

Response to the reviewers comments on manuscript cpd-10-4385-2014

We thank the two anonymous reviewers for their time reviewing this manuscript, for their valuable comments and for raising interesting points of discussion. Our responses to the specific comments are attached below, together with a list of other minor changes.

1. Response to Rev#1:

Rev#1.: Overall, the study is well designed and executed. It would have been valuable to have dD_{wax} data from modern vegetation in the watershed, in order to constrain local fractionation factors and reduce uncertainty in absolute values of reconstructed dD_{ppt} , although this lack does little to diminish the significance of the study. One minor concern is that the lower half of Figure 4b lacks scale bars and labels for the monthly mean climate variables.

Response: we agree that it would be valuable to have data from the vegetation in the catchment, however the field work was limited to lake coring in this case. Labels and scale bars were not included in the original version of Fig. 4b due to layout-reasons, but have now been added.

2. Response to Rev#2:

Key-comment (1): temperature vs. precipitation:

Rev#2: As soon as the abstract, it is stated: "Instrumental evidence and isotope-enabled climate model experiments with the Laboratoire de Météorologie Dynamique Zoom model version 4 (LMDZ4) demonstrate that dD values of precipitation in the region are influenced by both temperature and precipitation amount. We find that those parameters are inversely correlated on an annual scale; i.e. climate varies between cool/wet and dry/warm over the last 50 years." The instrumental evidence the authors refer to is a meteorological station (described in chapter 5.2.1. and data shown in figure 4a). The isotopic values of rainfall shows a very clear seasonality, with only 2 data points out of more than 20 significantly deviating from a nearly perfect sine curve. Such a pattern clearly demonstrate that TEMPERATURE is by far the first-order controlling knob of isotopes. Instead of interpreting the isotopic signal in terms of temperature, the authors acrobatically state "However there are also small amount effect observable", and they do list the couple of months that make the 2-years long record of isotopic composition of rainfall deviating from the sine curve... Later in the article, they tentatively interpret the isotopic ups and downs as going towards "warmer/drier" vs. "cooler/wetter" conditions, which I find awkward given the apparently little effect of seasonality of rainfall amount shown in Figure 4. The LMDZ, to me, can't be of great help to justify the interpretation of the isotopic ratios as long as the model

simulates an order of magnitude more annual rainfall than the meteorological station (shown in Figure 5, the feature being noticed by the authors themselves).

So why pushing precipitation in such case? It clearly obscures the discussion. This would help, in my opinion, to clearly mention that rainfall may play a minor role in the isotopic signal, but is very likely of second order. The discussion (and maybe interpretation) may then be much more straightforward and convincing.

Response: the question what -besides vapor source- actually influences the isotopic signal of the precipitation is very important and answering this is not straightforward, hence we are glad that the reviewer picks up this issue for discussion.

First of all the sinusoidal curve of isotopic data from the 2 years interval in Taxkorgan (Fig 4a) does imply a temperature control on monthly isotopic values. The question is if this also implies a temperature control on annual average precipitation. Let's assume -for instance- two years with equally warm summers, but the first being drier and the second wetter. From first principals, and evidence for the amount effect in these latitudes, we anticipate that would be accompanied by less negative δD -values in the first year and more negative δD values in the second. We think there are indications for this effect from the short timeseries in Fig. 4a. As described in the manuscript, June has the highest precipitation amount and an amount effect on the order of -5 to -7.5‰ in $\delta^{18}O$ values, which lowers the annual mean precipitation isotopic values. The amount effect is also supported by the LMDZ-derived correlations between isotopes and climatic parameters, which in the summers show a significant negative correlation with both temperature and precipitation (Fig. 6).

This, in our opinion, gives strong enough evidence that precipitation amount influences the overall isotopic values and needs to be taken into account in the integrated signal as recorded in sediments. However, we agree with the reviewer that the amount-effect is probably secondary compared to the temperature control, especially if considering the very low absolute changes of precipitation amount (as seen on the data from Taxkorgan in Fig. 5).

⇒ We rephrased the last paragraph of section 5.2.1 and included this assumption here.

A second aspect to consider is the co-correlation of climatic parameters. Fig. 5 shows this for the last 50 yrs on two spatial scales for temperature vs. precipitation: (1) locally i.e. point-data from Taxkorgan climate station (3090m) which –even if located at ca. 500m lower and 80km to the south- could be representative for the situation at Lake Karakuli. (2) regionally i.e. LMDZ4-grid cells, which include the complete altitude range, hence higher altitudes with higher precipitation. Both scales, i.e. (1) and (2) show a negative correlation between temperature and precipitation (except for winters if taking the whole grid-cell/LMDZ-model). Extending the timescale from 50 yrs to centennial-intervals than (3) proxy-data for the last few hundred years also generally agree that the LIA was a cool/wet episode and that the MCA was rather dry in large parts of arid Central Asia (Fig. 8c,e).

If temperature and precipitation are anti-correlated we can overcome the problem of deciphering the dominant climatic parameter on δD by not distinguishing between what *influences* the isotopic signal but what is *correlated* with the signal. Thus we are confident that our assumptions are true that low δD -values indicate cooler/wetter conditions and that higher values show warmer/drier conditions and that this holds for the whole time interval covered by the record.

