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Climate of the Past
Discussions

Interactive comment on "Seasonal variations recorded in cave monitoring results and a 10 year monthly resolved speleothem δ^{18}O and δ^{13}C record from the Han-sur-Lesse cave, Belgium" *by* M. Van Rampelbergh et al.

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Anonymous Referee #2 Received and published: 22 May 2014 This manuscript presents a cave monitoring study from Han-sur-Lesse cave in Belgium. Several cave parameters were monitored for one year in a two weeks rhythm. The monitoring results were used to interpret an annual laminated speleothem record from this cave. This link between cave monitoring results and speleothem records is very important for the progress in understanding and interpretation of past climate variability from this palaeocliamte archive. It is a very interesting story and a nice dataset, which is a good

contribution to the scientific progress. Further, the manuscript is well written and structured, but there are several points were the manuscript should be improved. In the following I will list general comments and suggestions and give a list of specific comments and technical improvements for the manuscript. The manuscript needs major revisions before accepting it for publication.

General Comments:

It is clear that you have a monthly measuring resolution for the speleothem record and a two weeks resolution for the monitoring parameters. But you, by yourself, wrote in the paper that there is "only" a seasonal change between a summer and a winter mode, which causes the annual lamination of the speleothem. Due to this the proxy resolution is "only" on a seasonal base. Please go carefully through the complete manuscript and correct this, because you wrote this at several points. The manuscript have to be reworked in matters of clarify the measuring resolution and the proxy resolution.

As correctly indicated by the reviewer, in the speleothem we have a monthly measuring resolution and for the cave monitoring we have a two-week measuring resolution. When looking at the results, we can clearly see that the cycles in the cave monitoring are seasonal. This same seasonality is seen in the isotope results of the speleothem. The lamination in the speleothem is seasonal (- not annual such as commented by the reviewer -) and is linked to the seasonal changes in the cave atmosphere. Since this was apparently confusingly written in the paper, we reworked it so that this message comes out more clearly to the reader.

In the context of the d13C composition of the calcite, which precipitated on the glass slaps, you mention evaporation. This is wrong in the context of d13C in drip water and calcite. I think you mean the degassing of CO2 from the drip water, which has an essential influence on the isotopic composition of the d'13C in the drip water and in the calcite. Evaporation is dependent on the humidity of the cave air and the temperature of the cave air and influences the d'18O system of the drip water and the calcite.

Therefore, please go carefully through the manuscript and rework these parts and clarify that you mean the degassing of CO2 from the solution. To this topic there are several publications from Dreybrodt, Mühlinghaus and Scholz.

We gratefully thank the referee for this comment. We absolutely agree that evaporation is not the correct term when referred to the CO2 escape from the drip water. The term “degassing” is the correct term to use. This was adapted in the manuscript.

The structure of the Abstract could be improved by adopting the following structure: 1.Background, 2. Methods, 3. Results and 4. Conclusion.

We agree that the structure of the abstract in the submitted manuscript was not optimal. Therefore, we reworked it according to the following structure that is very close to that suggested by the reviewer: 1. Context: provides background for less specialized readers 2. Need: Describes what we have and what is still needed 3. Task: Sates what the authors undertook to address the need 4. Findings: States the main results 5. Conclusion: Interprets the findings

New abstract:

Speleothems provide paleoclimate information on multi-millennial to decadal scales in the Holocene. However seasonal or even monthly resolved records remain scarce. Such records require fast growing stalagmites and a good understanding of the proxy system on very short time scales. The Proserpine stalagmite from the Han-sur-Less cave (Belgium) displays seasonal layers of 0.5 to 2 mm thickness per single layer that allows a measuring resolution at a monthly scale. Through a regular cave monitoring, we acquired a good understanding of how δ18O and δ13C signals in modern calcite reflect climate variations on seasonal scale. From December to June, outside temperatures are low inducing low cave air and water temperature, bio-productivity in the soil is limited leading to lower pCO2 and higher δ13C composition of the CO2 in the cave air, higher pH and higher discharge due to the inactivity of the plant coverage. From June to December, these parameters display an opposite behavior. The δ18O

and δ13C signals of fresh calcite precipitated on glass slabs also vary seasonally. Lowest δ18O values occur during the summer when the δ13C values are high. The δ18O composition of the calcite is deposited close to equilibrium with the drip water δ18O and display seasonal variations due to changes in the cave air and water temperature. In contrast to the δ18O signal, δ13C values of the calcite precipitated on the glass slabs seem more affected by disequilibrium effects. Highest δ13C values occur during summer, when drip water discharge rates are low increasing the degassing effect on the thin water film covering the stalagmite. This same anticorrelation behavior of the δ18O versus the δ13C signals is seen in the monthly resolved speleothem record that covers the period between 1976 and 1985 AD. Dark layers display low δ18O and higher δ13C. The cave system varies seasonally in response to changes in the activity of the vegetation and outside air temperature. From June to December, the cave is in ‘summer-mode’ while a ‘winter-mode’ occurs from December to June. The low δ18O and high δ13C values of the dark speleothem layers indicate that they are formed during summer, while light layers are formed during winter when calcite deposition occurs fast. The darker the color of a layer, the more compact its calcite structure, the more negative its δ18O signal and the more positive its δ13C signal.

At several points in the manuscript you give a list of references (e.g., P 1824, L 3). Please add “e.g.,” at the beginning of these lists, because it is a selection of studies which you cited and not all available once in this field.

As correctly stated by the referee, at several points in the manuscript, we refer only to a selection of existing studies. We fully agree that the listed references represent only a small selection of all the available literature for the cited examples. However, we think that it is not necessary to indicate that the listed references are only a selection of the existing literature on this topic. The same would have to be done for most of the references cited in the paper. With the increasing number of publications over the last 20 years, it would be totally unrealistic to list all the previous studies illustrating the statement made; or would lead to an unreasonably long paper, with an extensive reference list. As authors, we selected the references that we consider the most relevant for the case illustrated. Furthermore, the list of references always follows a sentence that clearly indicates that the references are examples illustrating the statement. Therefore we argue that adding the "e.g," at the beginning of such a listing adds no extra information for the reader.

At several points in the manuscript, for example on page 1824, line 25, you do not give the appropriate references and/or some important once are missing. Please check the manuscript for this. For example (P 1824, L 25), Dreybrodt (1999) is not an appropriate reference for a study of cave dynamics. One further example (P 1824, L 28): there are some extensive monitoring studies missing for example Spötl et al. (2005).

We thank the referee for pointing out that these references are indeed not the best possible examples for the statement. We adapted both references in the manuscript: P1824 L25: We changed the reference of Dreybrodt, 1999 to Mattey et al. 2008 P 1824 L 28: Spötl et al., 2005 was added.

The introduction of this manuscript could be better structured and focused. The structure should be: 1. The "problem" studied, 2. State of the art for this "problem" (findings by others), 3. The goals of your study.

We think that our present structure clearly indicates to the reader what is the background of the study. Moreover, it is very close to the improved structure suggested by the reviewer. First, we discuss the (1.) context of speleothem used in climate reconstructions. Still in part (1.), we illustrate that speleothems are valuable climate archives but that they are difficult to use to resolve the climate at a seasonal scale for seasonal paleoclimate studies based on speleothems (= "problem"). In part (2.) we discuss in more details the problem and provide a state-of-art of what has been done to address it (=state of the art). The third part (3.) discusses our "approach to the problem" and what we will do to help resolve it. Here, we argument that the Han-sur-Lesse cave and the Proserpine stalagmite do not have the limitations discussed in part (2.) and that

they can thus be used to tackle the problem stated in part (1.). Finally, we explain what we will do to deliver answers. Therefore, we think this structure is clear enough, very close to that suggested by reviewer X and does not need to be reworked.

In the "Study Area" you mentioned that the Han-sur-Lesse cave is a show cave where tourists are walking thorough. But in the following you did not discuss the influence of the tourist in the cave on your monitoring results. Do you measure in the cave when there were no tourists? Do you know how fast the CO2 values in the cave decease to normal values after a group of tourist were in the cave? How many tourists visit the cave every day? Are there more tourists in summer than in winter? These are important questions, which have to be mentioned and discuss in the manuscript. People in the cave have a big influence on cave air temperature, humidity and especially the CO2 values. There are several publications dealing with this issue.

Indeed, as correctly mentioned by the reviewer (and also commented by reviewer 2), the Han-sur-Lesse cave receives several thousand visitors during the whole year with the high season lasting from April to September. Although a large number of visitors passes trough the cave every day, we expect the effect of the visitors on the measured parameters to be negligible due to the size of the cave chamber. With a height of 60m and a width of 150m good for a total volume of 124 000 m2 and a river flowing at the bottom of the chamber causing good air mixing, the effect of visitors is negligible. This assumption has been confirmed with a test carried out at the beginning of the monitoring campaign. The reported parameters were measured before the first tourists entered the cave and after several groups have passed by the speleothem location. No significant difference could be measured confirming that visitors do not influence on the cave parameters. To be sure that the measured parameters are reflecting the natural conditions as closely as possible and to do a consistent measurement campaign, the cave parameters were always measured around 9 AM before the visitors are entering the cave. We understand that this discussion point should be better clarified in the paper therefore, we added the following paragraphs: Methods P1826 L28: "To make sure that the measured parameters are closely reflecting the natural conditions and to guarantee a consistent measurement campaign, the cave parameters were always measured around 9 AM before the first visitor enters the cave. To investigate the visitors possible influence on the measured cave parameters, a test was carried out by measuring these parameters before visitors were allowed into the cave and after the passage of different groups."

