Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-35-RC2, 2018

Climate of the Past Discussions
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Interactive comment on "Vegetation and geochemical responses to Holocene rapid climate change in Sierra Nevada (SE Iberia): The Laguna Hondera record" *by* Jose Manuel Mesa-Fernández et al.

Anonymous Referee #2

Received and published: 15 July 2018

Dear authors and editor:

I have thoroughly red with much interest the excellent paper of Jose Manuel Mesa-Fernández and his colleagues. The paper deals with climate and environmental reconstruction based on a core record from Laguna Hondera, a small lake located at ~2900 m above mean sea level in Sierra Nevada (southern Spain). Using a series of parameters and proxies, the authors utilize the sediment record to reconstruct ~9000 years of both regional climate and environmental variability, while concurrently emphasize the impact of long distance dust transportation from the Sahara into the lake region.

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I find the paper extremely interesting and timely for publication. The authors indeed point to highly important conclusions that are relevant to the scope set by the CP journal and of interest for the wide-scale paleoclimate scientific community. Yet, I believe that some points need to be better emphasized or explained in order to strengthen the article and reach the high-impact level as requested by the journal.

Below these lines I portray the main concerns:

1) Chronology and possible presence of hiatuses and/or unconformities in the sedimentary record. The authors do not consider at any step of the article the possibility that the record might not be complete or continuous. Considering the small-scale lake morphology (which unfortunately information on size and depth are missing), I believe that these issues definitively need to be taken into consideration. Furthermore, the authors state that when coring procedures took place, the lake size was even smaller than in previous years, testifying for the great variability such a lake system may have suffered in the past. Regarding the sedimentological record, just by looking at the lithological changes (Figs. 2 and 4) I estimate that depths characterized by sharp lithological variabilities (and that are associated with abrupt petrophysical and geochemical variations; e.g., at ca. 34 cm depth), may also result from interrupted sedimentation. I also think that the age/depth model in figure 2 is misleading as the dots in the graph does not necessary needs to be connected. Moreover, if you decide that the curve should be shown as is, so definitively a plus/minus range (such as a strip in the entire plot) should be given as well.

2) The suggestion that Pb/Al is a reliable recorder for human impact in the region. I sincerely do not see any important or drastic change in this parameter that can be connected with human impact, especially not in the suggested age of ~2800 cal yr BP (Fig. 8). The authors suggest that anthropogenic impact in the environment can be identified at this time, but this assumption is definitively not sustained by the data. There is only a single peak occurring at this time and not a trend towards increase values, as should be expected. In my opinion, the change on those elements are a

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direct result of a local variability in the source watershed, which can also be appointed (in this case) to natural causes. Yet, I totally agree with the trend identified for the younger sedimentary sequence (~150 years BP). The authors indeed explain the possible dilution of Pb in the LH record as a consequence of an increase catchment area and more humid conditions and they rely the assumptions solely on comparison with a nearby record (LdRS). I suggest the authors to abstain of reaching such a concise statement only by comparing with a single site, and neglecting that their own data does not strongly support those evidence.

3) Saharan dust. The idea is definitively well presented and well discussed against datasets from the African continent (although some references are missing, such as Krueger et al. Atmospheric Environment 38, no. 36 (2004): 6253-6261). Yet, I argue that the increase in Ca in their record (Fig. 7) does not necessary need to only imply dust coming from the Sahara. I suggest the authors to also consider the possibility that during increase intervals of Ca/Al or Ca/Ti (especially ~3300-2500 cal yrs ago, according to their figure), Ca elements could derive from exposed areas of continental shelves in southern Spain or Northern Africa. Those regions will probably include much greater amount of Ca, when compared with datasets from the Sahara. Please refer to the necessary literature for discussing this issue.

I have made further corrections directly on the PDF associated with this letter.

In light of the major comments listed above and those directly written on the PDF, I recommend the authors to carry out a comprehensive re-structuralization of their paper prior to further acceptance by the editor.

Kind regards,

Reviewer #2

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-35/cp-2018-35-RC2-supplement.pdf

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-35, 2018.