Terminations paper

Responses to Reviewer and Editor comments

In grey – Reviewer comments

In red – Editor comments

In blue – author comments

Lines 69-72: dragon sentence :). Consider to rephrase to: "While such tuning approaches are promising, confidently applying speleothem chronologies to multiple Terminations and millennial events during Terminations requires robust proxy interpretations across multiple climate archives. Ideally, modelling studies point to common drivers of proxy variability.

Thank you for this suggestion. We have made the change.

Lines 348-349: The TEX86 proxy is not used in these references.

You are correct. We have now modified the sub-heading to reflect that this sub-section only refers to speleothem carbonate oxygen and carbon isotope records rather than TEX86 or fluid inclusion records which are instead referred to in sub-section 3.3.

This is a review paper presenting speleothem records covering terminations (T) with the aim to (i) synthesize the available speleothem records covering T II, TIIIA, TIII, TIV and TV, (ii) description of their proxy quality, and (iii) Evaluation of the leads and lags of the records. The impact of a review paper depends on a well-organized structure and clear figures and tables. Unfortunately, the revised version is lacking clarity and a clear structure. For instance, the selection criteria for the stalagmites remain unclear, which is also related to the fact that the quality of the main table is still very bad.

I think the criteria are set out sufficiently. I don't necessarily agree with all the criteria (e.g. combining multiple speleothem records is designed to improve timings/comparisons, etc., not make them worse), but I appreciate the need for some sort of consistency when screening records. You should consider removing T-IV and T-V altogether – the two records from each (from SE Asia/EASM only) add nothing, and you cannot compare records between regions. The very imprecise chronology of the Green Cathedral record (T-V) also does not help the cause. The table definitely needs a makeover. Please consider presenting only the most essential metadata information for the main text and keeping the current expanded version for the supplementary data. A landscape (rather than portrait) format might work best for the supplementary (full) version (or both).

We recognize the challenges in the table and we have made a full revision.

Regarding T-IV and TV, we would like to retain them in the manuscript to highlight the need for records from these Terminations and these regions and to lay a template for comparisons across all terminations as additional records on these Terminations will become available (e.g. preprints and conferences highlight upcoming works from Torner et al covering T-IV from the Iberian region (https://doi.org/10.5194/cp-21-465-2025), upcoming works on the SE Asian speleothem fluid inclusion records from Nele Meckler's group

(https://doi.org/10.5194/egusphere-egu24-15117) and the Drysdale group presentations at the Karst Records conference (Ulasi et al, Pollard et al: https://airdrive.eventsair.com/eventsairwesteuprod/production-uctcmc-public/b00000003b3d4576996c7d71aefad4da).

This is also true for some of the figures, particularly for Figure 2, which would stretch over three pages in a publication.

For Figure 2, please: in your textbox descriptions of each plot, just use the cave names to avoid clutter. Add the description of what each proxy series represents in the caption. Also, the textboxes are placed over the time series in places – please avoid this. best not to combine blue and green (for readers with colour resolution problems). See https://www.nature.com/articles/nmeth.1618 offset the U-Th dates/error bars vertically in a better way so that symbols/bars do not superimpose one another. provide the full description of the insolation metric of Tzedakis et al. 2017 for your first plot (and x-ref to this caption in the following plots) and specify the unit of measurement. Also, offset the maximum insolation value in each case from the upper x-axis by expanding the y-axis range, and consider using the same y-axis range for each plot so amplitude differences in the metric between terminations can be compared more readily. be more generous with information in the caption. A figure and caption should be 100% self-contained as much as possible (i.e. all the essential information for interpretation should be provided with the need to consult the main text).

I have been working on improving these figures as we have presented at conferences. I'm sorry that they were rather unclear in the publication. The figures have been improved.

- Plots include only cave names,
- textboxes and U-Th ages and errors are offset from time series,
- colours have been changed,
- the insolation metric has been described more fully and includes the units of measurement,
- the insolation values on Y axis have been set to the same range across the different Terminations,
- more full descriptions have been given below figures.