Results P1829 L19: "No difference in the measured cave parameters is observed before the start of the visits and after a large number of visitors have entered the chamber."

Discussion P 1833 L 19: "A large number of visitors enter the cave and the Salle-du-Dôme chamber every day. The studied Salle-du-Dôme chamber has a height of 60m and a width of 150m good for a total volume of 124 000 m2 and a river flowing at the bottom of the chamber causing good air mixing. Due to the large size and the good ventilation of the chamber, the effect of the visitors on the measured parameters is expected to be negligible. This was confirmed by a series of tests, where similar cave parameter values were measured before and after groups visited the chamber. For the Salle-du-Dôme chamber, we consider the measured values to reflect the natural conditions of the cave atmosphere."

Results and interpretation/discussion of the data should be strictly separated. This is not always the case. Please go through the results and move all interpretation to the discussions part.

This has been done according to specific and technical comments made.

At some points of the manuscript the data are a bit over interpreted. For example the six drops in the d18O and dD values of the rain water, which are interpret as rain events and due to the amount effect. When I take a look on the rain data, I also could see some rain events in summer, which do not have an effect on the d18O and dD of the rain. Further, in the temperate climate zone the amount effect is very low. The most influencing factor on the d18O and dD of the rainwater is the temperature (go into the

corresponding literature). The only striking thing in the d18O and dD record from the rainwater is the drop in March, which is clearly related to the snow in March. There are several other points where the data are over interpreted. Please go carefully through the manuscript and rework this.

We do not agree with this over-interpretation comment. We stand by our interpretation, which is supported by the following argumentation. The isotope data displays 6 small but clear drops that relate to increased rainfall events detected in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014. We agree that in temperate regions, the temperature-effect is the main factor affecting the rainwater isotopic composition. The smaller drops in what do not relate to temperature drops but rather to small periods of increased rainfall. As suggested in the general comments, the referee also indicates a period of increased rainfall in summer. He suggests that this event does not affect the rainwater isotopic composition. We do not agree with this either. These two smaller summer rainfall events (we assume that the referee refers to the increased rainfall in August and September) do cause the rainwater isotopic composition to also display two small drops (highlighted in purple in the figure added).

Initially, we did not discuss these two drops because of their smaller amplitude compared to the ones occurring in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014. Therefore, we still believe that 6 smaller drops in the isotopic composition of the rainwater observed in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014 are significant and relate to the increased amount of rainfall in these periods. We therefore choose to keep this discussion part in the manuscript.

Always take your measuring accuracy and/or the standard deviation into account and think about if a fluctuation of your data is significantly above this accuracy and could be meaningful. Perhaps, it will help to put error bars at all data sets. I think there are also some error bars missing. For example the cave air CO2 values. You said you measure three times in the cave and then averaged these values. I think they will not be exactly the same. Therefore, error bars, in this case standard deviation, are missing. Please check also the other data for this.

To explain this in the manuscript, the following paragraph was added on P 1828, L6: “The standard deviation of the three measurements reports the error on the measured δ13CDIC-value. The 2σ values average 0.6 ‰Åll values are reported in ‰ VPDB.”

In the discussion part an in depth discussion and comparison of the result with other studies is missing, but this is an important part. Please read the corresponding literature.

We are well aware of the relatively important literature existing on this topic. Indeed throughout, the discussion part of the manuscript, we compare the main conclusions from our work to selected cave monitoring studies. We do not agree with the suggestion of the reviewer that further comparisons are necessary here. In order to do this comparison thoroughly and rigorously, the length of the MS would have to be increased significantly. Such comparison can indeed be interesting but would lie too far from the goal of the paper. It would be more appropriate in some kind of review paper, which we would gladly write for CP in the editor shares this vision. Here, we wanted to present the results of the specific cave monitoring of the Han-sur-Lesse cave and document its behavior over a period of 1 year, and we would prefer to keep this MS precise and to the point.

The Conclusions have to be shortened to the half. They should include the important findings of this study and do not repeat too much of the results and discussion part. Further, they have to be adapted to the changes, which should be made in the results and discussion part.

We agree with the referee that the conclusions are somewhat extensive. We therefore shortened them and included the new parts from the results and the discussion.

The figures have to be adapted to a reworked version of the paper.

This has been done according to suggested specific and technical comments.

Specific and technical Comments in chronological order:

Authors list: Please change “M Allan” to “M. Allan”.

Adapted

P 1823, L 2-3: These two sentences would fit better in the Introduction part than in the Abstract.

We thank the referee for this suggestion but we do not agree with removing these two sentences from the abstract because we argue that they are necessary for the reader to understand the context and the problem addressed in the study. Removing these sentences would immediately focus the abstract on the work carried out in the paper without indicating why this research is important. If these two sentences were removed, most readers would not know or realize that such fast growing speleothems such as the Proserpine are exceptional and very scarce and that the data resolution reported in the paper is rather unique.

P 1823, L 4: It is not clear what or who is meant with “They”. Please clarify this.

‘They” is referring to the subject of the previous sentence being “seasonal or monthly resolved records”. Since the referee indicates that this is not clear, “They” has been adapted to “Such records” in the concerning sentence. The new sentence on P 1823, L4 is: “Such records require fast growing stalagmites and a good understanding of the proxy transfer function on very short time scales.”

P 1823, L 6: Please clarify if the thickness of 0.5 to 2 mm is related to a layer pair (dark and white layer together) or on a single layer (dark or white layer).

The thickness is the thickness of a seasonal layer being equal to the thickness of a single layer. Since this is not clear for the reader as suggested by referee 2, we added “per single layer” in the sentence. The new sentence on P 1823 L6 is: “The Proserpine stalagmite from the Han-sur-Less cave (Belgium) displays seasonal layers of 0.5 to 2 mm thickness per single layer that reconstruct paleoclimates at a monthly scale.”

P 1823, L 6: A stalagmite could not reconstruct palaeoclimate. Please rework this sentence that the coherence is correct.

Based on the general comment where the structure of the abstract had to be reworked, this sentence was removed from the text.

P 1823, L 17: Here you did not put summer-mode in quotation marks, but before you did this. Please be consistent.

We thank the referee for this comment and adapted it in the manuscript.

P 1824, L 3: Please replace “systems” with “variations”.

We thank the referee for this comment and adapted it in the manuscript.

P 1824, L 9: I do not know a speleothem record with a monthly proxy resolution. If there is one please give a reference. To my knowledge the only palaeoclimate proxy, which could give a monthly climate proxy resolution are tree-ring. But also this archive show seasonal climate signals in most cases.

We thank the referee for pointing out that it is not clear that the authors are using a monthly measuring resolution but a seasonal proxy resolution. As correctly commented by the referee, a monthly resolved speleothem proxy record is not yet available in the literature. However, Mattey et al., 2008, managed to produce a record with a bi-monthly resolved measuring resolution reflecting seasonal variations. To clarify this the concerning part in the manuscript was adapted. P1824, L 5-10: “With the increasing number of studies on cave calcite deposition dynamics (Dreybrodt, 1999, 2008; Lachniet, 2009; Oster et al., 2012; Ruan and Hu, 2010; Scholz et al., 2009; Verheyden et al., 2008) and with the help of modern analytical tools (Fairchild et al., 2006; Jochum et al., 2012; Spotl and Mattey, 2006), progresses are being made to measure resolution at sub-seasonal and even bi-monthly scales. However, only a few studies, so far, reach

such high temporal resolution for the measured proxies (Mattey et al., 2008; Treble et al., 2003) mainly due to following two limitations.”

P 1824, L 10: What are these “limitations”? Please give a bit more information.

The limitations are listed in the two paragraphs following this sentence. The first limitation is that the growth rate needs to be sufficiently high to allow monthly resolved sampling. The second limitation is the that a good understanding of the measured proxies and cave dynamics is needed in order to translate the proxy signals in terms of climate. Since this is not clear from the text such as indicated by the referee the concerning sentence and the beginning of the two following paragraphs are reworked. P 1824 L 9-10: “However, only a few studies, so far, reach such high temporal resolution for the measured proxies (Mattey et al., 2008; Treble et al., 2003) mainly due to following two limitations.” Start of the paragraph below, P 1824 L11: “The first limitation to study the paleoclimate at sub-seasonal scale from stalagmites is that their growth rate needs to be significantly high. . . Start of the paragraph below, P 1824 L19: “A second limitation is that a good understanding of the cave system is needed to understand what the measured proxies are reflecting.”

P1824, L 19-23: There are some studies where a transfer function between the stable oxygen and carbon isotopic composition of a speleothem to climate parameters were applied, but it is not a “well established” method in speleothem research. The interaction between the climate parameters, the soil, the host rock and the cave environment are very complex, as you show in this study and due to this a straight transfer function is not established at the moment.

