITCZ, respectively. Our data provide evidence for a spatial and temporal distinction of NEB climate patterns in the past that can be interpreted as differences in seasonality during the last millennia. Furthermore, contemporaneous dry or wet events in both N-NEB and S-NEB suggest the occurrence of larger regional climate changes with higher environmental impacts."

L432: very vague and unclear which characteristics are meant

L429ff (the whole section) difficult to follow here, you jump from describing a trend to single events, and then to processes again - not clear where this leads to? please provide the reader with some kind of guidance in between, maybe in form of a summary and/or statement which observation will be tested/explained now…

L437: unclear what information your record adds to this aspect, and how this relates to the discussion?

We rewrote the paragraph to adjust it according to the reviewer's suggestions. Please see revised version below.

"When comparing N-NEB and eastern Amazon conditions, it is evident that the RN Composite shares some similarities with the Paraíso stalagmite record (Wang et al., 2017), due to the contribution of ITCZ precipitation in both places. But there are also important differences (Fig 4). The RN Composite shows lower δ18O values between 500 and 1000 CE, compared to the earlier period, while Paraiso shows decreasing values around the same period, suggesting a slight increase in precipitation in both areas. From 1160 to 1500 CE, abrupt increases in $\delta^{18}O$ values are seen in both records, which indicates abrupt and prolonged drought conditions due to a northward ITCZ migration. However, around 1100 CE, and the period from 1500 to 1750 CE, Paraiso is antiphased with the RN Composite and in phase with the Cariaco Basin (Haug et al., 2001), which is inconsistent with the notion of an ITCZ-induced regional precipitation change. Instead, a zonally-oriented precipitation change within the ITCZ domain over Brazil is required to explain the anti-phased behavior between precipitation in N-NEB and the eastern Amazon, and similarities between Cariaco and the eastern Amazon."

L441: now the discussion jumps again back in time to another event… I would bring this example later to showcase a potential relationship to Atlantic temperatures…?

We will adjust the entire manuscript according to suggestions and comments of the reviewers. Certainly the discussion about Bond events can be combined with the Atlantic temperature discussion and its relationship with the ITCZ. It will become clear where the best position for this paragraph is once we finalize the revision of the manuscript.

L447: I suggest to turn the argument around - the idea is that ITCZ displacements are forced by temperatures, so we check if there is a relationship of our record to AMV?

L448ff This sentence seems incomplete

We rewrote this paragraph in order to clarify our ITCZ displacement hypothesis related to meridional temperature gradients in the Atlantic. Please see the revised text below.

"We investigate the potential relationship between $\delta^{18}O$ values in our RN speleothems and an ITCZ displacement toward the warmer hemisphere to explain paleoclimate variability observed in N-NEB. In order to test this hypothesis, the RN Composite was compared with a reconstruction of Atlantic Multidecadal Variability (AMV) (Lapointe et al., 2020) (Fig. 4). Some studies suggest that the warm phase of the AMV forces the mean ITCZ to shift to the north of its climatological position, thereby causing a reduction in NEB

rainfall (Knight et al., 2006, Levine et al., 2018), while a recent study suggests that the warm phase of the AMV would cause a weakening of the ITCZ from February to July (Maksic et al., 2022)."

L452: Have you checked other records of AMV / Atlantic SSTs that allow to check if the Lapointe record is representative for the entire basin during these times or not?

Lapointe et al. (2020) present a record that is in good agreement with other temperature records from the North Atlantic and with other AMV reconstructions (Mann et al., 2009; Cunningham et al., 2013; Miettinen et al., 2015; Reynolds et al., 2016; Wang, et al., 2017; Spooner et al., 2020) and also with records from the Cariaco Basin (Black et al., 1999), which suggest that their AMV reconstruction is reliable and indicative of a large-scale teleconnection with the tropics.

L462ff As far as I understand the plot, there is no PDV record in the plot, so how do you infer a cold phase of PDV during that time? I guess you refer rater to Fig 5, but still I suggest to explain which record / curve you are referring to here exactly and what they are showing? Is the Pacific SST gradient a measure of PDV? this curve shows centennial scale variability, but not at all decadal?

The Figure presents only the AMV. The discussion about the relationship between AMV and PDV was only based on an observed precipitation analysis. We made some adjustments in the paragraph to clarify this aspect. Please see the revised text below.

