Dear Climate of the Past Editorial Board, Dear reviewers,

On behalf of all the authors, I would like to express my gratitude to the three reviewers who have published comments on our work in the interactive discussion. Below, we provide a point-by-point reply to all questions and remarks raised by our reviewers and provide suggestions to how we can improve our manuscript taking into account the comments of our reviewers. We reiterate comments by reviewers in italics and provide our answers (in red) directly below the comments.

Reviewer 1 (Robert Andrew Jamieson)

This is an excellent paper which pushes speleothem science forward in a significant way. The last paragraph of the conclusions in particular is an absolutely key insight which all speleothem scientists should bear in mind going forward.

In addition to vital considerations of the challenges in interpreting records within a stalagmite where the underlying cause (or causes) shift in importance through time, the authors also present useful information about historical climate variations in central Europe

I recommend that this manuscript should be published, and only have a few (hopefully constructive) minor comments and tweaks which I would suggest are made by the authors:

In section 2.1 (lines 92-101) the modern climate is summarised and reference made to calculated evapotranspiration effects. This information is essential, and is further discussed later on in section 5.3 where further calculations and a figure are included. However, no figure is shown here to summarise the data. This could be fixed with a reference to the subsequent figure (Figure 8, currently).

We will refer to current figure 8 (moving it forward in the manuscript) displaying a climatogram of the modern climate

Additionally in this section, average (mean?) values are given for both rainfall and temperature. Given the main thrust of this paper is about seasonality and variability it would seem important that some discussion of the variability is also included (SDs on means, some kind of measure of mean ranges?).

We agree that this is a good idea and will add a sentence describing the present-day climate in terms of seasonal variability.

Line 107: "Drip Water" within the cave is discussed. It would be useful to indicate whether this is a general cave value, or specific to the drip site where the stal in question was collected, as with flow path variations these may be different.

Drip waters were sampled by Van Rampelbergh (2014) directly at the site of the Proserpine speleothem and are therefore representative for this study. We will make mention of this in the revised manuscript version.

Line 108: Average pCO2 is given, but no clear indicator of the range. If dissolution or non-deposition are a threat to speleothem growth the maximum value is key.

The pCO_2 value at the site of the Proserpine varies through the year (see Van Rampelbergh et al., 2014). We will make mention of the range of values for clarity.

Line 234: Typo, missing the word "of" at the end of the line

We will add the word “of” here.

Line 241: (This is very nitpicky of me, and I do apologise). "the median is used instead of the average" is an odd phrasing, since the median is a form of average. I assume here that "average" refers to the mean. Here and throughout the rest of the manuscript I would ensure that either "mean" or "median" are used in the relevant places to avoid any confusion.

We agree that this is confusing and will replace any mention of "average" in the manuscript with the word "mean" to avoid this confusion. This was also noted by other reviewers.

Line 371: Typo, "chances" should read "changes"

We will correct this in the revised version.

Section 5.3: I like this section in general, but I find the idea that a speleothem "switches" between two transfer functions to be a little simplistic. My way of thinking about it (as expressed, perhaps poorly in my 2016 paper which is cited in this manuscript) is to view correct transfer functions as a compound function with multiple terms where the weighting changes. PCP and IDD are both taking place the entire time, but the weighting shifts to make one or the other more dominant. This is perfectly compatible with the results, interpretation and conclusions drawn in this paper. The point stands that one can't simply assume a transfer function based on very short-term monitoring, because there may be a currently low weighted additional term in that function which becomes significant in other time periods. Rather than abrupt switches from one to the other (such as may sometimes be the case, e.g. with flowpath activation) the more common result may be a gradual transition. The author's shouldn't feel that this comment requires a change to the text, I just think they should consider this as an alternative way of describing the phenomena.

We think this is a valid comment and thank the reviewer for better explaining his point made in the paper we cite in this section. We will very likely modify this section in reply to comments from the other reviewers and to leave room in the discussion for alternative hypotheses (such as the land use change proposed by Reviewer 3, which is indeed a plausible alternative).

Line 450: Supp. Mat. Fig. 1 is referred to. Incorrect? S1 is a set of frequency analysis plots.

