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Reply on RC2

Andre Baldermann et al.

Author comment on "Palaeo-environmental evolution of Central Asia during the Cenozoic: new insights from the continental sedimentary archive of the Valley of Lakes (Mongolia)" by Andre Baldermann et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-32-AC2, 2021

Review of paper Preprint cp-2021-32: "Palaeo-environmental evolution of Central Asia during the Cenozoic: New insights from the continental sedimentary archive of the Valley of Lakes (Mongolia)" by Baldermann et al.

RC2: Jeremy Caves Rugenstein

Baldermann and co-authors provide new data from the well-studied Valley of Lakes section in central-southern Mongolia to understand the sedimentological and paleo-environments during late Paleogene and early Neogene Mongolia. The authors find that a number of paleo-environmental indicators, such as CIA, track global climate signals, but that d18O and d13C do not; they conclude that stable isotopes of authigenic carbonates in this section reflect, to a much greater extent, uplift of the Altai and Tian Shan.

I found this paper easy to read; the figures support the text, and; the paper is well-referenced. I believe this paper is appropriate for a journal such as Climate of the Past subject to minor revisions. Below, I present a few comments, which I think will make the paper more robust. Please note that I am not an expert on Ar-dating of clays; I therefore restrict my comments to the paleo-environmental aspects of the paper.

We thank the reviewer for the very positive evaluation of our work. Below, we comment on the specific comments provided by the reviewer and demonstrate how we will revise the text of the manuscript accordingly.

I'm curious why the stable isotopes—particularly the d13C—do not track with the weathering indices, such as CIA. The authors interpret their d13C record in terms of precipitation; strictly, this isn't correct particularly over long timescales. Rather, d13C records the balance between atmospheric CO2 and the soil respiration flux (Cerling, 1999, 1984; Cerling and Quade, 1993). Over this time frame, changes in atmospheric CO2 need to be considered. However, for most of Asia, changes in plant productivity—probably driven by changes in the atmospheric CO2 via the CO2 fertilization effect—seem to be the larger driver of soil carbonate d13C changes (Caves et al., 2016; Caves Rugenstein and

Chamberlain, 2018). This is likely to have an effect on weathering, since plant-produced CO2 plays a vital role in breaking down primary minerals. Thus, it is curious why these weathering indices and d13C are decoupled, and some speculation from the authors on why would be helpful. We recently published a paper that dealt with this issue in the late Cretaceous Songliao Basin in NE China (Gao et al., 2021).

We fully agree but want to note here that Richoz et al. (2017) have commented on this issue: "From ~33 to 22 Ma, the atmospheric CO_2 concentration decreased from 800 to 200 ppm (Zhang et al. 2013), which should be translated in a trend towards lighter $\delta^{13}C$ soil values. We do not see this trend in our data, and thus, changes in aridification in Central Mongolia may have overprinted this effect." We will add the following explanation to the text for clarification (end of second paragraph, section 5.5): "We note here that the atmospheric CO_2 concentration decreased from 800 ppm to 200 ppm from ~33 to 22 Ma (Zhang et al. 2013), which should have shifted the soil carbonate $\delta^{13}C$ signatures towards lighter values. However, due to changes in aridification in Central Mongolia at the same time, this trend is not seen in the data. Indeed, an increase in aridification results in a restricted soil moisture content that can i) increase the $\delta^{13}C$ value of soil carbonates, ii) causes the plant productivity to decrease, which affects the ratio of atmospheric CO_2 to soil respired CO_2 and iii) reduced the formation depth of the soil carbonates and thus the relative contributions of atmospheric CO_2 and soil-derived carbon (Cerling and Quade 1993; Caves et al. 2014). Consequently, the $\delta^{13}C$ isotopic signature of the soil carbonate is ultimately linked to aridification pulses, which also affects the weathering intensity of the sediment source areas, explaining the inverse relation between the isotope record and the chemical alteration indices."

The relative lack of change in d18O is not too surprising. In such a continental, semi-arid setting as the Valley of Lakes, small changes in hydroclimate are unlikely to produce changes in d18O, given that most moisture is recycled in this setting and there is very little runoff. Such predictions for meteoric water d18O in continental settings has been detailed in a number of studies (Caves et al., 2015; Chamberlain et al., 2014; Kukla et al., 2019; Winnick et al., 2014).

We thank the reviewer for this excellent explanation and will add the following sentence after the aforementioned insertion: "On the contrary, large changes in the $\delta^{18}O$ isotopic record of pristine soil carbonates are not to be expected given that the hydroclimatic variations are small in the semi-arid setting of the Valley of Lakes and that most moisture is recycled (Caves et al., 2015; Chamberlain et al., 2014; Kukla et al., 2019; Winnick et al., 2014)".

I'm curious why the authors attributed many of the paleo-environmental changes to uplift of the Tian Shan and Altai mountains, rather than uplift of the Hangay mountains to the north. There is, of course, some dispute about the paleo-elevation of the Hangay mountains through time (McDannell et al., 2018; Sahagian et al., 2016) and my own work (Caves et al., 2014) suggests that the Hangay play an important role in blocking moisture to this part of the Valley of Lakes. Some discussion of why the authors have decided to attribute hydroclimatic changes to uplift of the Tian Shan and Altai versus changes in Hangay paleo-elevation would be appropriate and would be of interest to a broad swath of researchers who are interested in tectonics and paleoclimate in Mongolia.

We fully agree with the reviewer. We will add the following explanation after L 582 (section 5.6): "Moreover, the progressive uplifting of the Hangay mountains to the north ever since the early Oligocene also blocked Siberian moisture transport to the northern

Gobi, as inferred from $\delta^{13}C$ and $\delta^{18}O$ isotope records recorded in paleosol carbonates from different transects at the northern edge of the Gobi Desert and in the lee of the Altai and Hangay mountains, and consequently contributed to aridification of this area (Caves et al., 2014; Sahagian et al., 2016; McDannell et al., 2018)."

Minor Comments:

Line 90: I think you mean to cite Xiao et al., 2010 here.

We will change the reference accordingly.

Figure 8: How is the position of the dashed yellow, vertical lines in the d18O panel positioned? For the uppermost samples, is this line placed along the minimum values because there is evidence that there is evaporative effects for the higher d18O samples? What evidence is this?

The dashed yellow, vertical lines represent the moving average. We will move the line to the right of the $\delta^{18}O$ isotope record, thank you for this comment.

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