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Climate of the Past Discussions

Interactive comment on "Late Pleistocene to Holocene climate and limnological changes at Lake Karakul (Pamir Mountains, Tajikistan)" *by* Liv Heinecke et al.

Anonymous Referee #3

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General comments:

This is an interesting case study dealing with the reconstruction of palaeoenvironmental change over the course of the Pleistocene and Holocene in Central Asia, high in the Pamir Mountains. The main objective of the study is to provide new data (e.g., sedimentological and geochemical data) aiming at deciphering the contribution of lake external and lake internal controls on lake hydrology, in order to evaluate past dominant trends of atmospheric circulation systems regionally. The study is in general appealing; however the paper is rather long and the general structure of the manuscript should definitely be revised before further decision can be reached. In particular, I am concerned that most of the Results/Interpretations are developed in the Discussion

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chapter, not in the Results chapter. This leads to a far too long discussion (9 pages!) in which results, interpretations and general discussion are processed together, and this renders the manuscript difficult to read. The quality of the figures is in general acceptable, although Figure 3 is not legible at all and would benefit from a complete redrawing. I am also concerned that no synthetic figure is provided in the Discussion, although I am pretty convinced that a graphical comparison of proxy data for Central Asia (including Pamir and Central Tien Shan, in particular) would definitely be helpful for the reader. In doing so, besides making the discussion more accessible to the reader, it would definitely help to build a bigger picture for the data presented in this study. Finally, and above all, I have several important reservations regarding most of the interpretations, (i) which are for some of them not supported by the data (see below the specific remarks) and (ii) to my impression most often over-simplistic.

For these reasons, and in particular because some of the interpretations are not supported by the data, I would not recommend this study to be published in Climate of the Past. However I leave this decision to the editorial board, who should appreciate the other reviewers' comments and recommendations.

Specific remarks:

Introduction : Page 1, Lines 14-17 : There would be many more focused literature focusing on western Central Asia (and high-altitude lakes) for references here.

Page 1, Lines 21-23: Very puzzling that so few literature regarding the Tien Shan Mountains is referred to in the introduction in general. We strongly encourage the authors to update their reference list in this regard.

Materials and Methods: Why no description of the lithology is provided in the manuscript ?

Page 4, Line 21 : Please define CNS here.

Page 4, lines 25-28 : If the material consists of an in-situ living charophyte collected in

April 2012 (as shown in Table 1), it should be kept in mind that the material most likely suffered severe bias due to nuclear bomb testing, with apparent 14C ages showing too young ages. Ideally, the correction of lake reservoir effect in Lake KaraKul should be determined using 'pre-bomb' water organisms for correct interpretation of Holocene radiocarbon dates. If this is not possible, the authors should explain more convincingly how the apparent ages obtained on living charophytes may be used to determine apparent lake reservoir effects, as it was discussed in Mischke et al. (2010a) based on another modern charophyte. As it occurs in the present version of the manuscript, it is simply not possible to rely on the proposed lake reservoir effect estimation of 1315 years, as crucial information is lacking to support this value. Apart from this, it would have been of interest to use 210Pb/137Cs dating to get a refined chronology in the topmost part of the core.

Further, the working hypothesis assuming that the lake reservoir effect did not change through time is quite puzzling : in such a deep (and likely stratified) lake, in which inherited topography (and lake chemistry) certainly plays an important role in determining local sequestration of old carbon, it is all the more likely that lake reservoir age change over time, and especially at the transition between the Pleistocene and the Holocene. More radiocarbon data would be needed to tackle this important issue.

Page 4, line 25 : Please correct Mischke et al., 2010b into Mischke et al., 2010a.

Page 5, lines 4-5 : Please revise this sentence (a verb is missing).

Page 5, line 17 : Please correct carbonat into carbonates.

Page 6, lines 28-29 : The obtained proxies. . ... and explain the recorded signals. The authors should explicit, in detail, how they have discriminated between lake-internal and lake-external parameters. We are not provided with such development so far, and therefore can not evaluate if the attribution of one proxy to lake-internal or lake-external paramaters is sound or not.

