

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

Received and published: 8 May 2016

Dear Referee #3,

Thank you for your comments and notes on our manuscript. We feel certain that we can follow most of your comments and are sure that they will contribute to an improvement of the paper.

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 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.

Referee 3 Comment 1

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.

Authors Response 1

Thank you for this comment, we will revise the literature list and add more appropriate references.

Referee 3 Comment 2

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.

Authors Response 2

We will add more studies from the Tien Shan Mountains in the revised version.

Referee 3 Comment 3

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

Authors Response 3

Thank you for this comment. A section dedicated to the core description will be added.

Referee 3 Comment 4

Page 4, Line 21 : Please define CNS here.

Authors Response 4

We will define CNS.

Referee 3 Comment 5

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 ^{14}C 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 $^{210}\text{Pb}/^{137}\text{Cs}$ dating to get a refined chronology in the topmost part of the core.

Authors Reply 5

The lake reservoir effect calculation based on the macrophyte remains collected alive is based on the ^{14}C concentration of the atmosphere in the year of sampling. Thus, the postbomb ^{14}C concentration of the atmosphere in the year of sampling was used to calculate the lake reservoir effect. More details of this

procedure are described in Mischke et al. (2013) which will be added in the reference list. The proposal to use organism remains from pre-bomb times for the determination of the lake reservoir effect requires the determination of the sediment age for deposits accumulated before the start of nuclear bomb testing which is not an easy task. In addition, a single gastropod shell from an assumed pre-bomb testing level within the sediments does not necessarily represent the time of the sediment accumulation due to its possible relocation from older sediments at shallower depth. Thus, the collection of living material and calculation of a reservoir effect with the ^{14}C concentration of the atmosphere at the time of collection is regarded as the most precise way of the lake-reservoir effect determination.

(Mischke, S., Weynell, M., Zhang, C., Wiechert, U., 2013: Spatial variability of ^{14}C reservoir effects in Tibetan Plateau lakes. *Quaternary International*, 313-314: 147-155.)

We did not use $^{210}\text{Pb}/^{137}\text{Cs}$ dating as we are not focusing on the last 1000 yrs or the late Holocene and rather focused on radiocarbon dating to cover longer timescales. It is moreover likely that the dense modern cover of macrophytes at the core site cause bioturbation of the sediment, especially in the upper part of the core. *Radix* and especially ostracods and insect larvae living in the sediment were also found and are likely to disturb the sediment at least in the upper centimetres. So, we expect that the use of $^{210}\text{Pb}/^{137}\text{Cs}$ dating is not providing robust results.

Referee 3 Comment 6

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.

Authors Reply 6

Thank you for this comment. We are aware that a constant lake reservoir over time is not a realistic approach and that the reservoir effect likely changed over the time span covered here. However, a higher number of radiocarbon dates does not improve the situation as long as terrestrial organic remains are not found in the sediments. Terrestrial material was not found in the sediment core. The plant remains used for radiocarbon dating are all of aquatic origin. We will make this clearer in the manuscript by adding this information in Table 1 as well as in the Material and Methods section.

We do think that the shallower eastern basin including the core site, has as stratified water column during summer (or the rest of the year) due to the common existence of a thermocline at 20 m or below in lakes on the Tibetan Plateau. The situation will have changed over time with lake level changes and we will make this clearer with the integration of the lithological section and core description.

We sampled a parallel core for an independent age assessment by OSL dating. Although not fully assessed already the preliminary dates from the parallel core suggest that the lake reservoir effect remained constant over time. However, the data are not fully evaluated so far and they will be presented in a separate paper by another author discussing the correlation of both cores and a unified age-depth model.

Referee 3 Comment 7

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

Authors Reply 7

We will correct this reference.

Referee 3 Comment 8

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

Authors Reply 8

We will revise this sentence.

Referee 3 Comment 9

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

Authors Reply 9

We correct this typo.

Referee 3 Comment 10

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.