Key-comment (2): connection to large-scale climate dynamics

Rev#2: Another thing I'd like the authors to avoid is to blindly connect their isotopic records (along with the already published grain size fraction curve) with other reference curves, and interpret their record within a broader context on the basis of similarities between all curves. While the fact that there is no one-to-one coupling between their own records with other curves taken from the North Atlantic, Greenland, etc., the authors still courageously drop a line stating "Our interpretation of lower δD values indicating both relatively cool and wet conditions fits well with results from other late Holocene records in arid Central Asia (Fig. 8)." Figure 8 indeed displays panels where many – if not all - curves just don't look like each other (in terms of trends, rapid variability, etc.) They briefly deal with some discrepancies invoking large-scale atmospheric patterns, but the reader's feeling of the tone employed by the authors is that all those curves kind of tell the same story. While having a look at their comparisons (figure 8) and reading the above-quoted sentence, I guess climate dynamicists and high-res paleo-stat folks will just ignore your observations.

I understand it is important to connect one new climate record to other reference records to better understand large-scale climate patterns. But central Asia is far enough from some climate records shown in figure 8 to have a more descriptive discussion on what's happening at the site prior to try connecting the δD record to other reference curves situated very far away from central Asia (and that don't really fit with your ones). And to be honest, even the %silt and δD curve shown in Figure 7 are not really well fitting with each other (sometimes they look negatively correlated!), unlike what is suggested in the text. The authors shouldn't be shy and make everything to build their own "new reference curve" for the late Holocene climate in central Asia.

Response: we did not generally intend to suggest an one-to-one coupling between for instance the Northern Atlantic or other parts of Central Asia and our study area. Instead we tried to emphasize (e.g. in the conclusion) that responses of regional climate are complex due to the interplay between the influencing atmospheric circulation systems. But, nevertheless, that there is an imprint of changes in the dynamics and interplay of those systems (represented by proxy data such as GISP K+ for the Siberian Anticyclone or NA hematite for Northern Hemispheric circulations), observable in our record.

- ⇒ While we feel that these issues have been discussed already in detail in section 5.4. we rephrased parts of the conclusion and added one sentence to the abstract to transport our message clearer.

The sentence "*Our interpretation of lower δD values indicating both relatively cool and wet conditions fits well with results from other late Holocene records in arid Central Asia (Fig. 8).*" refers mainly to the LIA, as compiled in the Central Asian Wetness index from Chen et al. (8c) and also the Guliya accumulation rate (8e).

- ⇒ This is explained in the following paragraph, but to make this clearer we changed the figure callout after the above sentence to (Fig. 8c, e and g). Here, as suggested by the reviewer (see below) we also included a reference to the newly added compiled temperature plot of Asia (Pages2k-Network, Nat Geo 2013), which is in Fig. 8g in the revised version of the manuscript.

Although there is no 100% 1:1-similarity between the Karakuli silt and δD curves we still think that the general ups and downs of the two proxy curves (represented by non-shaded and shaded areas) vary somewhat consistently.

Further comments:

Rev#2: If they want to invoke large-scale atmospheric rearrangements for interpreting their isotopic signals in light of other studies, they should at least acknowledge that a change in moisture source (local vs. remote) can drive a change in the isotopic composition of rainfall without a corresponding change in temperature (and precipitation rate)

Response: potential changes of moisture sources always need to be considered when interpreting isotopic data. In our manuscript this is mainly handled by discussing shifts of the paths of Westerlies throughout section 5.4.

- ⇒ Further one additional sentence has been added to the first paragraph of section 5.2.1.

Rev#2: If the authors decide to opt for a "temperature-driven" isotopic record, then they should not miss the occasion to compare their results to the temperature results published in the PAGES2k consortium paper (2013, Nature Geoscience) where a large set of data from Asia were used to derive a continental-scale temperature record.

Response: this was an oversight and we have now compared our results to PAGES2k and the comparison is shown in Fig. 8g of the revised version of the manuscript and associated text.

Rev#2: I understand why the authors opt to use the C26 and C28 for the dD. Still, the supplementary information figure S3 shows some significant shifts in the d13C of those acids that are paralleled, in particular for the 4-2 ka time interval, that find some echoes in the dD, which suggests there were some contributions from different plant types to those d13C curves that affected the dD as well.

Hence I would have liked to see a figure with temporal changes in the d13C and dD of all individual fatty acids in the main article (not in the supplement), along with their own respective concentrations. This would help convincing more the reader that shifts in vegetation types does not significantly complicate the interpretation of dD that can have had been obscured by changes in the C3/C4 contributions (having different fractionations on the dD) of the different fatty acids homologues.