We agree with the author that a straight transfer-function is not the appropriate term to use in this part. Therefore, we removed the term “transfer function” from the paragraph and replaced it with ‘cave system’. The first three sentences fro the paragraph on P 1824, L19-23, was adapted to: “A second limitation is that a good understanding of the cave system is needed to understand what the measured proxies are reflecting.

The interaction between the climate parameters, the soil, the host rock and the cave environment needs to be well understood for the studied cave and time frame. On classical multimilennial and centennial time scales, the processes influencing the stable isotopes of oxygen and carbon are well established (Baker et al., 2007; Fairchild et al., 2006)."

P 1824, L 26: Please replace "systems" with "studies".

We thank the referee for this comment and adapted it in the manuscript.

P 1825, L 22: Please replace "19th" with "19th".

We thank the referee for this comment. This was done in the submitted manuscript but must have been reformatted for the online discussion paper. The authors will mention this formatting problem during the resubmission so that the 'th' is in superscript in the final version.

P 1826, L 1-3: From this description, I think, the "Proserpine stalagmite" is more a flowstone than a candle shaped stalagmite. Please replace "stalagmite" with "flowstone" in this context in the complete manuscript.

The referee is absolutely correct that the Proserpine is not a traditional candle shaped stalagmite. Due to its extremely high growth rate and the continuous drip this speleothem became very big and large. We thus agree that the Proserpine could appear more as a mix between a flowstone and a stalagmite rather than a classic candle shaped stalagmite. However, we think that using the term "flowstone" trough the whole manuscript is not the most appropriate. The Proserpine remains a speleothem that grows vertically under a clear drip. Using the term flowstone, would in our opinion confuse the readers, and provide the wrong idea that the Proserpine grows from water flowing over a surface, which is absolutely not the case. Furthermore, the studied core Proserpine was sampled in the center of the stalagmite where calcite is deposited from a clear drip and not from flowing water.

P 1826, L 3: What is meant with "high drip flow"? Is this water flowing along the surface of the Prospine flowstone? Is there a soda straw, stalactite or drapery above the Prospine flowstone from which the water is dripping on the surface? Is there one drip site or more dripping on the Prospine flowstone? This is not clear from this description. But this is a very essential point to know, because this has an influence on the interpretation and understanding of the monitoring results and further on the speleothem record. Please give some detailed information to this issue.

We agree with the author that a more detailed description of the 'flow' above the Proserpine is necessary to have a good understanding of how the Proserpine is formed. We also agree that information on the stalactite or drapery growing above the Proserpine provides relevant and needed information. Therefore the following wad added on P 1829 L 16 at the beginning of the results part: "The drip water feeding the Proserpine falls from a small drapery-shaped stalactite indicating that only a small part of the dissolved calcite precipitates from the drip water when hanging on the ceiling of the cave. The drip falls for approximately 30 m before reaching the surface of the Proserpine. The Proserpine grows under a 'flow' or continuous 'rain' that falls on the surface of the stalagmite at 4 points. The core used in this study was sampled in the center of the speleothem, where water is dripping on the stalagmite during the whole year."

P 1826, L 3: Please replace "stalagmite" with "cave chamber", because the epikast thickness covers the complete cave chamber.

We thank the referee for this comment and adapted it in the manuscript.

P 1826, L 4-9: This part, describing the residence and reaction time of the epikarst and the drip rate, respectively, should be moved to the results part, because these are specific results, which are essential for the interpretation of the signal in the monitoring results.

This comment concerns two sentences. The first sentence describes the conclusion from the study of Verheyden et al., 2008 describing a 6-month residence time of the drip water in the epikarst of the Han-sur-Lesse cave system. The second sentence is an observation that the water flow increases within 24h following a rainfall event. Together, both sentences show that the Han-sur-Lesse epikarts is acting as a piston flow system.

For the first sentence, we do not agree with the referee that it should be moved to the result part. This conclusion does not result from this study but is made in the previous study of Verheyden et al., 2008. It thus adds to the current knowledge of the study area and hydrological setting and belongs to the discusion. Putting the conclusion from another study in the result part would be incorrect and in some way might be seen as an attempt to expropriate Verheyden et al. 2008 of this results. Therefore we moved the sentence to the discussion part where the epikarst system is discussed. The sentence was moved and slightly adapted to P 1833 L9-11: “This same residence time of a part of the water has been suggested by Verheyden et al., (2008) based on a comparison between the isotopic signals of rainwater and drip water in the Pere Noel cave, formed in the same karst system as the Han-sur-Lesse cave.”

We do agree that the second sentence, describing the reaction of the drip flow within 24h following a rainfall event, is more a result than a cave setting. Therefore this sentence was moved to the result part on P. 1830, L4: “Superimposed on this seasonal cycle, very short events of increased drip flow are observed within 24h following a heavy rainfall event.” However, the last part of the sentence describing that the epikarst system is acting like a piston-flow system is already an interpretation and therefore not suited for the result part. The interpretation of the piston flow is already carried out in the discussion part (P1833, L11-15).

P 1826, L 9-12: This part about the ventilation pattern of the cave should as well be moved to the results part. At this part it should be only mentioned that two passages connect the “Salle-Du-Dome” to the neighboring chambers. In general, these connections in the cave do not only control the ventilation pattern. The most important feature controlling the ventilation of a cave is the number and wideness of the cave entrances

and how big is the difference in altitude between the entrances. Further, these cave conditions are very important for the interpretation of the cave air monitoring data. Therefore, please take this into account in the results and discussion part and give the necessary information about this here for the Study area. You could take a look in several of the monitoring publication for suggestions how to in cooperate these information into the manuscript.

We fully agree with the referee and agree that part 2, the ‘study area’, is not the best part to describe the ventilation of the cave. Therefore the sentences were removed from part 2 and replaced by (P1826, L9-10): “Two passages connect the “Salle-Du-Dôme” to the neighboring chambers and the Lesse-river flows at the bottom of the chamber.” However we think that the results part is not the best-suited place to discuss the ventilation of the cave. No ventilation measurements were carried out so no real results can be given. We do agree that the ideas on the ventilation pattern should be discussed in the discussion part. Therefore we added the following sentences on P 1833, L26: “Another factor also increasing the chamber ventilation is the fact that the Salle-du-Dôme is connected to two neighboring chambers allowing an airflow trough the chamber. However, no data of cave air flows is available to discuss the ventilation patterns of the cave in more details.”

P 1826, L 15-16: For the description of the climate please use the terms of an international climate classification (for example Köppen and Geiger). The expression that the “rainfall is spread all over the entire year” is colloquial. The correct term for this is “fully humid” (see climate classification after Köppen and Geiger).

We thank the author for this constructive suggestion. However the correct climate class for Belgium is not a “fully humid” climate but rather a “maritime temperate climate”. This was added to the sentence on P 1826, L15-26: “While the temperature displays a well-marked seasonality with cool summers and mild winters, the rainfall is spread all over the entire year indicating a maritime temperate climate according to the Köppen and Geiger classification.”

P 1826, L 19: Please mention here that the very cold March 2013 was an exception and does not represents the average weather conditions of the month March.

We thank the author for this constructive suggestion. The additional sentence was added on P1826, L19: "The coldest temperature of -4.2°C was reached on 15 March 2013. Such an extremely cold March month is an exception and does not represents the average weather conditions for the studied region."

P 1826, L 21: Please describe the soil with the terms of the international soil classification. Therefore, it will be understandable for everyone.

The soil description in the submitted manuscript is derived from the official soil map of Belgium. The soil above the cave is a typical karst soil as commonly found above caves.

P 1827, L 2-4: The measurement of pH-value in a small pool on the surface of the flowstone is not ideal. Due to the degassing of CO2 from the drip water the pH is strongly altered. It is also altered, when it is measured directly after sampling of the water, but in a lower amount. Why did you not measure the pH from the water you collected in the plastic swimming pool? Or did you sample drip water for pH measurements in an extra bottle? This is not clear form the text. But never the less it should be mentioned in the manuscript, that the pH is always altered. This fact is mentioned in several publications dealing with cave monitoring.

From the comment of the referee it is clear that our description of how the pH was measured is not clear. The small 'pool' the pH was measured in is a really small (6 cm wide and 3 cm deep) pool on the surface of the stalagmite where drip water is continuously dripping in. This pool thus forms an ideal place to measure the pH since it is constantly renewed with fresh drip water. The values obtained for this pool are thus less altered then water that is collected in the inflatable pool or sampled in a bottle and measured afterwards. Because we think it is important that the readers understand this well, the following sentences were added on P1827, L2: "The drip water temperature

and pH were determined in a small pool (6 cm wide and 3 cm deep) formed on the stalagmite's surface where drip water is continuously falling in. The extremely short residence time of the water is this small pool guarantees that the temperature and pH suffer minimal alteration."

P 1827, L 6: You measured CO2 concentration in the cave air tree times per cave visit. Where in the cave chamber do you measure? This could give some information about the cave ventilation patter, when you measure CO2 near to the ceiling and at the floor.

We measured the pCO2 directly above the speleothem surface. This was done three consecutive times at exactly the same spot. Therefore, the pCO2 values cannot be used to determine ventilation patterns. To clarify the measurement location the following part is added to the sentence: "Three times per visit, pCO2 values were measured at the same spot, right above the surface of the speleothem and reported as an average of the three values."