Authors Reply 10

Thank you for your comment. We present a number of proxies in this manuscript. In order to gain better insights into the developments within the system we choose to investigate lake internal and lake external parameters. We choose lake external parameters based on their outer, externally forced nature, such as the grain size distribution which is assumed to mainly represent external input. The same holds true for example for the minerogenic data such as Zr, Sr, Rb, Ti data. These elements reflect the detrital sediments which were transported to the lake from the catchment or even farther sources (in the case of dust) and were not formed within the lake.

On the other hand, the lake internal parameters are related to lake internal processes and are mainly formed within the lake. We hope to be able to decipher also changes within and outside the lake system and add more detail to the individual parameters.

Referee 3 Comment 11

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.

Authors Reply 11

Thank you for this comment and suggestion. Climate of the Past does not have precise regulations on how to structure a manuscript with regards to the mid sections structure. In this version of the manuscript we followed the common structure of Introduction, Study Site, Material and Methods, Results, Discussion and Conclusion. Thereby the Results section only includes the data and no interpretation, while the discussion includes the interpretation of the data and results. In our experience it is important to separate description of the results from interpretation in the discussion. However, if the Editor agrees with your suggestion we can change the structure of the manuscript. In the case the Editor does not agree with your suggestion we hope you understand that we will not follow your suggestion of restructuring the manuscript.

Referee 3 Comment 12

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.

Authors Response 12

Thank you for this comment. We can see how this is confusing and will clarify it. In order to calculate a reservoir effect as good as possible we used the already available data from the in situ macro-algae from the study of Mischke et al. (2010a) and newly dated one which was retrieved during the expedition in 2012. The two available results are more or less similar (1420 and 1315 years) and we used the average of both data (1368 years) as lake reservoir effect in the study presented here.

Referee 3 Comment 13

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.

Authors Response 13

In this part of the manuscript, the Results chapter, we solely focus on the data which was retrieved for the investigated parameters. The interpretation and reasoning follows in the Discussion chapter with the XRF related section from page 10 line 33 onwards.

In general only the heavier elements could be taken into consideration, as high χ^2 values prevented the usage of lighter elements. The Sr/Rb and Zr/Rb values are especially useful as high resolution grain-size indicator in addition to the actual grain-size measurements. However, this is discussed in more detail in the Discussion section of the manuscript.

Referee 3 Comment 14

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 separately to over-all/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.

Authors Response 14

Thank you for this comment. In order to investigate the data set in more detail we examined the internal and external parameters separately and used the stratigraphically constrained cluster analysis (CONISS). Through the separation of the parameters a more detailed and thus also differing zonation followed. Regarding your concerns of organic matter export from the lake catchment, we are certain that the majority of organic matter originates from lake internal plant and algae growth. Especially sediments of zone 3 contain very abundant aquatic macrophyte remains. A significant increase in TOC was ascertained in the zone. These remains are very abundant in the upper 450 cm, which will become clearer in the added lithology section in the revised manuscript. Only few TOC/TN ratio data are available for zones 1 and 2 due to very low TN contents and the available data are rather low and in the range which is typically associated with phytoplankton. If we furthermore take the location of the coring site, several hundred meters from the shoreline, and the sparse catchment vegetation due to low precipitation into account, we are certain that the majority of the organic matter is derived from within the lake. However, terrestrial organic matter input from the catchment vegetation or soils cannot be completely ruled out.

Referee 3 Comment 15

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 provided 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.

Authors Response 15

So far the manuscript is subdivided into the chapters Introduction, Study Site, Material and Methods, Results, Discussion and Conclusions. As the journal does not provide a strict framework for the structure, we are happy to follow your main concern about this study, if the editor agrees. We would then move the interpretation part from the first section in the discussion into the results chapter and retitle it to Results and Interpretation.

Referee 3 Comment 16

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

Authors Response 16

Thank you for this comment. We are certain, that the higher values of TOC are due to increased bioproductivity within the lake. The macrophyte content in the upper part of the core increases considerably and submersed water plants are very likely contributing the majority of preserved organic matter in this study. We are certain that the addition of a lithology description section will clarify this.

