Clim. Past Discuss., 10, C1982–C1989, 2014
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Climate of the Past Discussions



# *Interactive comment on* "A 500 year seasonally resolved $\delta^{18}$O and $\delta^{13}$C, layer thickness and calcite fabric record from a speleothem deposited in equilibrium of the Han-sur-Lesse cave, Belgium" *by* M. Van Rampelbergh et al.

**Anonymous Referee #1**

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Review of the manuscript "A 500 year seasonally resolved d18O and d13C, layer thickness and calcite fabric record from a speleothem deposited in equilibrium of the Han-sur-Lesse cave, Belgium" by Van Rampelbergh et al., submitted to Climate of the Past

General comment:

This is an interesting paper aiming to reconstruct past climate variability during the last ca. 500 years from a seasonally layered, exceptionally fast growing stalagmite from a cave in Belgium. The authors have already published a detailed study of the proxies and the underlying processes based on a cave monitoring program. Thus,



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Interactive Discussion
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the interpretation of the proxy data is relatively robust. The chronology is based on both counting of annual layers and U-Th-dating and, thus, also relatively robust. In summary, the paper is well written, and deserves publication in Climate of the Past.

However, one aspect is completely omitted: The potential occurrence of hiatuses. The authors report one major hiatus between 1810 and 1860, which is clearly documented by straw pieces embedded into the calcite. However, at least some of the described "anomalies" in the proxy records (Fig. 5), which display large and very abrupt changes in both d18O and d13C as well as in growth rate, show the typical signature of a hiatus, which is subsequently followed by a new onset of speleothem growth. Considering the relatively large uncertainties of the U-Th-dating chronology (Table 2), hiatuses and – as a consequence – missing layers may not be detected from comparison of the U-Th- and the lamina counting chronologies. This aspect needs to be discussed. Maybe the authors can use petrographic evidence to demonstrate (or exclude) the occurrence of hiatuses in their records.

Furthermore, the authors discuss the changes in the stable isotope signals and layer thickness throughout the paper in terms of colder/dryer vs. warmer/wetter winters. However, the same proxy signals could also result from a re-routing in the karst aquifer (resulting in increased PCP, lower drip rates, etc.). This is a general problem of all speleothem based palaeoclimate records, but since they do not present another, coeval record from the same cave showing the same variability in the proxy signals (reproduction!), such effects cannot be excluded for the presented record. This may particularly be the case for the anomalies. This aspect should at least be mentioned in the discussion of the proxy data. In this context, is there any evidence for anthropogenic influence above the cave (e.g., agriculture), potentially affecting recharge conditions during the last 500 years?

Similarly, they should discuss the effects of water residence time in the karst aquifer and the related effect of smoothing on the drip water d18O signals. Even if the (analytical) resolution of the stable isotope data may be seasonal, the d18O signal may reflect a



CPD
10, C1982–C1989, 2014

mixture of several years or even decades (Genty et al., 2014). This aspect is crucial for the interpretation of the stable isotope data and needs to be discussed.

Finally, some of the data/interpretations (e.g., correlations, seasonality) should be illustrated rather than just mentioned in the text. See my corresponding detailed comments below.

Detailed comments:

Title: I would delete “deposited in equilibrium” from the Title. Firstly, this is not the case for the whole record. Secondly, this very specific information makes the title quite long.

Title and throughout the paper: I am puzzled by the use of the term “fabric”. To my knowledge, this has been mainly used to describe crystallographic features in speleothems (e.g., columnar fabrics, compare for instance Frisia et al., 2000). In the paper, the authors only differentiate between darker and whiter as well as more compact and more porous calcite. Thus, I would either delete the term “fabric” or present more detailed (microscopic) data.