P 1827, L 7-11: Where is the drip water coming from? Is this water flowing over the surface of the flowstone? Or is this drip water from one or more stalagmites above the flowstone? This is not clear and has to be clarified for the reader.

The drip water is 'dripping' from a drapery shaped stalactite at different places on the Proserpine surface. The dripwater falls for approximately 30m before hitting the surface of the stalagmite. To clarify this to the readers the following part was added as the first paragraph under results (P1829, L16): "The drip water feeding the Proserpine falls from a small drapery-shaped stalactite indicating that only a small part of the dissolved calcite precipitates from the drip water when hanging on the ceiling of the cave. The drip falls for approximately 30 m before reaching the surface of the Proserpine. The Proserpine grows under a 'flow' or continuous 'rain' that falls on the surface of the stalagmite at 4 points. The core used in this study was sampled in the center of the speleothem, where water is dripping on the stalagmite during the whole year."

P 1827, L 24-25: What is the three point correction method? Is this a correction of the measured data with a linear regression line of three different standard materials with different isotopic compositions? Please give some exact information to this. Further, you mentioned the three “home made standards”. What material are these standards? Please use the term “in house standards” instead of “home made standards”. And please give the isotopic composition for all three standards for d18O and dD.

We agree that the method part of the Picarro measuring corrections was not clear. Therefore the paragraph was reworked to (P1827, L22-27): “The measured values were then corrected using house standards with a strong different isotopic composition. The first house standard, called DO1, has a δ18O value of -0.79 ± 0.04 ‰ and a δD value of -26.2 ± 0.2‰ and was made by collecting the damp of boiling water. The second house standard, called DO2, is Milli-Q water with a δ18O composition of -7.38 ± 0.04 ‰ and a δD value of -48.8 ± 0.2 ‰Ṫhe third house standard, called DO3, is water from Antarctic glacier ice that was filtered and has a δ18O composition of -14.77 ± 0.04 ‰ and a δD composition of -105.1 ± 0.2 ‰Ṫhe most positive (DO1) and the most negative (DO3) house standards are used to obtain a two-point calibration line. The DO2 house standard, with intermediate values, serves as a “target” or “control” point. By using these three standards, we can correct the measured values for a lateral difference as well as for a stretch that can occur in the measurement range. All three work-standards were made in the lab and calibrated against the international standard VSMOW2, GISP and SLAP2. These three international standards were used to correct the house standards in the same way the DO1, DO2 and DO3 standards are used to correct the measurements. The calibration curve was obtained using VSMOW2 and SLAP2, and the GISP standard was used as ‘target’. Every house standard was measured 55 times on the PICARRO L2130-i.”

P 1827, L 27: Please replace “two measurement rounds” with “two measurements”.

We thank the referee for this comment and adapted it in the manuscript.

P 1827, L 27: Do you mean the long term reproducibility of your “in house standards”

with the “analytical uncertainties”? Please give the long term reproducibility of the standards in your lab, when this is not the case in the text. Further, please mention in the text to which international reference material your “in house standards” are calibrated.

This was added in the manuscript. See two comments above. Therefore the paragraph was reworked to (P1827, L22-27): “The measured values were then corrected using house standards with a strong different isotopic composition. The first house standard, called DO1, has a δ18O value of -0.79 ± 0.04 ‰ and a δD value of -26.2 ± 0.2‰ and was made by collecting the damp of boiling water. The second house standard, called DO2, is Milli-Q water with a δ18O composition of -7.38 ± 0.04 ‰ and a δD value of -48.8 ± 0.2 ‰Ṫhe third house standard, called DO3, is water from Antarctic glacier ice that was filtered and has a δ18O composition of -14.77 ± 0.04 ‰ and a δD composition of -105.1 ± 0.2 ‰Ṫhe most positive (DO1) and the most negative (DO3) house standards are used to obtain a two-point calibration line. The DO2 house standard, with intermediate values, serves as a “target” or “control” point. By using these three standards, we can correct the measured values for a lateral difference as well as for a stretch that can occur in the measurement range. All three work-standards were made in the lab and calibrated against the international standard VSMOW2, GISP and SLAP2. These three international standards were used to correct the house standards in the same way the DO1, DO2 and DO3 standards are used to correct the measurements. The calibration curve was obtained using VSMOW2 and SLAP2, and the GISP standard was used as ‘target’. Every house standard was measured 55 times on the PICARRO L2130-i.”

P 1827, L 28: Please add that these isotopic values are given in respect to VSMOW.

We thank the referee for this comment and added that all values are reported in ‰ VSMOW. We added on P 1827, L28: “The reported values are the average of the two measurements and reported in ‰.”

P 1827, L 28: Please check the uncertainties of 0.5‰ for the dD. To me it seems to be too low for a 2s.

The analytical uncertainties are indeed very low, but they are correct. The uncertainty is calculated every measurement round and the reported values are the average of all measurement rounds. The low values are due to the high precision of the Picarro L2130-i Cavity Ring down spectrometer and because the measured samples, and especially the drip water samples, share similar isotopic compositions with very low variation. The typical 2sigma variations given for this type of PicarroL-2130-i are 0.06‰ for δ18O and 0.4‰ for δD and correspond with our obtained values. Furthermore, the average 2sigma values from all measurements made on the PicarroL-2130-I in our lab on water with an isotopic composition similar to the measured rain and cave water equal 0.1‰ for δ18O and 0.6 ‰ for δD. These 2sigma values measured on all waters are slightly higher due to the larger difference between samples measured by the other researchers. The more similar the sample in the batch, the smaller the error on the Picarro.

P 1828, L 6: Please make a new paragraph at the end of the sentence.

We thank the referee for this comment and made a new paragraph at the end of the sentence.

P 1828, L 18-23 and P 1830, L 17: Please check if Mattey et al. (2008) is the correct reference for this method. It is not exactly described in the same way in Mattey et al. (2008) as you describe it. Further, this method is also used by Spötl et al. (2005) during the Obir Cave monitoring. Perhaps there was a mistake in the references.

We thank the referee for the comment. Indeed, the first paper describing the method to measure the δ13CDIC in karst waters is Spötl et al., 2005. This was adapted in the manuscript and in Table 1.

P 1828, L 25: Please give the standard material, which was used to determine the analytical uncertainty, the international reference material to which a possibly used "in

house standard" was calibrated, and add VPDB.

The following paragraph was added on P 1828, L25: "The injected CO2 is measured against an house standard that consists of CO2-gas with a δ13C composition of -34.07‰.The measurement series starts with 5 house standard injections. After a series of 5 samples a new house standard injection is measured to correct the drift. The house standard is calibrated against two international standards, IAEA-CH6 (sucrose) and IAEA-CH7 (polyethylene). All measurements are reported in ‰ VPDB with an analytical uncertainty of 0.4 ‰ (2σ)."

P 1828, L 28: You took of five samples from each glass slap. Where on the glass slap you scratch of the sample? Were the glass slaps completely covered by calcite or was there only calcite on the rim? Please add some more information to this issue.

The glass slabs were always fully covered with calcite. The calcite of the whole glass slab was scraped of, collected in a tube and sampled for measurements. Since we agree that this information can be necessary for the reader, we added the following paragraph on P 1828, L28: "Every collected slab was always fully covered with calcite. The freshly precipitated calcite was then scraped off the entire slab. Five samples per slab were sampled from the collected powder and measured for their δ18O and δ13C composition."

P 1829, L 1-2: Where were the glass slaps placed on the flowstone? Are the positioned under different drip sites? Or is the water flowing over the glass slaps? This is quite important due to the interpretation of the results. If there are different drip sites, which could have different drip rates, this could influence the stable carbon isotopic composition of the water and the calcite. Therefore, the values of the d13C from the different glass slaps should not be averaged.

We thank the referee for this very good comment. The glass slabs were always placed under the drip point where the speleothem core was sampled. All three slabs were lying next to each other and the water or rain was dripping on them. Since the water is coming from the same drip site and since this drip site is the one growing the Proserpine speleothem, we believe that the results from the three slabs can be averaged. To clarify this to the reader we added the following paragraph on P1828, L 28: “All three slabs were positioned under the same drip site, where the speleothem core was sampled and collected during the next visit.”

P 1829, L 5: Did you use a microscope for the layer counting? If this is the case please add the specific microscope you used.

Layer counting was carried out using the Mechantec Micromill microscope. As suggested by the referee, this was added in the text on P1928, L7 : “Layer counting was carried out on high-resolution scans using Adobe Photoshop, by counting on the slab itself and by using the Merchantec Micromill microscope.”

P 1829, L 5: Is the age model of this flowstone only based on layer counting or were there some absolute age datings performed?

U/Th-ages were carried out later on the new drill core, but since we are studying the upper 16mm, which are very recent, the errors of the U/Th ages are too large to deliver significant conclusions. Therefore these results were not added to the paper. The core argument for the age model is that the second core, used in this study, was drilled 10 years after the core studied by Verheyden et al. (2006) and that when comparing the two cores, exactly 10 extra layers couplets could be counted in the new core compared to the old core.

P 1829, L 9: Please replace “Merchantic” with “Merchantec”.

We thank the referee for pointing out this typing error and adapted it in the text.

P 1829, L 13: Please add information about the used standard, to which international reference material it is calibrated, and that the values are given to VPDB.