"According to Kayano et al. (2020, 2022), during the last century, dry conditions over N-NEB and the eastern Amazon are present when AMV and Pacific Decadal Variability (PDV) are in both in their warm phase, or when the AMV is in a cold phase and the PDV in its warm phase. On the other hand, when AMV and PDV are both in their cold phase, precipitation over the Amazon is anti-phased with NEB, resulting in decreased precipitation over the Amazon and increased precipitation over NEB. This zonally aligned precipitation signal over eastern tropical S. America is the result of joint perturbations of both the regional Walker and Hadley Cell's, produced by teleconnection between the Atlantic and Pacific (Kayano et al., 2022, He et al., 2021). These conditions can explain in part our results, however during the decoupling of our record with AMV (between 1500 and 1750 CE), increasing precipitation over N-NEB and decreasing precipitation over the eastern Amazon can be better explained by the positive gradients both in Atlantic and Pacific Oceans forcing a south ITCZ migration (Fig. 4)."

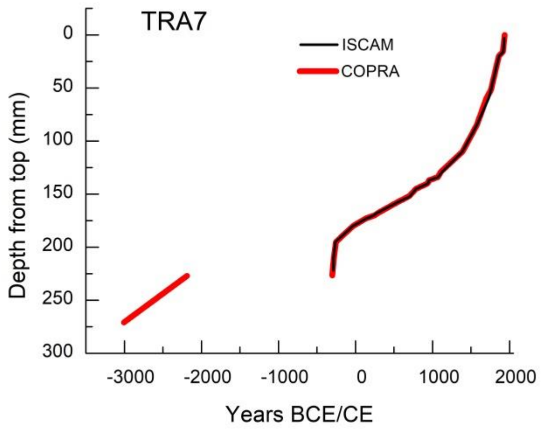
L494ff: what does the $\delta^{13}C$ record tell in this time? extreme events could be also visible there, the record looks quite "spikey"

The $\delta^{13}C$ and $\delta^{18}O$ records of TRA5 show similar characteristics during this time. These data can be interpreted in the same way as the rest of our record, indicating increased (decreased) precipitation ($\delta^{18}O$) and soil production (erosion), combined with a decrease (increase) in Prior Calcite Precipitation (PCP) at the epikarst ($\delta^{13}C$). All mentioned processes drive oxygen and carbon isotope variability in the same direction (Novello et al., 2021).

L495: How do the other speleothem records compare during this time? I understand that they have lower resolutions, but to support your point and strengthen your arguments (e.g. concerning age model uncertainties, etc) a zoom into the comparison of the different proxy records might be helpful? Also, how would ISCAM move the TRA7 record with respect to the original agemodel? This gives also a hint regarding dating uncertainty…

Unfortunately, the TRA7 record is not suitable for this kind of comparison because of its lower resolution. The deposition rates (DR) of TRA7 and TRA5 are different, 0.18mm/yr and 0.33 mm/yr. We tried to sample TRA7 at the maximum possible resolution to achieve such a comparison. Considering the uncertainties of the age model, some peaks are too smoothed and the $\delta^{18}O$ data are not suitable for comparison. We believe that including such a comparison would not aid in our interpretation. The TRA7

ISCAM age model does not significantly change compared to the COPRA model, since both use the linear method. We did not plot them together in the Figure S4 because the superposition would not be visible. We show here a figure to demonstrate the similarity of both TRA7 age models and to clarify this question.



L497ff: statements like this require a proper report of dating and age model uncertainties. From visual inspection there are some ages which have quite high uncertainties, which could limit the fidelity of such a record to absolutely date extreme events with annual precision! It could be also short-term hiatuses, that last longer than a single year…? I understand that the TRA7 age model is part of another paper, but then please still give a statement here, because this is relevant for your conclusions. It is also not clearly visible from the plot of the age model in the supplement.

We will improve the methods, results and discussion sections when referring to the stalagmite ages. We also improved the figures in the supplemental material to better describe the age models and uncertainties of our record and we modified the text in this paragraph. Please see revised text below.

