

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

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This manuscript presents a new set of trace element data from a speleothem from Pere Noel's cave. The study is focused in addressing whether solar variability during the Holocene has been recorded in this speleothem.

Even though there is a potential for a very interesting study on trace element variability in speleothems during the Holocene, I cannot recommend this manuscript for publication in its current form.

General comments

My main concern is that the authors do not acknowledge the fact that correlation does not imply causation. The authors base their interpretations solely on correlations and

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wavelet analysis, without attempting to invoke the physical mechanisms controlling the observed trace element variability and its potential link with solar variability. Therefore, the scientific methods and assumptions used in this study do not support its main claim: “Our study (. . .) emphasizes that speleothem trace element profiles may be considered as a new solar activity proxy on decadal to centennial timescales over the Holocene” (L331-333).

Since I do not consider the author's interpretation to be robust, I will not comment on that for now. But I hope to be able to review a revised version of this manuscript at some point!

Specific comments

- The overall presentation of the paper is structured and clear. In this regard, I would only suggest to merge Figure 4 and 7, as most time-series are shown in both figures.
- The English is correct throughout the manuscript (I've listed some typos at the end of this review).
- The significance level of the correlations is not provided for any correlation mentioned throughout the manuscript and therefore the reader does not know whether these are significant or not.
- The authors do not use the age model uncertainties in any of their calculations. They are, however, assessing specific periodicities that may not be significant at all if a Monte Carlo approach is used to take include one of the few uncertainties in the speleothem records that we can quantify!
- No information is provided on how the age model has been constructed. This would be useful to understand the reason behind the asymmetric errors for some sections of the speleothem shown in Fig 2.
- Most of the interpretations are based on wavelet analyses, but information on how they've been constructed is only available in the supplementary data. This should be

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part of the main "Methods" section.

- It is not clear how have the authors resampled the trace element data at constant time interval to perform the frequency analyses (has it been with a simple mathematical two-point interpolation?). One needs to be very careful with how the interpolation is done as the reconstructed periodogram (spectral power versus frequency for the resampled series) may differ from the original one. And this would make the reconstructed series unsuitable for wavelet analysis (ie frequencies shown in the wavelet spectrum may be just an artefact, predominantly at high frequencies).

- Regarding the wavelet analyses shown in Fig.6, the authors mention frequencies that I do not manage to see in the plots. For example, in L189, "the temporal stability of those periodicities is confirmed by wavelet results". I cannot see any predominant band (redish colour) in the top three panels (the trace elements) lasting more than about a century over a millennial length record. Therefore, stability of those periodicities is not proven.

- Along the same lines as the comment above, the 1000-yr band mentioned in L269 is only present in Ba but not in Mg or Sr. At the moment the authors mention this band as if it was present in all panels but this difference deserves some discussion.

- Also regarding the wavelet analyses, the authors need to be careful not to interpret frequency signals that are outside of the cone of influence, as these may be under/overestimated. See for example, the discussion on the Early Holocene (section 5.3.1, c. L258 ) where bands outside of the cone are used in the discussion.

- Considering that the three comments below are the same, why do they mention different correlation coefficients? Also, please consider not repeating sentences this similar. "Statistically significant positive correlations ( $r(\text{Sr}, \text{Ba})=0.77$ ,  $r(\text{Mg}, \text{Sr})=0.72$ ,  $r(\text{Mg}, \text{Ba})=0.48$ ) are found between Sr, Ba and Mg over the Holocene period suggesting either a common or strongly related controlling process." (L175); "The higher positive correlations between trace elements concentrations ( $r=0.5-0.9$ ) observed during cold

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periods which also suggest common control factors." (L221); "The positive correlations ( $r=0.47-0.98$ ) were observed between trace elements concentrations during those periods which confirms common control factors." (in L259).

- Assuming that the three main trace elements are controlled by similar mechanisms (which most probably do), the following statements cannot be used to interpret dry conditions: "positive correlation ( $r=0.45-0.95$ ) was observed between element trace concentrations confirming dryer conditions" (in L290); "The highest positive correlation ( $r=0.78-0.97$ ) observed between trace element concentrations during this period confirms dry conditions" (in L301).

- The authors say that this is a "high-resolution" record, but no information is provided of the temporal resolution between samples. Moreover, "high-resolution" is a vague term that strongly depends on everyone's expertise and background so, I'd suggest the authors to refrain from using it unless extra information is given.

- I don't understand the following sentence: "I calculated consequently the analysis of 50 micrometers each 300-1000 micrometers interval corresponds to a sample of 1 to 3 years every 0.5 to 50 years" (L126). Please, rephrase.

- How would you explain the peak observed at c. 5750 ka BP (figure 4 and 7), which is higher than some of the other peaks interpreted on the basis of Bond's cycles? (see L221).

- "The larger diameter of the stalagmite compared to its mean diameter suggests that there is enough water to flow down on the flank of the stalagmite and to precipitate calcite" (L236), when? Is this observation enough to interpret a wetter period? Could that precipitation take places many years later?

- In the discussion about the Younger Dryas, the authors present "humid" and "warm" is if they were the same concept (which they're absolutely not!): "All proxies measured in in the PN stalagmite suggest that a humid period occurred between 12.7 and 12.1

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ka BP. This is in agreement with the warm climate during the early YD (12.9-12.15 ka BP) . . . found in lake sediments from. . .” (from L239 onwards).

- It would be helpful for the reader if the caption of Figure 6 explained the meaning of the y-axis as well as the cone of influence. The authors could consider adding an extra panel of Period vs Power for all series (Mg, Sr, Ba, temperature and Sunspot number). This would help comparing the periodicities of the records.

- L190 and L194: Wavelet analysis is mentioned here as being part of first and third points.

- L180: Figure 3 as well.

Grammatical errors / typos

L89: Treble instead of treble

L40: “. . .and/or. . ., and. . .” in a list of items. Please use and just between the last two.

L126: calculated instead of calculted

L144: ka BP instead of BP ka

L205: there are two commas together after et al.

L301: high instead of highest

Best wishes,

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