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Solid Earth
Discussions

Interactive comment on "Sediment history mirrors Pleistocene aridification in the Gobi Desert (Ejina Basin, NW China)" *by* Georg Schwamborn et al.

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Thanks go to all reviewers and commentators! Find below our responses to individual issues raised by the reviewers and commentators:

RC1: 'Sediment history mirrors Pleistocene aridification in the Gobi Desert (Ejina Basin, NW China)', Attila Ciner, 18 Nov 2019 - Figure 9 shows a section a conceptual model illustrating the progradation of the Heihe AF into the Ejina basin. A map view of this model with different stages would help the reader to better visualise the development of Heihe AF in time and space.

AC1: We replaced the figure; now there is an oblique map view into the study area including different stages of alluvial fan formation. We hope to meet the reviewer's

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demand.

RC2: 'Review of Schwamborn et al Ejina Basin paper', Anonymous Referee #2, 09 Dec 2019 - Figure 1 needs a better image for part B, and a better representation of the major faults in the area. Some poorly-defined faults are sketched, but major and well-understood faults in the Heli Shan and the northern Qilian Shan are left out. Use a better color scale for the regional topography. Everything looks much the same in the crucial range. Adding a satellite image will help; the fan is beautiful on GoogleEarth, so it is poor that you can't even see it on a figure for the paper. Part A should be replaced by a map closer to the study area – cut out the regions beyond 30-50 N and 80-110 E, and replace by a more detailed map. Figure 1 really is bad...

AC2: We replaced the figure; now we use an optical satellite image. The new figure includes the delineation of the Heihe catchment. However, we removed any fault lines from the map figure, since the article focuses more on the sediment rather than the tectonic history. In the text we added more details on seismicity of the area (and references). We hope to meet the reviewer's requirements.

- Add more description of the lower contact with the Red Clay Formation. As noted in the text, the age of this unit is not well known, with published estimates ranging from Neogene to Cretaceous. But, this study describes intercalation at the base of the core; if correct, this implies a rapid but gradational transition from conditions at the top of the Red Clay to the undoubted Quaternary units. With this relationship, the Red Clay cannot be Cretaceous – at least in the study area.

AC2: We added more lithologic description at due place and refer to imagery of this core part; i.e. at the core bottom between 223.7 and 222.6 m red sandy clay with angular clasts occurs, which is interpreted a fanglomerate. This subunit has a sharp boundary with the grey (anoxic) medium sand layers overlying them at 222.66 m (see also hs.pangaea.de/Images/Cores/Lz/Gaxun_Nur/GN200_images_31-223m.pdf).

- A change in sediment type and provenance is linked to "opening" of the Heli Shan, about 1 Ma. Again, more description and discussion is needed here. A schematic figure would help.

AC2: We added a statement at due place in the text: The authors concluded that the Heli Shan opening occurred around 1.1 Ma BP and allowed the Heihe to flow northward into the Ejina Basin (Pan et al., 2016; Fig. 11 therein).

- I don't think Wang et al (2017) found any evidence for a Pleistocene stepwise uplift in the region – where did this claim come from?

AC2: We changed this statement to better reflect the reference. Now: Wang et al. (2017) suggest an emergence of the Qilian Shan during the late Miocene, the area where the Heihe (engl. = Hei River) evolves from its upper reaches on the northern flanks.

- The faults on the fan drawn in Rudersdorf et al (2017), reproduced here, are not credible: faults are shown right along the two major streams on the fan surface, there is no evidence for these structures.

AC2: We deleted the fault lines from the figure.

SC1: 'a few suggestions', Isla Castaneda, 02 Dec 2019 - I noticed that many details regarding the organic geochemical analyses are currently missing. The one methods reference cited is a textbook chapter that discusses only in very general terms how biomarkers are analyzed instead of the specific methods information pertinent to this study. o 1. Leaf wax concentrations: what were the methods used for extraction and column chromatography? How much sample was extracted? Were additional cleanup steps required to separate saturated and unsaturated alkanes prior to isotopic analysis? Where were the organic geochemical analyses preformed and what make/model instrument was used? Details including type of column, the oven temperature program and flow rates, and the types of internal standards run should be given. o 2. Leaf wax deuterium isotopes: The instrument make/model, column type, reactor conditions (temperature, carrier gas and flow rate), and isotopic standards analysed should be reported. Were the samples analyzed in duplicate or triplicate? What was the minimum peak size used (1000 mV)? How was instrumental error assessed? How often was the H3+ factor determined and what was its range during the analytical period? Was hydrogen isotope drift throughout the life of the reactor evaluated? The recent publications of Goldsmith et al., 2019 JGR Biogeosciences and McFarlin et al.,2019 Quaternary Science Reviews are good examples of the level of detail that should be included when publishing leaf waxδ2H data o 3. In Figure 8, the top two panels should plot the error for theδ2H measurements. For the other panels, it is unusual to present leaf wax concentrations as centered-log ratios. Plotting it in this manner makes it difficult to compare these data with other studies. Usually leaf wax data are presented as concentrations (ng or mg per g sediment extracted) or fractional abundances of the different chain lengths – adding such a plot would be helpful. o Finally, given that this is an endorheic basin, which should be highly sensitive to changes in aridity, and given that leaf waxδ2H at this location likely reflects both temperature change and shifting moisture sources, has theδ2H difference between a terrestrial compound (nC31) and an aquatic (nC19 or nC21) compound (εter-aq) been examined? This approach has been used by a number of studies to help identify past arid intervals (e.g. Thomas et al., 2018 GRL; Rach et al., 2017; Sachse et al., 2004) and it might work well in an endorheic basin.

A to SC1: For covering the n-alkane analysis more appropriate one more co-author has been called (M. Burke). Chapters 3.6, 4.4, and 5 have been revised to meet much of the comments of SC1. Figure 8 has been modified accordingly.

SC2: 'Suggestion for manuscript improvement', Christian Leipe, 03 Dec 2019 - I am aware that also pollen analysis was initially planned and supposedly also performed. However, no pollen results are shown in the submitted version of the paper. I am wondering why the authors did not include the pollen analysis results, if they are available?

A to SC2: Pollen results arrived late from lab (after first version of the manuscript), but they are indeed available now. We added the new chapters 3.7 Pollen analysis and biome reconstruction and 4.5 Pollen record, and more statements in the text at due place (see chapters 2 and 5). In addition, a new figure has been inserted: Figure 9. Percentage pollen diagram summarizing the results of pollen analysis presented in this study. . . For covering the pollen analysis appropriately, more co-authors have been called (M. Schlöffel, F. Kobe, P. E. Tarasov).

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