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Climate of the Past Discussions

Interactive comment on "Terrestrial responses of low-latitude Asia to the Eocene–Oligocene climate transition revealed by integrated chronostratigraphy" *by* Y.-X. Li et al.

A. Licht (Referee)

alicht@email.arizona.edu

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The paper provides interesting new paleomagnetic data from the Maoming Basin, China, to locate the stratigraphic interval of the critical Eocene-Oligocene Transition (EOT). I must first say that I am sympathetic with the effort made by the authors to identify and study the EOT in the East Asian sedimentary record, because this event is virtually undocumented in continental Southeast Asia and is particularly critical to understand the impact of Eocene Greenhouse conditions on the proto-monsoons. Accordingly, the topic of this paper is potentially suitable for CotP. However, I think that the manuscript still needs a fair amount of work to make it ready for publication. First,

many important details about the sedimentology and the biostratigraphy of the localities are lacking. I acknowledge that a big part of this initial work seems to have been previously published in Chinese journals, but this work is not available for the common, non-Chinese reader and needs to be synthesized and summarized (at least in the introduction of the paper). Moreover, this paper has some critical issues with specific scientific points that significantly weaken their paleomagnetic correlations and I am not sure that there is the potential for the authors to address these concerns by reorganizing their arguments or providing more data.

Sedimentological interpretations

The main -and critical- sedimentary change in the studied section is a shift from lacustrine to deltaic conditions, eventually attributed to the EOT. But the sedimentological part of the paper is very weak, and most of the sedimentary interpretations are referred to a Chinese MS thesis. The results of this previous study must be synthesized, with a clear explanation of the different lithofacies / architectural units that are found in the basin. Among the questions that remain unanswered: -What is the environmental interpretation of the different facies that are described by the authors? "lacustrine" and "deltaic" are too vague and do not qualify facies. For instance, how are the "massive sandstone" beds of the Haungniuling Fm interpreted? Are those channel body, mouth bar, or delta front deposits? What is their lateral extent?

-How do the authors interpret their "parasequences" in terms of deltaic environment? note that fining-upward sequences as they are described in the paper are not very common in deltaic setting. The few information provided in this paper would rather suggest sequences made of stacked channel bodies, and thus a fluvial environment.

-the authors described a colored mudstone layer at the interface between both Paleogene units. Could it be a paleosol? If so, that would significantly change their paleomagnetic correlation; if not, what is it?

-Whatever is the origin of the "parasequences" in the Haungniuling Fm (fluvial or deltaic), channel / delta mouth migration is not necessarily controlled by orbital forcing. Avulsions /migrations can be endogenic as well. Orbital forcing must be shown, for example by proving that parasequences alternate with a regular period that corresponds to one of the Milankovitch periods. But there is no data about the frequency of parasequences in the Haungniuling Fm, neither a clear log of the unit.

Weaknesses of the paleomag correlation

The chronostratigraphic correlation proposed in this paper is based on several assumptions that are not very well addressed and should be discussed in more details.

-The authors claim a "late Eocene" age (what is their definition of "late" Eocene? Upper Eocene?) based on one fossil mammal: Lunania youngi. I can not read the original papers relating this discovery (in Chinese), but Russell and Zhai attributed this taxa to the Middle and Upper Eocene of China in their anthology of 1984 ("The Paleogene of Asia"). Note that Lunania are still poorly described and understood (Remy et al., 2005, CR Palevol), as well as their exact stratigraphic range. Moreover, the study of pollens from the Maoming Basin by Aleksandrova et al (2014, Stratigraphy and geological correlation) attributed the Youganwo Fm to the Lutetian / Bartonian and the Haungniuling Fm to the Priabonian. It thus appears to me that the biostratigraphic context contradicts the authors' correlation.

-The authors argue that sedimentation rates in the Haungniuling Fm should be higher that in the Youganwo Fm because of "changes of lithology" and coarser grain-size. This assumption is clearly incorrect. Changes in lithology and grain-size increase can be caused by simple paleoenvironmental changes (lake level fall, for example) without any change of sedimentation rate.

-Finally, the authors argue that accumulation rates above 1.5 cm.kyr-1 are too high for oil shales. This is incorrect as well. In lacustrine context, accumulation rates can be up to 5-10 times higher. See, for example, the accumulation rates in Paleogene deposits of the Greenriver Basin, Wyoming. For all these reasons, it appears to me that almost

all the other chronostratigraphic hypotheses introduced in the paper are as pertinent as the one that is eventually proposed. Actually, Hypothesis 1 (previously rejected) seems the most reliable, because it works with Aleksandrova et al's pollen study and yields reasonable accumulation rate estimates.

Paleoclimatic discussion

The paleoclimatic interpretation of the correlation proposed in the paper is virtually non-existent. Among the questions that should be addressed:

-How do the authors explain the shift from lacustrine to deltaic at the EOT? What does this mean for the hydrological cycle?

-How to explain the impressive increase of accumulation rates at the EOT, if their correlation is right? -How does it compare with other records in East Asia?

-What does the hypothetical eccentricity signal found in their section mean in terms of paleoclimate? How does it compare with other contemporaneous orbital record?

Finally, a few additional comments:

-The authors state that the magnetostratigraphy of the area was already study by Wang et al. (1994). They should clearly indicate what has been done in that study and where, and how it overlaps with their own work.

-Table 1 should be reorganized (It is unclear, too much infos in parentheses), Fig. 2 should be enlarged (and subfig 2d should be explained).

The scientific English writing is for me comprehensible. I am not a native English speaker so I leave this to the discretion of the editor. I have noticed a few spelling mistakes, as well as unclear statements, suggesting that the manuscript should be proof-read by an English speaker. My feeling is that I am not sure that this manuscript can be saved, unless the authors succeed to clean their sedimentological interpretations and strengthen their correlation by additional biostratigraphic data.

A. Licht

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