The major weakness, however, is the discussion about similarities and differences between the Terminations (chapter 5), which should be the most important part of the review. The absolute timing and nature of Terminations in the speleothems is key to understand the interplay between external (orbital parameters) and internal (e.g., glacial boundary conditions, ocean circulation, carbon dioxide) climate forcing mechanisms. These important aspects are not really discussed in paragraphs 5.1 and 5.2, which are very short compared to the preceding paragraphs. This surprises me, since the authors state in the introduction that "we evaluate the whether there are consistent leads and lags in the manifestation of terminations across different aspects of the climate systems and different regions as tracked by speleothem proxy records.".

Given the authors' intent, it is reasonable to expect the Discussion section to be central to the paper, so I agree with the reviewer that this part needs more work and better organisation. For example, I do not see the point in comparing NISA for T-II with Sofular for T-III (lines 613

onwards). The main features of similarity and difference for how speleothems are recording local/regional climate through each termination, and why this might be the case, should be the focus. Do the apparent leads and lags makes sense? Are they confounded by uncertainties in the age models being used (not to mention the fact that some papers use more than one age-model algorithm)? It is from these observations that deficiencies in the current state of knowledge can be highlighted. Therefore, I urge you to carrying out the most plausible and meaningful comparisons between the speleothem records for each termination (still looking at regional differences), bearing in mind the dating constraints.

We have clarified in the introduction the purpose of the paper (final paragraph of introduction):

In this review paper we identify how currently available speleothem records constrain the timing of climate events across different regions within a given termination, and describe the similarities and differences in the sequences of a processes across different Terminations.

Additionally, we retitled heading 5.1 and have rewritten the first main paragraph to emphasize the comparison of a similar process (meltwater release) among TII and TIIIA detected in two speleothems (NISA and Sofular).

Both the NISA and Sofular $\delta^{18}O$ are inferred to track the release of isotopically light meltwater from Northern Hemisphere ice sheets, albeit in different regions, and in both TII and TIIIA, meltwater release happens similarly early in the Termination prior to the warming in North Europe, and recovery of the EASM. The differences in magnitude of the $\delta^{18}O$ signals in NISA and Sofular is a manifestation of the different sources of moisture for the two records rather than a reflection of the differences between Terminations, since the NISA records the ~2‰ freshening of the North Atlantic is thought to have altered the source composition by ~2‰ (Stoll et al., 2022) whereas a combination of factors including the inflow of isotopically depleted Caspian Sea waters resulting from Eurasian ice sheet melt, diversion of rivers, higher river runoff coefficients and reduced evaporation to the Black Sea during Terminations contributes to the strongly depleted $\delta^{18}O$ values of the Sofular cave record (Badertscher et al., 2011 and references therein).

Chapter 5 is rather poorly organized and it is very difficult to follow their arguments and selection criteria. To give an example: the sequence of events for Termination II in speleothems is shown in Figure 4, whereas only insolation changes are displayed as one external forcing factor. What about other key-forcing factors, such as AMOC, IRD or carbon dioxide? Changes in the intensity of the AMOC, for instance, are responsible for cold/dry snaps during Terminations (YD-like events), and they should be discussed in greater detail as they are one important aspect of almost all Terminations.

Whilst this was not requested in the first round of reviews, I believe adding additional information in the figures is useful for contextualising the comparisons you are making. The difficulty of assigning chronologies to ice-core and marine records is not trivial so you should make this clear when introducing these time series in the text. For example, time series from the marine record would be useful – e.g. a benthic and SST from the same core (=same chronology) from the Iberian margin).

We have added a new figure to illustrate an example of speleothem age tuning of marine records. We have expanded the discussion of this in the first paragraph of section 5.3.

Tzedakis et al., (2018) match the relative abundance of temperate pollen in marine sediment records on the Iberian margin pollen with Corchia (Italy) speleothem $\delta^{18}O$, assuming a common rainfall amount signal dominates both records (Figure 8). This provides an independent, speleothem tuned chronology for other proxies recorded in the Iberian margin sequence, such as the benthic $d^{18}O$, and reveals how the chronology of events in the marine sediment core would be shifted earlier by several thousand years compared to a marine age model based on tuning benthic $d^{18}O$ to stacks (e.g. Spratt and Lisiecki, 2016).

