

Reviewer 1

General assessment

*This manuscript presents a high-resolution (annual to sub-annual), multi-proxy speleothem reconstruction covering the last millennium, supported by modern cave monitoring. The density of the dataset, the meticulous chronological framework, and the effort to integrate active monitoring data are highly commendable. The record has great potential to contribute significantly to our understanding of Western Mediterranean paleoclimate during the last millennia. The study combines U-Th chronology, lamina counting, trace elements, stable isotopes, petrography, and forward modelling to reconstruct hydroclimate variability and proposes changes in Atlantic versus Mediterranean climatic forcing, including variations in DANA activity. The manuscript is generally well written and presents an impressive amount of high-quality data, the chronology appears carefully developed, and the site has clear potential to provide an important high-resolution record for southern Iberia. The annual lamination, modern cave monitoring, and multiproxy approach are valuable contributions. All these together would support publication in a well-known journal like *Climate of the Past*.*

Response: We thank Reviewer 1 for this positive assessment of the dataset, chronology, and multiproxy approach. We also appreciate the detailed comments.

Comment 1. Geochemical inconsistencies and proxy decoupling ($\delta^{13}\text{C}$ vs. Mg/Ca)

The authors heavily rely on Prior Calcite Precipitation (PCP), longer residence times, and dolomite dissolution to explain their proxy variations, but their arguments contain a major thermodynamic contradiction:

-The decoupling paradox: The authors state that $\delta^{13}\text{C}$ and Mg/Ca are poorly correlated. In classical speleothem geochemistry, if prolonged dry periods cause slower drip rates, enhanced degasification, and PCP in the epikarst, both $\delta^{13}\text{C}$ and Mg/Ca must increase in tandem. The manuscript simultaneously invokes several different mechanisms. First, PCP may be suppressed under very dry conditions because of reduced soil $p\text{CO}_2$ and lower initial saturation. Second, Mg enrichment is attributed to longer residence times and dolomite dissolution. Third, Mg is interpreted as partly reflecting detrital inputs because of its correlation with Al. Finally, Mg variability is also linked to seasonal ventilation and to centennial-scale hydroclimate variability. These mechanisms are not clearly reconciled, making it difficult to understand what Mg/Ca what the proxy signal truly reflects. The discussion repeatedly shifts among these interpretations depending on the climatic interval considered, making it difficult to understand what Mg/Ca actually records. Similarly, Sr/Ca is interpreted as a growth-rate proxy, a PCP proxy, a seasonal proxy, and a wetness indicator. These interpretations are not always mutually compatible and require a much clearer mechanistic framework. The PCA is presented as supporting these interpretations, but PCA alone cannot uniquely identify geochemical processes. The loading patterns are not sufficient evidence for assigning individual components to PCP, residence time, or detrital inputs.

-Circular reasoning: To explain the lack of visual correlation during certain intervals, the authors argue that severe droughts reduced soil CO_2 , which lowered the initial calcium carbonate saturation state of the drip waters, thereby limiting PCP. However, if PCP was restricted, it cannot simultaneously be invoked as the primary driver for the enrichment in $\delta^{13}\text{C}$.

The authors should resolve this contradiction more clearly and present a coherent mechanistic framework for the reader. If they maintain that PCP is the driver, they must explain the decoupling in a quantitative geochemical way (e.g., $\ln(\text{Mg/Ca})$ vs. $\ln(\text{Sr/Ca})$ systematics, verify if there are higher or lower correlations between the sections where $\delta^{13}\text{C}$ and Mg are visually correlated or not). Otherwise, they must temper their claims and explore alternative drivers for $\delta^{13}\text{C}$ and Mg/Ca variations. Overall, the trace-element discussion should be substantially simplified and focused on mechanisms that are directly supported by the observations.

Response: We agree that the mechanistic hierarchy was not sufficiently clear in the original version. Mg/Ca is treated as a recharge-sensitive hydrochemical signal based on the combined modern observations, I-

STAL results, proxy relationships, and published process understanding. In the revised manuscript, Mg/Ca is interpreted primarily as reflecting the composition of the water reaching the stalagmite, linked to effective recharge, flow regime, epikarst residence time, water–rock interaction, and upstream hydrochemical evolution, including possible upstream degassing and PCP. This hydrochemical signal may be further modified by coupled degassing and PCP during calcite precipitation at the stalagmite where sufficient supersaturation and continued calcite deposition are maintained, while extreme drying may additionally limit its preservation in calcite.

