Reply to Reviewer Dr. Jovane

We thank Dr. Jovane for his helpful comments and we are grateful for his time, efforts, and suggestions. Dr. Jovane's comments are mainly focused on the cyclostratigraphic component of the manuscript. Dr. Jovane questioned that 1) details of our "close inspection" of reddish layers in the Youganwo Fm are lacking; and 2) statistical analysis of cycles is lacking. We will address these concerns by clarifying our field investigation of lithology and interpretation of the rhythmic occurrence of the reddish layers in the Youganwo Fm as well as by providing spectral analyses of the depth series of magnetic susceptibility data to strengthen our interpretation. Below are our point-by-point responses (in blue) to Dr. Jovane's comments.

The paper is well written and fluid. The topic is interesting and deserves to be published.

The topic is indeed interesting and our work could make important contributions to understand the Late Eocene environment in low-latitude Asia and better understand the impact of Eocene greenhouse conditions on the proto-monsoon.

However, the paper does not present at this stage a mature study. This study pretends to show a lacustrine response to global paleoclimate in a continental Asia stratigraphy sequence based on paleomagnetism and cyclostatigraphy. The paleomagnetic work is reliable and solid. Nevertheless, the cyclostratigraphy is not ready yet.
There are two major lacks in this study:

- The poorness of the phase relation. They associate the cycles to couplets of red beds, which they states, without any proof, that are represented as to weathered bands related to fresh exposure. They says that this is build up from a "close inspection" but I do not see any sedimentological data to show this inspection. If cycles are represented by red beds, then the authors really have to make an effort to explain the environmental process that is behind the cycles and red beds, which they just go around avoiding a real explication of the phase relation.

The confusion probably arose from the wording of "close inspection". The rhythmic occurrence of reddish layers on the outcrop is striking at distance (Fig. 2). The wording "close inspection" was meant to say that when we approached the outcrop and looked at these reddish layers closely, we found that reddish color occurs only at

the surface of these layers because once the reddish surface is hammered off, fresh exposure of the same layer does not show reddish color. Therefore, the reddish color should represent recent weathering, not the depositional signature. But weathering did help enhance the expression of subtle changes in lithology and make the subtle lithological changes more distinctly visible on the outcrop. The repeated occurrence of these sedimentary rhythms was probably related to fluctuating lake levels that can cause subtle changes in deposition, thus in lithology. In the revision, we will clarify this issue and provide an explanation for the processes that caused the sedimentary rhythms.

- The absence of spectrum, tuning and the wrong calibration. The cycles must be expressed as simple statistical analysis of a depth or temporal series as a frequency analysis, which they do not make neither on the magnetic susceptibility nor to the lithology. This quick exercise is needed to support the cycle analysis they underway. The mathematical approach would also allow them to attempt a tuning of the sequence to the target cycles of Laskar. Surprisingly, at the end of the magnetotratigraphy they show an astronomical calibration of the EO boundary, which is completely non-sense since they even do not show a spectrum of the depth or time sequence.

Spectral analyses of the depth series of magnetic susceptibility data will be performed in the revision. Together with the observed sedimentary rhythms, the cyclostratigraphic component of the paper will be strengthened.

As to calibration, maybe it was not presented clear enough because we did not intend to calibrate the EO boundary per se. Our goal was to find the best correlation that can satisfy most, if not all, of the constraints by examining six possible correlations. Ensemble 6 happens to present a correlation that places the transition at the EO boundary and yields sedimentation rates, using which the observed sedimentary rhythms would be in short and long eccentricity bands. Since orbital cycles have been observed from records of similar time intervals in other regions, this line of evidence adds more weight to Ensemble 6 and makes it the preferred correlation.

The paper should not be accepted if those two major issues are resolved.

Then, minor corrections are also requested to improve the paper:

The title
They uses “responses” should be singular because they study only one process. If there are different processes aging in the upper and lower part of the section they have to study them sepatelly.
“of low-latitude Asia” should be “at low-latitude”. Is the English correct?

The above and other language related concerns raised by Dr. Jovane will be taken care

of in the revision. The English language of the revision will be polished by a native English speaker.

I do not see much integrated chronostratigraphy because I see a quick lithology description and not a lithostratigraphic analysis, the biological part is quite poor and uses previously published data and the cyclostratigraphy is completely lacking. Consequently, I only see a tentative magnetostratigraphy and not an "integrated chronostratigraphy".