Correct, this reference should be to Figure 2 (which we moved from the supplement in an earlier version). We will update this reference.

Figure 5: In Figure 5 the P16 section of the record appears to undergo a transition at the 9mm mark where the magnitude of multiple trace element cycles markedly alters. Y, Zn and Mg all seem to increase in cycle maximum magnitude by almost double. This sub-sub-section variability isn't discussed in the text. It probably should be at least mentioned in the results section.

Agreed, we have overlooked this apparent change in the current discussion of the data and will make mention of it at least in the results of the new version.

Reviewer 2 (Anonymous)

The manuscript of Vansteenberge et al presents a number of geochemical analyses across three short time intervals from the well-studied Proserpine stalagmite core from Han-sur-Lesse cave, Belgium. The analyses are very high-resolution, multi-proxy, and high quality, and yield interesting insights into the climate particular to those intervals. The manuscript is very well-written and high-quality throughout, and I recommend publication following the authors' consideration of the following points.

In particular, I suggest that the authors investigate ways of strengthening their discussion regarding the palaeoclimate implications of their results, which I think at the moment are too limited. An enhanced *discussion of how intervals of high-resolution data derived from stalagmites can help understand climate further back in time (e.g., beyond the instrumental era) would help increase the impact of the research.*

This is a helpful suggestion that will indeed make our manuscript stronger. We will implement it by adding a short paragraph at the end of our discussion in which we more clearly explain the implications of high-resolution data for climate reconstructions from speleothems on different timescales.

More specifically, we will emphasize that the expression of trace element records should not be seen as a result of a constant transfer function (see Reviewer 1's comment on our section 5.3). Instead, we propose that the application of high-resolution, multi-proxy transects placed on strategic places along the length/growth axis of a speleothem may be used to check for changes in the response of these proxies to long-term trends and with respect to each other. These changes have implications for the interpretation of the longer, lower resolution proxy records, highlighting the importance of including multiple proxies (e.g. trace element in combination with stable isotope, sedimentary and/or crystallographic analyses) as well as multiple sampling densities (high- and low resolution sampling) to reliably interpret speleothem archives in terms of climate and environmental evolution.

More details regarding the U-Th dating should be included in the main text, including a better description of how U-Th dating and layer counting were combined (line 161). I appreciate that this was done in previous publications and is in the SOM, but a short review (two or three sentences) outlining how layer counting and the U-Th were combined would be useful in the main text. Similarly, the U-Th-derived growth rate does not really feature, but it would be useful to compare to the presumed annual cycle wavelengths; they should be broadly similar (e.g., near lines 271-272).

We will add a few sentences about our age model to our methods section and discuss the (differences in) growth rates derived from the model in our discussion.

The number of counted layer couplets over the annually layered 500-years determined the seasonal character of the layers and demonstrated that two layers (one dark, one light) were deposited per year. The number of years obtained by layer counting between two U-Th datings was compared with the number of years suggested by the U-Th ages. We combine results of both independent dating methods to produce the final age model.

I think that there must be a better way of referring to the time intervals other than the 'P16, P17, and P19'. I did not see any reason stated for why the intervals are named this, and I assume there must also be other intervals (e.g., 'P18') that also exist (maybe as slabs or pencils? Although Fig S5 doesn't imply this) but are not discussed. It almost seems that the number is linked to the century of growth, but of course this doesn't quite work ('P16' is partly in the 16th Century, 'P17' is in the 17th Century, but 'P19' is in the 20th Century). Perhaps simply changing 'P19' to 'P20' and stating that the number corresponds broadly to the century C.E. might help. I suspect that the numbers represent a label of some sort, in which case the authors may not want to change these for bookkeeping reasons. In which case, I suggest that i) somewhere on lines 75-77 it would be useful to explain why the intervals are given these labels, and ii) it might also be useful to occasionally remind the reader what these labels refer to. If changing the labels isn't possible, this will help reader keep track of the time intervals represented.

This is a useful suggestion and we agree that the naming of the slabs might be confusing to the reader. We like the suggestion of changing 'P19' to 'P20' to refer (broadly) to the centuries and will follow this suggestion in our revised manuscript.