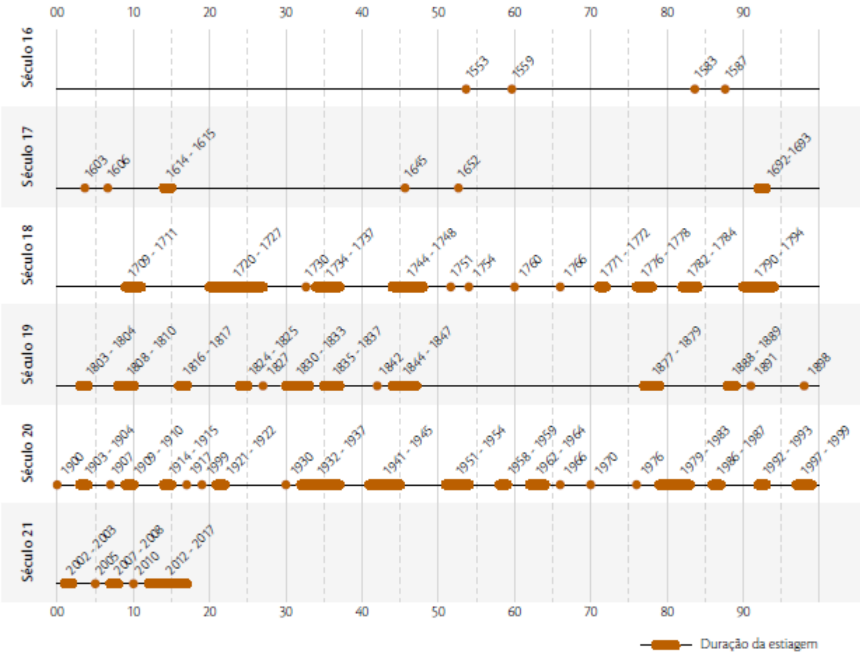
"In NEB, the low water availability has been one of the major challenges faced by its people during the last centuries (Marengo and Bernasconi, 2015; Marengo et al., 2021; Lima and Magalhães, 2018). On the other hand, the last 500 years were the wettest of the last two millennia, according to our results (Fig. 3). Superimposed on these long-term negative $\delta^{18}O$ anomalies, distinct peaks are recorded in the TRA5 $\delta^{18}O$ record from 1500 to 1850 CE (Fig 5). These drought events are visible in this record thanks to its higher deposition rate (faster growth) and thus higher temporal resolution of the $\delta^{18}O$ record when compared to other stalagmites used in our study. Although the age model errors of TRA5 are larger and could limit our ability to attribute $\delta^{18}O$ peaks to specific single-year events, it still allows for a comparison between these abrupt events with historical records to demonstrate the long-term context of abrupt drought events in modern human history."

L538ff: how many droughts are not recorded in your stalagmite record? the reference is not accessible, so please provide a clear statement, or, better, a plot/histogram of all droughts reported by the other study in Fig 6

The historical record of Lima and Magalhães (2018) (Graph 1 in the original paper) mentions drought events compiled from different historical letters and books from all of Northeast Brazil (NEB). Hence, some of these events might be located in the southern and/or northern part of Northeast Brazil. Our data record a smaller number of events than are listed in the historical data, probably recording primarily the most intense events that affected all of NEB, or ITCZ changes that affected only the northern portion of NEB. According to our correlation maps, southern and northern NEB have different precipitation sources and seasonality. Therefore, the TRA5 stalagmites do not record all events mentioned in the compilation of Lima and Magalhães (2018).

The paper can be accessed using the link below. The link will be updated in the reference section of the revised manuscript.

https://seer.cgee.org.br/parcerias_estrategicas/article/view/896/814



Graph 1 – Historical drought events in Northeast Brazil. Extracted from Lima and Magalhães (2018).

L551: Discussion ends quite abruptly, following from your section 5.1 one would at least expect a hypothesis of a forcing mechanism of the drought occurrence?

We will include a concluding paragraph at the end of section 5.1 suggesting a hypothesis related to our main conclusion. Please see the revised text below.

"We suggest that progressive changes in the mean ITCZ position over the course of the last 500 years might be responsible for historical droughts that affected the seasonality of N-NEB and caused abrupt and strong drought events. No preferred periodicity of these events is apparent in our record. Additional drought-sensitive high-resolution records will be required to improve our understanding of these historical droughts events in NEB."

How is the drought frequency related to what you found out from your record of the past 2.5ka? I suggest to elaborate this a little bit more…

Our stalagmite and RN Composite records contain variability at multidecadal and interdecadal frequencies. However, the wavelet analysis did not show a temporally continuous signal at a preferred wavelength. We therefore chose not to discuss this aspect in greater detail.

Figures

Figure 1: Locations hardly visible, please increase the size of the text and the stars. Also No. 5 is barely visible, please choose other colors.

We updated all figures to improve the font size of text and symbols. Please see the revised Figures 1 and 5 below. Figure 5 was updated and changed to Figure 4.

