

Reply to anonymous referee #2

In the following we reply to the main point of criticism. Minor aspects (e.g., related to language and grammar) will be directly considered in a revised version. Referee comments are in italic and grey, the author response in black

Kluge 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 comparison to other speleothems chronologies from the Peloponnese and Greece shows that the Hermes Cave stalagmite has age uncertainties comparable to other records from the region. For example, the stalagmite chronology of Finné et al. (2014) from Kapsia Cave (Peloponnese) has age uncertainties of ca. 170 to 1600 a. Depending on the detrital contribution lower age uncertainties on the order of a few decades may be achieved for the Holocene (e.g., Finne et al., 2017). This is also visible for GH17-04, for which the 4.2 ka event was dated with reasonable uncertainties for low-uranium speleothems of 70-130 a.
- Additional U/Th analyses will further constrain the chronology. We'll address in particular the oldest and the youngest part of the stalagmite
- The proxy signals reflect the high alpine environment and are therefore potentially attenuated regarding rainfall amount variations and, based on the clumped isotope results, not enhanced by kinetic effects

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.

- The most pronounced and clear signal of the whole d18O record occurs around the 4.2 ka event. In the light of the ongoing discussion on the nature of the 4.2 ka event and the previous special issue on this topic in *Climate of the Past* we deem it important to add and discuss related data that is unique for Greece
- A more coherent discussion with adjusted abstract and conclusion will be prepared

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 generalcomment I have. However, there are many points along the manuscript, which needs to be improved. I try to give some example below.

- We'll improve the chronology by additional radiometric analyses and check if a statistical robust correlation is possible with other, better dated stalagmite records from the Peloponnese or Greece in general
- The discussion will be oriented at the revisited uncertainty of the chronology and correspondingly restructured and rephrased

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.

- We remove the reference to this event and the highlighting in the figure

Pag. 1 Line 20 the record is reported continuous between 800 and 5300 ka differently from the conclusion

- Typo to be corrected

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).

- Will be removed from the abstract

Introduction Pa.g 2 lines 14-15. I think we must be aware that speleothems can be precisely dated if clastic contamination is negligible.

- We add the restriction that the absence of significant detrital contamination is an important condition for the possible high-precision radiometric dating of speleothems

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.

- We restructure and rephrase introduction and discussion accordingly to match the description of events on various durations

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.

- We embedded our presentation and discussion of the oxygen isotope ratios in rainfall in the larger regional context and also cite the IAEA-WMO data base. In the supplementary we show recent isotope data of Athens (IAEA-WMO) that also indicates a rainfall amount-d18O relationship for Athens. The relatively close proximity of the Peloponnese to Athens (about 100 km to Hermes Cave) justifies its use for interpretation at the cave side
- The negative d18O-rainfall relationship holds for most of the Eastern Mediterranean (s. Nehme et al., 2019; Bar-Matthews et al., 2003; IAEA-WMO), but should be investigated also for the cave site. As we are missing local hydrological and long-term isotopic data for the Hermes Cave site we'll amend the text by a critical assessment

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.

- We'll highlight the soot layer (already visible as blackish layer at about 15mm from top) and add a photograph of the thin section, including discussion of fabrics and a discussion on possible hiatus

Pag. 4 Line 17 “: : where manually pre-treated to obtain pure carbonate: : ..” please can you explain more precisely?

- To be deleted (the speleothem carbonate is already pure)

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

- Contamination of clastic carbonates is in general possible but unlikely for speleothems. Detrital contaminants are flushed in with the drip water and typically relates to clay minerals.

Results Pag. 6 line 24 “may be”? “is” better.

- Will be changed as suggested

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

- Already shown in supplementary figure S5
- Depending on the algorithms implemented in the program, slight differences in the chronologies may occur

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

- Will be changed as suggested

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

- We’ll add a supplementary figure focusing on the long-term trends with a statistical discussion and polynomial curves

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.