The authors should consider a plot similar to the one that they refer to from Jamieson et al., 2016 (lines 451-455). The approach used in Jamieson et al.'s Figure 6 could really help to differentiate some of the processes particular to the three intervals discussed here. Perhaps carbon isotopes versus Mg would yield *interesting insights into the different seasonal cycles inherent to the three intervals. The composite monthly geochemical proxy values shown in Figs 6 and 7 could be plotted as X and Y-axes, with the months labelled for all three intervals.*

We like this suggestion for a cross plot of mean monthly changes in different proxies and will try to implement it in the revised version. We will try to focus on Mg, Sr and d13C for this plot as those are the proxies we discuss most in combination in our discussion.

I am surprised that P does not show an annual cycle. Although the explanation suggested in lines 321-323 is certainly possible (albeit vague), it is still surprising that with a clear 'autumnal flush' of soil-derived material that P should remain unaffected. Is it possible that there are some P cycles, but that these are discontinuous so that the FFT results suggest that there are no annual cycles? Adding P to Figure 5 could help the reader can evaluate this.

We will add the P records to Figure 5 to demonstrate that, while there is definitely some variability in P over the records, this variability is not very periodic in character and does not follow the same pattern as the other soil-derived elements (raw data is also given in supplementary tables).

Surprisingly, no seasonal changes were observed in P-content. The difference between records of P and other soil-derived elements (e.g. Zn and Y; which exhibit clear seasonality) is difficult to explain. The finding is, however, in agreement with minimal seasonal variability observed in $\delta^{18}O$ and δD values of cave water (Van Rampelbergh et al., 2014). This suggests that seasonal changes in the epikarst, linked to water availability, were dominant over seasonal processes related to surface (soil) processes. Another explanation might be that the limit of detection of P in our LA-ICP-MS data is higher relative to the measured values than that of other elements of interest (e.g. Mg, Zn and Sr), causing higher analytical noise on the P record compared to the other trace element records. The reason might be that P measurements are sensitive to interferences in a Ca-rich matrix (such as calcium carbonate in speleothems).

Technical points:

Line 15: The stalagmite does not have to be annually laminated to reconstruct monthly-scale climate. Rephrase, maybe emphasising the other benefits of annual laminations.

We will rephrase this to make it more general and then add one additional sentence explaining the specific advantage of an annually layered speleothem.

24: PCP will probably occur all the time, so best to say something like ': : :enhanced PCP occurs during: : :'

We will change this and mention "enhanced" PCP.

29: What is it in the trace element concentration profiles that reflects increased recharge? Increased specificity will help build your case early on.

We will be more specific here and mention already in the abstract that higher peaks in soil-derived elements (e.g. Zn and Y) and lower host-rock derived elements (e.g. Mg, Sr, Ba) point towards lower residence times in the epikarst and higher flushing rates during the 17th century compared to present-day conditions.

36: What about other factors not discussed here? P and large organic acids in particular can depress calcite growth rates and influence partition coefficients, particularly for Sr.

As we will show when we add P records to Figure 5 (as discussed above), P does not show the annual variability seen in other trace elements and the P records are not significantly different between the different intervals. We will make brief mention of this record in the results but will only briefly discuss processes based on the P records, because it does not show a clear correlation with what's going on in the other proxy records (see above).

42: Again, annual layering is not inherently linked to our ability to discern seasonality. Also, reconstructions can reach monthly- or even daily-scale using the right techniques, so this should be rephrased.

We will rephrase this.

76: I'm sure that there is a good reason, but it would reduce confusion to explain why these intervals are called 'P16' etc. Are these the names of the stalagmite slabs or pencils?

As mentioned above, we will implement the suggestion to rename 'P19' to 'P20' and motivate the names by stating that they derive from the (approximate) centuries in which the intervals grew.

126: I generally disagree with the requirement for stalagmites to necessarily be at isotopic equilibrium. They grow via kinetic processes so some kinetic fractionation is inevitable, and if there is more it usually enhances the primary climate signal. So I am not concerned if there are occasional intervals with more or less disequilibrium fractionation within the context of the current study.

We would like to emphasize that we never in the text stated that speleothems are necessarily in isotopic equilibrium. Since our sentence around line 125-126 seems to make this impression on the reader we will rephrase it to clarify that this is not what we mean.