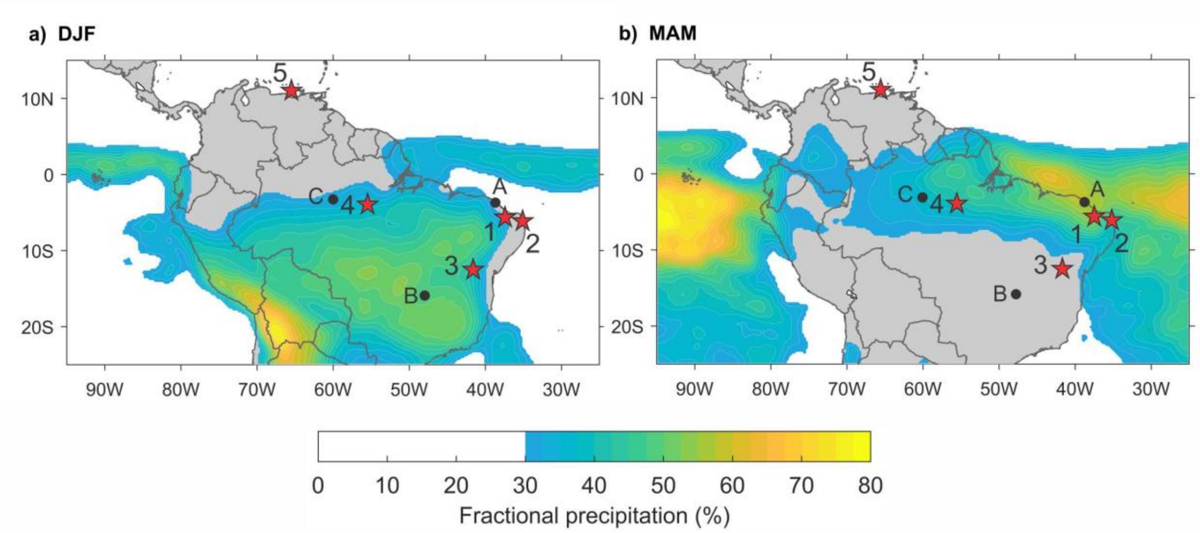


Figure 1 – Location and precipitation climatology of study sites during the austral summer (DJF - December to February) and autumn (MAM - March to May). Color shading indicates percentage of the annual precipitation total that is received during either DJF or MAM and highlights the extent of (a) the SASM over the continent and (b) the ITCZ over the ocean. Precipitation data is from the Global Precipitation Measurement (GPM) mission, with averages calculated over the period 2001–2020. 1) Trapiá and Furna Nova Cave (this study), 2) Boqueirão Lake (Utida et al., 2019), 3) Diva de Maura Cave (Novello et al., 2012), 4) Paraíso Cave (Wang et al., 2017), 5) Cariaco Basin (Haug et al., 2001). GNIP stations: A) Fortaleza, B) Brasília, C) Manaus.

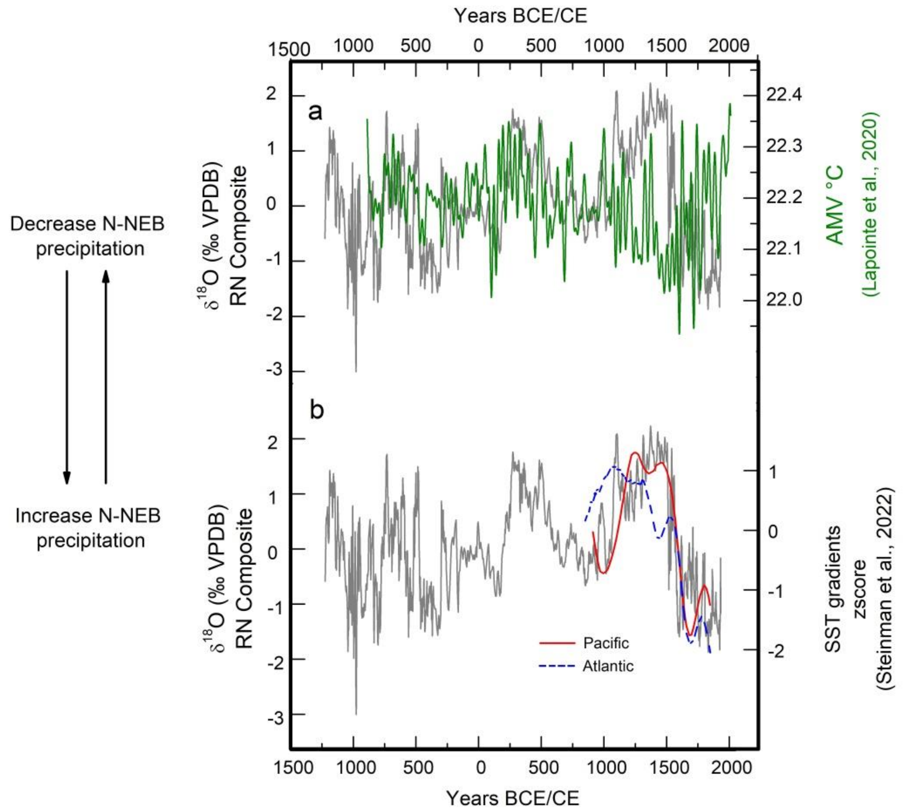


Figure 4 - δ18O RN Composite compared with (a) Atlantic Multidecadal Variability (Lapointe et al., 2020) and (b) Pacific and Atlantic Sea Surface Temperature gradients calculated (z-score) according to Steinman et al. (2022). Atlantic: 2σ range of 1,000 realizations of the Atlantic meridional SST gradient (north – south). Pacific: median of 1,000 realizations of the Pacific zonal SST gradient (west – east).