This hierarchy addresses the apparent Mg/Ca– $\delta^{13}\text{C}$ contradiction. Lower soil $p\text{CO}_2$ may shift the initial soil–epikarst carbon signal toward higher $\delta^{13}\text{C}$ while simultaneously reducing initial dripwater saturation and limiting the coupled degassing-and-PCP response of both $\delta^{13}\text{C}$ and Mg/Ca. It is therefore not invoked to produce a PCP-related $\delta^{13}\text{C}$ enrichment independently of Mg/Ca. Although this nonlinear behaviour was already considered in the original manuscript, the revised text now clarifies that the partial decoupling reflects the nonlinear response of PCP to drip interval and initial saturation, superimposed on distinct initial Mg/Ca and carbon signals. The revised text also states this sequence explicitly and separates it from preservation filtering when calcite growth becomes strongly reduced or absent.

To address the requested quantitative evaluation, we added to the revised manuscript ordinary least-squares $\ln(\text{Mg}/\text{Ca})$ – $\ln(\text{Sr}/\text{Ca})$ and $\ln(\text{Mg}/\text{Ca})$ – $\delta^{13}\text{C}$ regressions and evaluated Mg/Ca– $\delta^{13}\text{C}$ separately by lamina type. The $\ln$ – $\ln$ relationship is weakly negative ($r = -0.26$, $p = 0.008$, $n = 107$), which is inconsistent with a simple, continuously dominant coupled degassing-and-PCP trajectory but does not exclude a secondary contribution under favourable depositional conditions. Mg/Ca and $\delta^{13}\text{C}$ covary modestly within dark compact laminae ($r = 0.280$, $p = 0.029$), but not within light porous laminae ($r = 0.016$, $p = 0.935$) or across the complete transect, supporting a restricted common overprint rather than PCP as the sole control. We have prepared supplementary tables with the lamina-specific statistical results and a supplementary figure showing the $\ln(\text{Mg}/\text{Ca})$ – $\ln(\text{Sr}/\text{Ca})$ and $\delta^{13}\text{C}$ – $\ln(\text{Mg}/\text{Ca})$ regressions. These additions would accompany a revised manuscript; the correlations and their limitations are reported here so that our response can be evaluated without those materials.

The modern 17-fold Mg/Ca contrast between the dry 2016 and very wet 2018 campaigns is consistent with this recharge-sensitive hydrochemical framework and reflects changes in both Mg and Ca concentrations. However, because the monitoring consists of two discrete campaigns, the relative contribution of the individual hydrochemical processes cannot be separated quantitatively.

The modest Mg/Ca–Al/Ca relationship is restricted to a possible episodic particle-associated contribution and is not used to infer a dominant detrital control. Seasonal ventilation is treated as part of the depositional and lamina-forming framework rather than as an alternative first-order control on Mg/Ca. PCA is now presented as an exploratory summary of covariance structure rather than as independent evidence for identifying geochemical processes. Process interpretations rely on PCA together with geochemical, monitoring, petrographic, and modelling evidence; individual PCs are not assigned uniquely to PCP, residence time, growth effects, or detrital input.

Comment 2. Contradictions between observations and interpretations

Several interpretations presented in the manuscript appear inconsistent with the data. For example:

Mg/Ca is interpreted as recording aridity, yet it shows only weak or absent relationships with $\delta^{13}\text{C}$, despite both supposedly responding to PCP.

The manuscript argues that PCP is an important control, while also explaining the lack of Mg– $\delta^{13}\text{C}$ covariance by invoking reduced saturation during dry periods.

Dark laminae are interpreted as wetter conditions because of elevated Sr/Ca and Ba/Ca, but simultaneously display higher $\delta^{13}\text{C}$, which is generally interpreted as drier conditions.

Although the authors attempt to reconcile these observations through multiple interacting processes, the final conceptual model remains difficult to follow and is not fully convincing. The discussion would benefit from explicitly acknowledging the limitations of interpreting trace elements in such a complex hydrological system rather than proposing several simultaneous explanations.