This last sentence clarifies precisely why we have not included on figures the curves of climatic parameters from marine sediment or ice core records which are not well tied to absolute chronology – because the chronologies of these records can be offset by many thousands of years, so it is not possible to describe the sequence of timing across a termination from records derived from different archives. Thus, we refrain from adding comparative records from non-tuned climate parameters from marine or ice core records

In Figure 7, the x-axis ranges should be extended to capture the complete error bars, and I would recommend either excluding the 'start of insolation' indicator, or apply it to the first derivative of the insolation metric, which appears as though it will show an uptick in insolation as the termination kicks in. Also, here and through the paper, please be consistent with expressing time (both age and duration): it should be expressed in ka or kyr respectively (with 1 decimal place where necessary, but not more), and not in years (e.g. lines 629 and 643).

- We have increased the horizontal size of this figure to better represent the information but expanding the Y-axis to cover complete error bars makes the figure unwieldy.
- We have removed the start of insolation metric and instead simply show the insolation curve on the figure with the same Y-range across all Terminations to enable comparison.
- We have expressed time everywhere in ky BP or ka now and restricted age expression to one decimal place.

Figure 7 is only of limited use as the authors focus solely on oxygen isotope magnitudes, whereas growth phases (important for some alpine sites and related to cold snaps) are not included. In addition, the amplitude of the few existing paleotemperature estimates is also not included. The sequence of events in Figure 7 is therefore incomplete and rather selective.

I agree it is useful to include in Fig. 7 growth phases and temperature estimates as suggested by the reviewer (incorporate these into the main text) but urging caution regarding interpretation of any single speleothem records of growth/interruption.

We fully agree with the editor that extreme caution would be necessary to make robust interpretations of climate from speleothem growth phase/interruption. For example, work in the recent paper on growth phases in the British isles by Panitz et al (https://doi.org/10.5194/cp-21-261-2025) really exemplifies both the power and the complexity of using growth phases to derive paleoclimatic information. For this reason, we propose to not overextend an already long paper by introducing an additional proxy (growth phases/hiatus). Rather, we hope that future projects and collaborations can take on the task of integrating information from growth phases and growth stops in a transparent and robust way to provide complementary data. Regarding the proxies discussed in this paper and in Figure 7, we compare the oxygen isotope records of timing of temperature changes and extend these

by describing in the discussion the absolute temperature estimates from Hungarian caves and from Schrattenkarst.

More importantly, it is still unclear how the timings, durations and amplitudes of these events/intervals were calculated.

Please specify these in the text or figure caption where applicable.

Ah that's a good point. I'm sorry. I have expanded on this in the methods section in sub-section 2.1.

Not to mention that the chronologies of the records in Figure 4 are based on different chronology building approaches.

I agree that this is an issue, but it cannot be dealt with in this paper (it's a separate and long overdue study itself).

Though one could find some information in the table, some of the records shown in figure 4 are not included in Table 1 (e.g., Siebenhengste, Schneckenloch, Diamante).

Please rectify

We have elaborated on the source of the information and corrections further in the figure captions. I'm sorry that this is causing confusion.

Furthermore, my initial concern regarding a more scientific and statistically sound approach to calculate ages for the onset/end of the termination and timing of dry/cold or warm/wet phases were not addressed in the revised manuscript. This is still a major weakness, particularly because the authors give sometimes extremely precise age estimates for some terminations, such as "~6985 years from the initial weak monsoon event to monsoon recovery".

See comment above re expression of time in ka/kyr, and add uncertainties in quadrature when specifying durations (although some age models seem to underestimate interpolation uncertainties!).

I'm sorry about this. We have made this correction.

Overall, I think that the current manuscript is not suited for publication in CoP as it reads more like an initial draft. In my first review, I had hopes that the authors would revise their manuscript thoroughly, but they missed this opportunity, leaving me with no choice but to

reject it. However, a review on the timing of terminations in different speleothem records is overdue and I would encourage the authors to revise and resubmit their manuscript to CoP.

I agree – with a little more work, this ms can make a useful contribution

We appreciate the clarification on additional aspects of the paper which require further revisions and have diligently followed through on these points.