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Interactive comment on “A 500 year seasonally resolved $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$, 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 #2 General Comments: This is an interesting and quite detailed study of a 500 year seasonally resolved speleothem stable isotope record from Han-sur-Lesse cave, Belgium. My general impression is that the authors have completed a careful multi-proxy study and for the most part have matched their careful analytical work with thoughtful and well considered and supported interpretations. The U-series data are clearly problematic and I think that even less discussion should be given over to these data. It is finally stated near the top of page 4161 that the layer counting model

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is used to establish the chronology for the stalagmite; perhaps convey this information to the reader earlier in this section. My other main concern is that the manuscript is currently too long c.50 pages) and I would strongly recommend that the authors try to reduce the overall length of the text, paying particular attention to possible repetition in places. A more concise text will improve the readability of the manuscript and ultimately will improve the impact of the work.

In the interpretation/discussion section, changes in the NAO state are often invoked to explain the periods of anomalously high $\delta^{18}\text{O}$ values (cold-dry periods. These are often linked to negative NAO periods in the reconstruction of Trouet et al. (2009). I would like to see the Trouet et al. curve plotted on the same diagrams (e.g. Figs. 4 and 5) so that the reader can more easily assess these putative links. It should also be noted that negative NAO conditions are known to be associated with more easterly-derived moisture that typically has more lower $\delta^{18}\text{O}$ values (e.g. Baldini et al., 2008, *Geophys. Res. Lett.* 35, GL032027 and Baldini et al. (2010) *Climate Dynamics* 35, 977-993). The point is that negative NAO conditions would be expected to produce lower rainfall $\delta^{18}\text{O}$. One would then have to argue that this effect is overwhelmed by the non-equilibrium effects that apparently give rise to higher $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$ in the Prosperine stalagmite during these periods. I do not wish to claim that the authors' interpretation of a link to the NAO is incorrect, but the magnitude of the non-equilibrium effects on $\delta^{18}\text{O}$ and $\delta^{13}\text{C}$ would have to be quite large to produce the observed 'anomalies' (high values) if rainfall values for $\delta^{18}\text{O}$ were lower than usual. Are there any particular ventilation effects that might make the cave particularly sensitive to NAO conditions (wind directions etc.)?

The authors fully agree that the general length of the manuscript was too long. Therefore different parts of the text were re-written and made more concise. This was done for the Abstract, part 2. Study Area, part 4. Results and part 5. Discussion.

In the submitted manuscript the $\delta^{18}\text{O}$ variations were suggested to be linked with variations of the NAO with more positive $\delta^{18}\text{O}$ values reflecting colder and drier periods. However, we agree that this link is somehow difficult to establish. Therefore the dis-

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cussion has been re-written in the reviewed manuscript. The focus is not put more on the similarities between our the climatic interpretation of the Proserpine records and the findings in climate reconstructions mostly based on historical archives such as the studies of Le Roy Ladurie 2004, Luterbacher et al., 2004, Dobrovolny et al., 2010. The link with the NAO is discussed more carefully at the end of section 5.3 Anomalies in the proxy records.

Given the absence of Mg/Ca and Sr/Ca data I think the authors should steer clear of making interpretations that include statements about prior calcite precipitation. In the absence of the trace element data, such interpretations are probably overly speculative. Given the absence of Mg/Ca and Sr/Ca data' indeed, we present a study that does contain many other data, leading to interpretations that are the most plausible in our opinion, including the occurrence of prior calcite precipitation (PCP). The latter is indeed not supported by Mg/Ca and Sr/Ca data, because we don't have them. This absence of Mg/Ca and Sr/Ca data is however not a reason to a priori disregard the occurrence of prior calcite precipitation, as a possibly valuable explanation for certain observations based on several other data. Moreover, this interpretation is based on the observations made by some of us (Verheyden et al., 2014), and referred to in the Introduction, in a speleothem in the Père Noël cave, which is part of the same Hansur-Lesse cave system, in which the similarly varying isotopic ($\delta^{18}\text{O}$ and $\delta^{13}\text{C}$) and geochemical (Mg/Ca and Sr/Ca) proxies could be interpreted in terms of alternations of wetter and drier phases, causing changes between weaker or absent PCP and more intense PCP respectively. We added this argument to '5.2 Factors driving decadal and multi-decadal changes in the measured proxies in the text' of the new version of the manuscript. Mg/Ca and Sr/Ca- analyses would be among the first things to be done to verify certain of our interpretations.