144: 'number' instead of 'amount'

Agreed, we will correct this.

156: I seem to recall that a core C3 from the same stalagmite contained petrographic evidence for a reduction in visitation to the cave during both world wars (Verheyden et al., 2006). I assume that this is the same core - is the chronology presented here still consistent with this interpretation? If so, it might be useful to state here because it would help confirm the chronology.

This is indeed the same core through the same speleothem, and the chronology presented in this study remains consistent with this previous interpretation. We will reiterate it here (referring to Verheyden et al., 2006) to help give our chronology more confidence.

163: 'coupled to a Leica GZ6 microscope'

We will correct this typo.

190: 'laser points'?

We will rephrase to "laser pits" or "laser spots" to clarify.

191: 'because'

This will be corrected

193: elements are lowercase – here and throughout

We will correct this throughout the manuscript.

239: 'mean' instead of 'average' here and in most other occurrences (other than when not referring to a statistical mean, e.g., 'the average cave is: : :' etc.)

Similar to a previous comment by Reviewer 1, we will use "mean" throughout the text.

271: How does this compare to the U-Th derived growth rate?

See earlier comment: We will describe and discuss the results of our age model in more detail in the revised version and compare estimated growth rates with the thicknesses of the layers to lend more confidence to our interpretation of annual cyclicity.

295: ': : :the Han-sur-Lesse Cave: : :'

We would like to keep this sentence as it is, referring to this monitoring study specifically as done in another part of the cave system compared to the previously cited studies by Van Rampelbergh et al., which were done at the Proserpine growth site to differentiate between these studies.

457: Is deposition on the roof (i.e., on the stalactite) not enough? Is another void space really necessary? If there is a large stalactite over the Prosperpine stalagmite, deposition on this might be enough to cause the observed PCP signal (I agree that it is PCP).

This might indeed be the case and we will specifically mention this possibility here and state that voids in the epikarst are not strictly necessary as an explanation.

477: I agree with how the authors handle evapotranspiration, but I'd like them to consider the possibility that the concept isn't really relevant in highly karstified regions. If a summer month experiences an intense rain event, the rain may be channelled into a doline and into the subsurface before 'evapotranspiration' has a chance to really occur. If the Proserpine stalagmite is fed by a rapid drip with high variability, it is possible that the whole evapotranspiration concept does not apply. Hess and White (in the book "Karst Hydrology: Concepts from the Mammoth Cave area") suggest a 13% decrease in the relevance of evapotranspiration in karst regions, but I suspect it could be even more in certain situations. The authors approach is correct and is what is conventionally done, so if reducing the amount of summer evapotranspiration is useful they may want to consider this possibility – otherwise I'll leave it up to the authors whether or not they want to rephrase

This is a valid point and we will add a short (1-2 sentence) discussion of the relevance of evapotranspiration in karst systems (including reference to the suggested citation) for sake of completeness. However, we still believe that evapotranspiration is an important process in this case. As shown in Figure 8 (which will be moved up in the manuscript, see other comments), seasonality in precipitation is rather limited in the area, except for a slight increase in the summer months, as discussed. We must therefore conclude that the large seasonal changes we observe in our data, which are controlled by seasonal changes in drip rate (as demonstrated by Genty and Deflandre, 1998), are driven for a large part by seasonal changes in evapotranspiration. We will clarify this line of reasoning in the revised discussion.

Conclusions: As mentioned above, I think that it would be beneficial for the authors to conclude with a greater focus on the implications for climate science. It is impractical to perform an LA-ICPMS analysis across an entire meter long stalagmite. Does this research provide any guidance as to how short high-res intervals would be useful across very long records? I think that it does, and the authors should discuss this more than they currently do.

This is a very helpful suggestion and we will make sure to add a paragraph at the end of the discussion (pre-conclusion) or in the conclusion in which we elaborate more on the implications of our work for similar studies. In this paragraph we will give some recommendations on how to optimize LA-ICP-MS measurements to obtain chemical information on different time scales through a speleothem, combining high resolution intervals with longer, lower resolution transects.

517: Couldn't cooler regional temp also explain the decrease in $\delta^{18}O$?