Figure 2: Increase symbols for locations. Please improve visibility in general. Caption should be streamlined, “precipitation amount” is mentioned twice in the first sentence (L199-201). Correlation maps is repeated in L199 and L204. GNIP is repeated in L200 and L207. No need to repeat all information to all caves again, it is also ok to refer to the previous figure…

We updated all figures for size and to render them suitable for color-blind readers. Please see the revised Figure 2 and caption below.

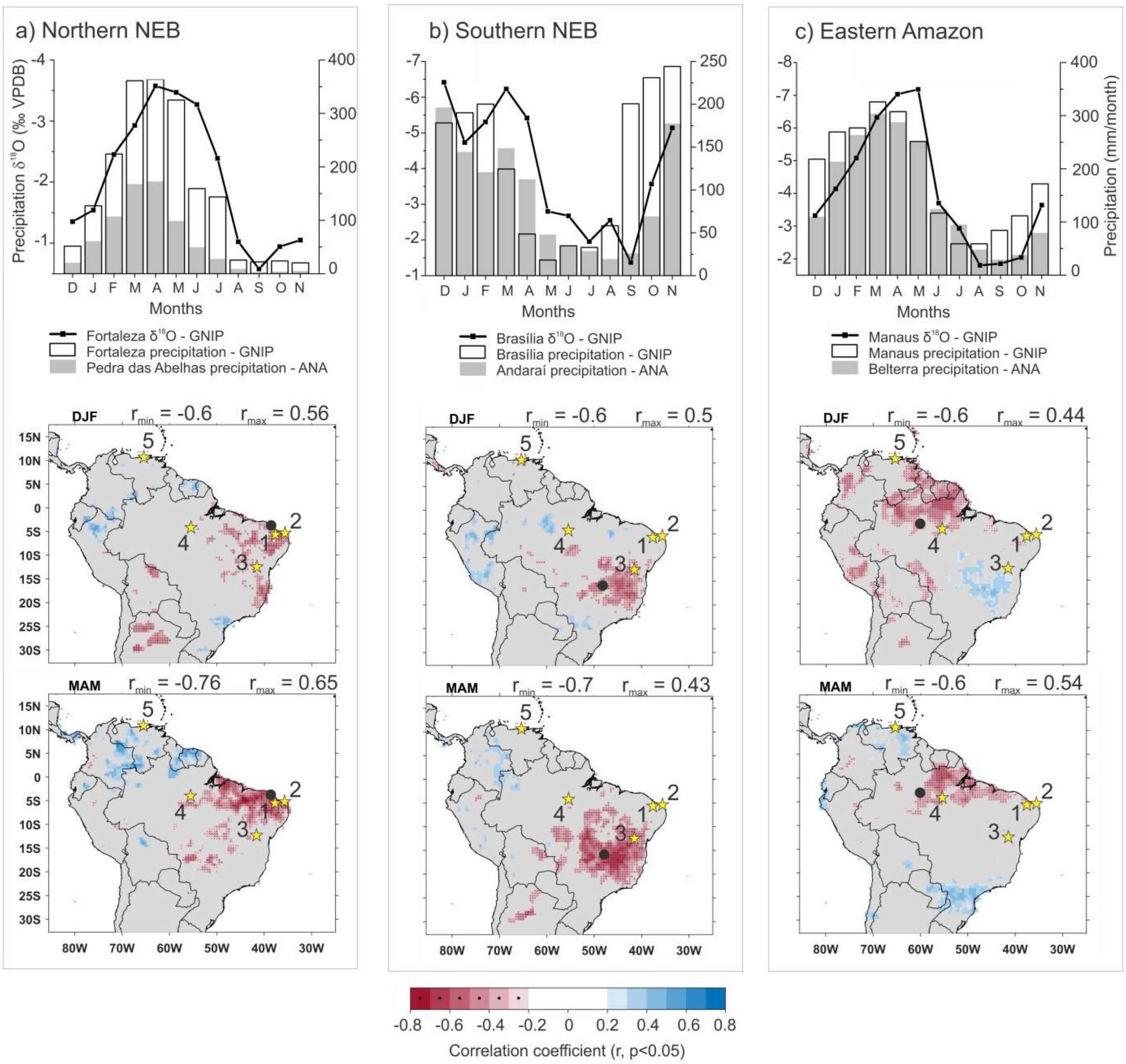


Figure 2 – Monthly mean observed precipitation amount collected at ANA and $\delta^{18}O$ values for GNIP stations (IAEA-WMO, 2021) (black dots) and correlation maps between gridded precipitation and $\delta^{18}O$ anomalies from the same stations (black dots) for: (a) Northern NEB, Fortaleza and Pedra das Abelhas stations (star 1), (b) Southern NEB, Brasília and Andaraí stations (star 3), c) Eastern Amazon, Manaus and Belterra stations (star 4). The maps show the spatial correlation between $\delta^{18}O$ anomalies at GNIP stations and GPCC gridded precipitation anomalies based on the period 1961-1990 for December to February (DJF) and March to May (MAM) for Fortaleza, Brasília and Manaus stations (Ziese et al., 2018). The $\delta^{18}O$ values (left y axis) and precipitation (right y axis) for each station were obtained from the GNIP IAEA/WMO database. Stars indicate the site locations: 1) Trapiá Cave, Furna Nova Cave and Pedra das Abelhas ANA Station (reference period 1910-2019), 2) Boqueirão Lake (Utida et al., 2019), 3) Diva de Maura Cave (Novello et al., 2012) and Andaraí ANA Station (reference period 1960-1986), 4) Paraíso Cave (Wang et al., 2017) and Belterra ANA Station (reference period 1975-2007), 5) Cariaco Basin (Haug et al., 2001).

Figure 3: Please check if colors are color-blind friendly (red and green mixed…?)

We updated all figures to render them suitable for color blind people and we checked them using the website that simulates color blindness, as suggested by the journal. Please see the updated version of Figure 3 in this comment. Please note that we merged Figure 3 and 4 because of the overlapping data. The complete TRA7 record is in the supplemental material. Please see Figure S7 below.

