

## ***Interactive comment on “Global aridity synthesis for the last 60 000 years”***

**by Florian Fuhrmann et al.**

### **Anonymous Referee #2**

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→ We thank reviewer#2 for his constructive and helpful comments to improve the manuscript. We have revised it in detail. Our point-by-point response to your suggestions are provided in red in the attached PDF file.

This study is an attempt to provide a global synthesis of aridity over the last 60 ka using a number of selected terrestrial (speleothem, lake, loess) and marine records of 10 regions on the globe. While the major outcomes (including the aridity index) of the manuscript results from an immense effort of synthesising various records having different chronologies, resolutions, proxies and associated uncertainties, it is particularly hard to judge what has been really done in terms methodology and if this is sound or not. In agreement with the opinion of referee #1, the methods section (+Supplementum) should be much more transparent to the reader, and the sometimes sloppy text and superficial statements, inappropriate usage of specific terms must be carefully revised. This also applies to some of the argumentations (e.g. Europe-Greenland aridity relations).

In general, it is suggested that the authors should 1) clearly present the core concept of proxy record selection for this synthesis, and 2) exclusively include records having independent absolute chronologies (i.e. NGRIP/MIS tuned chronologies should be avoided). In my view, the concept of excluding proxy records, which are otherwise well-dated, but do not extend back to 60 ka, should be revised or at least some justifications for this decision are required. Just to mention one excellent example: the Nussloch loess record in Germany (Central Europe), which has a quite well-defined, robust and precise <sup>14</sup>C-chronology (extending back to 55 ka), has been omitted. Moreover, further details on the aridity index and age uncertainty calculations of proxy records must be provided. In my view, any proper assessment of the scientific content of this work can only be provided after a thorough revision of the methodological part.

→ Well cited papers - where data were not available - were not included into the compilation. This was a prerequisite from PalMod, that is why the paper is presented in the special issue. We fully agree, that these data would be useful, but they are not publically available.

- 1) Is done now in the revised method section
- 2) This would be the perfect attempt. Unfortunately, those records are rare and often not publically available. Especially the Nussloch loess record is not available in databases to our knowledge. Also many other striking paleoclimate records either do not cover a larger part of the last 60 000 years or are not available from official data repositories. We move a section with not used, but important records from the supplement to the paper and extend it for more clearness.

Specific comments

#Manuscript

Page 1, lines 25-26, “MIS2 (Last Glacial Maximum (LGM) 24 000-14 700 yr b2k)”: This is misleading, as the LGM was a globally recognizable, peak glacial period between 26-19 ka (broadly speaking), while the ages given are the widely accepted boundaries of MIS 2.

→ Yes, changed to “the MIS2 (24 000-14 700 yr b2k)” and explanation of abbreviation “LGM” at page 1, line 13.

Page 2, lines 7-12: I would say dust is dominantly from deserts, but other dryland ecosystems (shrublands, grasslands and even forests with 300-500 mm annual rainfall; Breshears et al. 2003) can also produce fair amount of dust.

→ agree on that comment. We changed that according to referee#1 to “indicate an arid climate”.

Page 3, lines 3-8: This is corroborated by other studies of loess records, 14C-dated in high (Nussloch, Germany; Moine et al., 2017) and extremely high (Dunaszekcso, Hungary; Ujvari et al., 2017) resolution. Why not using at least the Nussloch record for Central Europe, beyond the ELSA stack?

→ Well cited papers - where data were not available - were not included into the compilation. This was a prerequisite from PalMod, that is why the paper is presented in the special issue. We fully agree, that these data would be useful, but they are not publically available. We include a section with not used, but important records within the paper.

Page 3, line 15: Provide more details on GICC05 (b2k) timescale conversion. Does this simply mean a 50 yr addition to the calibrated radiocarbon chronologies? How this approach was applied to the luminescence chronologies?

→ We used the original stratigraphy of all records on the age scale of yr b2k. Sometimes this means an addition of 50 yr, yes. We compare data on a multi-millennial scale, thus uncertainties between BP, ka and b2k age scales are not that important. This is mentioned now in the revised method section.

Page 4, lines 8-9: What does this sentence mean?

→ rephrased to “The grainsize record from the loess plateau in China shows phases of aridity. The larger the sediment grains, the lower the precipitation and temperature and the higher the wind speeds.”

Page 4, lines 9-10: Does “eolian content” mean eolian fraction of sediments?

→ yes, rephrased to “Dust or eolian content of the sediment is...”

Page 4, lines 12-14: Fuzzy text (K/C ratio and related interpretations) must be revised.

→ rephrased to “Kaolinite / chlorite ratio can be used as a dust proxy for the Mediterranean Sea region. Higher K/C ratios (more kaolinite than chlorite) indicates increased eolian dust transport. During humid periods, kaolinite was stored within lakes or basins - due to increased erosion - and deflated during arid periods (Ehrmann et al. 2017).”

