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Climate of the Past Discussions

Interactive comment on "Reconstructing seasonality through stable isotope and trace element analysis of the Proserpine stalagmite, Han-sur-Lesse Cave, Belgium: indications for climate-driven changes during the last 400 years" *by* Stef Vansteenberge et al.

Anonymous Referee #3

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Cp-2019-78 Review

My general comments are embedded in the specific sections pertaining to Lines in the text. Overall, I believe that the principal hypothesis has to be better supported by data and the discussion has to encompass other alternate hypotheses. . .based on the data.

Line 42: Perhaps it is best to replace "their ability to hold. . ." with: Some speleothems are characterized by distinct physical and/or geochemical layering that enables past climate and environmental reconstruction at the seasonal scale (Ref. is needed here, such as, for example: Mattey et al., 2008; Boch & Spötl, 2008).

Line 48: it is not entirely correct that visible laminae develop because of change in drip rate. They can also be caused by variation in cave ventilation, and, therefore, in the pH of the film of fluid driven by seasonal intensity of the degassing process, which then produces laminae consisting of compact calcite and dark, impurity rich, more porous calcite (see for example Frisia et al., 2003) or white, porous calcite (Boch et al., 2010). In particular, Boch et al (2010) suggest that drip rate influences the thickness of the laminae, whilst the fabrics reflect seasonality in degassing. True that the two may be related (degassing and fabric) but this needs to be somehow written, just to show that there is complexity in this two-fabric system. Actually, such clarification would explain best the following paragraphs.

Line 51: PCP can also occur in the epikarst, in fractures or pores, not just in the cave. So, it is not entirely appropriate to state that the ability of the cave waters to degas is increased. Infiltration waters is best.

Line 54: replace is known with "has been known"

Line 55: replace stalagmite collected with "Proserpine stalagmite, which was collected in the cave of Han-sur-Lesse and first studied by Verheiden. . .. Line 57: replace "according to" with "as inferred from U/Th dating and lamina counting between the radiometric ages (. . ..).

Line 65: there are many papers before the Regattieri et al. (2016) which demonstrated the importance of trace elements in tracing the seasonality of hydroclimate in stalagmites. Please refer to Treble et al., 2003; Fairchild and Treble (2009), Griffiths et al., (2010).

Line 70: actually, the first to use trace elements to fingerprint volcanic events were Frisia et al. (2005). The most accurate expression here would be: and can be used to identify changes in atmospheric load of anthropogenically and volcanic derived aerosols, as well as volcanic ash

Line 71: please add after geochemical layering : “of Proserpina” line 73: replace “the relation” with its relation. Replace “this work” with the present study. Line 75: I would rephrase for clarity: To achieve....three sections of the Proserpina stalagmite, each encompassing 12 years of growth, were analysed, namely : P16 (from....to), P17 (from to) and P 19 (from ...to). Each section was analysed at sub-annual resolution ..... this sentence is not entirely clear. Do you mean: the results from the selected sections were then applied to interpret in terms of seasonality of hydroclimate variability climate variability the Proserpina stable isotope ratio curves for the last 400 years.

Please, check if I understood correctly what was meant with your sentence. I looked at the figures ... but I may be wrong.

Line 78 replace elemental with element

Apologies, I have become a bit annoyed at having to correct grammar and typos...I have started highlighting issues in yellow on the pdf to save my time.

Line 87 to 91 : local is too vague. What does it mean? Partially dolomitized limestone? As the geology is relevant to the data and discussion, a geologic section of the beds above the cave and inferred distribution of dolomite should be provided. Fig. 1 does not give any geologic information. Somehow, the text is referring to formations that are not shown, the relevance of which is, thus, unknown to the reader. In the opinion of this reviewer, this is not scientifically acceptable, as it fails to provide crucial information. Lines 95-97: Just a suggestion – it would be great help to the reader if a figure with distribution of rainfall and temperatures throughout the year (or a 30 year mean) were presented. True, we are referred to Genty &Deflandre (1998) and Genty & Quinif (1996), but still it is beneficial to have it visually in the present manuscript, even as Supporting Material. It would help understanding the significance of proxy data. Line 103: I suggest to write that it was removed in 2001. Line 108: colder than what? Line