Abstract: The abstract appears very detailed and relatively long to me. I would focus on the most robust findings here, which would make the abstract much more concise.

p. 4151, lines 14-15: I suggest to remove isotope slang (“heavy”, “light”) throughout the paper and use more positive/negative delta values.

p. 4152, line 25 ff.: Do the referenced records have a particularly high resolution?

p. 4154, line 7 ff.: Perhaps the authors should mention other annually laminated speleothem records here (e.g., Boch et al., 2009; Scholz et al., 2012).

p. 4153, line 22 ff.: I suggest to move this detailed paragraph presenting previous work on the Proserpine stalagmite to the material and methods sections.

p. 4157, line 17 ff.: The layering is almost impossible to see in the current figures. I suggest to include an additional figure showing high-resolution pictures of specific



CPD

10, C1982–C1989, 2014

sections of the speleothem illustrating the layering, changes in thickness, the sequence of dark and bright layers etc.

p. 4158, line 7 ff.: I do not see any data marked in light grey in Table 1.

Table 1: In some cases, the corrected age is older than the uncorrected age. This cannot be the case. Please explain/clarify! Why are some ages bold?

p. 4158, line 10 ff.: Actually, the speleothem does not contain large amounts of detrital Th (less than a ppb for almost all ages). However, the uppermost samples are very young and, thus, contain only very low amounts of radiogenic 230Th. Please clarify.

Table 2: Please report uncertainties for the calculated growth rates (both based on U-Th-dating and layer counting).

Section 4.2: I would like to see the U-Th-ages and the layer counting chronology in a diagram of age vs. depth. The StalAge age model could also be included in this diagram. This would make it much easier for the reader to understand the construction of the chronology.

p. 4158, line 26 ff.: The discussion of the correlations would strongly benefit from a calculation of the running correlation between d18O and d13C. If a proper size of the window (e.g., 50 years) was used, this could nicely illustrate different parts of the record. This should be included in Fig. 3.

p. 4159, line 11 ff.: As mentioned above, the “very clear seasonal variations” are not visible in Fig. 3. Please include a high-resolution picture showing the seasonal nature of the laminae. Furthermore, I strongly suggest to plot the seasonality in the stable isotope signals rather than just mentioning it in the text. Following your reasoning, this quantity has been calculated and is available. It would be very illustrative if this was included in Figs. 3, 4 and 5.

p. 4160, line 27 ff.: “...the good agreement between the changes in growth rates suggested by the layer counting model and the changes in growth rate indicated by the



CPD

10, C1982–C1989, 2014



layer thickness measurements suggests that the layer counting age model is the most accurate." This appears as circular reasoning to me since both quantities are based on the counting of annual layers.

p. 4162, line 7 ff.: As mentioned in my general comment, it appears to me that some of the "anomalies" could reflect hiatuses. Please include this hypothesis in the discussion, in particular since the U-Th-chronology is not precise enough to exclude the presence of missing laminae (hiatuses).

p. 4162, line 13 ff.: Please provide the (running) correlation between the stable isotope signals and lamina thickness. It is hard to see the correlations only based on the figures. A running correlation would be particularly useful to identify temporal changes in the correlation between the individual proxies.

p. 4162, line 27 ff.: Please provide the temporal evolution of the seasonality of the stable isotope signals in a plot. It is impossible to deduce this from the current figures.

p. 4163, line 7 ff.: Changes in speleothem d18O values may also result from changes in seasonality as the authors themselves point out. This should be included.

p. 4163, line 26: The paper from Baldini et al. (2002) has been retracted. Please remove the corresponding reference.

p. 4163, line 27 ff.: The effect of the residence of the water in the karst aquifer on the d18O values of the drip water and speleothem calcite is completely omitted from the discussion. Perhaps this has been discussed in the monitoring paper, but since this point is crucial for the interpretation – at least on the seasonal to annual scale – this must be included here. In a recent paper, Genty et al. (2014) have shown that the residence time may have a large effect of drip water d18O values. This aspect should be discussed.

p. 4164, line 16 ff.: I agree that the majority of the recharge water originates from winter and that summer rainfall probably contributes less. However, Genty et al. (2014) have