Page 4, line 25, Table 1: It is still not entirely clear how these aridity values are calculated from dust. Dust MARs or grain size or what has been used and in which way? What does the internal normalization mean?

→ The whole methods section including data treatment and aridity index calculation is revised.

Page 5, section 2.2: I suppose this section describes age uncertainties. State this clearly. Have you considered proxy uncertainties?

→ We used the original stratigraphy of all records on the age scale of yr b2k but we are aware of a general error of up to  $\pm 2\,000$  years for all MIS 3 dates. This is now incorporated into the introduction. Beside this, uncertainty estimation is revised as well.

Page 5, line 7: Does the “error of our aridity index” mean uncertainties related to dating/chronological uncertainties?

→ We estimated uncertainties of the proxies, as no original uncertainties are aware beside the age uncertainties of the speleothem growth phases.

Page 5, line 10, Table 2 (header): Clarify that the “tree pollen/eolian dust uncertainties” are dating/chronology uncertainties. Provide more details on the method used for uncertainty estimations. Has this been done by Monte Carlo simulations?

→ This was some kind of a simple Monte Carlo simulation, yes.

Page 5, line 14: In what sense is Central Europe a “feedback region”? Clarify.

→ rephrased according to Review#1: “Central Europe is related strongly to North Atlantic climate changes.”

Page 6, lines 4-5: Provide numbers for “low dust concentration”. Do you refer to Greenland or dust source regions (or Central Europe) when talking about “intermediate to low aridity” in this sentence?

→ rephrased according to Review#1: “An intermediate dust content in the ELSA-Dust-Stack suggest an intermediate to low aridity, which is supported by a similar pattern of low dust concentration in the NGRIP ice core. This corresponds to an underlying process, affecting both regions during this time.”

Page 6, line 6, “49.000 yr b2k”: Provide uncertainty for this date.

→ as previously mentioned, we are aware of up to  $\pm 2\,000$  years uncertainty for MIS3 dates (see reply to Page 5, section 2.2). Therefore, p.6 l.6 is rephrased to “The pollen composition change began around 49 000 yr b2k towards more grass and herbs pollen.” The updated manuscript is changed according to this suggestion.

Page 6, line 25: Provide numbers of “extreme cold temperature” for the NGRIP site based on reconstructions of Kindler et al. (2014) and state clearly that these temperature estimates are not only from  $\delta^{18}\text{O}_{\text{ice}}$ , but a combination of  $\delta^{18}\text{O}_{\text{ice}}$  and  $\delta^{15}\text{N}$  measurements (+ $\Delta\text{age}$ ).

→ We assume, you mean Page 6, line 21 instead of line 25: We agree about your addition of  $\delta^{18}\text{O}_{\text{ice}}$  to this sentence, but we refer to Figure 3a and Figure 5 of the cited Paper, where the temperature reconstruction is performed using  $\delta^{18}\text{O}$ . Therefore, we specified the sentence according to your suggestion: “The NGRIP  $\delta^{18}\text{O}_{\text{ice}}$  whereas shows a phase of extreme cold temperatures ( $-45^\circ$  to  $-50^\circ$  C) during this time”. For age uncertainty, we refer to the previous comment, as no uncertainty is given with the original paper, even within the supplement. We are aware of age uncertainties within the GICC05 age scale of Rasmussen et al. (2014) but would not make any statement about age uncertainty beyond that.

Page 6, lines 24-25: In this sentence you suppose a direct link between Central Europe and Greenland in terms of dust transport. On what basis? To my knowledge, the possibility of European dust sources for central Greenland (over the LGM) has been proposed in Ujvari et al. (2015), specifically based on

Sr-Nd isotopic compositions. I would rather emphasize that the ELSA record reflect regional conditions and these could have differed from those in Greenland.

→ The dust records of Central Europe and Greenland correlate over most of the last 60 000 years. ELSA-Dust-Stack (2009) shows the GI / GS changes in detail. The biggest difference between ELSA-Dust-Stack and NGRIP-Dust can be seen in the period of 26 000 to 23 000 yr b2k, where the dust content in the NGRIP core has two distinct maxima, while the dust content in the Central European record increases only slightly. We do not want to suppose a direct link in dust transport as we know, that the majority of NGRIP dust is from Asian deserts (Steffensen et al., 2008, Mayewski et al., 2014 etc.). We interpret the observed correlation in such a way that similar climatic conditions must have prevailed in both regions.

Page 7, lines 10-11: This is a bit strange suggestion or at least not explained properly. Central Europe cannot be taken as a reference, as no other regions. All regions have their own climatic history. The Greenland ice core records are usually taken as stratigraphic correlation targets as they have an unprecedented resolution, layer-counting chronology and reliable proxies.

→ Agree. We rephrased this paragraph to: “The Central Europe region acts as an example for the nine other regions. For further detailed information on the other nine regions, see Supplement S1-S9.”

