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Climate of the Past Discussions

Interactive comment on "Diatom-oxygen isotopic record from high-altitude Petit Lake (2200 m a.s.l) in the Mediterranean Alps: shedding light on a climatic pulse" *by* Rosine Cartier et al.

Anonymous Referee #1

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This paper presents new data from ∼4.2ka from a part of the world where data from this time are lacking. The data collection (e.g. purification of diatom samples) and the treatment of uncertainty in the age model is thorough and allows us to have more confidence in the results. The introduction presents a good hypothesis based on the previously produced proxies to test in this paper with the d18Odiatom data.

However, my main concern is regarding the interpretation of the (slight) increase in d18Odiatom around 4.2ka as primarily a water balance signal (i.e. indicating a shift to drier conditions). If the isotope shift were of a higher magnitude, then it would be possible to have more certainty in the interpretation. But, and the authors acknowledge

this at points, a change in precipitation source could also account for some of the isotope shift. In fact, potentially it could account for all of the isotope shift. Also, a decrease in snow (with its very low d18O) around 4.2ka with everything else staying the same could account for the d18Odiatom rise. While your argument about temperature not being the main driver if valid, more thought and caution needs to go in to your interpretation. There are only a couple of modern day lake water isotope values, but even the summer one is fairly low, so how evaporatively-driven is the isotope system? While I agree that something definitely happened in the lake ∼4.2ka as the d18Odiatom changes are outside of uncertainty and other proxies show changes too, with so few d18Odiatom data points and the relatively small magnitude of the change means it is difficult to unequivocally say that a change to drier conditions, rather than a change in precipitation source, or decrease in snow, or a combination of these factors, was responsible for the d18Odiatom change.

Therefore, I think the argument of the driver(s) of d18Odiatom needs to be more cautious and more thought through.

Nevertheless, this is a valuable new dataset that is robustly analysed and adds to our knowledge of what was going on in the Mediterranean region around 4.2ka, so I support its publication if my points are addressed.

Specific comments: First line of abstract: Holocene not holocene Page 2 line 5: discussion not discussions Page 2 line 13: change to "In the Central Mediterranean. . ." Good summary of previous literature. In addition to Walker et al. 2012, could now reference the new Holocene subdivisions brought in this year. Detailed purification strategy and XRF check to ensure contamination removed. Good SEM work to check diatoms not dissolved. Good, thorough treatment of uncertainty with age model. Page 6 line 7 ". . .temperature and d18Olakewater. . ." rather than "temperature and hydrological balance as reflected by d18Olakewater. . ." Page 7 line 5 "does not" not "doesn't" Good discussion of difficulties of comparing local and climate signals in other records from the region from 4.2ka time. Diatom samples look clean as no correlation between

d18Odiatom and %contamination.

In answer to the review criteria questions: 1. Yes 2. Yes, new data 3. Yes, although need to be more cautious about whether evaporation is the major driver of d18Odiatom. 4. Yes 5. Yes 6. Yes 7. Yes 8. Yes, although I'm not sure of the term 'climatic pulse'. Something like 'event' or 'interval' might be better. 9. Yes 10. Yes 11. Mostly 12. Yes 13. Yes – see my comments on d18Odiatom interpretation 14. Yes 15. Yes

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