The revised version of Li et al.'s manuscript is much clearer and easier to read than the first draft. The authors have been doing a great job simplifying their discussion. The introduction and geological context are now well explained, and all the important details that were lacking about the sedimentology, biostratigraphy, and the previous work in the area are present in the manuscript. However, I still have major concerns about critical issues that were not addressed during the first round of revision. I will focus my comments on the sedimentological part and on the discussion (I am not competent to discuss magnetic properties matters).

The sedimentological part of the manuscript (section 4.1) is still very weak.

1) There is still a mix of description and interpretation throughout the entire section. Please separate both.

2) Most of the sedimentological description is still based on colors (lines 21-30 page 7). You need to use lithofacies (including the description of grain-size, sedimentary figure and texture, thickness and size of the beds etc...). You can not base paleoenvironmental interpretations on descriptions that are mainly based on sediment color.

3) The 'log' in Fig. 2 is not a sedimentary log. Please make a standard sedimentary log with varying grain-size and displaying sedimentary textures. If your red layers are finer grained than the rest of the section, describe them as fine-grained and display them as finer-grained on Fig. 2. You have partly based your correlations on grain-size consideration. You must provide (at least qualitative) grain-size data in your log!

4) You argue that there are cyclic red beds in the Huangniuling Fm, but a) you do not provide any frequency data for this unit; b) your log from the Huangniuling Fm is incomplete and has no scale (please provide a real log for this unit, not a schematic one); c) the ~30 m log of the base of the Huangniuling Fm that is provided in Fig. 8 does not display any clear cyclic pattern.

5) Stop using evidence for orbital forcing in other Eocene sections as a justification for orbital forcing in yours! (pages 9 and 14). You can find sections with evidence of orbital forcing for every geological period, it has nothing to do with the late Eocene. Actually, most of these other sections do not display the same kind of orbital forcing (for instance, in Xining -the closest late Eocene section, obliquity is the prominent orbital parameter in the record). This should be discussed.

6) You keep using the word 'parasequence' without describing them. If you have identified parasequences in your section, make them clear. Evidence of parasequence IS NOT evidence for orbital forcing, as discussed during the previous round of comments.

Their preferred correlation for the GPTS is still poorly justified.

Note that I think that the correlation chosen by the authors might be right, because I would expect a major hydrological disturbance at the EOT. However, nothing in the present manuscript has convinced me of the accuracy of their choice. All the scientific arguments that are brought by the authors to justify their choice are either controversial or fallacious.

1) As I already discussed during the previous round of revision, **increase in grain-size does not imply increase in sedimentation rate**. The authors argue *"the sedimentation rate for mudstone is generally slower than that of sandstone"*. This is a big mistake. There is no room here for a lengthy lesson on sedimentology, but I invite the authors to read Miall's books about deposition dynamics in continental setting or any general book about continental sedimentology basics. The coarsening upward trend in grain-size that you observed in your section likely reflects the shift from lacutrine to deltaic setting. If you want to show that there is a major change in erosion dynamics and a potential increase of sedimentation rate at the transition between both geological units, you need to show that the increase of grain-size is basin-wide, and provide logs from different parts of the basin.

2) The reply of the authors about accumulation rates in lacustrine context is not satisfying. Accumulation rates are just 2-3 times higher in hypothesis 5 compared to hypothesis 6. This is nothing compared to the actual range of accumulation rates in lacustrine environments. The authors argue that hypothesis 6 is more relevant because it has sedimentation rates that are closer to mid-Cretaceous oil shales from the same area. But I do not see why those mid-cretaceous oil shales can be use as analog; as I already noted last time, accumulation rates in lakes are highly variable, in profundal or nearshore setting.

3) I acknowledge that the authors have made a great effort in describing the paleontological context of the basin. However, their biostratigraphic considerations are based on one single taxon (*Lunania*) found only in two other basins. Though one taxon is better than no taxon at all, this is still too weak to make this a key argument for the correlation.

After this first round of revision, almost all the other chronostratigraphic hypotheses still look as pertinent as the one that is eventually proposed.

The paleoclimatic discussion is still weak. The authors do not discuss the meaning of obliquity forcing in their section (cf, for ideas of interpretation: Zachos et al., 2008, Nature; Abels et al., 2011, Paleo3; Xiao et al., 2010, CotP). They do not explain the reasons of a potential increase of aridity at the EOT. Is it related to summer or winter rainfall, to monsoonal dynamics? e.g. Huber and Goldner, 2012 JAES, Licht et al., 2014, Nature. What about the increase of accumulation rates: how does it compare with other Asian records? e.g. Peter Clift's paper in the South China sea, Metivier's paper in southeast Asia and Tibet.

Finally, the scientific English writing is for me comprehensible, but there are many repetitions throughout the paper and a few paragraphs that are particularly unclear (cf the legend of Fig. 2).

After this round of revision, my feelings have not changed and I think that this study needs more biostratigraphic or geochronological data to constrain the chronology of the section. I am clearly not convinced by the current set of arguments brought by the authors.

Alexis Licht