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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.

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In my review I will concentrate on the rock- and palaeomagnetic part. For the sedimentological and biostratigraphic part I am not a specialist but I feel the discussion stimulated by the comments of Prof. Licht and the future response by the authors will considerably improve the paper. This is true also about Prof. Licht's comments on the weakness of the palaeomagnetic correlation.

The topic of the paper fits the framework of CotP. However, I am not fully convinced by the presented results, especially on the palaeomagnetic part of the paper. In this part the paper needs more work before it is ready for publication.

The applied methods in palaeomagnetism are well described and fulfil modern standards. The thermal behaviour of susceptibility was only measured for two samples in the whole profile. In my opinion this should be enlarged by measuring many more samples to have a well established rockmagnetic profile. The described lithological units are not homogeneous enough to be thoroughly described by only two samples. I am sure that the authors have measured more K/T curves; did they all show the same behaviour?

What is the reason for the repeated occurrence of sedimentary rhythms and why are they correlated to peaks in susceptibility? If the reddish colour represents weathering, is there some hematite formed colouring the beds? The fact that fresh exposure of the beds shows no reddish colour hints on a present event. Why did you not investigate the rock magnetic characteristic of these rhythmic occurring beds?

Remarks about the rockmagnetic chapters

4. - IRM for oil shale did not enter saturation before 800 mT. - The dominant magnetic minerals show coercivity around 40 mT.

I am not at all convinced by your interpretation of Fig. 4b. Peaks of the heating and cooling curves show quite different characteristics between 500 and 580°C. Your K/T curve also shows a newly-formed phase (due to heating of the sample) and it is not dominated by the phase that was studied by the IRM acquisition.

Your interpretation of titanomagnetite is not convincing. Do all K/T curves show similar behaviour? Is the K/T curve that you present in the paper the best? Conversion at 400°C during heating more likely represents the formation of a new magnetic phase than in situ titanomagnetite. The clearly higher K signal at room temperature after the heating cycle hints at newly-formed magnetic minerals.

Your interpretation of the K/T curve of the oil shale (17.2 m) also raises questions. Again your susceptibility value at room temperature is near to zero – this is suspicious of newly-formed minerals during the K/T experiment. In the case of hexagonal pyrrhotite, which is characterized by the sharp peak at 240°C, you should see the ïĄň-transition to monocline pyrrhotite. This alone is distinctive and diagnostic of hexagonal pyrrhotite (Dunlop and Özdemir 1997). During further heating the irreversible oxidation of monocline pyrrhotite to magnetite should occur. This is not shown in your K/T curve. In your paper you argue that the sharp decay at 350°C - derived from your first differentiate, which, in my opinion, is dispensable here (no additional information in comparison to fig. 4c) - indicates the presence of greigite. Roberts et al. 2011 name different parameters (Mrs/Kappa; hysteresis parameters; no low temperature transition) to be diagnostic of greigite, but this is not addressed in your rockmagnetic chapter. In my opinion your evidence of greigite is not convincing enough and should be better justified.

Please use mA/m instead of A/m, because it is better to read.

The discussion of your demagnetization experiments is comprehensible. But its interpretation depends on your rockmagnetic statements and that should be strengthened.

5.

Your discussion of your magnetozones is transparent. The correlation of these magnetozones to the standard GPTS is difficult to follow. The problems recognised by Prof. Licht in relation to your basic assumptions, i.e. the correlation of the magnetozones to chronostratigraphy seem to me to be correct, and I do not feel competent enough to argue for or against that argument. Why not try a cyclostratigraphic study, especially in the oil shale, to estimate the sedimentation rates based on susceptibility values, for example (keyword sliding window technique)?

Your construction of a geomagnetic polarity timescale is hard to read. I suggest you describe your technique on one example and then refer to this and describe in few words your data listed in Table 1. This would make your discussion more readable.

6. The missing palaeoclimatic discussion is well described by Prof. Licht. I have nothing more to add.

Final comments:

The results of the study carried out by Wang et al. (1994) should be taken into account and/or matches and contradictions should be described.

Shorten the discussion of the magnetozones and their stratigraphic correlations, by using a better constructed Table 1, and avoid the lengthy descriptions of your different correlation possibilities.

Since I am not a native English speaker myself I have had good experience of using professional journal experts to edit my texts. This should be considered in this case too.

The paper deserves to be published after a thorough revision.

Interactive comment on Clim. Past Discuss., 11, 2811, 2015.