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Climate of the Past
Discussions

Interactive comment on "A 500 year seasonally resolved δ^{18}O and δ^{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.

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Anonymous Referee #1 Received and published: 25 November 2014 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 grow-

ing 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, 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. The Proserpine stalagmite is a rapidly growing stalagmite with a clear layering in the upper ca. 56 cm we studied. It was demonstrated, based on comparison between U/Th dating and layer counting that one layer couplet is deposited per year (even if two parts, IV and VI, suggest possible problems of opening or contamination of the system by modern dripwater). Moreover, some of us (Van Rampelbergh et al., 2014) studied the seasonal isotopic functioning of the vadose zone and the seasonal calcite deposition. The deposition dynamics in the Proserpine stalagmite is therefore well understood. The layer counting provides results that are very close to the expected period based on U-series data. It is therefore highly improbable that even minor hiatuses exist in the period studied in the speleothem. Especially because the Proserpine stalagmite currently fed throughout the whole year by a 'real shower' from many different drip sites, leading to a growth rate of the order of 1 mm per year. The 'hiatus' observed around 1850 is not due to non-deposition of calcite but is rather a perturbation due to the insertion of straw pieces in the calcite by humans. This most probably happened in a period of strongly decreased drip rate since there are strong indications that a fire has been lit on the stalagmite (Verheyden et al, 2006), which is impossible nowadays due to the dense dripping. The isotopic signatures with rapid and important δ18O and δ13C are therefore linked to changes in terms of more or less close to isotopic equilibrium, and consequently to changes in climatic conditions that are at least regional.

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? That some proxy-signals can change related to the routing of the water used through the vadose zone is certainly a crucial point as highlighted by the reviewer. This is certainly the case for elemental composition which is very spatially dependent in caves and was studied in Han-sur-Lesse caves (cfr Verheyden et al, 2008: Verheyden S., Genty D., Deflandre G., Quinif Y. and Keppens E., 2008. Monitoring climatological, hydrological and geochemical parameters in the Père Noël cave (Belgium): Implication for the interpretation of speleothem isotopic and geochemical time-series. International Journal of Speleology, 37(3): 221-Âň234.) This can be the case for δ13C. Depending on the routing, the effect of Prior Calcite Precipitation (PCP) can be less or more important, which can change the δ13C of the carbonate ions due to repeted degassing of the water during the PCP process. However, as explained in chapter 5.2, this is not the case for the δ18O of the vadose water, which has been

studied in the Han-sur-Lesse cave (cfr Verheyden et al 2008; Van Rampelbergh et al., 2014) and which does not display spatial variation of more than 0.5 permil, which corresponds to the largest seasonal changes observed in the calcite δ18O. This means that the rapid and large δ18O changes are changes related to real changes of overall conditions in the cave (which may be possibly linked to regional changes). It suggests the possible existence of threshold conditions inducing rapid and important change in isotopic composition due to changes in isotopic equilibrium status, linked to changing conditions and/or temperature changes (as explained in 5.5.).

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 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. Van Rampelbergh et al., 2014 largely discussed the smoothing effect of the vadose zone on the δ18O composition of the drip waters in the Han-sur-Lesse cave. They demonstrated that the δ18O and δD of the dripwater display almost no variation throughout the year, indicating that the residence time is sufficiently long to homogenize its isotopic composition. The change in isotopic signal can therefore not be due to seasonal variations or to rain or snow events. Moreover, the fact that the δ13C changes at the same time as the δ18O suggests a change due to other factors than a change in vadose water δ18O. Therefore a discussion on the mixture of one or several years of the vadose water is not crucial in deciphering the specific problem of important rapid changes. It is crucial however in the discussion of the longer-term trends.

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. We thank the reviewer for this suggestion. The title of the manuscript was adapted to "ÂňA 500-year seasonally resolved δ18O and δ13C, layer thickness and calcite aspect record from a speleothem deposited in the Han-sur-Lesse cave, Belgium."

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. We agree that the term "fabric" is not the best-suited term to use for describing the difference between darker and whiter as well as more compact and more porous calcite. A better term can be the calcite "aspect". The term fabric was adapted throughout the manuscript with the term calcite aspect.