109: what is the time scale of “temporary”? Line 114 replace “record” with values. Line 117: what does tabular mean? Better to rephrase: flat topped surface. Even if tabular was used by Rampelbergh et al. (climate of the past 2015), still the speleothem looks more a composite edifice showing, clearly, from the insert in Fig 1. From Fig. 1 (and Fig. 1b in Rampelbergh et al., 2015) Proserpina it is not a typical stalagmite, but a speleothem that formed through the coalescence of several stalagmites, because of the fast drip rate, into a stalagmitic flowstone characterized by mm- to cm- scale rimmed pools (which is also clear from Verheyden et al., 2006, Figures 1 and 2 showing cahaotic “pool like” deposits). Which hints to the fact that the feeding drip points may be multiple, shift in time and drip rates may have been extremely variable because of rupture of feeding straws (Verheiden et al, Fig 1). And, in fact, the sentence continues by stating that it is fed by a drip “flow”. Thus, I would call it a stalagmitic flowstone. Line 122. State entirely what DCL and WPL are the first time. In any case. Line 156: The way the sentence is written, it would seem that there 50 years of uncertainty > I suppose that “at” should be replaced by from 1960 (to 2000). Line 231 It is unclear to this reviewer why line P17 was taken at an angle with crystal growth. In Fig. 2 B one can see that the directions of columnar crystal growth at the core of the photo is from bottom to top. The P17 was taken laterally. It is unclear, from the photo, if it is meant to be perpendicular to an outer layer , roughly at 45 degrees from the “page vertical direction). Yet, from the “blurred” image I could retrieve, it seems that the fabric is more complex, there could be more “mosaic-type” crystals as in P 19. Please explain the direction of P17, which may actually explain the isotope ratios data in Fig 2, specifically the abrupt shifts. I suggest the Authors to provide a better fabric control for their data. His may also be the reason why the thickness are different for P16 and 17 relative to 19. In P 19 it is clear that the transect cross cuts laminae perpendicularly. Line 234. Perhaps there is an “of” missing? Line 288. Perhaps better to expand for those who are not familiar. Rather than “same samples” elaborate: whilst stable isotope ratios are measured on powders drilled... trace elements are...thus the exact location of the analyses in the same sample may not coincide.... Line 297. What is intended here for mineral? Dissolved ions? And what is intended for organic matter? Please explain. Have any analyses for the nature of the DOC in the H sur L waters been carried out? The type of Organics? LMW-A, LMW-N, building blocks etc? Works by Hartland et al. (2012), Rutlidge et al. (2014) are showing a connection between the NOM and trace element transport in cave waters (and trace element content in stalagmites). Thus, in order to better understand the trace element variability, a knowledge of DOC in H sur L waters is needed (see the speculation at Line 8. This can be solved by having a characterization of NOM in the feeding waters). Line 316 to 323. The discussion about P becomes highly speculative and citing Frisia et al (2012) is, here, not the case, as the case-study in the cited paper is from locations where P is known to be present in the host rock and/or is related to stromatolite-like features. Unless the Authors show petrographic features that resemble microbially induced ones, the hypothesis has no grounds. And the same applies for the host rock, Authors do not provide a geochemical characterization of the rock. Hence, Authors simply do not know where P may come from and why it behaves the way it does. This is why they need to provide a DOC characterization of the dripwater. As there are organic acids that favour the mobilization of P. Each cave has its own environment and diverse types of soil. Also: Vegetation dieback for P was hypothesized in a seminal paper by Treble et al (2003), which I believe should be cited, as it provides a diverse context that that of Baldini et al (2002).