Referee 3 Comment 17

Page 10, lines 13-16 : If I overall 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 and 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 assumption.

Authors Response 17

Thank you for your comment. We understand your concern regarding the EM1 as purely of aeolian origin. We also state, that EM1 and EM2 could also be associated with glacially derived sediments with fine grain sizes (glacial milk, page 10 line 19-21). There is not proxy available here to differentiate between aeolian derived and glacially derived sediment input. EM1 and TiO₂ correlate and plot similar in the PCA for the external parameters and it has been shown in Sorrel et al. (2007) that the majority of Ti input comes from dust accumulation in arid and semi-arid regions.

However we will formulate our assumptions more carefully and will discuss different possibilities for the sources and transport mechanisms of EM1 in the revised manuscript version.

Referee 3 Comment 18

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.

Authors Response 18

We will revise this paragraph and the related references and will provide more detail regarding the possibility that EM2 might be associated with glacially derived deposits.

Referee 3 Comment 19

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. . .

Authors Response 19

Thank you for this comment. We appreciate your concerns and will take the two possibilities for inferences based on EM 1 into account in order to improve the manuscript. We will also phrase the inferences from our results in a more careful way.

Referee 3 Comment 20

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).

Authors Response 20

We will add that the grain-size range of EM3 is also represented in the modern pond and slack water silt reference samples. Thank you for pointing this out.

Referee 3 Comment 21

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.

Authors Response 21

We understand the reviewers concern and will attribute the EM3 to runoff triggered by local summer precipitation. Thus, we will revise our assumption that EM3 is reflecting Indian Monsoon induced precipitation. As the northern boundaries of the Indian Monsoon are still under discussion it is also not

possible to rule out a monsoonal influence. However, more detailed studies with additional or more appropriate are required for a better discussion of moisture sources. Thank you for pointing this out. We will discuss the possible sources in the discussion section of the revised manuscript and evaluate the different possibilities. We are aware that contributing a single mechanism to an endmember is not realistic. We will consider different scenarios and mechanisms related to the sediment origin and transport of EM3. However, we expect a significantly less well sorted sediment distribution if indeed glacial tongues reached into the lake and were located close to the coring position. The EM3 sediments are relatively well sorted and we favour precipitation-controlled runoff as the main factor. We are happy to provide a climatic diagram of the area in the manuscript or supplementary material.

Referee 3 Comment 22

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 comparison 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.

Authors Response 22

Thank you for this comment. As mentioned above, there is no evidence so far that glaciers reached the coring location and could have dropped high amounts of coarser grain sizes into the lake. This is also shown by the study of Komatsu and Tsukamoto (2015) who investigated ancient shorelines above Lake Karakul and glaciation remains in the lake's catchment. Satellite images show a valley which was most likely glaciated during the LGM to the east of the lake and the coring location. Therefore it is possible that glacier-derived materials contributed to the sediments in the lake. We will discuss this possibility in the interpretation section.

Detailed information or literature about the ELA is not available to our knowledge.

We will be happy to search for literature concerning this issue for lakes in the Tien Shan and will include relevant information. Thank you for pointing this out.

Referee 3 Comment 23

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).

Authors Response 23

If the editor agrees with to a restructuring, we will follow your suggestion. If not we will keep the current structure and will present results separate from the interpretation in the results chapter.

Referee 3 Comment 24

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.

Authors Response 24

Thank you for your comment. We will re-examine the data statistically to ensure that the mean grain-size and TiO₂ data are anti-correlated, especially the lowest two meters of the core as well as in the transition from zone 1 to zone 2.

Referee 3 Comment 25

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

Authors Response 25

We will restructure the manuscript accordingly if the editor agrees that the interpretation of data will be incorporated in Results and discussion section.

Referee 3 Comment 26

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 run-off (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).

Authors Response 26

Thank you for this comment. We will revise this paragraph and consider the PCA in more detail for a more thorough discussion of potentially involved sediment sources and transport mechanisms. We will also consider common grain size characteristics of modern fluvial and aeolian sediments in similar settings more intensively to provide a better assessment of the different EMs and the involved sources and transport mechanisms.