Response: We agree that the earlier conceptual model may have allowed the meaning assigned to individual proxies to shift between intervals. Mg/Ca is now treated as a recharge-sensitive hydrochemical signal that may receive a secondary, condition-dependent depositional overprint, rather than as a direct or uniquely calibrated aridity proxy. $\delta^{13}\text{C}$ integrates an initial soil–epikarst carbon component and a possible in-cave overprint. Lower saturation may limit the response of both $\delta^{13}\text{C}$ and Mg/Ca to coupled degassing and PCP and can therefore explain why some dry intervals lack a clear response to coupled degassing and PCP in either proxy; however, it is not invoked to generate enrichment of $\delta^{13}\text{C}$ through coupled degassing and PCP independently of Mg/Ca. Their partial decoupling is instead interpreted as being consistent with the nonlinear response of coupled degassing and PCP to drip interval and initial saturation, superimposed on variations in their respective initial signals. Dark compact laminae are also no longer interpreted as a simple wet end member. Their higher Sr/Ca and Ba/Ca are assigned to a contrasting growth- and partitioning-sensitive depositional regime, whereas their higher $\delta^{13}\text{C}$ reflects a different initial carbon signal with a possible secondary in-cave overprint; their co-occurrence is therefore not used to imply a direct Sr/Ca– $\delta^{13}\text{C}$ mechanism (also clarified in the updated seasonal interpretation). Finally, the manuscript explicitly acknowledges that the proposed hierarchy is qualitative in terms of causal attribution and that the available observations do not uniquely constrain the numerical contribution of each individual process. For clarity, Figure 7 has been simplified to retain only the conceptual Mg/Ca seasonal trajectory. Sr/Ca is no longer shown as a monthly curve because its interpretation is primarily growth- and partitioning-sensitive rather than a direct hydroclimatic trajectory. We have also prepared a supplementary table to show the expected relative behaviour of Mg/Ca, Sr/Ca, and $\delta^{13}\text{C}$ qualitatively. It would accompany the revised manuscript.

Comment 3. What is truly an extreme event in Mg/Ca changes?

In the chapter explaining changes in Mg concentrations when monitoring cave drips, it states that during abrupt transitions that show changes between wet and dry years, the concentrations can increase up to 17 times the normal Mg/Ca concentration. The appendix clearly shows how the concentrations of the dripping water on the stalagmite can be compared, making it clear that an event of the magnitude of those between 2016 and 2018 has not been recorded on the stalagmite. Do all of the inferred dry events represent statistically or geochemically significant excursions? At least the transition from the beginning of the dry medieval period to its second phase shows a decrease of at least 3 to 4 times in Mg/Ca concentrations, but the others do not differ in the same way. For example, is the shift around 1500 CE statistically distinguishable from background variability?

Response: We agree that the four excursions are not equivalent. The manuscript now describes them as relative dry excursions of differing magnitude: those near ~1125 and ~1560 CE are the most pronounced, whereas those near ~1250 and ~1500 CE are more moderate (Abstract, Section 5.5, Conclusions). To address the reviewer's specific question about ~1500 CE, we standardized the unsmoothed Mg/Ca series and used $z = +1.96$ as a descriptive reference threshold. The ~1500 CE interval contains more than one value above this threshold. This supports describing it as a relative excursion, but we did not perform a formal event-detection test accounting for serial dependence. This z-score check is provided only in this response to the reviewer; it has not been added to the manuscript or adopted as an event-detection criterion. The modern 17-fold dripwater contrast is retained as hydrochemical context, not as a magnitude expected to be preserved in calcite; extreme drying may substantially reduce calcite deposition or produce a hiatus (Sections 5.1 and 5.5; Appendix A).

Comment 4. Speculative Atmospheric Dynamics and the NAO

The manuscript attempts to frame the regional climate evolution entirely through the lens of the North Atlantic Oscillation (NAO), but the dynamic mechanisms presented are weak:

The transition disconnects: The authors admit that during the key transitional periods of 1320–1450 CE and 1600–1700 CE, their speleothem record shows a very weak relationship with Atlantic forcing.