They could in theory explain some variation in d18O, but only if these cooler temperatures resulted in cooler cave temperatures as well, and only if fractionation was in equilibrium with ambient air, which is not certain for the 17th century part of the Proserpine. In addition, the local temperature trend of ~1°C trend is not visible in the long-term d18O record of Figure 2, suggesting that other factors have a stronger influence on the oxygen isotope fractionation in this speleothem (e.g. the d18O of precipitation). Nevertheless, we will add a sentence highlighting this possibility since we cannot exclude it.

Table 2 caption labelled as Table 1

We could not find this typo in the version we received directly after submission from the system. In our version, Table 2 caption is correctly labeled also in the CP discussions PDF. We will make sure the labelling remains correct in the revised version.

Reviewer 3 (Anonymous)

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).

We will rephrase the sentence following the reviewer's suggestion and include the suggested citations.

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.

This is a useful suggestion and we will implement this nuance in our description of the causes of annual lamination in speleothems in general, including the suggested references. Note that in our original sentence we tried to cover changes in cave ventilation in our mention of "cave climatology" but we understand that this is a bit vague and that it would be better to give a more detailed explanation since the annual laminations are of central importance to our manuscript. We thank Reviewer 3 for his suggestions that will help us do so.

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.

We will rephrase and change "cave waters" to "infiltration waters" to clarify.

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

We will rephrase this.

Line 55: replace stalagmite collected with "Proserpine stalagmite, which was collected in the cave of Han-sur-Lesse and first studied by Verheiden: : :.

We will make the suggested change.

Line 57: replace "according to" with "as inferred from U/Th dating and lamina counting between the radiometric ages (: : :.).

We will rephrase this as suggested. In addition, in response to the comment by Reviewer 2 we will add a bit more detail about the dating methods we used to arrive at our age model for this study.

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).

We thank the reviewer for the suggestion and will update our references with the suggested papers.

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

We will rephrase this sentence to clarify and refer to Frisia et al., 2005.

Line 71: please add after geochemical layering : "of Proserpina"

We will add this to the sentence.

line 73: replace "the relation" with its relation. Replace "this work" with the present study.

We will make the suggested changes in the revised manuscript.

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.

We understand the confusion caused by this sentence and thank the reviewer for pointing this out. We will rephrase to clarify that we will interpret the chemical changes in the three intervals in the context of the longer stable isotope record that is available from Van Rampelbergh et al. (2015) to arrive at a discussion of climatic changes over the past 400 years. Please note that the most recent interval ('P19' now to be renamed to 'P20') is longer than 12-years and we will mention this specifically to avoid confusion.

Line 78 replace elemental with element

We will correct this typo.

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.

We understand the choice of Reviewer 3 to annotate typographic comments on the PDF to save time and regret that this was necessary. We appreciate the time and effort the reviewer spent to make these corrections and will go through her/his annotations in detail and amend them in our revised manuscript.

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.

This is a valid comment and we will make an effort to update Figure 1 to contain information about the local geology of the region. With "local" in this context we refer to the geology directly overlying the Han-sur-Lesse cave system. This geology is more or less consistent throughout the region and is part of the larger synclinorium of Dinant which is a structure of folded (mostly) Devonian limestones that covers most of the Ardennes region in southern Belgium. We will limit our description of the geology to the direct ("local") area of the cave system for sake of brevity:

Specifically, the Salle du Dôme opens in the Devonian Givetian limestone beds forming an anticline structure, which explains the surface geomorphology of the hill in which the cave is located (Delvaux de Fenffe, 1985). The limestone reaches a thickness of 20-50 m above the cave, as estimated by the map of the cave and the surface. Some of the Devonian beds are dolomitized. Since no impermeable formations are present above the cave, precipitation directly seeps through the thin (~25 cm) soil and enters the epikarst.

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.

This comment was also made by Reviewer 1. The necessary information is currently present in Figure 8, but we will move this figure forward in the manuscript to refer to it earlier and give the reader an overview of the present-day (1999-2012 average) climate in the region.

Line 103: I suggest to write that it was removed in 2001.

We agree that this is information we should add and will do so in the revised version.

Line 108: colder than what?

Colder than the air temperature in the cave mentioned in the previous sentence. We will add this for clarity.