CPD
10, C1982–C1989, 2014

impressively shown that summer rainfall may still have a substantial effect on the d18O value of cave drip water. Since the basic assumption for the interpretation of the stable isotope records is that the recharge water mainly reflects winter precipitation, this is a crucial point and should be critically discussed. Please expand the discussion on this and also on smoothing effects due to mixing in the karst aquifer (see above).

p. 4165, line 4 ff.: One aspect I am missing in the discussion of the d13C values is the effect of host rock dissolution occurring in the closed/open system. This may have a large effect on the d13C values of the drip water both on short (annual) and longer (centennial) time-scales (see e.g., Fohlmeister et al., 2011). This should be included.

p. 4167, line 12 ff.: I am not sure that changes in soil productivity cannot occur on much shorter time-scales (i.e., decadal if not even shorter).

p. 4167, line 28: Please show the correlation between colder and dryer winters in the instrumental data or at least present the correlation coefficient.

p. 4168, line 2 ff.: I am not convinced that the large isotope excursions (the "anomalies") are due to disequilibrium fractionation. I rather suspect that at least some of them are related to hiatuses (see above). Please present further evidence that this is not the case.

p. 4168, line 22 ff.: I do not agree that the anomalies are related to "exceptionally cold and dry winters". I rather think that these events are related to short-term hiatuses, which may also be related to re-routing in the karst aquifer. This possibility should at least be mentioned. The authors mention "non-climatic factors" below to explain some of the anomalies, but this discussion should be expanded.

p. 4170, line 26 ff.: "This observation corresponds with colder conditions in Europe (Fig. 5h–j) (Van Engelen et al., 2001; Le Roy Ladurie, 2004; Luterbacher et al., 2004; Dobrovolny et al., 2010) and confirms, that although calcite is white matte, the isotopes still record climate variations." I do not agree with this statement. It is encouraging that



CPD
10, C1982–C1989, 2014

the other reconstructions also show colder conditions for the corresponding periods. However, the duration and shape of the cold phases is very different. The duration is much shorter in the other reconstructions. Furthermore, the speleothem record suggests a progressive cooling during the interval, which is not visible in the other records. This statement is thus associated with large uncertainty. I would rephrase the whole paragraph and definitely not use “confirm”.

p. 4170, line 5 ff.: This interpretations seems OK to me, but I would again mention the possibility that the anomalies are related to non-climatic or even anthropogenic factors.

Section 5.6: This paragraph only makes sense if seasonality is plotted and included in the figures. As these data seem to be available, it should be no problem to plot them. This would allow the reader to follow the reasoning much more easily.

p. 4176, line 24 ff.: The “speleothem data from the Alps” are mentioned (without a reference) for the first time here. Please provide a reference and include the corresponding data in the discussion and in the figures.

p. 4177, line 1 ff.: This section should only be included if seasonality is plotted and compared with the other proxies (see my comment above).

References

Boch, R., Spötl, C., and Kramers, J., 2009. High-resolution isotope records of early Holocene rapid climate change from two coeval stalagmites of Katerloch Cave, Austria. Quaternary Science Reviews 28, 2527-2538.

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Frisia, S., Borsato, A., Fairchild, I. J., and McDermott, F., 2000. Calcite fabrics, growth mechanisms, and environments of formation in speleothems from the Italian Alps and southwestern Ireland. Journal of Sedimentary Research 70, 1183-1196.



CPD

10, C1982–C1989, 2014

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Scholz, D., Frisia, S., Borsato, A., Spötl, C., Fohlmeister, J., Mudelsee, M., Miorandi, R., and Mangini, A., 2012. Holocene climate variability in north-eastern Italy: potential influence of the NAO and solar activity recorded by speleothem data. Climate of the Past 8, 1367–1383.

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Interactive comment on Clim. Past Discuss., 10, 4149, 2014.