Results : Should this Chapter be re-entitled Results and Interpretation ? This would definitely allow to lighten significantly the Discussion (which is so hard to read..), in which results, interpretations and discussion are totally overlapping. One of my main concerns is that the Discussion contains a large number of results and interpretative developments, which should occur in the Results chapter and not be duplicated/developed elsewhere.

Page 7, line 3: Two in-situ collected living charophytes ? The authors mentioned only one in Chapter 3.2.1 and in Table 1. Please check for homogeneity and clarify it. See also our comment provided above (Page 4, lines 25-28) and elaborate on that.

Page 8, line 11 : The authors should elaborate on the choice to use the Sr/Rb and Zr/Rb ratios, in particular. If those proxies are indeed sound, we should be at least provided with some detail explaining why these ratios are meaningful for the interpretation of (palaeo-) environmental and climatic parameters.

Pages 8-9, lines 26-31 ; 1-7 : Results from statistical (e.g., stratigraphically constrained cluster analysis) data treatment reveal a different division of core KK 12-1 for lake-internal and lake-external parameters. In particular, zones 1 and 2 have different boundaries : 13.3 cal kyr BP for zones 1-2 in lake-external parameters and 19.2-17.5 cal kyr BP for zones 1-2 in lake-internal parameters. How could we explain that both set of parameters would behave differently, and thus responding separatly to overall/regional environmental change ? For instance, how do we know that the organic matter (e.g., TOC) in Lake Karakul is predominantly produced in situ within the lake, and not exported from the catchment and/or soils from the shores (thus reflecting precipitation events through sheetwash) ? The authors should elaborate on that issue.

Discussion: Page 9 : Most of the data discussed here should be treated in the Results chapter, not in the Discussion. I would suggest that the Results chapter is re-entitled Results and interpretations, which would allow to lighten the Discussion chapter and focus on a more integrated discussion. Hence most of the data interpretation pro-

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vided in this chapter (including the successive sub-chapters of the Discussion) should be moved to the previous Results and interpretation chapter, for the sake of clarity, consistency and legibility.

Page 9, lines 20-21 : The combined. . ..in Lake Karakul. One can not rule out the fact that part (or even, a predominant contribution !) of the organic matter is delivered to the lake through run-off and/or sheetwash. In that case, elevated input of terrestrial TOC in Lake Karakul would drive TOC/TN to higher values during the last 7 cal kyr BP. Please clarify on this.

Page 10, lines 13-16 : If I overally agree that the clay fraction (EM1) MAY well be aeolian in origin, one can definitely not rule out the possibility that this very fine-grained fraction might correspond to glacial by-products delivered to the lake through run-off activity. How could the authors argue on that ? Would there be any proxy allowing to discriminate aeolian vs. glacial (i.e., catchment) sources ? Please elaborate on that at that stage since (i) a significant part of the following discussion relies on the interpretation of grain-size fractions and (ii) that the possibility that this fraction consist of a mixing of (totally) different sources may impact on the general picture of (palaeo-) environmental anc climatic change. At that stage, I am not convinced that the interpretation of a (sole) aeolian origin of the clay fraction is supported by the data, all the more that no modern data (see in Fig. 4B) seem to confirm such an sssumption.

Page 10, lines 16-18 : How does this reference add on and is related to the present dataset/study case ? This is purely speculative to me.

Page 10, line 19-21 : This fact is certainly of great importance for the interpretation of grain-size data (see also the comment above). However, it is totally under-estimated in the rest of the discussion, and I would recommend to strongly keep this fact in mind before conducting to over-interpretation of the data. . .

Page 10, line 21 : EM3 covers a wide grain-size range similar to the reference samples of fluvial deposits. I would also add that EM3 is also represented in the modern pond and slack water silt modern samples (in addition to the former lake sediment sample).