Line 109: what is the time scale of "temporary"?

See Figure 8 (to which we will refer in the revised version): This increase in rainfall in summer lasts for approximately 2 months (July-August). We will mention this in the revised version.

Line 114 replace "record" with values.

We will rephrase this.

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.*

Agreed, we will adapt the terminology accordingly to be more precise. We would like to note that the Proserpine did not form through the coalescence of multiple stalagmites. The type of speleothem is described in the French literature as a "tam-tam stalagmite".

Line 122. State entirely what DCL and WPL are the first time. In any case.

We will write out "Dark Compact Layers" and "White Porous Layers" here.

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).

We will rephrase this for clarity, stating that layer counting is accurate between years 1960 and 2001.

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.

All three transects used in this study were sampled perpendicular to the prevailing local direction of the annual layering. In the case of piece P17, this direction is indeed at an angle of ~25° with respect to the vertical in the picture. This direction is chosen because the layers at this location in the speleothem are roughly parallel and at an angle of 25 degrees with the horizontal in the picture. To emphasize this, we will accentuate the layers (which we concur are hard to spot on the current image) in all three pieces in Figure 2 to show that the transect is indeed chosen perpendicular to the layering.

Line 234. Perhaps there is an "of" missing?

We will add "of" after "effect".

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: : :.

We will rephrase this according to the reviewer's suggestion and agree that this would be clearer.

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).

We did not conduct a separate analysis of the nature of OM in the drip water, but we have information about conductivity (ion concentrations; Genty and Deflandre, 1998) and variability in Mg/Ca and Sr/Ca of dripwaters (Verheyden et al., 2008), which are higher during higher drip rates in autumn after the more intense July-August rainfall. At the same time, Verheyden et al. (2008) postulate that autumn flushing brings humic and fulvic acids that accumulate due to intense biological activity above the cave during spring and summer. This lag in response is also in agreement with the results of cave monitoring done by Van Rampelbergh et al. (2014) which shows a peak in discharge in October-November as a delayed response to summer rainfall. This seasonality explains much of the observed trace element and stable isotope variability as well as the presence of the layering in the speleothem mineralogy. We will try in the revised version to explain this more clearly earlier in the manuscript to help the reader to better understand the cave system and refer to it in our discussion.

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).

In the revised version, in response to the comment from multiple reviewers, we will provide records for P in Figure 5. However, P does not show the same seasonality that is so clearly expressed in the other trace elements. Therefore, even adding this record to Figure 5 will not help is explain why P is behaving the way it does. If P was predominantly mobilized by humic and fulvic acids, one would expect a seasonal pattern that follows the autumn increase in discharge, in which these acids are supposedly enriched. We do not observe this however, so the P record remains hard to discuss (see also comment above). We will mention the studies suggested by the reviewer, but will likely not venture beyond highlighting the different processes that might influence the P concentration in the Proserpine speleothem as we are not certain about which processes are dominant.

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.

We appreciate the discussion by Reviewer 3 and will try to incorporate as many elements as possible from it into our discussion of the phase relationship between trace elements and stable isotopes. The cross plot between these proxies which was suggested by Reviewer 2 might also help here, and we will add it to our revised manuscript. We agree that a change in land use might be an aspect we currently overlooked in our discussion and we will add it in the new version. As we will try to show in an updated version of our Figure 2, all transects are in fact taken perpendicular to the speleothem layering. Nevertheless, we will acknowledge that a change in the morphology of the speleothem itself, resulting in prolonged degassing, might be part of the explanation for the correlation between d13C and Mg and Sr concentrations. Finally, the previously suggested addition of information about the overlying geology should clarify whether or not dolomite or shale dissolution contributed to the trace element budget in a significant way. We will update our discussion in this section taking this new geological information into account. Evidence for the fact that the overlying rock is partly dolomitized comes from a characterization of the rock in Verheyden et al. (2000), which we will cite in this section. We will also add more literature references to discuss the antiphase correlation between Mg and Sr (e.g. Pingitore, 1978; Pingitore and Eastman, 1986; Pingitore et al., 1992; Van Beynen et al., 1997). In addition, we will list the different processes that could cause the observed anticorrelation between Mg and Sr, as suggested by the reviewer and by our reply above. In this discussion, we will acknowledge that incongruent dissolution is one of the possible hypothesis.