The international standard used to calibrate the measurements is the calcite powder NBS-19 standard. All values are indeed given in VPDB. Since we agree with the referee

that this information is improving the paper we added the following sentence on P1829, L14: “All values were corrected using the international calcite powder standard NBS-19 and reported in ‰ VPDB.”

P 1829, L 19-21: Please rework the cave temperature description. Please give the month and C of the maximum and the minimum and describe the trend between these extremes. Therefore, it will be consistent with Table 3.

We thank the referee for this suggestion and also agree that describing the temperature that way is more consistent with Table 3. Therefore we reworked the temperature description on P 1829, L19-21 to: “The cave air temperature (Fig. 2c) in the Salle-Du-Dôme varies seasonally over a range of ca. 4°C with highest values reaching up to 14.5°C in August and lowest value of 10.5°C in February. After the warm August month, temperature values decrease gradually until December. From December through the end of May, temperatures remain stable and vary around an average of 11°C. In July, the temperature increases quickly to reach the warmest values in August.”

P 1829, L 22: Please deled “(Fig. 2b)” in this line, because the average is not seen in this figure.

Since this sentence has been reworked (see previous comment) “(Fig. 2b)” is not mentioned anymore.

P 1829, L 24-25: Please add some information about the seasonality of the drip site. Also here the questions from before arises. Is it one or more drip sites, which were averaged, or is it water flowing along the surface of the flowstone? Please categorise your drip site/sites after the classification of Smart and Friederich (1987) and Baker et al. (1997). This makes the comparison with other studies much easier.

We thank the referee for this suggestion and incorporated the comment in the result part. Since referee 1 also commented on the drip flow characteristics, both comments were combined in a new paragraph at the beginning of the results part on P 1829, L16:" The drip water feeding the Proserpine falls from a small drapery-shaped stalactite indicating that only a small part of the dissolved calcite precipitates from the drip water when hanging on the ceiling of the cave. The drip falls for approximately 30 m before reaching the surface of the Proserpine. The Proserpine grows under a 'flow' or continuous 'rain' that falls on the surface of the stalagmite at 4 points. The core used in this study was sampled in the center of the speleothem, where water is dripping on the stalagmite during the whole year."

The following paragraph describing that the drip flow was measured using the 4 drip sites is added on P1829, L25: "The drip flow of the Proserpine was always measured for the whole stalagmite, thus including the 4 drip sites, to have an idea of how much water is dripping on the whole surface of the speleothem."

The following sentence describing in which class the drip site is classified according to the Smart and Friederich (1987) classification is added on P 1829, L 28: "With a discharge of 3.10-3 L/s and a coefficient of variation of approximately 2, the drip site can be characterized as a "percolation stream" according to the classification of Smart and Friederich (1987)."

P 1830, L 4: Please make a new paragraph at the end of the sentence.

We thank the referee for this suggestion and made a new paragraph on P1830, L4.

P 1830, L 7-8: The pH value is not related to the drip rate. Due to your measurement in the water pool on the flow stone surface the pH value is altered and not the initial one. It is much more related to the CO2 in the cave atmosphere. There are a lot of studies describing this. Please, go into the literature and clarify this.

As an answer to a previous comment, we have already clarified that the water is in the pool, where the pH is measured, is always refreshed with new water dripping in. Therefore we consider the pH to be only very little altered. Concerning the seasonal pH variations, we also agree that they are related to the seasonal pCO2 variation. Referee

1 also made this comment. Therefore we reworked the whole paragraph on P 1834 L 17 to the following: "The drip water pH clearly displays a seasonal variation that is interpreted to be related to the seasonally varying cave air pCO2. In winter, when the pCO2 is low, more CO2 will degas from the drip causing the pH of the drip water to increase. The effect of CO2-degassing on the pH can slightly be modulated by the drip rate. During periods of extremely high drip rate, full CO2-degassing may not be completed. The drip water will therefore be lower in pH during strong drip flow events. This is very clear during the May wet event of 30 May 2013, where the discharge peaks to high values and where the pH (Fig. 2f) drops from 8.2 to 7.9."

We also added the discussion of the seasonally changing δ13CDIC below this paragraph on P 1834 L 17:"More degassing of the drip water due to low cave air pCO2 also results in the preferential loss of light C-isotopes. This will cause the δ13CDIC in the remaining drip water to be more positive during periods of strong degassing such as is the case in winter. This effect on the δ13CDIC is rather small but visible in the monitoring results, with more positive values during winter."

P 1830, L 11: Please replace "opposite trend" with "anticorrelation".

We thank the referee for this comment and adapted it in the manuscript.

P 1830, L 15-18: The comparison of the two measuring methods of d13CDIC is not necessary for this paper. It could be perhaps a small study by its own, but it is not fitting in the interpretation of the monitoring data and the speleothem.

We agree with the referee that discussing the comparison between the previously established δ13CDIC measuring method by Spötl et al. (2005) and Mattey et al. (2008) and our new established method is not necessary in the paper. Therefore we removed this comparison in the method and results part. To clarify that our method delivers the same results as the method of Spötl et al. (2005) and Mattey et al. (2008), we added the following sentence in the method part on P 1828 L23: "This sampling and storing method was tested against the method described by Spötl et al. (2005) and delivers similar results within the analytical uncertainties."

P 1830, L 21-28: This is all interpretation and not only results. Please move this to the discussion part.

We do not fully agree with this comment. In this part, we are describing the important patterns seen in the rainwater δ18O and δD results. However, we can understand that relating these observations to other measurements such as the temperature and the precipitation intensity can be seen as an interpretation. Therefore we agree to delete the following parts in the results: "They occur in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014. They appear to correspond to periods of higher rainfall recorded in the site (Fig. 3b). One larger drop (red arrow in Fig. 3c and d) of 9 ‰ for the δ18O and of 90 ‰ for the δD signal occurs at the beginning of March and seems to correlate with unusually cold air temperatures of -6.3°C (Fig. 3a), measured at the RMI station of Han-sur-Lesse."

P 1831, L 17 and 23: Instead of "contrasting behaviour" and "opposite trend" you could describe this as an anticorrelation pattern.

We thank the referee for this suggestion and adapted it in the manuscript.

P 1832, L 3-21: Please reduce this discussion to the interpretation of the snow event. See general comment in relation to over interpretation.

We do not agree with this over-interpretation comment. We stand by our interpretation, which is supported by the following argumentation. The isotope data displays 6 small but clear drops that relate to increased rainfall events detected in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014. We agree that in temperate regions, the temperature-effect is the main factor affecting the rainwater isotopic composition. The smaller drops in what do not relate to temperature drops but rather to small periods of increased rainfall. As suggested in the general comments, the referee also indicates a period of increased rainfall in summer. He suggests that

this event does not affect the rainwater isotopic composition. We do not agree with this either. These two smaller summer rainfall events (we assume that the referee refers to the increased rainfall in August and September) do cause the rainwater isotopic composition to also display two small drops (highlighted in purple in the figure added).

Initially, we did not discuss these two drops because of their smaller amplitude compared to the ones occurring in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014. Therefore, we still believe that 6 smaller drops in the isotopic composition of the rainwater observed in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014 are significant and relate to the increased amount of rainfall in these periods. We therefore choose to keep this discussion part in the manuscript.

P 1832, L 26: Please add at the end of the sentence "(Fig. 3e)".

We thank the referee for this suggestion and added it in the manuscript.

P 1832, L 22 to 1833, and L 3: Please rework this passage. Write that the drip water signal is quite constant, and the only noticeable thing is the negative peak in July/August. Say that it is in the range of the uncertainty, but that it is probably related to the strong decrease in the isotopic composition of the rain water in March. Do not make this passage longer than necessary.

We agree that the paragraph can be shortened and adapted it. The new paragraph on P 1832, L22 to P1833 L, 3 is: "The δ18O and δD compositions of the drip water display almost no variations throughout the year indicating that the water residence time is sufficiently long to homogenize the isotopic composition of the drip water (Fig. 2j). In July and August, a small negative excursion in the drip water δ18O of 0.1 ‰ occurs (Fig. 3e). Although it is only of the size of the analytical uncertainty, we consider it meaningful because several points support it. This small drop in the drip water composition is most probably related to the strong decrease in isotopic composition of the drip water in March."

P 1833, L 4-15: The residence time of the water is probably much longer than 6 month, because the ï ÌÍAd’18O signal in the drip water is really constant. To verify the exact time a dating of the water with tritium could give some more information. There is no doubt about that a portion of water reaches the cave after six month. Otherwise the decrease in the ï ÌÍAd’18O drip water data will not appear, but is very well mixed with water staying much longer in the epikarst.

We agree with the referee. The residence time can indeed be much longer than one year, but the first water is already percolating trough the cave after 6 months. Therefore the concerning paragraph on P1833, L4-15, was adapted to: ”The intense mixing of the percolating vadose water in the epikarst reduces the δ18O shift of about 8 ‰ in the meteoric to a hardly detectable one of about 0.1 ‰ in the drip water. This indicated that the drip water residence time is most probably much longer than 6 months due to the really constant signal of the drip water δ18O. However, a part of the water reaches the cave within 6 months as is indicated by the shift in drip water δ18O in August-September. With an increase in drip rate within 24 h following a heavy rainfall event and the long residence time of the drip water in the epikarst, hydrological system appears to act like a piston flow system with a the first waters coming trough after half a year.”