Page 9, lines 11-12: I’m wondering why so many recent papers include one completely off-topic sentence about the migration of anatomically modern humans into Europe? Just one sentence pops up without any further discussion in most of these papers, including this one. This is pure hypothesis without any further evidence, therefore I strongly suggest deleting this sentence.

→ Agree, we deleted this sentence

Page 12, lines 17-19: Talking about Heinrich-events, these should be indicated in Figure 6. Also, from where do you know if these are H-events or not in the studied dust records? Timing?

→ As you suggest, we incorporate HE times within Fig. 6. Heinrich Events either have been interpreted by the original authors (Hodell et al. 2013; Collins et al., 2013 for Southern Europe, Portuguese margin, Collins et al., 2013 for NW-Africa region) or phases of increased dust fall within the timings of H-Events.

Page 13, line 4, “turning point”: Do you refer to tipping points here?

→ To our knowledge, turning point and tipping point is used in similar ways. We accept your suggestion and change this to tipping point

Page 13, line 19: I suggest deleting the Gobi after “China” (in parenthesis), as there many other deserts in China, including the Taklimakan, Tengger, Hobq, Mu Us etc. deserts.

→ We fully agree on that comment, as especially Taklimakan desert is more important on dust transport than Gobi.

Page 14, lines 13-17 and Page 16, lines 2-6: These text parts should go somewhere in the Methodology section, in my opinion.

→ We moved the methodological part of the model simulation to a new subchapter of the methods and extended it for more clarity (p.14, l.12-17). We do not want to move P.16,l.2-6 as they describe Table 3. Therefore, these lines should remain close to it.

Page 14, line 28: Which simulation do you refer to? Barron and Pollard's?

→ Yes, added this to the sentence.

## #Supplementum

Page 21, line 21: Records with tuned chronologies should be excluded, in my view.

→ There are no records used within this paper, which are exclusively dated by tuning. All records, which are used, are 14C or Th/U dated (or OSL dated for Jingyuan). Most of the data sets are correlated crosswise or tuned afterwards with other nearby data sets to strengthen the stratigraphy.

Page 22, lines 2-3: This is exactly the reason, which precludes unambiguous GI identifications in OSL-dated records, including Jingyuan in China. Such an "exercise" is difficult even using 14C-chronologies, having an order of magnitude lower uncertainties.

→ As you can see on Page 6, line 15 of the Supplement, we are aware of this. Therefore, Sun et al., 2010 talk about loess interstadial / stadial, which we also did for this paper.

## Technical corrections

Page 3, line 23: write "pollens" → Most Palynological papers use 'pollen' as plural, thus we follow this notation.

Page 3, line 30: dropstones? I would use "lithic clasts" or "detritus" or something like that

→ As IRD layers are not part of the aridity index but comparable to the records of Southern Europe and Portuguese margin. Therefore, the whole sentence has been deleted.

Page 5, line 19: write "varved" (same later) → Yes

Page 5, line 22: specify this abbreviation: Greenland Interstadial (GI) → This is done previously on Page 3 line 4-5

Page 6, line 2: write "caves" → Yes

Page 6, line 3: replace "strong precipitation amount" by "wet climate" or a similar expression → Yes

Page 6, line 5: write "beginning" (same below) Page 6, line 10: write "hiatus" → Yes

Page 6, line 14: replace "on" by "to" after "apparently" and use "underlying" instead of "overlying" → Yes

Page 9, line 18: I can't find these red bars. Or do you refer to figure 4? → Yes, we mentioned the wrong Figure!

Page 9, line 23: delete "bevor" and write "before" → Yes

Page 10, line 3, Figure 4 caption (second row), "red bars indicate high humidity": this should be aridity, I guess → Yes

Page 14, line 3: delete "at" and use "in" before "Central Chinese" → Yes

Page 14, lines 7-9: first half of sentence makes no sense, rewrite please → Rephrased to “In order to our reconstructed precipitation we employ the coupled climate model COSMOS which was developed at the Max-Planck Institute for Meteorology in Hamburg.” → deleted “with the large-scale pattern of model simulation,”

Page 16, line 12: “large humidity” is bad phrasing, write “increased humidity” or simply “wet phase” → Yes

Page 16, line 13: write “considerably” → Yes

## References

Breshears, D.D. et al., 2003. Wind and water erosion and transport in semi-arid shrubland, grassland, and forest ecosystems: quantifying dominance of horizontal wind-driven transport. *Earth Surf. Process. Landf.* 28, 1189–1209.

Moine, O. et al., 2017. The impact of Last Glacial climate variability in west-European loess revealed by radiocarbon dating of fossil earthworm granules. *Proc. Natl. Acad. Sci. USA* 114, 6209–6214.

Ujvari et al., 2015. Two possible source regions for central Greenland ice core dust. *Geophys. Res. Lett.* 42, 10399–10408.

Ujvari, G. et al., 2017. Coupled European and Greenland last glacial dust activity driven by North Atlantic climate. *Proc. Natl. Acad. Sci. USA* 114, 10632–10638.