Response: The reviewer raises an interesting concern about C₃ versus C₄ plant types. We had previously assumed that C₄ plants were unimportant in the catchment and region. However in response to the reviewers point we have search more deeply and found that there at least partly C₄ plants of the taxa *Chenopodiaceae* could occur in some high altitude mountainous deserts of the eastern Pamirs (newly included reference Sage et al., 2011), and thus a contribution of C₄-derived lipids cannot be totally excluded. Further there is a possibility of low levels of distal transport of C₄ waxes from surrounding Central Asian deserts; however, this does indeed seem unlikely to contribute significant amounts to the sedimentary lipid pool.

Nevertheless we have acquired a $\delta^{13}\text{C}$ record that allows us to explore the influence of vegetation changes on the δD wax record. In particular we were more concerned about the potential changes between aquatic and terrestrial sources, rather than the C₄ contribution. However, in contrast to other densely macrophyte covered high-altitude lakes on the Tibetan Plateau, that I have studied (Aichner et al., OG 2010), there are few macrophytes observable in Lake Karakuli and thus the aquatic contribution is currently minimal in Lake Karakuli. Furthermore, given the relative abundance distribution of fatty acids in an aquatic plant collected close to the shore (Fig. S4), we would expect much higher abundances of C₁₆ and C₁₈ in our sediments if the aquatic contribution is high. But this is not the case throughout the whole sediment core (Fig. S2).

We agree that the slightly enriched $\delta^{13}\text{C}$ -values (i.e. reaching values higher than -30‰) of C₂₈ during the period 4.0-3.5 kyrs BP must be either due to enhanced C₄ input or macrophyte productivity could have been increased; and this might have additionally enhanced the corresponding δD -C₂₈-values during this interval because of the different fractionation factors for C₄-plants and/or different source water. However, this doesn't change anything about the overall interpretation that this was a relatively dry and warm episode because this inference can be made from both δD and $\delta^{13}\text{C}$ (even though the δD -amplitude might be biased towards more positive values for a few permil).

- ⇒ We have now adjusted the discussion to develop both of these points (C₄ and aquatic) further (new last paragraph in section 5.1.2; slight modification in section 5.1.3;

additional sentence in the first paragraph of 5.3). We have also added another figure to the supplement (S4) to illustrate the relatively low abundance of aquatic plants in the lake at present which is limited to scattered patches close to the shore.

Concerning the potential changes of aquatic vs. terrestrial input, or C₄ vs. C₃ plants, the δ¹³C values give the most important information which makes compound concentrations obsolete (also the latter do not show many changes throughout the whole record and thus do not add a new perspective to the story). The rather stable δ¹³C-values of C₂₈ (except for the above mentioned interval between 4.0 and 3.5 kyrs BP) speak for relatively constant terrestrial C₃-contribution to this compound. Slight ¹³C-enrichments of C₂₆, e.g. in the lower and middle core section, could speak for a bias due to enhanced aquatic or C₄ input here. But since the δD-curves of C₂₆ and C₂₈ run mostly parallel (except for the interval ca. 2500-2000 yrs BP), we assumed this to be of minor relevance for the overall interpretation.

We have produced a study that takes a variety of approaches including proxy data, model experiments and instrumental data. We attempt to communicate the main findings in the main paper, and then to provide ancillary information in the supplement, as a resource to those interested to delve deeper. The nuances of the different information contained in the various chain lengths can be interpreted with knowledge of the biomarker production and carbon isotopic compositions characteristic of different sources, and this is of interest to organic geochemistry readers. But for the readers of *Climates of the Past*, the paleoclimate story that emerges from the long chain hydrogen isotope record is most of interest for the research question of climatic reconstructions.

3. Other changes:

p. 4389, l.5 (section 2): changed time range of Bulun Kul climate data from 1956-1986 to => 1956-1968

p. 4395, l.25-28 (last sentence of paragraph 5.1.2): we removed the last sentence "We estimate the macrophyte contribution....(Fig.7)." and the resulting scale bar in Fig. 7 showing the estimated quantified macrophyte contribution. Instead we added a relative scale bar illustrating higher/lower macrophyte productivity and/or C₄-contribution. As described above, we added another paragraph discussing the potential relevance of C₄-contribution to the lipid pool of our studied lake.

p.4397, l.4: changed year of reference Bowen and Revenaugh, 2013 => 2003

p.4398, l.19: changed reference "Bowen, 2014" to "Bowen and Revenaugh, 2003".

p.4406, (conclusions): several "Eastern", "Western" and "Central" have been decapitalized => "eastern", "western", "central", except for "Central Asia"

p.4406, (acknowledgements): the acknowledgements have been slightly modified

There are a few other minor improvements/corrections of language/grammar which are not listed here but which are annotated in the attached revised version of the manuscript.

Newly included references:

Sage, R.F., Kocacinar, F., Kubien, D.S.: C₄ photosynthesis and temperature, in: C₄ Photosynthesis and Related CO₂ Concentrating, Advances in Photosynthesis and Respiration Mechanisms, 32, Springer Netherlands, doi: 10.1007/978-90-481-9407-0_10, 161-195, 2011

PAGES 2k Network.: Continental-scale temperature variability during the last two millennia. Nat. Geosci., 6, 339–346, doi:10.1038/ngeo1797, 2013.

Newly included supplement:

A .kml-file showing positions of coring site and climate stations have been included as supplement S7.