P 1833, L 18: Please replace “evolution” with “cycle”.

We thank the referee for this suggestion and adapted it in the manuscript.

P 1833, L 21: You say that the temperature difference between cave air and drip water is also made in other cave, but possible reasons for this are not given here. Please add some information.

We understand that stating this makes the reader want to know more about the difference between the drip water and cave air temperature. Adding more explanations on the link between the drip water and the cave air temperature can be a long discussion and out of the scope of the discussion here. Therefore we removed the last part of the sentence. The new sentence on P 1833, L21 is: “However, the temperature range,

which is around 20°C outside the cave, is reduced to about 3°C inside the cave.”

P 1833, L 22-23: The river could play a role in cave ventilation, but the biggest influence should have the number and altitudinal location of the entrances of the cave.

We agree that the Lesse-river flowing at the bottom of the chamber is most probably not the only factor influencing the ventilation of the chamber. We know of two existing connections of the Salle-du-Dôme chamber with two other chambers, but the altitude of the connections is not known. Since we lack more details on the cave ventilation pattern, we choose to not go further into this. Therefore we replaced the text on P 1833, L29 by: ”The Lesse-river and the connections of the Salle-du-Dôme with neighboring chambers are most probably influencing the cave ventilation. However, no data of cave air flow is available to discuss the ventilation patterns of the cave in more details.”

P 1834, L 17-18: Please be very careful with such a statement. Your measured pH is highly altered and is also highly dependent on the CO2 concentration of the cave air.

We absolutely agree with this comment. A similar comment was made by referee 1 and the whole paragraph on P1834 L17-28 to P 1835 L1-8 was adapted. In the new paragraph we link the pH variations to variations in pCO2 in the cave air. The new paragraph is the following: “The drip water pH clearly displays a seasonal variation that is interpreted to be related to the seasonally varying cave air pCO2. In winter, when the pCO2 is low, more CO2 will degas from the drip causing the pH of the drip water to increase. The effect of CO2-degassing on the pH can slightly be modulated by the drip rate. During periods of extremely high drip rate, full CO2-degassing may not be completed. The drip water will therefore be lower in pH during strong drip flow events. This is very clear during the May wet event of 30 May 2013, where the discharge peaks to high values and where the pH (Fig. 2f) drops from 8.2 to 7.9.”

P 1834, L 21: The water in the epikarst is not more acidic, due to a pH value above 7. Further, the water in the epikarst could be completely saturated with calcium carbonate and have therefore a pH of for example 8. Please discuss the issue of prior calcite precipitation, which could have an important influence on the pH values of the drip water.

This part was removed from the manuscript. The new paragraph is mentioned in the comment above.

P 1834, L 25: The delay of half a year is unlikely. It is much more likely that it is one and a half year or two and an half year.

This part was removed from the manuscript. The new paragraph is mentioned in the comment above.

P 1834, L 26-27: The pH value has nothing to do with the amount of dissolved ions. To say something about the ion content of the drip water you have to measure the elements and/or the conductivity. Therefore, please correct this sentence.

This part was removed from the manuscript. The new paragraph is mentioned in the comment above.

P 1834, L 27-28: This statement is not correct. One important factor for the precipitation of calcite is the saturation index. Further, the CO2 concentration of the cave air plays a role.

This part was removed from the manuscript. The new paragraph is mentioned in the comment above.

P 1835, L16-17: Also during short residence times in the epikarst the water get saturated with calcium carbonate quite fast and the pH is altered when the water reaches the cave or when prior calcite precipitation takes place in the epikarst zone.

We absolutely agree with this statement. Because this was also a comment of referee 1 the part in the text has been removed and adapted to the following on P 1835, last sentence of point 5.2: "During the 'summer-mode', cave air and drip water temperatures increase. The plant coverage reactivates leading to lower discharge, higher

pCO2 and lower δ13C values of the cave air CO2. The drip water pH is lower and the δ13CDIC values are slightly lower due to less CO2-degassing from the drip water."

P 1835, L 23: Lachniet (2009) is not the best reference for this. There are several studies about the physics of calcite precipitation in cave environments.

We changed the reference to Dreybrodt and Scholz, 2011.

P 1835, L 25: Please replace "kinetic effects" with "disequilibrium effects", because disequilibrium encloses all possible effects.

We thank the referee for the suggestion and adapted it in the text.

P 1835, L 26: Please replace "transfer-function" with "system".

We thank the referee for the suggestion and adapted it in the text.

P 1836, L 4-5: Please replace "the calcite-water fractionation factor" with "a calcite water fractionation factor".

We thank the referee for the suggestion and adapted it in the text.

P 1836, L 7 and L 8: Please replace "in-lab" with "laboratory". We thank the referee for the suggestion and adapted it in the text.

P 1836, L 12: Please replace "category of" with "approach to determine".

We thank the referee for the suggestion and adapted it in the text.

P 1836, L 19-29 and P 1837, L 1-2: Please delete "suggesting that it is deposited close to equilibrium with the drip water" in line 19-20, because 1‰ offset means that it is not in exact equilibrium. Depending on the fractionation factor it is more or less close to equilibrium. It should also be discussed that the best fitting fractionation factor of Tremaine et al. (2011) is a fractionation factor from the natural system and that it is questionable if real equilibrium could exist in nature. Please clarify these sentences.

We do not agree that the part "suggesting that it is deposited close to equilibrium with the drip water" should be deleted. We do not state that the calcite is deposited in exact equilibrium. This is not written in that sentence. We state that the calcite is close to equilibrium. However, we agree with the second part of the comment where the questionability of the fractionation factor of Tremaine et al. (2011) is reflecting equilibrium conditions. We added to following at the end of the sentence on P 1836, L23 : "Among the in laboratory established relationships, that of O'Neil et al. (1969) modified by Friedmann and O'Neil (1977), best corresponds to our observations with on average 0.25‰ difference with the values measured on the glass slabs." At the end of the paragraph we added: "However, the good match between the measured values and those calculated from the Tremaine et al. (2011) relationship does not constitute a proof of equilibrium condition. The fractionation factor from Tremaine et al. (2011) is derived from the natural system and it is questionable if real equilibrium does exist in nature."

P 1837, L 3-4: Please replace "deposition of calcite occurs in oxygen isotopic equilibrium" with "deposition of calcite occurs near to oxygen isotopic equilibrium".

We thank the referee for the suggestion and adapted it in the text.

P 1837, L 12, 13 and 18: What do you mean with "fresh calcite", please clarify this.

We mean calcite that is "freshly" deposited on glass slabs. Since this may seem confusing as commented by referee 2, we changed it in "glass-slab" calcite.

P 1837, L 17: The aspect of drip rate influencing the ï ÌÍAd'18O of the calcite is not mentionned here. Please also discuss this point here.

We do not agree with this comment. Indeed, the drip rate can influence the equilibrium state of calcite deposition and thus the δ18O composition of the deposited calcite. But drip rate is not the only factor. Also degassing rate or ventilation can affect the equilibrium state and thus δ18O of deposited calcite. Since the δ18O signal is deposited close to isotopic equilibrium we think that discussing all the possible factors that could affect

the δ18O composition of the drip water is not necessary. Furthermore, in the studied cave setting the drip rate has no influence on the calcite δ18O composition because periods with higher drip should cause more negative δ18O signals. The exact opposite is observed with more positive δ18O values during the wetter months.

P 1837, L 19-20: Please replace "ï ÌÍAd'18O composition of the glass slaps" with "ï ÌÍAd'18O composition of the calcite on the glass slaps".

We thank the referee for the suggestion and adapted it in the text.

P 1837, L 24-25: Please discuss the possible disequilibrium effects (e.g., evaporation) here in much more detail and why they did not play a role.

We do not think that it is necessary to discuss the all the disequilibrium processes that could influence the δ18O composition of the calcite since the oxygen isotopes are almost deposited in equilibrium with their drip waters. This would make the discussion too heavy and the reader may loose the scope of the story.

P 1837, L 27-28: Please delete "reflecting the C3 type vegetation growing above the cave", because this is not a thing which is clearly visible in a calcite composition of -10‰ of d13C.

We thank the referee for the suggestion and adapted it in the text.

P 1838, L 3-6: There are much more factors in the cave environment influencing the d13C composition of the calcite (e.g., drip rate, CO2 of the cave air). Please give more information.

In the sentence on P 1838, L3-6, we discuss which factors determine the δ13C composition of calcite deposited in equilibrium with its drip water. Discussing the possible disequilibrium effects is not suited at this point.

P 1838, L 13: You gave the number "1.001" here, but it is not clear what it is at that point ï ÌÍA ÌÍa or ï ÌÍAËĞe?

We agree that this is not clear. The sentence has been adapted and the ε was used to be consistent with the following sentence where the temperature dependence of the fractionation factor is given in ε. The new sentence on P 1838, L13 is: “The fractionation factor between the DIC and the deposited calcite is estimated to be ε= 1 ‰ for temperatures between 10 and 40°C (Romanek et al., 1992).”

P 1838, L 14-15: Here you gave a formula for ï ÌÍAËĞe. Please make it consistent.

We adapted the previous sentence to be consistent with this sentence. See previous comment.