Page 10, lines 23-25 : Here I strongly disagree with the attribution of local, summer precipitation to the Asian Monsoon. Please provide accurate references showing that summer precipitation events in the Pamir are mechanistically related to summer Asian Monsoon forcings. Why summer precipitation signals would not be controlled by the Westerlies ? If we rely on Aizen et al. 1995 ; 2001 (for instance) it is emphasized that most of precipitation signals in summertime result from cold air moist air fluxes from the west in Central Asian, in part due to the merging of northern and subtropical jet streams leading to heavy summer precipitation. Interestingly, Aizen et al. (2001) document that Pamir experienced most of its precipitation values during winter, but not in summer as it is stated in the present manuscript. I would thus recommend that the authors clarify on that crucial issue. Please also provide an ombrothermic diagram from nearest meteorological stations of Lake Karakul. Above all, it is encouraged not to attribute one grain-size fraction (such as EM1 and EM3 for instance) to one single mechanistic origin and/or over-simplified mechanism, when the situation is in fact much more complex. Hence I do not understand why the authors attribute EM3 fraction to a summer precipitation signal. This point, in particular, would be worth investigating in a far more convincing and reliable way. This is partly considered in lines 24-26 (in considering a much more complex pattern for EM3), but the interpretation/conclusions for EM3 remain untouched, and over-simplified, afterwards. Please elaborate on that.

Page 10, lines 26-31 : Same problem here for the EM4 fraction. I agree that EM4 shows similarities with a modern reference sample of well-sorted and coarse aeolian sand. However, here again, one can not rule out, especially in such high-altitude and glacial settings, the influence of coarse, local, inputs linked to meltwater run-off and/or glacial by-products. This is for instance the case in another lake setting from Central Tien Shan (Lake Son Kul), but no comparaison with that lake was undertaken in this study. What is the equilibrium line altitude of glaciers (ELA) in the vicinity of Lake Kara Kul today ? During the Little Ice Age (LIA) ? During the Pleistocene ? This is however an important issue, not considered so far.

Page 10-11 (XRF data) : Here again this part should rather be included in the Results chapter, rather than in the Discussion... Please change it accordingly (see also my previous comment).

Page 11, lines 5-6 : I am not sure if this anti-correlation is obvious in Zone 1 (I would say no correlation at all in that zone). This is also very difficult to evaluate it in Zone 2, as very few data are available in this zone. Please be careful not to over-simplify and over-interpret data and trends, in general.

Page 11, lines 15-18 : The explicited choice for using Sr/Rb and Zr/Rb ratios should occur much earlier, in the Results Chapter, not in the Discussion !

Page 11, lines 26-29 : If the PCA biplots indeed show obvious opposite relationships between EM1/EM2 and EM3/EM4, I would stick with the interpretation that such opposite trends most likely reflect different grain-size fractions delivered to the lake, and this is further supported by the correlation between grain-size fractions and XRF data. However, as also explained previously, I do not see any reason to attribute in a simplistic way EM1/EM2 to airborne-derived material and EM3/EM4 to fluvial/precipitation signals (and in particular EM3 to a summer proxy and EM4 to a winter proxy). For instance, EM4 plots with proxies for fluvial minerogenics, and I here again wonder whether this fraction may not be related to coarse fluvial delivery during seasonal runoff (in spite of the fact that the modern reference aeolian sand shows similarities with EM4). Similarly, the EM2 fraction outcomes as an overlap between different sources (mixture between EM1 and EM3) rather than to an indubitable fine-grained fraction... Therefore, if the PCA biplots definitely add on for the discussion, they certainly do not allow to interpret the data in such a simplistic way (as it is presented here).

Page 11, lines 32-35 : Do you mean carbonate precipitation as aragonite/calcite crystals and layers/laminations from the water column and/or at the sediment interface ? Or the precipitation of carbonates mediated by organisms (e.g., bivalves, gastropods, ostracods) as it is stated Page 9, lines 24-25 ? If shells (e.g., molluscs and ostracods) are present (dominant ?) in sediments of Lake Kara Kul, it is likely that oxic (or sub-oxic) conditions were prevailing in the bottom (or even in the pore water) to sustain growth and development, although this seems unrealistic taken into account the general lake setting. Thus depending on the mineralogy – aragonite/calcite vs. dolomite (usually formed in the pore water), and the predominance/contribution of biogenic vs authigenic carbonates – the correlation between TIC and Fe/Mn may be more complex again. Please clarify on that. An interesting discussion dealing with the forcings involved in the interpretation of oxygen and carbon isotopes is available in Huang et al. (2014) for Lake Son Kul. In this regard, the discussion dealing with oxygen isopotes of authigenic carbonates is far too simplistic in the forthcoming Chapter 5.2, and would thus benefit from a reconsideration.