Lines 339 to 393 The issue here is that the different behaviour from P19, P16 and P17 is that P19, from Fig 2, is the only clear transect that is perpendicular to parallel growth layers that are perfectly flat. The delta 13C values and flat tops with a seemingly predominant compact fabric indicate that P19 formed from a thin film of fluid that resided for a longer time (than P17 and 16) at the top surface of the growing stalagmite. This allowed prolonged degassing. At this stage, the correlation between delta 13 C, Mg and Sr may be slightly different than just PCP, the. . ..delta 13 C may be also influenced by in- cave degassing, but all processes lead to the same conclusion: less infiltration. For the antiphase correlation between Mg and Sr, the Authors speculate about a role for dolomite dissolution, albeit the dolomite bodies are not presented in a geologic section. If this were the case, it should hold also forP19, unless the hydrological pathway changed completely. But there is another explanation, for which the Authors should consult Rutlidge et al (2014). The Authors should actually analyse other elements, such as Al, K, Si and check their behaviour in relation to Mg and Sr. Works that use Sr isotopes have highlighted that Sr may have a twofold provenance: from soil and from rock (Belli et al.,2017). Mg has also a similar twofold provenance (Rutlidge et al., 2014). Now, if Sr had provenance from dolomite and limestone, the Authors should explain why the overall Sr values in P19, 16 and 17 are similar. If Sr in P19 came only from limestone, why is it that the baseline is the same (about 60 ppm?). Also, the Mg concentration in P 19 is overall higher than in P16 and 17, which should not be expected if the hydrological pathway was in a pure limestone, or dolomitized limestone. So, the actual possibility is that something changed between 16 and 17, rather than "somewhat" between P16 and P19., but has nothing to do with dolomite (or little to do with it). It is the Y and Zn cycles that actually point to that. It could be soil. The other problem is that the Author cite shales. And shales may be rich in Sr. And Zn and Y Thus, trace elements and/or Sr isotopes that point to provenance of Sr (soil or shales) would give robustness to the speculation. Line 415Incongruent dolomite dissolution is not really demonstrated by the data, as I mentioned above, there should be a change in the mean Mg and Sr values from P19 and P17/16, there is not. The baseline is similar. Also, in P17 and 16 the Mg is not always co-varying with the delta13C (Fig.5). If IDD were acting, I would expect that higher Mg coincided with more positive delta 13C, because of dolomite dissolution. But when the Mg is high and delta 13C is more negative, that there is an issue. Unless Mg comes from the soil, or the dolomitized shales have a low delta13C because they are rich in organic matter (I suppose they are dark, but I may be mistaken). To support the IDD hypothesis, I sugget that the Authors provide a geochemical composition for the limestones, the shales and the dolomite, which would help understanding the effects of the processes they invoke (PCO vs IDD) Line 448 I do not understand why a change in "cave morphology" is cited here. IWhat would be important, from the data presented by the Author, is a change in ENVIRONMENTAL parameters. Not necessarily climate. This reviewer admits a poor knowledge of the studied region, but being in Belgium, I suppose that humans have somewhat impacted the land. Even if it does not appear “visible”, land use (tree cutting, particularly in the LIA, when people had to find wood somewhere….) may have altered the hydrological pathways, changed soil (uprooting). These options…of anthropogenic disturbance, need to be investigated also for P 17 and 16, not just P19. Line 427: may also indicate that soil disturbance/forest disturbance above the cave resulted in faster infiltration for P19, because of less soil humidity retention. And, obviously, the rise in temperature in the last 150 years related to the end of LIA, industrial “revolution” and increase in GHG in the atmosphere. Line 473-489 This part of the discussion does not account for land use. Because the data are discussed with the assumption of PCP versus IDD. But this reviewer fails to see unequivocal proof of IDD from the trace element data. Lines 490-517 Actually, the best piece of information in favour of higher recharge in the 17th century is the lamina thickness, given that the delta13C values seem similar over the three periods. The delta 18O could reflect cooler air masses in the 17th century, or a diverse provenance. So it may not be a n unequivocal marker of enhanced rainfall in winter. But the thicker laminae in a cold period need to have water supply and an efficient soil/microbes/fungal hyphae. The marked seasonality could well be a marker of snowmelt, which should be considered as hypothesis. Actually, if the bulk of infiltration in P16 and 17 came at snowmelt rather than in Autumn (as I suppose there is no historical record of rainfall patterns for the 17th Century), then the behaviour of P may be explained…there is no vegetation dieback in Spring.

Hope these comments help.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2019-78/cp-2019-78-RC3-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-78, 2019.