P 1838, L 19-24: Please think carefully about the term equilibrium at this point. It is depending on the used fractionation factor (Mook or Romanek) and the other question is if really isotopic equilibrium fractionation could appear in a natural system. Also the fractionation in winter of 1.4 0.5‰ does not show real equilibrium conditions.

We fully agree with this comment. Since it was not clear from the paragraph, we reworked the discussion. Also, as suggested by the reviewer, we added the discussion that isotopic equilibrium fractionation is mostly improbable in natural systems. The new paragraph on P 1838, L9-24 is: “With the seasonal variation of the calcite δ13C and the rather stable δ13CDIC, the fractionation factor of the C-isotopes also varies seasonally between a low value of 1.4 ± 0.5 ‰ during the cave ‘winter-mode’ and a higher value of 3.2 ± 0.5 ‰ during the cave ‘summer-mode’. The ε-values during the cave ‘summer-mode’ are higher than the suggested ε-values by Romanek et al. (1992) and Mook (2000) indicating out of equilibrium deposition during the summer months. During the winter months the ε-value is smaller compared to the Mook (2000) value but higher compared to the Romanek et al. (1992) value indicating that during winter the calcite is deposited closer to isotopic equilibrium.”

P 1838, L 28-29: Please be careful with such a statement. For this you should know the amount of CO2 in the drip water, which could be calculated with the programme PHREEQC.

We absolutely agree that we have to be careful for such statement. We adapted the sentence to the following on P 1838 L28-29: “The larger the difference between the cave air pCO2 and the drip water pCO2, the stronger the degassing of the drip water and consequently the heavier the δ13C value of the formed calcite.”

P 1839, L 12-17: Please specify the temperature influence on the d13C system. Please give some numbers as you did for the d18O system.

We agree that some number can clearly illustrate that the variations in δ13C are not related to temperature variations. Therefore we added the following sentences to the paragraph on P 1839, L12-17: “With a temperature dependence for the fractionation factor of 0.036‰-1°C (Emrich et al., 1970) the temperature variation leads to changes of 0.15‰ in the calcite composition. This calculated variation is much smaller compared to that of 1.7 ± 0.12‰ measured on the glass slabs. The temperature dependence of the carbon fractionation factor between the DIC and the forming calcite is indeed too small to cause the δ13C shifts of the formed calcite. The temperature is thus not the only factor influencing the δ13C of the deposited calcite.”

P 1839, L 26-28: Please delete the term “kinetic evaporation” this is wrong in the context of CO2. See also general comment to this topic. Further you be contradictory. First you say “kinetic” and in the next sentence “equilibrium” for the same issue. Please rework this.

We thank the reviewer for this comment and changed the word “kinetic evaporation” to “degassing” in the sentence. We also reworked the following sentence to make it consistent. The new text on P 1829, L26-29 is: “In winter, with a higher discharge, degassing of CO2 from the water film on the stalagmite top is smaller and calcite deposition occurs closer to isotopic equilibrium.”

P 1839, L 28-29: Wherefrom do you know that the cave air is dryer in summer? You did not show cave air humidity data. Please rework this! Show humidity data or delete this statement.

We removed the sentence since we have no measured data about it

P 1840, L 2-3: Please calculate a correlation coefficient between d13C from the calcite and the drip rate.

The correlation coefficient between the δ13C from the calcite and the drip rate equals 0.3. This is a rather low since a statistical correlation calculates the correlation between two graphs point per point. It is not necessary to add this since we do not state that the d13C and the drip rate are strongly correlated. From the monitoring results, they display a clear visual inverse relationship. This is also how it is written in the manuscript.

P 1840, L 5: It is Riechelmann et al. (2013) instead of (2011).

We thank the reviewer for pointing this out and adapted it in the manuscript.

P 1840, L 10-11: “It is however not visible in the d13C record of the DIC (Fig. 2i), which is at its lowest value than.” This is contradictory.

We agree that this sentence is contradictory. Further we think it is not necessary to mention this. Therefore the sentence was removed from the manuscript.

P 1840, L 13: Please replace “Heavier” with “Higher”.

We thank the reviewer for pointing this out and adapted it in the manuscript.

P 1840, L 14-15: In relation to “evaporation” see comments above.

We thank the reviewer for this comment and adapted the sentence on P1840, L13 to: “Higher values occur during the cave ‘summer mode’ when the drip rate decreases and more CO2 degassing can occur from the water film.”

P 1840, L 18-20: Small or elongated crystals are not visible in Figure 4. Did you make thin section analyses of the crystals? Please clarify this. If you made some microscopy analyses please verify what an elongated crystal is and what a small crystal is. Please read Frisia and Borsato (2010) in relation to this topic. To my knowledge the crystal

often do not “care” if it is a summer or a winter layer, they grew continuously over several layers. The difference of the crystals between summer and winter are more or less inclusions and in growth direction elongated holes. More inclusions and holes mean a faster calcite precipitation and therefore, a faster growth rate. Please clarify this.

We thank the reviewer for this comment and we agree that the paragraph needs to be reworked to clarify some points. First, no microscope analysis or thin section was made in this study. The form and structure of the crystals was described by Verheyden et al. (2006). Since this reference was not added at the end of the sentence describing the crystal structure, we understand that the reader may think this was done in this study. Second, we absolutely agree that the difference between summer and winter layers is more likely related to more inclusions and the growth direction of holes. Since we do not have any thin sections, we removed the description of the crystal structure. It is not necessary to discuss this in this paper since it was already done by Verheyden et al. (2006). Therefore, the first sentence of the paragraph on P 1840, L18-20 was rewritten to: “The Proserpine stalagmite displays clear lamination formed by alternating dark, compact layers and white more porous layers (Verheyden et al., 2006).” Also further in the paragraph, parts describing the crystal shape were removed since we did not study this ourselves.

P 1840, L 25-27: How is it confirmed that the layer couplets are annual and how is the age model established? Did you make some datings (Th/U or 14C bomb peak or cross dating with a dated sample from the same flowstone)? Please clarify this and give some more information.

The main argument for annual layer couplets is that ten additional layer couplets could be counted in the new core compared to the core drilled ten years before and studied by Verheyden et al. (2006). U/Th-ages cannot be determined on the core due the too recent ages and the large error bars. They provide no additional information. Also, no 14C bomb peak was established. We believe that the argumentation for annual layer couplets is clearly discussed in the manuscript and that it is not necessary to add more information. Furthermore, the exact age of the layers is not the most important. The focus lies on the difference in isotopic signal between the layers.

P 1841, L 16-18: You wrote that the ion concentration in the drip water plays a role for calcite growth rate, but did not provide data for this (e.g., saturation index with respect to calcite). There are also other factors, which could influence the growth rate of speleothems (e.g., drip rate, CO2 concentration in the cave atmosphere). Please go to the corresponding literature and discuss this in more detail here.

We absolutely agree with this comment and we reworked the passage on P1841 from L10 to L27 has been rewritten. The different factors that could influence the growth rate of speleothems are discussed. We also added that since we do not have any data on the saturation index of the calcite, we could not make any statement on this issue. The new paragraph on P 1841 from L10 to L27 is:" This indicates that dark layers displaying more positive δ13C and more negative δ18O signals form in summer. The crystal structure of dark layers is compact while the white layers consist of more porous calcite (Verheyden et al., 2006). The fabric of calcite is dependent of its growth rate. The slower the calcite growth rate, the longer the time available to form a nice compact crystal structure and thus the darker bands. The very compact and dark structure of the summer layers compared to the porous structure of the winter layers suggests that the growth rate also varies seasonally. Different factors such as drip rate, amount of dissolved carbonate ions or amount of CO2 concentration in the cave atmosphere affect the growth rate of speleothems. In the case of the Proserpine, the strong seasonal difference in drip rate between summer and winter is supposed to be the main driver of the seasonality in growth rate. The high discharge in winter leads to an increase in growth rate and causes the formed calcite to be porous and full of small inclusions. The CO2 concentration of the cave air is also influencing the calcite deposition rate of the Proserpine. Low cave air pCO2 in winter leads to stronger degassing of the drip water and consequently to an increase in the deposition rate of

the calcite. Both effects, the drip rate and the pCO2, act in the same direction and cause the calcite to be deposited faster in winter compared to summer. Other factors such as the amount of dissolved carbonate in the drip water are most probably also having an influence on the Proserpine growth rate with an increase in calcite deposition rate when the concentration of carbonate dissolved ions in the water is higher. This link has clearly been observed in the drip waters from the Père Noël cave (Genty and Deflandre, 1998). Further investigation in the seasonal cycle of the carbonate dissolved ion concentration of the Proserpine drip water is necessary to understand its influence on the calcite deposition rate."

P 1842, L 1-2: The absolute color contrast could be verified by measuring the gray scale of the layers. There are several programs available for this. When you could not measure this, please relativize this statement. Further the picture in Figure 5 is diffuse and the layers are not very well visible. Please put a better picture in Figure 5.