Greater influence of Mediterranean-sourced precipitation: To bypass this lack of correlation, the authors attribute these transitional intervals to a dominance of Mediterranean-sourced convective precipitation

(e.g., DANAs). While meteorologically plausible on a weekly scale, invoking highly localized, ephemeral convective events to explain multi-decadal to centennial proxy trends in a speleothem record without physical, independent geological evidence is highly speculative. The interpretation is inferred from increased variability in Mg/Ca and associated proxies. However, enhanced variability could equally reflect: a) changes in recharge, b) karst hydrology, c) seasonal precipitation distribution, d) growth dynamics, e) analytical variability or f) changing moisture sources.

Lack of regional synthesis: One of the central conclusions is that the record reflects changes in NAO variability. However, the manuscript compares the speleothem primarily with one NAO reconstruction (Trouet et al., 2009). Given the large number of available NAO reconstructions covering the last millennium (e.g., Faust et al. 2016 and Hernández et al. 2020), relying on a single record is insufficient. Different NAO reconstructions differ substantially in timing, amplitude, and even sign over some intervals. Therefore, conclusions regarding NAO forcing should be tested against multiple reconstructions. The manuscript should include comparisons with additional NAO reconstructions and discuss the uncertainties associated with reconstructing medieval NAO variability. Without this broader comparison, the conclusion that the record primarily reflects NAO variability is not adequately supported. Furthermore, there are almost no comments on the possible effects that East Atlantic (EA) and Scandinavian (SCAND) patterns could have on this region.

The authors should compare their record against existing high-resolution continental archives from the Iberian Peninsula (e.g., other NAO records, speleothems from northern Spain or the Pyrenees, or high-resolution lake records). Here, the rainfall record of Andalusia (Rodrigo et al., 1999) is very helpful in arguing the changes from wet to dry during 1520 to 1560 AD, unlike records from Portugal and Morocco, which show clear differences with the record of the Ardales cave in several sections during the last millennium. This comparative spatial analysis is required to robustly demonstrate if the regional NAO signature actually breaks down during these transitional centuries or if local cave/karst routing effects are dominating the signal.

Response: We thank the reviewer for this detailed comment. In Section 5.5 and Figure 8 we have made four linked changes:

- (i) We compare several NAO reconstructions and explain their disagreement across time scales;
- (ii) we include reconstructions of the East Atlantic (EA) and Scandinavian (SCAND) patterns;
- (iii) we extend the comparison with Iberian and western Mediterranean archives; and
- (iv) we treat an increased relative contribution of cut-off-low-related precipitation as a working hypothesis, alongside alternative explanations for the observed variability.

As a result, the manuscript no longer states that the record primarily reflects NAO variability. The record is now compared with the NAO reconstructions of Trouet et al. (2009), Hernández et al. (2020) together with Ortega et al. (2015) and Cook et al. (2019), the EA reconstruction of Sjolte et al. (2018), and the SCAND reconstruction of Xue et al. (2026). The revised text states the factors that condition this comparison. The reconstructions differ in archives, regions, seasonal windows and statistical approaches. The persistent positive NAO during the MCA inferred by Trouet et al. (2009) has been questioned by robustness tests and by multi-proxy, model-tested reconstructions (Lehner et al., 2012; Ortega et al., 2015; Cook et al., 2019). The reconstructions broadly agree at centennial scales but diverge at decadal scales (Hernández et al., 2020; Figure 8). The Iberian reconstruction of Hernández et al. (2020) represents the local NAO impact in central Iberia on a decadal grid and is undefined for c. 1200–1270 CE; that's why this gap is masked in the modified figure 8. This comparison shows that the correspondence between Zerolín and the NAO is partial and reconstruction-dependent. In Trouet et al. (2009), the dry excursions occur within intervals of positive or recovering NAO. However, positive values also persist during the humid intervals between 1150–1200 and close to 1300 CE, and the mid-16th-century excursion overlaps a brief negative excursion. The Iberian reconstruction does not show a consistent NAO phase during the dry excursions. We have therefore removed the statements that each dry excursion aligns with a positive NAO peak and that the NAO interpretation is "unequivocally validated". The Zerolín record is now described as consistent with a hydroclimatically variable, rather than persistently positive-NAO, late

MCA. The NAO discussion is restricted to what the comparison supports: the intervals of enhanced Zerolín variability (1320–1450 and 1600–1700 CE) coincide with weak, near-neutral or rapidly changing NAO states and with greater disagreement among reconstructions.

