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Climate of the Past Discussions

Interactive comment on "Asian aridification linked to the first step of the Eocene-Oligocene climate Transition (EOT) in obliquity-dominated terrestrial records (Xining Basin, China)" *by* G. Q. Xiao et al.

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Dear editor,

We are glad to hear that all three referees were very positive about our manuscript stating that records like ours are rare though essential in understanding the atmospheric impact of Cenozoic climate change. Generally, also, they agree with the work done and minor comments are given. Below, we have tried to respond to all comments given by the three reviewers, which have been very useful to improve the paper.

Response to M. Fuller:

Dr. Fuller is very positive about our work in general and the magnetostratigraphic part in particular. No comments or recommendations are given.

Response to Referee #2:

Anonymous Referee #2 is generally positive as well, but gives minor corrections.

1) The referee doubts our correlation of the magnetostratigraphy to the time scale, while, in the mean time, Referee #1 states that the correlation looks good. Nevertheless, there is no doubt at all about the correlation. The correlation is not only based on the magnetostratigraphy presented here, but on an integrated set of data of different and much longer sections, in particular the Xiejia and Shuiwan sections extending from the Eocene up into the Miocene (Dai et al., 2006; Dupont-Nivet et al., 2007). An extensive justification of the correlation is already published in Dupont-Nivet et al. (2007). This primarily relies on the distinctive pattern of two long reversed polarity zones separated by a shorter normal zone that unequivocally correlates with C13n. In addition, the pattern fit is corroborated by smaller polarity zones, steady accumulation rates and some age diagnostic fossils found in the Eocene and lower Miocene part of the sampled sections. Alternative correlations are indeed investigated in Dupont-Nivet et al. (2007), but the C12r-C16n.1n correlation of the interval considered here remains robust whatever alternative is considered (see in particular the methods section of Dupont-Nivet et al. 2007). We understand the present text, that made only short references to these previous studies, might raise questions like these. Therefore, we rephrased the correlation paragraph including clearly these justifications.

2) The reviewer asks to provide details including declination and inclination in Fig. 4. We now provide these data in a Supplementary Table that can be easily plotted for readers preferring this layout over the VGP latitude. Table also indicates thermal demagnetization range and steps, VGP latitude and statistical parameters on the ChRM directions.

3) The reviewer asks to provide parameters of reversal test for the ChRM in Fig. 3. The

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parameters are now indicated in the caption of Fig. 3. The figure caption has changed to: "Figure 3. Equal-area projections of characteristic remanent magnetization (ChRM) directions (downward (upward) directions are shown as solid (open) circles) of 226 samples from the TS section (a) and reversals test (b). The angle between the mean of the normal polarity set and the mean of the reverse polarity set is 2.3°. The critical angle is 5.2°. The probability of exceeding this angle is 0.558 resolved between 5° and 10° thus defining a B-class reversals test (McFadden et al., 1990) and a positive reversals test at 95% confidence as indicated in lower panel (Tauxe, 1998)."

4) The reviewer asks to explain the open circles below VGP Lat. in Figure 4. Open dots are rejected and full dots are reliable VGP lat. As indicated in the text, these VGP latitude with open dots were rejected if they were more than 45° from the mean VGP or if they are isolated directions (polarity zones are defined by at least two successive reliable directions of same polarity as indicated in the text). The ChRM inclination and declinations of these samples are provided in the supplementary Table. The figure caption has changed to: "Figure 4. To the left, lithostratigraphy of the TS section showing dominant red mudstones with intercalated gypsum and gypsiferous mudstone beds arbitrarily labeled G0 to G23. Composite depth scale refers to a much longer section not shown here. In the middle are shown the magnetic susceptibility (MS), median grain size (MGS), and redness reflectance (a*) records with grey bars indicating gypsum bed intercalations. To the right, VGP latitude results from paleomagnetic analysis with full (open) dots indicating reliable (rejected) VGP latitude (see text). Polarity - interpreted polarity zones with black (white) for normal (reversed) result and grey for uncertain result. Normal (reversed) polarity zones labeled N1 to N4 (R1 to R4)."

Response to M. Sprovieri:

Dr. Sprovieri is also generally very positive about our MS. He gives some minor comments for improvement.

1) The reviewer states that we should improve the regional correlation of the three

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sections given in Figure 5 of our manuscript. However, we can not improve this correlation as we tried to gain a bed-to-bed scale without over-interpreting the data. The lithostratigraphic correlation lines are given on Figure 5 at a broader scale of packages of beds that undoubtedly correlate between the sections, while the cycle numbers for each gypsum bed represent the detailed bed-to-bed correlation. In the present Figure 5, we decided not to draw lines for this bed-to-bed correlation as minor differences indeed exist between sections at this scale and uncertainty would be introduced. In the text of the revised MS, we clarified our points.

2) As asked for, we now provide a lithostratigraphic column in the Figure S1. The reviewer further wants more explanation on the tuning procedure in order to convince the reader on the cyclostratigraphy. However, we do not tune the section in our cyclostratigraphic approach. Time series analyses and bandpass filtering are done in the 'untuned' depth domain. Afterwards, only for the comparison with marine records, the age model is slightly adapted with adding two supposedly missing obliquity cycles.

3) Dr. Sprovieri asks for more detailed climate-ocean dynamics that could have produced the observed teleconnection between the global climate change and lithofacies change in our section at the first step of the EOT. This is an excellent point that has yet to be solved. Dupont-Nivet et al. 2007 already stated that "The growing evidence that global cooling is associated with continental aridification along with intensification of monsoons and increased regional erosion remains to be tested by climate models." They further suggested the simple idea (along the lines of Lawrence et al. 2003) that cooling of global ocean temperatures at the EOT may generally reduce moisture transport to continental interiors. Unfortunately, we are not aware of climate modeling studies that specifically confirm that hypothesis and look at the impact of the EOT on the precipitation and moisture transport to Central Asia. This is now mentioned in the revised paper.

4) The reviewer suggests to evaluate our age model by comparison the potential 1.2-Myr obliquity cycle in TS section with the astronomic target. This is a good suggestion,

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however, our section contains only ∼2 Myr, which is less than two 1.2-Myr cycles. The minimum of the 1.2-Myr cycle indeed occurs at around the EOT as was shown previously by Coxall et al. (2005). However, it is not possible with the present data and length of the section to confirm or construct an astronomical correlation.

5) The reviewer would like to see open ocean records corroborating our results, with which we fully agree. Unfortunately, these do not yet exist, as far as we know. The results of Pälike et al. 2001 come closest and clearly show that after ∼36.6 Ma (GTS04 age; top chron C17n.1n) obliquity was the dominant cycle in Blake Nose, Atlantic Ocean, which these authors relate to incipient ice sheets and is in line with our findings (discussed in the text).

Interactive comment on Clim. Past Discuss., 6, 627, 2010.