We thank the referee for this suggestion. However, we think that a greyscale analysis is not necessary here since the link between color and isotopic signature is not the main goal of this discussion. We just want to highlight that on a visual base it is clear that the darker or the whiter a layer, the more pronounced the isotopic signature. We do not have statistical data to support this we agree with the referee that this statement can be relativized. Therefore we adapted the paragraph on P 1842 L 1-10 to: "In 1977 AD (Fig. 4), the visual color contrast between dark and white layers is stronger. In parallel, a stronger difference in the isotopic composition of two consecutive layers is observed for that year. More compact and darker layers have more negative δ18O and more positive δ13C values. Data from the Pere Noel cave, located in the same karst system as the Han-sur-Lesse cave, also show that during periods of lower drip rate, the calcite deposition occurs slower and the darker the formed layers (Genty and Deflandre, 1998). The observations gained from combined monitoring observation and stable isotopic analyses answer the remaining question in Verheyden et al. (2006). Darker layers are formed during the summer months when calcite precipitation is slower. "

P 1844, L 3: Please replace “withe” with “white”.

We thank the reviewer for pointing this out and adapted it in the manuscript.

P 1849, Table 1: Please replace “delta C” with “ï ÌÍAd’13C” in the table caption. I think this method test is not necessary in this study, because it is beyond the scope of this manuscript and would be better put in a method paper.

The table caption was adapted. We still believe this small comparison between the two methods can be used in this study for two reasons. First, this study is the first to use this method so we believe it is important to illustrate that it delivers similar results as the previously used methods. Second, it would not be realistic to write a paper on only this issue.

P 1850, Table 2: It would be easier for the reader, when there are ï ÌÍA ËĞe listed instead of 1000 x lnï ÌÍA Ìĺa, because the most readers are more in the “‰ world” that calculating withï ÌÍA Ìĺa.

We thank the referee for this suggestion and adapted it in the table.

P 1851, Table 3: Please replace “in equilibrium its drip water” with “in equilibrium with its drip water” in the table caption.

We thank the referee for this suggestion and adapted it in the table.

Please change “d18O” in the Table to “ï ÌÍAd’18O”.

The table was submitted using the “δ18O” symbol, but must have been reformatted by the journal. We will mention this during resubmission.

Why is there an error given for the results from the fractionation factor of Tremaine et al. (2011)? Where does it come from and why all the others do not have errors?

Therefore we refer the referee to the paper of Tremaine et al. (2011). Their fractionation factor has been established based on a serie of natural speleothem data. They report

their fractionation with an insecurity band, so we used it as they reported.

P 1852, Fig.1: The font in the cross section is too small. Please enlarge it.

We thank the referee for this comment and enlarged the font of the cross section.

P 1853, Fig. 2: There are no figure captions given for a), b), d), f), g) and h). add these. Further the font in the figure is too small, please enlarge it. c) & d): Please replace “Cave T (C)” with “T (C)”, because you also give drip water temperature. Sometimes you write “Cave air” but also “Drip water” please unitizes this. g): Please replace “pCO2 (ppm)” with “p CO2 (ppmv)”. h), i), j), k), l) and m): For all axes “(‰)” or “(‰ VSMOW)” are missing. Please add these.

We thank the referee for these constructive comments on figure 2. The font in the whole figure was enlarged. The new caption of Fig 2 includes a caption for all measures cave parameters. However, we think that adding the “‰ VSMOW” and “‰” in the axis of the figure makes the figure too heavy and less clear. Therefore, we added this in the figure caption rather than in the figure itself. The reviewed caption of Fig 2 is : “Precipitation intensities (a) and air temperatures (b) are measured at the Han-sur-Lesse station of the Royal Meteorological Institute. All measured cave parameters; the cave air and drip water temperature (c and d), drip flow (e), pH (f), pCO2 (g), δ13CCAVE AIR CO2 (h), δ13CDIC (i), δ18ORAINWATER (j), δ18ODRIP WATER (k) and the δ18O (l) and δ13C (m) composition of the glass slabs were measured during the cave monitoring campaign from November 2012 to January 2014. Cave monitoring results show that the cave conditions vary seasonally between a ‘summer-mode’ lasting from June to December and a ‘winter-mode’ lasting from December to June. The δ18O signal of the glass slabs (j) is deposited in equilibrium with the drip water and reflects the seasonally varying cave temperatures (c and d). The δ13C composition of the glass slabs (k) is not deposited in equilibrium and variation are affected by the seasonally varying discharge amounts (d). The water δ18O values (j and k) are reported in ‰ VSMOW. The isotopic composition of the δ13CCAVE AIR CO2 (h), the δ13CDIC (i) and the glass slabs (l and

m) is reported in ‰ VPDB.“

P 1854, Fig. 3: This is not an appropriate figure caption. Please give figure captions for a), b), c), d), e) and f). c), d), e) and f): For all axes “(‰ VSMOW)” is missing. Please add these.

We included the comments made on figure 2. The new figure caption is: “Fig.3. Precipitation intensities (a) and air temperatures (b) are measured at the Han-sur-Lesse station of the Royal Meteorological Institute. The δ18O and δD of the rainwater (c and d) and the δ18O and δD of the drip water (e and f) are all reported in ‰ VPDB. The δ18O composition of the drip water averages the annual mean composition of the rainwater δ18O. 6 small drops occur in the δ18Orain water in November 2012, December 2012, May 2013, July 2013, December 2013 and January 2014 (indicated in light blue) and correspond with increased rainfall periods. At the end of March a cold temperature peak and prolonged snowfall cause the rainwater δ18O to display a large drop. This negative spike in the 18Orain water in March can be found back in the 18Odrip water in August indicating that a part of the infiltrating water reaches the cave only 4 to 5 months later.“

P 1855, Fig. 4: The picture could be a bit defined.

The picture used for figure 4 cannot be more defined than presented here. The scans were taken at the highest possible resolution (=1200 dpi).

P 1856, Fig. 5: The picture should be much more defined to see the different layers. Further, “(‰ VPDB)” is missing for both isotope axes. Please add these.

The picture used for figure 4 cannot be more defined than presented here. The scans were taken at the highest possible resolution (=1200 dpi). The “‰ VPDB” was added in the first sentence of the figure caption rather than in the picture itself to not overload the picture.

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Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C850/2014/cpd-10-C850-2014-supplement.pdf

Interactive comment on Clim. Past Discuss., 10, 1821, 2014.

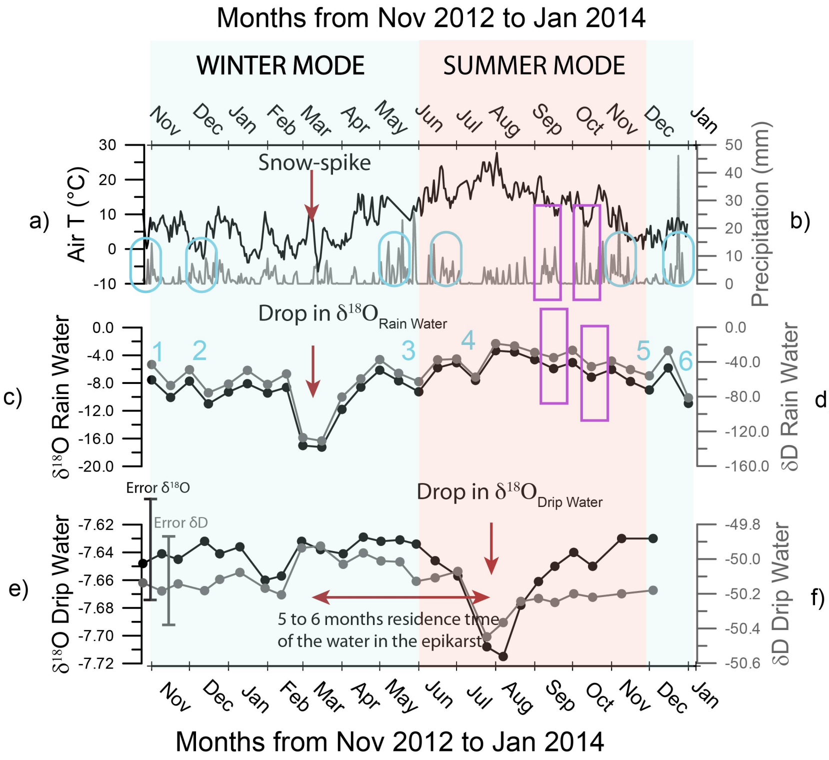
Months from Nov 2012 to Jan 2014
WINTER MODE
SUMMER MODE
Nov Dec Jan Feb Mar Apr May Jun Jul Aug Sep Oct Nov Dec Jan
Snow-spike
a)
Air T (°C)
30 20 10 0 -10
b)
Precipitation (mm)
50 40 30 20 10 0
Drop in δ18O Rain Water
1 2 3 4 5 6
c)
δ18O Rain Water
0.0 -4.0 -8.0 -12.0 -16.0 -20.0
d)
δD Rain Water
0.0 -40.0 -80.0 -120.0 -160.0
Error δ18O
Error δD
Drop in δ18O Drip Water
e)
δ18O Drip Water
-7.62 -7.64 -7.66 -7.68 -7.70 -7.72
f)
δD Drip Water
-49.8 -50.0 -50.2 -50.4 -50.6
5 to 6 months residence time of the water in the epikarst
Nov Dec Jan Feb Mar Apr May Jun Jul Aug Sep Oct Nov Dec Jan
Months from Nov 2012 to Jan 2014

Fig. 1.