

Clim. Past Discuss., referee comment RC3
<https://doi.org/10.5194/cp-2021-137-RC3>, 2021
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Comment on cp-2021-137

Anonymous Referee #3

Referee comment on "Humidity changes and possible forcing mechanisms over the last millennium in arid Central Asia" by Shengnan Feng et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-137-RC3>, 2021

This is an interesting case study dealing with the reconstruction of paleoclimatic change in arid Central Asia (ACA) over the past millennium, based on a new lacustrine archive from Lake Dalongchi (Tien Shan). The main objective of this study is to decipher the respective contributions of internal and external driving mechanisms of hydroclimatic variability in ACA, which may help to better understand the chain of reactions involved and better constrain future climate model simulations. If the topic is of interest, I find that the manuscript (in its present state) has several flaws, which require some further consideration and development (especially in the Results and Discussion) before it could be accepted for publication. Hence I would recommend major revisions, and I would like to see a revised version of the manuscript before final acceptance.

Major comments and concerns:

Lines 38-40: Some key references are missing in the state-of-the-art of the Introduction.

See for instance the recent paleolimnological contribution of Rousseau et al., 2020 (<https://doi.org/10.1016/j.palaeo.2020.109987>) depicting the sequence of glacier fluctuations and associated palaeoclimatic changes over the past millennium in the Tien Shan mountains. See also Zhang et al., 2009; doi: 10.1029/2009gl037375.

Furthermore, other recognized contributions focusing on hydroclimatic changes in ACA during the Holocene are curiously eluded in the Introduction, although they are crucial in understanding the mechanisms at work at decadal to centennial and longer timescales, (e.g., amongst others Mathis et al., 2014; Lauterbach et al., 2014; Huang et al., 2014, Schwarz et al., 2017 and more recently Sorrel et al., 2021).

Hence I feel that the introduction lacks, in particular, a concise but general overview focusing on humidity changes in ACA during the Holocene, as a brief introduction of the mechanisms controlling hydroclimate changes in this region. Here, a couple of key references are therefore required in the revised version.

Lines 42-43 : Can you be more precise and detail what is involved behind the general statement "*internal climate variability*"? Very imprecise. This is important as you build most of your Discussion on this issue..

Lines 60-62: As evaporation clearly predominates on precipitation (rain, snow) and riverine inputs in the annual hydrological budget of Lake Dalongchi, are there available information regarding the groundwater contribution on the hydrological balance (which could be very high in such lacustrine systems)?

Lines 66-67: Do you have more clues about which "*shrubs*" predominate on the northern slope? Which species precisely?

Line 66: Correct "*southern*" and "*western*"

Line 67: Correct "*northern*"

Results, Chronology, lines 107-113: There is no description provided for core lithology. Are there some hiatus identified in the studied core? I see on Figure 2 that part of the core is laminated, while other intervals look more homogeneous. Hence changes in sedimentation rates should be expected over the past millennium. This is of importance because the authors state (in the Abstract, in the Introduction and in the Conclusions, but never in the main part of the text, why not developed any further in the Results?) that their age model has a very high and constant resolution of ca. 1,8 year (!). Hence some more detail regarding lithological change and sedimentation rates should be provided, and developed, in this chapter.

Line 126: "... *as has a steep headwall*": More detail should be provided in the study site. By the way, we are not provided with any clue regarding the geological setting of the formations surrounding the lake, in particular in the catchment from which most of the inputs originate. Please provide some more emphasis on this.

Line 130: There must be a mistake here: the distance between the catchment and the lakeshore should be lower during highstands (compared to lowstands). Correct it.

Lines 133-134: This statement is not really convincing as one could expect higher riverine inputs (and thus higher magnetic susceptibility or MS, high silt content and higher C/N ratios) during more humid intervals (rather than during dry intervals). What do the pollen say at the local and regional scale? Are there existing and available palynological data, which would favour one of those two hypotheses?

Lines 139-140: What is the impact of wind activity around the lake? Is it possible to discriminate between aeolian and riverine inputs in the clastic fraction? Any smear slide analysis performed to check the shape/texture of minerogenic grains? By the way, what is the grain mineralogy of "exogenous materials"? Quartz? Oxides? Else? More information should definitely be provided in this section (and thus also ahead in the Study site section regarding the catchment). Besides, XRF data would have been of help to identify grain size variations, possible sources and discuss changes in clastic inputs over time. Any possibility to add such a dataset in a revised manuscript?

Figure 4: Please add the impact of wind processes on the two cartoons, i.e., during highstands and lowstands.

Lines 143-145: I doubt that the relationship/consistency between the reconstructed HI and the instrumental relative humidity can be used that straight, since the correlation (as well as the R^2 ; $R^2=0.298$) are rather low. This statement should thus be tempered down.

Lines 145-149: Here the results should be compared with those recently published by Rousseau et al., 2020 (Palaeo3) (<https://doi.org/10.1016/j.palaeo.2020.109987>) relying on a proglacial lake from the Tien Shan mountains in Kyrgyzstan over the past millennium. Such a comparison should also be integrated on Fig. 6.

Line 161: Replace "during in MWP to LIA" into "between the MWP and the LIA".

Figure 5: This figure (vs age) is finally very similar to Figure 4 (vs depth). Perhaps you could highlight more clearly (with vertical bands) the most important timeslices for the Discussion (driest / humid intervals) on Figure 5.

Lines 162-166: The authors shortly state the discrepancies between the HI index developed in this study and other humidity records in ACA, but strangely do not provide any reason for it. Then, how would you explain such discrepancies between the different ACA records? We are in the Discussion; hence this should at least be developed *a minima* (and I would not expect the respective age models to account for the differences observed). Very important.

Figure 6: Where is the Badain Jaran locality? Sugan Lake? Lake Gahai? Which country in ACA? This should occur on Figure 1, or on a separate panel in Figure 6.

Line 176: Here it is stated that periodicities of coherence occur from 88 to 146 years, although 88 to 157 years are quoted line 168. Please clarify it.

Lines 188-194: Here again we are in the Discussion, not in the Introduction. This paragraph is in fact almost devoid of any information, as we are only provided with very general statements mentioning that a solar forcing was also involved in other regional records, but without providing any clues regarding the chain of reactions and/or the mechanisms at work behind (at least an attempt could have been done). Such a relationship between solar activity and lake proxies has been long reported in the literature over the past 30 years, but we do not learn much more here. This part of the manuscript would deserve a more in-deep discussion and some more development.

Lines 196-219: Same comment here regarding the link between the HI and the ENSO. Even if wavelet analyses suggest a negative relationship between the HI and ENSO, this is however tricky to see any kind of correlation (or anticorrelation) between the two datasets. At least, kind of a correlation could be observed after 1800 AD, but interestingly

not before. How would you account for that? But, basically, I am puzzled again by the fact that we do not learn much more at the end of the manuscript that what has been widely elsewhere in the literature, especially regarding the driving mechanisms of hydroclimatic variability in ACA during the late Holocene. Hence I would recommend to revise the Discussion by bringing a far stronger case on proxy correlation between the different regional records presented in Figure 6, as when tackling the possible mechanisms at work controlling climate variability over the timespan studied.