

Interactive comment on “High resolution leaf wax carbon and hydrogen isotopic record of late Holocene paleoclimate in arid Central Asia” by B. Aichner et al.

Anonymous Referee #2

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The article "High resolution leaf wax carbon and hydrogen isotopic record of late Holocene paleoclimate in arid Central Asia" by Aichner et al. presents a high-quality paleoclimatic record from central Asia spanning the last 4 ka. It is based on isotopic measurements performed on leaf wax from a lacustrine sedimentary sequence, and the data are accompanied by a thorough review of regional climatic background, a detailed description of how and why the isotopic signals should be interpreted, with the help of an isotope-enabled atmospheric model output (LMDZ4).

As reviewer 1 I recommend publication of the article in Climate of the Past, but I also think the article can still be improved, in particular (1) on the justification of the interpretation the authors have done on the δD in terms of "temperature and precipitation",
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as well as (2) in the second half of the discussion chapter.

(1)

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 δD 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.

(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 dD 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 dD 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 dD 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.

Following on those two points, the other things I suggest the authors to address briefly are:

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-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)

-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.

-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, than 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.

In brief, I re-emphasize this is a very nice study that has to be eventually published after minor to moderate revisions. But after a first half of the manuscript which describes very well the site and the proxies, I also feel the authors did not put as much efforts on the results and discussion parts as they've done for the first half of the manuscript.

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