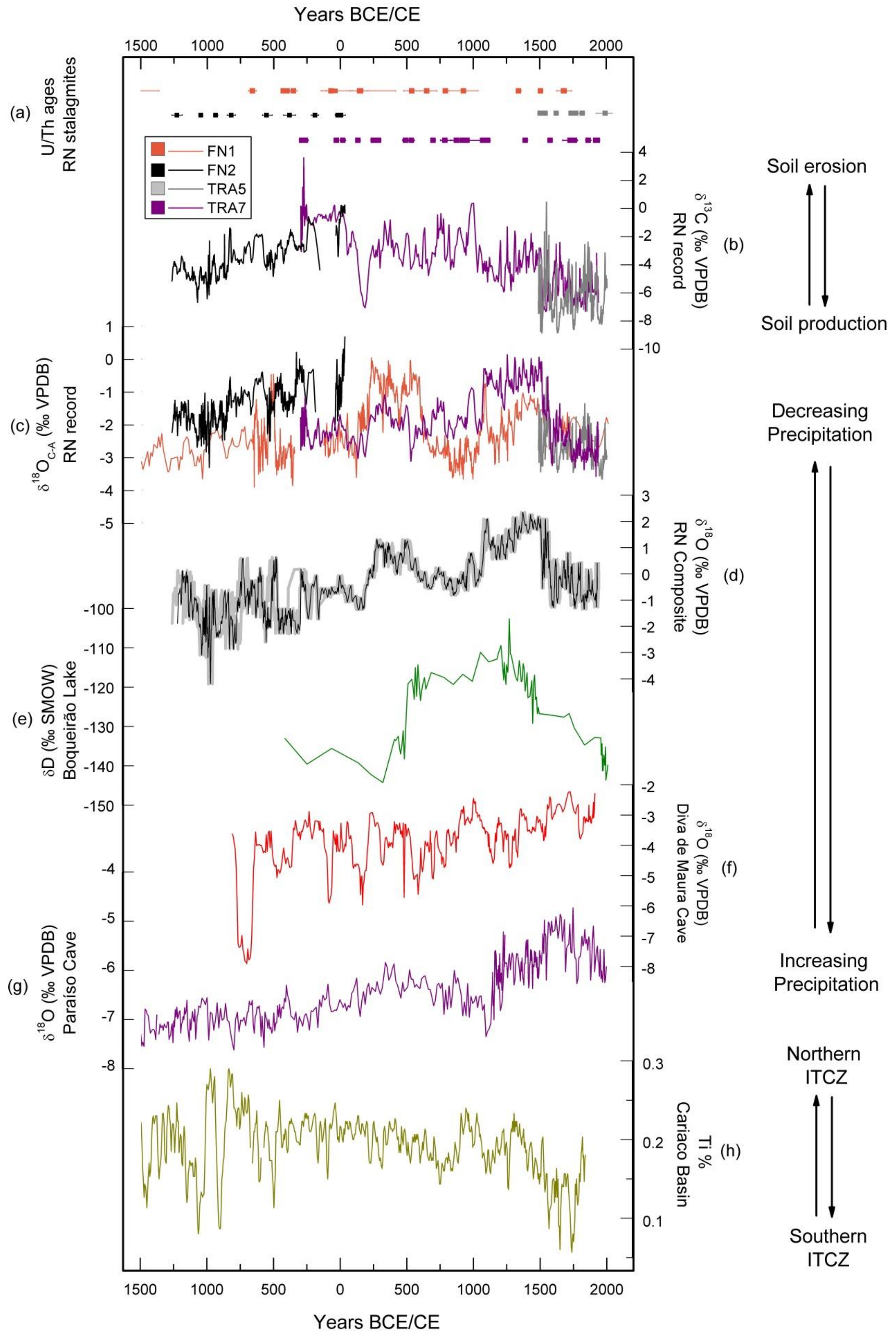


Figure 3 – Rio Grande do Norte stalagmite isotope records and comparisons with other records from South America. a) U/Th ages from each stalagmite studied. b) Raw data of $\delta^{13}C$. c) Oxygen isotope results corrected for calcite-aragonite fractionation ($\delta^{18}O_{C-A}$), according to weight proportion of mineralogical results. d) $\delta^{18}O$ RN Composite constructed using stalagmite records from NEB (black line). Grey shaded area denotes 99% confidence interval of age model. e) Boqueirão Lake δD record (Utida et al., 2019). f) DV2 $\delta^{18}O$

speleothem record from Diva de Maura cave, southern NEB (Novello et al., 2012). g) PAR01 and PAR03 $\delta^{18}O$ records from Paraíso cave stalagmites, eastern Amazon (Wang et al., 2017). h) Ti record of Cariaco Basin (Haug et al., 2001).