Page 12, Chapter 5.2 : As mentioned earlier, all the results presented in this chapter should go in a Results/Interpretation chapter, but not be developed here in the Discussion. Further, the significant issues outlined in my previous comments should be taken into account when processing/correcting the manuscript following reviewers' recommendations. As it is presented in its present form, I am not convinced that the interpretation and conclusions are supported by the data. In particular I have important reservations regarding the interpretation of grain size fractions in this chapter, during the Pleistocene and Holocene.

Page 12, lines 5-6 : Does Sr/Rb and Zr/Rb ratios document physical or chemical weathering as it is underlined here ? Page 11 (lines 14-15), but also in the Abstract (Page 1, line 23), it is stated that the main source of Zirconium (Zr). . .. is more often released by physical rather than chemical weathering... Please clarify on it as this is an important point. Similar mistakes occur in the text further in the discussion (for instance, Page 12 lines 10-11 and lines 16-17). In Page 13, lines 13-14, both physical and chemical weathering are involved from maximum values of Zr/Rb and Sr/Rb. So very difficult to read, and follow, as a whole.

Page 13, lines 18-20 : On the basis of what (which data ?) is based such a conclusion? Any references to strenghten this assumption ?

Page 13, lines 24-26 : Please add a line on Central Tien Shan lakes as well, such as Lake Sonkul on which many literature is available (e.g., Mathis et al., 2014).

Page 14, lines 13-15 : Do you mean physical or chemical weathering ? See also comment above.

Page 14, lines 22-27 : Please add a line on Central Tien Shan lakes as well, such as Lake Sonkul on which many literature is available (e.g., Mathis et al., 2014 ; Huang et al., 2014 ; Pacton et al., 2014).

Page 15, line 3 : Please change growthing into growing.

Page 15, Chapter 5.3 : Here again, most of the results developed here deal with the Results/Interpretation chapter, not within the Discussion, which gets longer and longer (as for instance in sub-chapter 5.2.3).

Page 15, lines 10-15 : See the comment provided above (Page 11, lines 32-35).

Page 15, lines 26-28 : Very simplistic explanation to account for the discrepancies observed between Lake Karakul and other regional lacustrine archives. What do we learn in such a case ? Any other alternative ? Age control ? Else ? I am not convinced at all by such an easy way of interpreting results.

Page 16, lines 29-32 : Here again I am not satisfied, and disagree, with this interpretation. Why should we believe that if we are not provided with data allowing us to evaluate if the Monsoon may indeed act as a trigger for the internal lake development during the early to mid-Holocene ? Very puzzling.

Page 16, line 33 and Page 17, line 1 : I do not see how the comparison with Lake Issyk Kul helps for deciphering the influence of the Westerlies and the Monsoon.

Page 17, lines 5-7 : If the influence of the Indian Monsoon on Lake Karakul hydrology is speculative, why such relationships look far less unrealistic when intrepreting lake-internal signals (e.g., chapter 5.2.2) ? Then, how could we reconcile these apparent contrasting scenarios between lake internal and lake external parameters ?

Page 17, line 12 : Rewrite the sentence (a word is missing).

Page 17, lines 21-26 : Please add a line on Central Tien Shan lakes as well, such as Lake Sonkul on which many literature is available to date.

Captions:

Caption Figure 4 : Please reverse captions for A and B, as the text does not match with the provided figure labelling (A and B).

Figures:

Figure 3 : Please add a scale (Depth, Age) at the right end of panels A and B, for the sake of legilibility. Please also delete the graph EM res. Scores (%) as it is not used further in the text. The quality of this figure is particularly low as a whole, and would therefore strongly benefit from reconsideration/redrawing.

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