In Table 1, the $^{230}\text{Th}/^{232}\text{Th}$ ratios should be given as activity ratios, not atomic ratios. Most readers will be able to tell at a glance that many of these samples are contaminated by detrital thorium if these ratios are given as activity ratios (e.g. is the activity

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ratio < 100 etc). As presented in Table 1, these numbers are difficult to convert to activity ratios unless the reader happens to have a calculator to hand and he/she happens to know the ratio of the decay constants of ^{230}Th and ^{232}Th . Help the reader – convert these ratios to activity ratios. The U/Th-ages are given as they are usually presented with their uncertainties and taking into account the possible contamination by detrital thorium. We think that the robustness of the age model we use is mainly in the careful combination of the two independent methods: layer counting and sufficient U/Th (20 in 56 cm or ca. 500 years), using StalAge.

Specific Comments and minor corrections: Abstract: The abstract is informative but is overly detailed and too long for an abstract. The purpose of the abstract is to convey the major findings of the study in a concise manner to the reader. Details of how the interpretations are supported can be omitted and discussed within the text of the manuscript itself. There are also a few typographical errors (e.g. upper case A is missing at the beginning of two sentences). We agree with this comment and reduced the length of the abstract.

P. 4153, line 5: 'To allow reconstruction of' instead of 'To allow reconstructing'-Adapted. P. 4154, line 3: 'in more detail', not 'more in detail'- Adapted. P. 4156, line 19: I suggest you use 'number of counted layer couplets' rather than 'amount of counted layer couplets'- Adapted. P. 4158, line 12: 'number of years' rather than 'amount of years'- Adapted. Similar comment on P. 4159, line 24.- Adapted. P. 4163, lines 14 and 15: The 'amount effect' causes $\delta^{18}\text{O}$ values to decrease during wetter periods, i.e. become more negative, (not increase as stated here). We agree with the reviewer that this sentence was not correct. However, to shorten the length of the manuscript, the discussion part was thoroughly rewritten and this sentence was removed from the manuscript. P. 4163, line 27: 'smaller scale' not 'smaller scale'. - To shorten the length of the manuscript, the discussion part was thoroughly rewritten and this sentence was removed from the manuscript. P. 4164, line 10: 'assuming' would be better than 'considering'. This is definitely an assumption. To shorten the length of the manuscript,

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the discussion part was thoroughly rewritten and this sentence was removed from the manuscript. P. 4165, line 4: 'large scale', not 'large scaled'. To shorten the length of the manuscript, the discussion part was thoroughly rewritten and this sentence was removed from the manuscript. P. 4171, line 8: The possibility of a lit fire on the Prosperine stalagmite does indeed suggest dry conditions, but they may have been short-lived, perhaps a season or two? I'm not convinced that this is necessarily evidence for drier conditions on decadal timescales, as implied here. The physical evidence that supports the presence of a lit fire on the Prosperine stalagmite in the past doesn't indeed suggest more than dry conditions for a short period of time, e.g. 'perhaps a season or two' at least. Apart from a maximum duration, that cannot have been longer than the estimated duration of the hiatus, we have no evidence for its duration neither for its continuous character. Therefor it cannot be used to support the duration of the drier conditions indeed. But the occurrence of the drier period, and its duration of decadal order, is derived from the stable isotope record. We only refer to the physical evidence of the presence of a lit fire, 'at some time' during this drier period, because it demonstrates that at least during a certain part within this period of time, the conditions on top of the stalagmite cannot have been too wet to lit a fire, as they are today, and most probably have been throughout the whole formation of the speleothem taking into account the generally high growth rate.

P. 4174, line 22: 'measured' not 'measure' - Adapted.

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

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