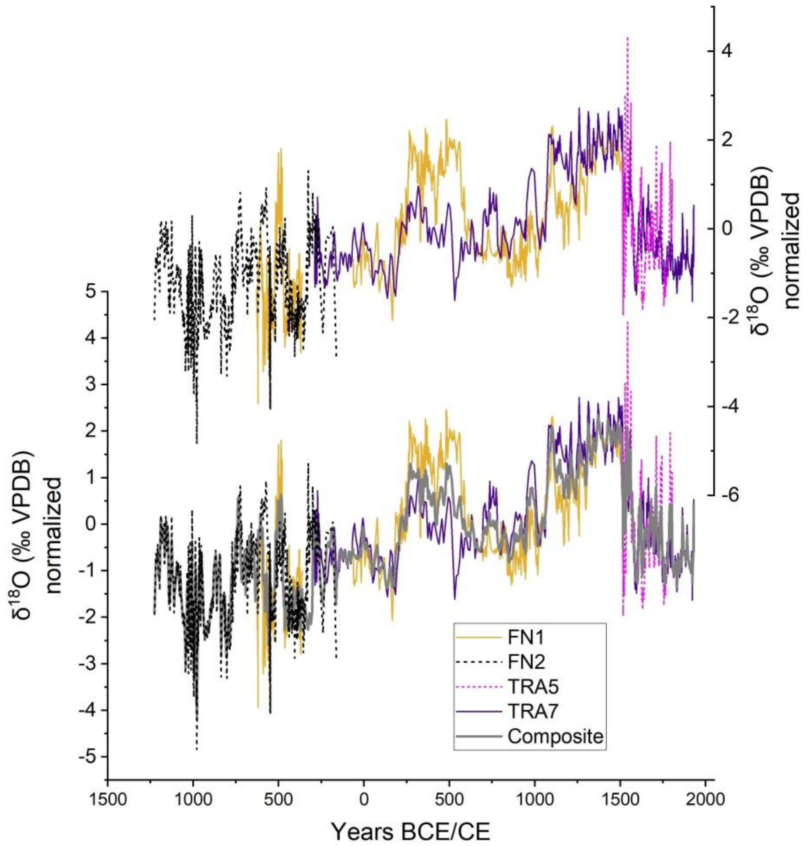


Figure S7 – Oxygen isotope records and age model results calculated by ISCAM for individual stalagmites and Composite. The normalization of the data is made by ISCAM (Fohlmeister, 2012).

Also, why is the early phase of TRA7 between 3 and 2k not included in the composite?

This part of the TRA7 stalagmite was not included in the ISCAM composite, because this interval was not the focus of our discussion. Even though it is new data, most of its interpretation is related to the 4.2 ky BP event and was described previously by Cruz et al. (2009) and Utida et al. (2020). Therefore, and following the suggestion of Reviewer 1, the Figure 3 was merged with Figure 4, and this older part of TRA7 was included in the Supplementary Material.