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. Abstract was shortened to make it 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. We thank the referee for this suggestion and adapted the manuscript by replace the isotope slang terms with more positive/more negative throughout the manuscript.

p. 4152, line 25 ff.: Do the referenced records have a particularly high resolution? With this sentence the authors illustrate that speleothems successfully can be used to reconstruct climate in Europe. Since the focus is of this sentence is not the resolution od the climate record, but rather the success of the used archive, the sentence was adapted to: "Speleothems have already often proven to enable climate reconstruction in Europe (Genty et al., 2003; Baker et al., 2011; McDermott et al., 2011; Fohlmeister et al., 2012; Verheyden et al., 2014).

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). We thank the referee for this suggestion and added the two proposed papers in the reference list.

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. We think it is the best option to keep 'this detailed paragraph presenting previous work on the Proserpine stalagmite' in the Introduction, also in the new thouroughly reworked manuscript because many of the findings in our previous work, especially Verheyden et al 2008 and Van Rampelbergh et al., 2014, are essential in our reaserch approach of the presented study, to which we were also trongly motivated by the prevous work we refer to.

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 sections of the speleothem illustrating the layering, changes in thickness, the sequence of dark and bright layers etc. Such a figure was already made and published in the paper of Van Rampelbergh et al. 2014 " Monitoring of a fast-growing speleothem site from the Han-sur-Lesse cave, Belgium, indicates equilibrium deposition of the seasonal d18O and d13C signals in the calcite. (COTP). To inform the reader that such picture was already published in a previous study we added the following sentence at the end of section 4.: "Sampling for the stable isotopes was done layer per layer in the parts II to VII and reflects seasonal variations in the δ18O and δ13C signals (for a high-resolution picture of the seasonally resolved isotope records, the authors refer to Fig. 4 in Van Rampelbergh et al., 2014)."

p. 4158, line 7 ff.: I do not see any data marked in light grey in Table 1. We thank the referee for pointing this out and will make sure that the color contrast will be preserved when uploading the paper in the COTP website.

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? The U/Th ages marked in light grey (numbers, 1,2,7,8,15,17,18 and 19) were measured by Verheyden et al. in 2001. When we correct these ages to be reported before 2013 we have to add 12 years to the corrected results obtained by Verheyden et al. This causes the U/Th ages with numbers 15,17,18 and 19 to have slightly higher corrected ages compared to the uncorrected age. The bold ages must be errors that have occurred during uploading of the file. They have no function and the authors will make sure that these bolds are removed in the published version.

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. We thank the referee for this comment and fully agree that the low amounts of 230 Th are responsible for the large uncertainties rather than the amounts of detrital Th. However, to make the whole manuscript more coherent, the Results part was rewritten. This sentence is not mentioned anymore in the manuscript.

Table 2: Please report uncertainties for the calculated growth rates (both based on U-Th-dating and layer counting). We thank the referee for this suggestion and added the uncertainties for the growth rates in Table 2.

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. As suggested by the referee we added an Age-Depth figure with the U/Th ages. See Figure 4. We did not choose to introduce the results of the StalAge model since this is not the model that was used to determine the final age-depth relation of the Proserpine.

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. The point the authors want to emphasize in the manuscript is that the d13C and d18O signals display similar trends above the perturbation. Below the perturbation the d13C and d18O are most probably affected by different factors and display a different evolution. Doing a running correlation between these two variables does not clearly indicate this difference since the correlation will be established by looking at the correlation between the two variables point by point. A better way is to report this difference is to calculate the Spearman's rank correlation coefficient or Spearman's rho. This is a nonparametric measure of the statistical dependence between two variables. It assesses how well the relationship between two variables can be described using a monotonic function, i.e. which is not necessarily linear. In the new version of the manuscript the difference in correlation between the part above the perturbation and the part below the perturbation are reported by the Spearman's rank correlation coefficient.

