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Climate of the Past Discussions

Interactive comment on "Does Belgian Holocene speleothem records solar forcing and cold events?" *by* Mohammed Allan et al.

Anonymous Referee #1

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General comment:

This paper addresses an interesting question, namely whether Holocene climate variability was influenced by past changes in solar activity and if these fluctuations are recorded in a speleothem from Belgium.

In general, the paper is well written, the data are interesting and may have the potential to tackle this question. However, in its current form, I cannot recommend the paper for publication in CP. My main concern is that the discussion and interpretation of the presented trace element records is mainly based on correlation and time series analysis. The similarity of the individual trace element signals and their similarity with the d18O record is – in my opinion – not sufficient for a robust interpretation in terms of climate variability. The same is the case for the interpretation in terms of solar forcing. The

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simple observation of cycles with a similar period as solar variability may indicate a solar origin, but to in order to prove this relationship, the authors should at least present a hypothetical mechanism, how the trace element records could be influenced by solar variability. In other words, I would like to see a much more detailed discussion of the potential processes controlling the trace element and stable isotope signals of the speleothems.

A few more detailed points are listed below.

Detailed comments:

Section 2 provides a nice summary of the potential processes influencing speleothem trace element signals. This shows that the authors are aware of all these complicated processes. I strongly suggest that they discuss their records in terms of these processes, rather than just relying on correlations and frequency analysis.

Line 114 ff.: The stalagmite was dated in three different labs. In terms of comparability, it would be good to provide a bit more information on the methods used in the individual labs (spike calibration, decay constants, etc.).

Table 1: Please use points instead of commas for decimal separation. In addition, please provide uncertainties for U content and activity ratios. Why are no (230Th/234U) ratios provided for the three samples (NA)? The (230Th/232Th) is relatively low for some samples suggesting significant detrital Th. Have the ages been corrected for this? Is this reflected in the age uncertainties? Please provide more information.

Line 124ff.: Please provide more information on the construction of the age model. Which model was used?

Line 150 and throughout the MS: “Trace element results are closely correlated (r = 0.48-0.77 ...” Please do not provide ranges for r-values, but report the individual correlation coefficients for all records. This information could also be listed in a Table. In addition, how were r-values calculated in case of different temporal resolution of the records?

Interpolation? Please provide more information.

Line 158: Fig. 5 is mentioned prior to Fig. 4 in the text.

Line 163ff.: "The d18O isotopic composition of the calcite in the Père Noël cave is largely controlled by water availability (drip rate) in the cave. Changes in isotopic equilibrium conditions are driven by the changes in cave humidity and linked to changes in precipitation and evapo-transpiration (Verheyden et al., 2008)." Please provide more information on this. Of course, you do not have to repeat all the details if you refer to published work, but the interpretation of the d18O values is crucial for the interpretation of the trace elements. Thus, at least a short discussion of the major drivers of the d18O signal should be given here.

Line 175: "(r(Sr, Ba)=0.77, r(Mg, Sr)=0.72, r(Mg, Ba)=0.48)" This is much better (see above) and should be used throughout the paper.

Line 180ff.: "The positive correlations of Al with Mg, Ba and Sr (r =0.45) suggest that Al content is controlled by the same process than the other trace elements." This is the major problem of the whole paper in its current form. I agree that a correlation may suggest a similar process influencing all trace element records. However, whereas Mg, Sr and Ba are mainly transported as solutes in cave waters, Al is normally transported attached to particles and colloids and thus a proxy for detrital content. Therefore, the correlation of Mg, Sr and Ba with Al – in my opinion – suggests that Mg, Sr and Ba are not only incorporated into the speleothem in dissolved form, but also associated with detrital material. Actually, the positive correlation suggests that the signals are dominated by the content of detrital material. As a consequence, the interpretation of Mg, Sr and Ba in terms of past hydrological variability is complex. For instance, a similarity of these signals has often been interpreted as a result of PCP in the aquifer above the cave. High values would then reflect drier conditions. This could also be proposed for the PN stalagmite. However, high Al content reflecting a high content of detrital material (particles/colloids) would suggest higher flow rates in the aquifer and

thus wetter conditions, which would be contradictory. In summary, as outlined in my general comment, the authors must provide a much more detailed discussion of the processes affecting the trace element and stable isotope records. In the current form, all is based on correlations, which can be – as described above – misleading. In this context, it would also be good to see the d13C record if available. As trace elements are strongly influenced by processes in the soil and karst, the comparison with d13C values my provide important additional information.

Line 187ff.: "First spectral analysis demonstrates that trace element time-series contain significant periodicities ..." Maybe I missed that in the paper, but how was the significance of the periodicities determined?

Line 191 ff.: "Second trace element PN time-series are compared with available temperature record from Mekelermeer core in the Netherlands (Bohncke J., 1991) in order to evidence a regional temperature influence on the PN record." I do not understand why the records are compared with a temperature record if all proxies are interpreted in terms of precipitation. This is a general problem of the paper, in particular in section 5.3. If the records are believed to reflect past precipitation, they should (at least mainly) be discussed in this context. In section 5.3, however, several cold events, such as the 8.2ka event, are discussed as well. Please be more precise here. In addition, it would be better to compare with another record than a relatively old non peer-reviewed record from a PhD thesis.

Line 213ff.: "Since the changes of trace elements in the PN speleothem were formerly demonstrated as due to changes in recharge, i.e. effective precipitation, the study suggests that variations in solar activity may be a significant forcing on either the precipitation or on the soil activity or vegetational activity intensity." If trace elements reflect soil and/or vegetational activity, this could be expressed by a similarity with the d13C signal (see above). I really suggest to show and discuss d13C as well.

Line 221ff.: "The higher positive correlations between trace elements concentrations

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(r=0.5-0.9) observed during cold periods ...” See above. Please provide more information here. Which signals were correlated? Which periods were used? Why are cold periods studied if the trace elements reflect precipitation?

Line 234ff.: “The oldest part of the PN stalagmite, from 12.7 to about 12.1 ka BP, is characterized by the lowest contents of trace elements and low d18O, suggesting wet conditions (Fig.4).” Except for d18O (and maybe Ba), I do not see this. Mg and Sr are on a similar level as in many other phases of the Holocene.

Line 260ff.: “The covariance between geochemical proxies support, as explained in Verheyden et al. (2008), their interpretation in terms of dry-wet changes with higher trace elements and higher δ18O values linked to drier conditions.” I agree that d18O and the trace element signals show some similarity on the orbital to millennial scale. However, on shorter time scales, and in particular during some of the trace element peaks, d18O values show an opposite behaviour (low d18O/high trace elements, Fig. 4). This suggests that the relationship is not as obvious as proposed by the authors. Please provide a more detailed discussion.

Line 324ff.: “Based on trace element time-series we demonstrate that several events at 10.3, 9.3-9.5, around 8.2, 6.4-6.2, 4.7-4.5, and around 2.7 ka BP alternate with periods of relatively stable and wet/warmer climate.” Here and throughout the paper: How do you infer warm/cold, if the proxies reflect precipitation? This needs to be clarified, based on a detailed discussion of the processes influencing the proxy signals ...

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