Supplementary material

Tables S1, S2, S3: Please check decimal and 1000s delimiter, there are different styles used (comma and points mixed, sometimes comma as 1000s delimiter, sometimes not). Also "delta"234U instead of d234U.

Thank you for mentioning the lack of harmonization. All data will be delimited consistently by using periods. The delta notation was also corrected in Tables S1, S2 and S3.

Figure S4: Any ideas for the outliers, e.g., in TRA7 or FN1? Also, why is the age model of FN1 systematically older than the stalagmite ages? Also, why do you show ISCAM uncertainties, but COPRA average age model? Why not show ISCAM and COPRA in comparison?

The outliers for TRA7 and FN1 were discussed above. We will include a more complete description of the U/Th ages when we submit our revision. The outliers can be explained by the ^{232}Th content and

$^{230}Th/^{232}Th$ results. Please, see our detailed response to this question in the third paragraph of this RC2 response, when discussing U/Th dating results.

We decided to show COPRA age models, because the age model of ISCAM failed to produce reasonable extrapolations for the first and last millimeters of the stalagmites or to bridge intervals where we had identified a possible hiatus such as in the sample FN1. COPRA produces an independent linear age model allowing us to evaluate them without changes as made by ISCAM. However, both age model methods use a linear interpolation and produce very similar results. The plot with two time series does not show any significant differences between them; hence the choice of age model does not affect our interpretation. Please see figure below.

We also revisited the age models and the caption of Figure S4. The systematically older ages for FN2 were the result of a plotting error. We also corrected the text concerning the age model errors in the caption. This was not an ISCAM age model error, but a COPRA age model error. We corrected the graph and caption and present the revised version below (Figure S4).

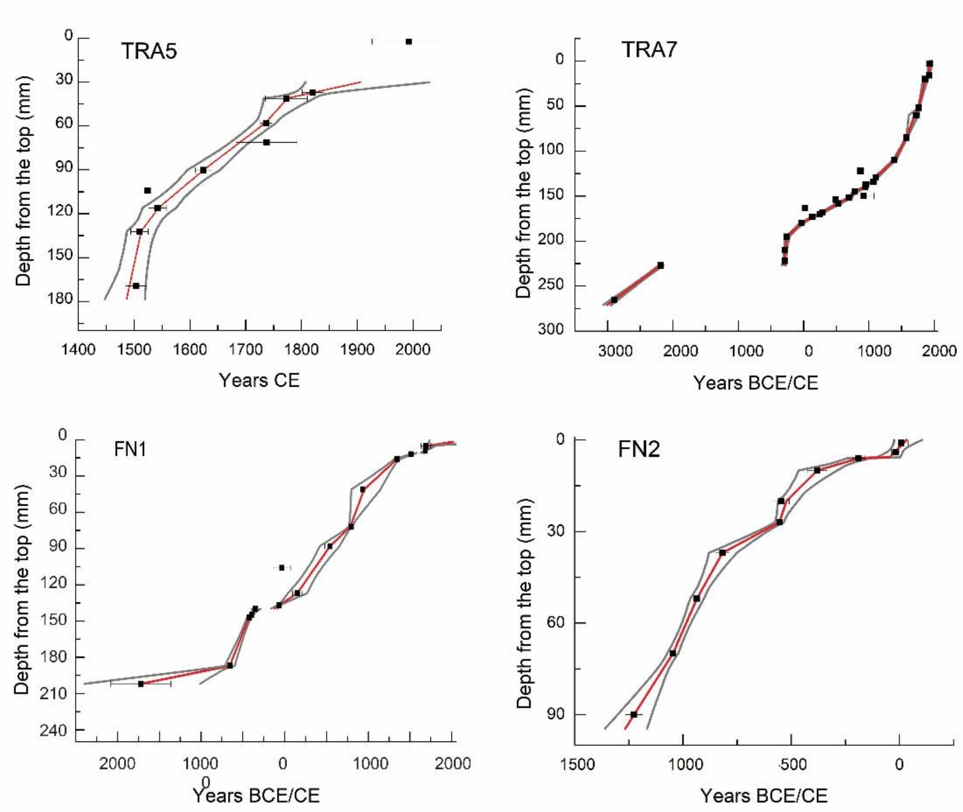


Figure S4 – Age models for each stalagmite from Rio Grande do Norte. Age models were calculated using COPRA (Breitenbach et al., 2012) through a set of 2.000 Monte Carlo simulations. The COPRA age model was produced for each sample and covers the entire stalagmite. Squares and horizontal bars: age results with error bars. Red line: COPRA average age model. Grey line: age model errors considering 95% confidence interval.

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