The reason why we think an integrated chronostratigraphy is better because none of the lithostratigraphy, biostratigraphy, or cyclostratigraphy *alone* can provide a refined chronological framework for the studied section. For example, magnetostratigraphy *alone* will only yield magnetozones that cannot be exclusively correlated with the standard GPTS. Similarly, biostratigraphy *alone* cannot provide adequate temporal resolution needed. Cyclostratigraphy *alone* will only provide duration estimates, thus can only serve as a floating timescale, if there is not an anchoring point. However, if these different types of records are used jointly, i.e., integrated, it is possible to establish a refined chronological framework. As we show in this study, 6 ensembles of correlations are possible and Ensemble 6 is preferred because it can satisfy most, if not all, of the constraints from the different types of records.

In the revision, the lithostratigraphy and biostratigraphy part will be strengthened following Dr. Licht's suggestions. For the cyclostratigraphy part, as mentioned above, besides the improved description of sedimentary rhythms, spectral analyses of depth series of magnetic susceptibility will be performed to strengthen the cyclostratigraphic component.

Abstract
Line 1: "clues to the impacts" not correct
Line 15; "reduction in hydrodynamics in low-latitude regions", their results do not show this! This sentence is not true! The abstract should re-written on the base of the two major improvements needed.

The phrase will be changed to "drying conditions in the low-latitude". This statement is based on the fact that a drastic change in lithology is observed between the oil sale dominated Youganwo Fm and the overlying siltstone and sandstone dominated Huangniuling Fm. The drastic change in lithology indicates that the depositional environments changed from deep lake, shallow lake, to delta front to delta plain settings, suggesting the overall shrinkage of the lake and most likely implying a gradually prevailing drying condition of the region.

With the additional data in the revision, the abstract will be revised accordingly.

Results

Line 22: how MS can facilitate the characterization of sedimentary rhythms? Here they need spectral analyses to proof it!

The reddish layers correspond to high magnetic susceptibility (MS) values. Less reddish levels display relatively lower magnetic susceptibility values. Thus, MS facilitates the characterization of sedimentary rhythms. As mentioned above, spectral analysis of the depth series of the magnetic susceptibility data will be performed in the revision.

Line 25: which close inspection? Show some sedimentological data!

The sedimentary rhythms are best seen at distance (Fig. 2c, e). When we came closer to the outcrop and examined the reddish layers, we found that the reddish color of these layers only occurs at the surface. When the reddish surface of these layers is hammered off, the fresh exposure of these layers show no reddish color. So the weathering is recent and the rhythmic occurrence of reddish color reflects rhythmic subtle lithologic changes, which would otherwise be difficult to discern at the outcrop.

Thanks to the recent weathering that makes the subtle lithological changes distinctly and expressively visible, our high-resolution measurements of magnetic susceptibility at the outcrop surface can capture the subtle lithological changes.

Line 26: weathering banding means continental exposure like paleosols? In this case the environmental interpretation is completely different!

As mentioned above, weathering bandings of the Youganwo Fm are the surficial features that were recent not during deposition. These surficial features enhance the subtle lithological changes and make them well expressed by distinct rhythmic occurrence of reddish color. Since the banding was not depositional in origin, it is not related to continental exposure.

Page 2819 line 1: the beds are red or not? Unclear sentence and scientifically weak! This is a major point!

The surface of the beds is reddish and the fresh exposure of the beds is not reddish.

Line 2: subtle compositional of what?

This was meant to say subtle lithological changes. This will be revised.

Line 4: which depositional environment? What cause the cycles? This is not explained at all! Is the level of the lake changing in elevation exposing sometimes this area? At this point is fundamental that they show some pictures and geology of the area where

they show that the strata are really continuous and horizontal. The lower part of the session, which is supposed to be deeper water, how can express paloesols?

Lake level fluctuation was likely the cause for the sedimentary cycles. Yes, the Youganwo Fm was deposited in a semi-deep to deep lake environment. As mentioned above, recent weathering makes the subtle lithological changes distinctly and expressively visible. So the reddish layers are not paleosols. The field photos of these strata are shown in Fig. 2 c-e.

Line 7: what makes them consolidated? CaCO3? Should be good to compare magnetic susceptibility with CaCO3 to build up the phase relation.

It is by the appearance of this layer because it often sticks out the outcrop, indicating that this layer is more resistant to weathering than the neighboring sandstones.