- Clumped isotopes are in general very sensitive to disequilibrium (references in the manuscript). Temperature offsets in case of disequilibrium could be on the order of 10°C and up to 40°C and are therefore clearly detectable.
- Measured clumped Δ_{47} values scatter and have partially elevated uncertainties. Calculated temperatures are, however, with one exception all within the range of modern cave temperatures (note that there has been another instrumental cave temperature measurement in 1996, see response to ref.1). The average temperature of all clumped analyses over the speleothem growth period is $8.5 \pm 1.4^\circ\text{C}$ (n=8, 1SE), well corresponding to the modern temperature measurements. Considering the small uncertainty of the average value of the full data set, the correspondence between D47-based temperature and cave T suggest no or negligible influence of disequilibrium

Discussion Pag. 8 lines 14-15. If the manuscript is focused on this interval this should be 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 clear focus. At the end what do the authors want to solve? What do they have then solved?

- Speleothem proxy signals provide an insight into paleoclimate and environmental changes. Depending on the chronology and the time resolution the short term signals can be discussed (around 4.2 ka, reasonably well constrained chronology) or the long-term evolution investigated (rest of the record)
- We provide continuous information on paleoclimatic changes for about 4 ka during the most interesting period of cultural evolution in Greece, complement existing speleothem records both temporally and spatially and add unique and detailed information on the 4.2 ka event in this region
- We’ll reorganize the structure and clearly outline the focus of our discussion

Pag. 8 lines 15-17. These two sentences are rather confounding. The chronological uncertainties are elevated or most of the record and not just on top. A detailed correlation with historical events is honestly not applicable (if we can exclude a brief interval). Indeed the second sentence is correct.

- will be rephrased

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

- it is a very important aspect for the interpretation and discussion of the $\delta^{13}\text{C}$ and $\delta^{18}\text{O}$ data. Furthermore, it impacts also the discussion of elemental ratios as PCP can have a major influence (e.g., on Mg/Ca ratios). Indications for the absence of PCP constrains the remaining causes and allows a more robust discussion.

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.

- we'll add a note of caution and rephrase the corresponding sentences

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

- We are intending to amend our data set with drip water samples from the Hermes cave site.

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

- see response regarding "study area" above

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

- the discussion will be restructured to better represent the main topics of the manuscript and to focus the content

Section 5.2 There are a lot of literature on 4.2 event, but the discussion proposed did not add relevant points.

- Absolute dated and reasonably well-resolved signals of the 4.2 ka event are rare. For Greece and the Peloponnese a corresponding signal has not yet been reported and is therefore a novelty.

Line 28 Rousseau et al., 2019 is not a paper. So difficult to quote.

- removed

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.

- based on the IAEA data the rainfall amount-d18O relationship has been observed throughout the Eastern Mediterranean and is also visible for Athens. See supplementary figure S1. We'll rephrase the text to clarify the relationship and to indicate that it does not only hold for the Levant.

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

- We'll be more specific and clearly indicate in detail similarities and discrepancies.

Pag. 14, lines 20-21. "Furthermore, both records show a high degree of consistency in medium and high-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.

- The data of Alepotrypa Cave are so far only available as a thesis (Boyd, 2015). We'll therefore remove the comparison. Note, however, that a comparison of both records shows indeed a high degree of overlap for the longterm trends and even higher frequency fluctuations (allowing for wiggle matching within the dating uncertainty).

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.

- The revised version will be complemented by a figure with direct comparison of the Hermes Cave proxy record with the other discussed lake records

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

- We'll complement the figures with all records that are discussed in the text

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.

- we'll restructure this section, add a discussion of all points that were not addressed before and move the list of drier periods to the conclusion

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?

- Reference to cooling trend will be removed
- we'll add an extra paragraph on the inter-relation between social evolution and climate and what our study could contribute to this topic

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

- we are sorry to hear that the manuscript in its current form is not ready for publication but are grateful for the detailed suggestions on possible improvements