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. For a clear picture of the seasonal laminae, the authors prefer to refer to Fig. 4 in the previous published manuscript of Van Rampelbergh et al. 2014 since this paper discusses the seasonal character of the layering and the seasonality in the resolved isotope signals. Since we agree that the discussion of the seasonality (=section 5.6) was missing a clear Graph illustrating the seasonal amplitude we added figure 6 to the manuscript. In this figure, the reader can clearly see that the part below the perturbation was seasonally resolved and that the amplitude of the seasonality changes throughout the different colder and warmer periods of the LIA.

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 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. We thank the reviewer to draw our attention to this point, which may indeed not be clear. This approach may indeed appear as circular reasoning, but we are convinced that it is not. In the discussion on the relations between apparent U/Th-ages, number of counted layers, growth rates, thickness of layers, lengths and duration of sections, numbers of years etc., all with their analytical and/or statistical uncertainties, and that are all functions of 'time' being the unknown, we do start our analysis with the 'a priori' assumption that , all layer duplets, consisting of a lighter and a darker one, correspond to one year, which is an observation that some of us (Van Rampelbergh et al., 2014) have demonstrated in a careful well documented monitoring study of the section covering the youngest 10 years of the same drill core. Without this observation all reasoning related to the unknown time would indeed be circular. In order to clarify this concept to the reader we briefly explain it in the text.

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). See our discussion on possible haituses in the 'general comment'

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. The main message in comparing the isotope values with the layer thickness values is that when isotope values (and mostly the d18O values) are more negative, the layer thickness increases. A (running) correlation analysis is not necessary to illustrate this relation. However, we agree that

if the term 'correlation' is used to describe the similar trends, such a s was done in the submitted version, the reader expects results of a (running) correlation analysis. Since a correlation analysis is not necessary to indicate that more negative d18O values correspond with thicker layers, the term 'correlation' was removed from this discussion.

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. The temporal resolution of the isotopes is not fixed fro the part above the perturbation since the sampling was done at fixed distances (every 1mm). Only below the perturbation (part between 1810 and 1479 AD) samples were drilled in every layer and are thus always seasonally resolved. Since this difference was not clear in the first version of the manuscript, the text has been re-written to better illustrate this difference. However, we do not think that adding a plot explaining this is necessary in the manuscript.

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. We think that this is better explained in the new version of the manuscript.

p. 4163, line 26: The paper from Baldini et al. (2002) has been retracted. Please remove the corresponding reference. The discussion of 5.3 The possible factors driving the d18O and d13C values layer thickness and calcite fabric was thoroughly rewritten to make a smoother text and this sentence was removed from the manuscript.

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. We think that this point is better explained in the new version of the manuscript.

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 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). We think that this point is better explained in the new version of the manuscript.

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. The open/closed system is a classical problem of the epikarst system, which is related to the varying impact of PCP. We think it is better explained in the new version of the manuscript, in which we refer to previous work by some of us (Verheyden et al., 2008) on the seasonal co-variation of Mg/Ca and Sr/Ca with changes in the effect of PCP, which is discussed in the new version of manuscript.

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). We think that this point is better explained in the new version of the manuscript.

p. 4167, line 28: Please show the correlation between colder and dryer winters in the instrumental data or at least present the correlation coefficient. As suggested by the reviewer, the correlation coefficient between the winter temperature and the precipitation intensities measured by the RMi were added to the manuscript in section 5.2: "Furthermore, a good Spearman correlation can be established between lower winter precipitation intensities (DJF) and lower winter temperatures (DJF) measured by the RMI since 1833 (= 0.47 and p = 3.99 x 10-11) suggesting that drier winters correspond to colder winters."

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. See our discussion on possible haituses in the 'general comment'

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. See our discussion on possible haituses in the 'general comment'

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 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". We think that this point is better explained in the new version of the manuscript.

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. We think that this point is better explained in the new version of the manuscript.

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. We fully agree with this comment and added a plot of the discussed data in Figure 6.

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. The discussion part of the manuscript was thoroughly rewritten and the comparison with the Alpine record was removed.

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). Figure 6, illustrating the seasonal evolution of the d18O and d13C signals was added to section 5.5. Seasonality in d18O and d13C values. Therefore conclusion point number 5 was kept in the manuscript.

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