Reply to Reviewer Dr. Rolf

We thank Dr. Rolf for his constructive comments and we are very grateful for his time, efforts, and suggestions. Dr. Rolf's comments are mainly focused on the rock magnetic and paleomagnetic part of the manuscript. Dr. Rolf criticized that 1) our interpretations of rock magnetic data are not fully convincing; and 2) the description of correlations of magnetozones with the standard geologic polarity time scale (GPTS) is lengthy and is hard to follow. We will address these concerns by providing additional rock magnetic data to strengthen our interpretation as well as by presenting a concise description of correlations and better-organized Table 1 to improve the clarity of presentation. Below are our point-by-point responses (in blue) to Dr. Rolf's comments.

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 rock magnetic 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?

We appreciate this comment. Visual inspection appears to show mainly two types of lithologies. As such, one sample from each type was chosen to measure the K-t data. Additional K-t data are being measured to better characterize the thermal magnetic properties of the profile. The additional data will help detect detailed changes in lithology in the profile.

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?

The reddish color likely represents recent weathering because fresh exposure of these beds does not show reddish color. Also, it is reasonable to believe that hematite may have been formed during weathering based on the facts that reddish color occurs and magnetic susceptibility values increase in these beds. Weathering may have enhanced the subtle changes in lithology by making them more distinctly visible. The repeated occurrence of sedimentary rhythms is intriguing and was probably related to fluctuating lake levels that can cause subtle changes in deposition, thus in lithology. We did not specifically investigate the rock magnetic properties of these reddish beds because, at the time of sampling, our primary goal was to collect fresh rock samples to obtain reliable paleomagnetic data for the magetostratigraphic study.

Remarks about the rock magnetic 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.

Additional K-t data are being measured to better characterize the thermal magnetic properties of the samples. Also, the experiments of thermal demagnetization of composite magnetization acquired along three orthogonal axes, i.e., Lowrie test (Lowrie, 1990), are being conducted to better identify the magnetic mineralogy. These additional rock magnetic data will provide new constraints on the interpretation of magnetic mineralogy.

Lowrie, W. (1990), Identification of ferromagnetic minerals in a rock by coercivity and unblocking temperature properties, Geophys. Res. Lett., 17, 159–162.

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 λ-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 rock magnetic chapter. In my opinion your evidence of greigite is not convincing enough and should be better justified.

We appreciate this comment. Additional experiments including the low-temperature measurements and magnetic hysteresis measurements will be conducted (these experiments have been scheduled and will take place after Oct. 7 because of the national holidays from Oct.1 to 7). The new data from these additional experiments will help better interpret magnetic mineralogy of the samples.

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

This has been revised as suggested.

The discussion of your demagnetization experiments is comprehensible. But its interpretation depends on your rock magnetic 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)?

Similar to the situation for the Huangniuling Fm, the lack of at least two numerical ages from the investigated Youganwo Fm does not allow precise determination of sedimentation rate. So a cyclostratigraphic study of MS values in the Youganwo Fm may detect sedimentary cycles only in depth scale, i.e., cycle wavelength, not sedimentation rates.

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.

We appreciate this comment. Following your suggestion, we have shortened the description of the correlations and re-organized Table 1 in the revision to improve the

clarity of presentation.

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

We have elaborated on the paleoclimatic discussions in the revision.

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.

More details of Wang et al. (1994) will be included in the revision.

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.

We appreciate this comment. We have revised Table 1 to make it easier to follow. Accordingly, description of different correlations is shortened as well.

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.

Thank you. The English will be improved in the revision to make it more readable than the initial version.

The paper deserves to be published after a thorough revision.

Thank you. We hope you would be pleased to see the improvements that we have made for this paper.