Pingitore N.E., 1978. The behavior of Zn^{2+} and Mn^{2+} during carbonate diagenesis: theory and application. Journal of Sedimentary Petrology 48 (3): 799-814.

Pingitore Jr. N.E. and Eastman M.P., 1986. The coprecipitation of Sr2+ with calcite at 25ºC and 1 atmosphere. Geochimica et Cosmochimica Acta, 50: 2195-2203.

Pingitore N.E., Lytle F.W., Davies B.M., eastman M.P., Eller P.G. and Larson E.M., 1992. Mode of incorporation of Sr2+ in calcite: determination by X-ray absorption spectroscopy. Geochimica et Cosmochimica Acta 56: 1531-1538.

Huang Y., Fairchild I.J., Borsato A., Frisia S., Cassidy N.J., McDermott F., Hawkesworth C.J., 2001. Seasonal variations in Sr, Mg and P in modern speleothems (Grotta di Ernesto, Italy). Chemical Geology 175: 429-448.

Van Beynen P.E., Toth V.A., Ford D.C. and Schwarcz H.P., 1997. Seasonal fluxes of humic substances in cave drip waters, Marengo Cave, Southern Indiana. Proc. of the 12th Int. Congress of Speleology, Switzerland (2): 120.

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)

As mentioned above, we will add this geological information earlier in the manuscript and refer to it in this part of the discussion to investigate the possibility of IDD or shale contribution playing a role. The hypothesis explaining the lack of covariance between Mg and $\delta^{13}C$ that does include IDD would be that if the water is saturated with respect to CaCO3, calcium carbonate starts to precipitate in the epikarst, leading to increased degassing and an increase in $\delta^{13}C$. The changes in $\delta^{13}C$ are therefore mostly related to degassing, not IDD, and the relationship between $\delta^{13}C$ and Mg is not necessarily always positive, as they are governed by independent processes. We will explain this more clearly in our discussion and mention that the dominance of these processes depends on the composition of the dolomites and limestones in the epikarst, as suggested by the reviewer.

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 C6 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.

This is a valid point by Reviewer 3, and we will broaden our discussion of "climatic change" to encompass "environmental change" which may also entail changes in land use as an explanation for the observed variations. In an earlier stage of working on this study, we tried to find data on the land use change in this area in the 17th century, but sadly such information is not available. We can therefore not venture beyond speculation about changes in land use. We will cite the discussion of land use change in the literature (e.g. Van Rampelbergh et al., 2015). Forest cover was reduced between the little ice age and the early 1900's, but mostly on the more humid slopes rather than the drier tops of the hills. Since the Salle du Dôme is situated under the top of the hill (formed by the anticline structure mentioned above), changes in local forest cover are relatively small, but cannot be neglected.

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.

Agreed, we will add this possibility in our discussion and provide the proper citations.

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.

See above, we will adapt our discussion to include the possibility of a change in land use explaining (part of) the changes in speleothem chemistry we observe in this study. In general, as mentioned above, we will more clearly list the different possible hypotheses and the evidence that could point towards them.

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 an 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.

We thank the reviewer for this suggestion and will try to implement it in our discussion in the revised version of our manuscript (albeit probably earlier on in the discussion where the P record is discussed). This comment is similar to a comment made by Reviewer 2, namely that the difference in temperature might have driven the difference in d18O between the 17th century and the present day. However, this change is not entirely clear from the longer d18O record, so we suggest that temperature is not the dominant factor. Rather, a change in the d18O of precipitation (more snowfall could indeed contribute to this) may explain the difference. More specifically, the different proposed processes might be linked, such that an change in land use causes a decrease in vegetation cover which affects infiltration of precipitation water. A change in the precipitation and evapotranspiration pattern can contribute to this effect. Finally, snow melt may have the so-called “piston effect” pushing older water out of the epikarst and therefore increasing the flow of water to the speleothem site, causing increased seasonality in Mg and Sr. We will add this explanation which links anthropogenic and climate and environmental changes in our discussion.

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

Kind regards on behalf of all the authors,

Niels de Winter