Considering EA and SCAND indexes, the revised text considers three pathways through which these modes could affect the record. First, EA and SCAND modulate the position and strength of the NAO dipole and hence the non-stationary NAO–precipitation relationship over Iberia (Comas-Bru and McDermott, 2014; Sánchez-López et al., 2016; Mellado-Cano et al., 2019; Hernández et al., 2020). A weaker Zerolín–NAO correspondence is therefore expected during intervals of weak NAO or changing NAO–EA/SCAND combinations. Second, summer SCAND has been identified as a leading dynamical control on Mediterranean warm-season hydroclimate over the last millennium (Xue et al., 2026). Summer precipitation is negligible at Ardales, but the intensity of the summer water deficit matters for our interpretation. It controls residence time, the length of the seasonal depositional hiatus, and the Mg/Ca enrichment of the epikarst water mobilized by the first late-summer–autumn recharge. Third, cut-off low occurrence over the European sector is season-dependent, being favoured by positive NAO in autumn and by blocking in winter (Nieto et al., 2007), and blocking configurations substantially modify Iberian precipitation (Sousa et al., 2016). Cut-off low related precipitation therefore cannot be tied to a single NAO phase. These reconstructions also have limitations. They differ in season (winter EA, summer SCAND) and in temporal coverage (EA from 1241 CE), and the SCAND amplitude is damped in earlier centuries. Neither shows a systematic one-to-one relationship with the Zerolín excursions. They are therefore used to identify physically plausible non-NAO pathways, not to attribute individual intervals to a specific mode.

As suggested, the Andalusian precipitation index (Rodrigo et al., 1999) is now used explicitly. It shows near-normal to wetter conditions in the early 16th century, drier conditions in the mid-16th century, and markedly wetter and more variable conditions from the 1570s onward. This supports the humid-dry sequence recorded by Zerolín around 1520 CE and during 1545–1570 CE. For the late 16th and early 17th centuries, documentary evidence independent of the speleothem reports a phase of enhanced catastrophic-flood frequency on the Spanish Mediterranean coast (1570–1630 CE; Barriendos and Martín-Vide, 1998). These sources do not identify the synoptic origin of individual events. However, they show that the onset of the second interval of enhanced Zerolín variability coincides with regionally documented heavy-rainfall activity. We also compare the record with Iberian lake, marine, and speleothem archives (Moreno et al., 2012; Martín-Puertas et al., 2010; Ramos-Román et al., 2016; Bartolomé et al., 2024).

We also agree that the western Portuguese and Moroccan records differ from Zerolín in several intervals. These differences are now discussed in terms of site exposure and seasonal sensitivity, including differences in exposure to Atlantic winter precipitation and Azores High variability (Thatcher et al., 2023; Ait Brahim et al., 2019), and in terms of chronological uncertainty. Importantly, we no longer use the lack of regional coherence during 1320–1450 and 1600–1700 CE as evidence for a Cut-off low (DANA) mechanism. Such incoherence is expected under a locally expressed precipitation regime, but it is not diagnostic of it. It could equally arise from dating uncertainties, different proxy sensitivities or local karst effects.

We now state explicitly that the proxies constrain a change in recharge regime. Its attribution to a greater share of Mediterranean-sourced late-summer–autumn precipitation, and specifically to cut-off lows, is an inference based on modern analogues. The hypothesis does not invoke individual ephemeral events to explain multidecadal trends. Instead, it invokes changes in the frequency and seasonal share of heavy rainfall within a recharge budget that, in southern and eastern Iberia, is dominated by a small number of intense events (Martín-Vide, 2004). These intervals are therefore described as compatible with an increased relative contribution of such precipitation, for which cut-off lows are a plausible but not independently demonstrated mechanism.