Referee3 Comment 27

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 isotopes of authigenic carbonates is far too simplistic in the forthcoming Chapter 5.2, and would thus benefit from a reconsideration.

Authors Response 27

Thank you for this comment. We will revise this section and will examine the paper of Huang et al. (2014) from Lake Son Kul. The carbonate which was used for the stable isotope analyses represents mostly aragonite crystals precipitated from the water column. The carbonate of the recovered sediments include also other carbonate species such as biogenic shells of ostracods and gastropods. However, the material used for stable isotope analysis was prepared in a way to concentrate authigenic aragonite crystals. Sediment material was examined by smear-slide analysis. We will make this inspection and the material used for stable isotope analysis more clear in the revised manuscript.

Referee 3 Comment 28

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.

Authors Response 28

Thank you for your concern. We will be more careful when formulating our inferences and derived implications and will make sure that our reconstructions are justified by presented and discussed data.

Referee 3 Comment 29

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.

Authors Response 29

Thank you for this comment. A comprehensive reinterpretation of the involved external parameters and statistical indications, as well as involving further literature, brought more clarity to the general interpretation of the used XRF ratios. The revised version of the manuscript, regarding external parameters, will focus on fluvial, detrital and eolian input rather than chemical weathering. Zr/Rb and Sr/Rb will be used as high resolution grain-size and fluvial indicators, supported by end-member modelling.

Referee 3 Comment 30

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

Authors Response 30

We will formulate our inferences more carefully and change this sentence. We will focus on the changing moisture conditions indicated by EM3 rather than assigning moisture source areas in the revised manuscript.

Referee 3 Comment 31

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).

Authors Response 31

We will be happy to add a reference from the Tien Shan, thank you for this comment.

Referee 3 Comment 32

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

Authors Response 32

Thank you for this comment. Weathering has been excluded as dominant driving force for Zr/Rb and Sr/Rb XRF ratios. Please see response 29.

Referee 3 Comment 33

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).

Authors Response 33

We will check the literature for reference studies from the Tien Shan and add a suitable study.

Referee 3 Comment 34

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

Authors Response 34

Thank you for this note. We will correct this typo.

Referee 3 Comment 35

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).

Authors Response 35

We will restructure the manuscript if the editor advises us to do so as well.

Referee 3 Comment 36

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

Authors Response 36

We will discuss the most important different places and sources of carbonate formation (precipitation of aragonite from warm surface waters, formation of carbonate shells by organisms) in a clearer way in the revised manuscript.

Referee 3 Comment 37

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.

Authors Response 37

We will revise this sentence and describe similarities and differences between individual climate records in more detail in order to show the complexity of the issue.

Referee 3 Comment 38

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.

Authors Response 38

We agree. We will revise the inferences and will eliminate the previous interpretations of monsoonal precipitation since the different possible moisture sources cannot be differentiated in a reliable way.

Referee 3 Comment 39

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.

Authors Response 39

We will revise this sentence and we will remove our previous interpretations of precipitation with monsoonal sources.

Referee 3 Comment 40

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 interpreting 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 ?

Authors Response 40

We will emphasize that the inference of monsoonal precipitation at the site of Lake Karakul remains fully speculative as is written in lines 5-7 on page 15. We will make clear that both lake-internal and lake-external proxies do not allow a good assessment of moisture sources.

Referee 3 Comment 41

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

Authors Response 41

Thank you for this note. We will correct this sentence.

Referee 3 Comment 42

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.

Authors Response 42

We will examine the literature from the Tien Shan on this subject and add a relevant reference or references.

Referee 3 Comment 43

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).

Authors Response 43

We will correct the captions. Thank you for your comment.

Referee 3 Comment 44

Figures:

Figure 3 : Please add a scale (Depth, Age) at the right end of panels A and B, for the sake of legibility. 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.

Authors Response 44

We will revise figure 3 and also follow your advice and remove the graph of EM res. Scores (%).

Interactive comment on *Clim. Past Discuss.*, doi:10.5194/cp-2016-34, 2016.