Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-47-RC2, 2020

Interactive comment on "A 4000-year long Late Holocene climate record from Hermes Cave (Peloponnese, Greece)" *by* Tobias Kluge et al.

Anonymous Referee #2

Received and published: 13 July 2020

Klunge et al present a new multiproxy speleothem record from Peloponnese ranging from ca. 0.8 to 4.7 ka with the aims to give new data on climate evolution to place societal and cultural evolution of this important part of the Mediterranean during Bronze and Iron age. Despite this ambitious target, the manuscript basically fails to obtain important insight on it, because the high clastic contamination produce a large spread of U/Th ages and associated error, and the signal of many proxies is not always pronounced. The authors are aware of this and at certain points of the manuscript tried to focus on a short interval close or corresponding to the so called 4.2 event. Also in this case the discussion is not able to focus on substantial new ideas on this period. So the general aspect of the manuscript is confused in the treatment of the data in the discussion and then in the abstract and the conclusion. First of all the manuscript needs a

complete reorganization in the aims and introduction. The present aims are good for a speleothems with better chronology and much better signal. Along the manuscript, we pass in a confounding way (some time with repetitions) between the description of the long-term record and the short term changes. These are some of the general comment I have. However, there are many points along the manuscript, which needs to be improved. I try to give some example below. Abstract Pag. 1 Line 15 the 3.2 even is mentioned, and in many figures is highlighted but along the text I never see a serious discussion on that. Moreover, the chronology of this interval is really poor. Pag. 1 Line 20 the record is reported continuous between 800 and 5300 ka differently from the conclusion Pag. 1 line 24 234U/238U: there is no particular discussion on this point along the text to be so relevant to be mentioned in the abstract (indeed is lacking in the conclusion). Introduction Pa.g 2 lines 14-15. I think we must be aware that speleothem can be precisely dated if clastic contamination is negligible. Pag. 2 lines 30-31 This the style of the manuscript a description of a short event and then a focus on long term trend. So, most of the introduction is not useful to justify this view.

Study Area Pag. 4 lines 2-5 The data from Nehme et al., 2019 and Bar-Matthews et al., 2003 cannot be used acritically for Peloponnese. They cannot be presented as valid data for your area. There is a general "paradigmatic" view on the interpretation of the d18O in the Mediterranean and I can agree this can be used for past reconstruction. Moreover, in your discussion you try to justify this view using also other proxies. I think this is a correct "qualitative" approach. In absence of regional-to-local convincing data on precipitation to show data from other sector is not good.

Material and Method

Pag. 4 line 10 "a soot layer. . ...can you please show the position in the figure 4. In the text there is no mention of thin section and just a brief description on the fabric would be useful also for the equilibrium conditions and to discuss if there are hiatuses. In some points it seems likely. Pag. 4 Line 17 ". . .where manually pre-treated to obtain pure carbonate. . ..." please can you explain more precisely?

Pag. 4 line 25. Clastic contamination can be also related to clastic-carbonate? I'm not an expert on U/Th measurements.

Results Pag. 6 line 24 "may be"? "is" better.

Pag. 7 lines 5-10. It Is unclear why you use two different Bayesian program and then choose one instead the other. Can you show both?

Pag. 7 line 14. "suggest" I understand what the authors want to say, considering the large error, but I prefer "indicate".

Pag. 7 line 19. Can you show this trend with a polynomial curve? Can you be statistically confident that this a trend or is just a visual impression? Please can you explain why in figure a different averaging is choose for Skala Marion. Can you show with a polynomial curve the trend described in the text of the Hermes Cave?

Pag. 8 Lines 5-8 "...the general correspondence of individual and average....." Considering there is only one single measure of cave temperature and the large T variability obtained using clumped isotopes and the associated error the conclusion would be: there is no secure conclusion. Discussion Pag. 8 lines 14-15. If the manuscript is focused on this interval this should declared since the introduction and the manuscript structure should be mostly different and most focused. But the general organization of the manuscript is not well done. There is no a clear focus. At the end what do the authors want solve? What do they have then solved? Pag. 8 lines 15-17. These two sentences are rather confounding. The chronological uncertainties are elevate for most of the record and not just on top. A detailed correlation with historical event is honestly not applicable (if we can exclude a brief interval). Indeed the second sentence is correct.

Pag. 8 lines 27-28. Once again I don't think to stress to this point is useful.

Pag. 8 lines 28-31. I don't think that the conclusion of Borsato et al. (2016) can be transported acritically from Alps to Peloponnese in a so strict sense without a general

monitoring program like the data presented by Borsato et al.

Pag. 9 lines 4-6. The ranges of values is quite large. In absence of more detailed local data many calculation are probably misleading.

Pag. 9 lines 15-16 The relationship reported by Bar-Matthews is very local, and it cannot used for Pelopennese.

Pag. 9 lines 20-24 this is one of the few point where other short term oscillation are considered.

Section 5.2 There are a lot of literature on 4.2 event, but the discussion proposed did not add relevant points. Line 28 Rousseau et al., 2019 is not a paper. So difficult to quote.

Pag. 12 lines 23-27 here there are some sentences and concept repetitions. Once again, I think is quite misleading to use the correlation defined for far areas.

Pag. 14, lines 14-16. It is hard to say that Mavri Trypa provides a similar climate picture.

Pag. 14, lines 20-21. "Furthermore, both records show a high degree of consistency in medium and hig-frequency fluctuation." Absolutely not. This is an overexploitation of the data. Moreover, there is a mention to high-frequency oscillations which have not been discussed in detail along the manuscript.

Pag. 14 lines 22-30. In some part of the manuscript the correlation with Lake Stymphalia is presented as strategic for the general interpretation. There are no proxy records show for this lake and the "comparision of the trends....is difficul". There are other lakes cited but the record are not shown. This section seems quite useless.

Pag. 14, lines 1-13 There is a discussion of records which are not show in any figure, so the comparison is difficult.

5.3 implications

Pag. 14 lines 23-24. This point appears here for the first time and there is no any discussion. This section is not “implication” but already a summary of the main result, some not discussed at all, like the list of drier events reported as last point at pag. 15.

Conclusion

Pag. 14 line 14. Where along the manuscript do emerge that there is a cooling trend? The introduction promise some conclusion related to social evolution and climate, but then?

Overall, I consider the manuscript not suitable for publication even if the data can have some interest. I suggest to change the target of the manuscript, basically deciding which is the main focus and what wants to solve really and not what would be interesting to solve. The chronology is relatively poor so, an honest and calibrated manuscript is necessary and in this case, for me, welcome.

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