Comment 5. Analytical discrepancy in colloidal elements

In Section 4.3, the authors mention that elements typically associated with detrital or colloidal fractions (such as Al and Y) yielded concentrations up to 400% higher when measured via laser ablation (LA-ICP-MS) compared to conventional wet-chemistry solution ICP-MS (which used a weak nitric acid digestion).

The explanation that the weak acid digestion simply failed to dissolve the colloidal/detrital phase is insufficient. A 400% discrepancy is massive and suggests either a major calibration issue in the LA-ICP-MS data (e.g., matrix effects, standard mismatch) or that the laser ablation was actively blasting and vaporizing micro-inclusions that are not representative of the homogeneous calcite matrix.

It would be useful to provide the exact details of the standard materials used for the LA-ICP-MS calibration, quantify the analytical uncertainty, and justify why the LA-ICP-MS Al/Ca and Y/Ca spikes can be reliably interpreted as paleo-environmental "extreme events" rather than analytical artifacts.

Response: We thank the reviewer for raising this analytical issue. We agree that the original wording could be interpreted as indicating a systematic LA-ICP-MS overestimation and as attributing the discrepancy too exclusively to incomplete weak-acid dissolution. We have therefore revised the text and added details on calibration and analytical reproducibility. ^{43}Ca was used as the internal standard, assuming 400,000 $\mu\text{g/g}$ Ca in calcite. NIST SRM 610, NIST SRM 612, and USGS MACS-3 were used to derive three independent Ca-normalised calibrations, and the reported concentrations correspond to the arithmetic mean of the three resulting concentration estimates. Al/Ca reproducibility ranged from 0.8 to 7.8 %, Y/Ca reproducibility from 1.2 to 5.8 %, and between-material calibration variability was 19.7 % for Al and 18.5 % for Y. We have prepared a supplementary table detailing these calibration and reproducibility results for the revised manuscript.

Across the common analytical interval, median Al/Ca values were nearly identical for LA-ICP-MS and solution analysis (0.210 and 0.206 mmol/mol), providing no evidence of a substantial systematic offset. Local differences of approximately 400 % occurred only at isolated stratigraphic positions and exceeded replicate and calibration-related variability. These maxima reflected increases in ^{27}Al and ^{89}Y rather than anomalously low ^{43}Ca signals. The sharp maxima are therefore interpreted as spatially localised Al- and Y-rich detrital or colloidal microphases intercepted by LA-ICP-MS but spatially averaged within the wider micromilled samples. Incomplete dissolution or recovery of some detrital or colloidal material during weak-acid treatment may also contribute, but is not considered the sole explanation. Individual maxima are not interpreted as palaeoenvironmental extreme events or used to infer rainfall magnitude; only broader, stratigraphically coherent enrichments are considered potential indicators of enhanced detrital or colloidal transport.

Comment 6. Sentence could go

Line 79-83: I think this sentence could go in chapter 2 describing the study site.

Response: We agree. The paragraph describing the seasonal contributions of Atlantic winter–spring recharge and less frequent Mediterranean summer–autumn precipitation has been moved from the Introduction to Section 2.2, where it now forms part of the hydroclimatic description of the study site.

Comment 7. Figure 2 chronology axis

Changing the x-axis ages to CE instead of years before present will maintain consistency in the units for ages throughout the paper. I recommend changing them to AD.

Response: We thank the reviewer for this suggestion. The x-axis has been changed from relative years since 2007 to calendar years CE to ensure consistency with the chronological units used throughout the manuscript.

Location in the prepared revision: Figure 2 and its caption.

Comment 8. Figure 8-alignment of the time series

Figure 8: The alignment of the time-series is difficult to follow. Please add the x-axis also in the top of the figure to help the reader visually track the proxy transitions.

Response: We agree. A second calendar-age x-axis has been added at the top of Figure 8, aligned with the lower x-axis, to facilitate visual comparison of transitions among the different time series (Figure 8 and caption).

Comment 9. Figure 8-not-useful Al/Ca

Figure 8: Is useful to show the Al/Ca concentration in this figure? I don't see it being used in the discussion and it could free up some space in the figure.

Response: We agree that Al/Ca was not required for the long-term comparison and occupied space without contributing directly to the discussion developed around Figure 8. The Al/Ca panel has therefore been removed, and the caption numbering has been updated accordingly (Figure 8 and caption).