Line 11: rewrite the sentence: “massive sandstone cycles vary”

From line 10 to line 11, we describe the thickness of the massive sandstone, not the cycle, varies from decimeters to meters. The massive sandstone together with a red layer and a thin mudstone bed constitute a parasequence, or a cycle (lines 13-14).

Line 12: thinner mudstones bed… is it a cycles? They have much better to explain this too!

Lines 13-14 provide the explanation that a thin mudstone bed is part of a parasequence, or a cycle.

Lines 17-20: this sentence is already said before. Line 21: what they mean for “ traced to the center of the basin”? ~50 cm seem to me quite abrupt for a geological change also if continue… have they considered a tectonic uplift?

These will be revised. The ~50 cm interval presents a gradual transition from brown grey mudstone to pale grey mudstone. And the pale grey mudstone is capped by siltstones and sandstones that show a coarsening upward trend. These features suggest that gradual environmental transition was the most likely cause because tectonic uplift would lead to sharp changes, not a gradual transition.

Line 27: I really do not see any data that show the lacustrine environment and the sedimentological characteristics of the transition. This point is fundamental and need to be resolved!

Detailed sedimentary facies analyses were carried out by Guo (2006), which we cited in the paper to interpret the depositional environment. The lithological changes of the transition are described in lines 20-26 and are shown in Fig. 2d.

Page 2820 line 24: the curie temperature of ~500C?? what is this? Wrong statement!
Line 26: it is not clear which are the proof to affirm that is it titanomagnetite and not only magnetite?
Page 2821 line 7: it does not seems to me that it is a “rapid increase between 580-500”. Line 21: write “the NRM intensity f the samples…” It does not seams to me they spell NRM.

The inference of this part of the rock magnetic data was mainly based on Hrouda (2003) (Page 2820, line 26). Additional rock magnetic experiments (e.g., k-t, Lowrie test etc) are being conducted and the additional rock magnetic data will help better constrain the interpretation of magnetic mineralogy of the samples. Also, NRM will be spelled out at its first occurrence in the revision.

Discussions

Page 2822 line 3: they stated before that there is titanomagnetite as main magnetic carrier and now they say iron sulfides? I do this that there is still lot of magnetite carrying the characteristic magnetic signal… but if they really think that the main carrier is iron sulfides they have to do more magnetic researches. Line 25: prolate occurrence is written in a weird way.

Titanomagnetite was for the uppermost part of the Youganwo Fm and the Huangniuling mudstones (Lines 4-6), whereas iron sulfides were for the Youganwo oil shale (Line 3). Additional rock magnetic experiments are being conducted to further constrain the interpretation of magnetic mineralogy of the samples.

Page 2824 line 4 and 27: “late” or “middle” Eocene are not capital letters.
Line 10: “pebbly coarse sandstone” that means that a strong energy depositional process was aging meaning that probable also a lot of erosion… this must be considered in the cycle reconstruction.

We appreciate this comment. It is possible that erosion could take place at the edge of the lake during the same time interval. Since the investigated section is located at a depositional center of the basin, erosion at the study site during this stage of the Huangniuling Fm would be less likely.

Line 14: “sediment composition”… where? Do I miss something? I have not seen any sedimentological or compositional analyses like XRD or XRF. Each affirmation must be scientifically proven!

We meant that the Youganwo Fm is predominately oil shale and lithological changes within the Youganwo Fm are subtle. This will be revised. So, compositional analyses

such as XRD and XRF would be beyond the scope of this study. However, we measured high-resolution magnetic susceptibility (MS) of surface of the outcrop and the MS data can capture the subtle lithological changes within the Youganwo Fm.

Page 2825 line 6: here too they must specify which orbital cycle because each one act in a different way on the climate and environment… spectral analyses can explain which is the orbital forcing.

Spectral analyses of the depth series of the MS data will be performed to detect the dominant sedimentary cycles.

Line 23: avoid using "+" sign

This will be revised.

Line 27: late Eocene
Page 2826 line 1 and 4: late Eocene, middle Eocene. Ensemble 5 and 6: I liked the effort they make to build up their model using a logical exclusion process.

Thank you. This is one of the thrusts of this study. Unlike many marine records that are relatively long in time and magnetostratigraphy can be relatively easier to be defined with the help of paleonotologic data, terrestrial records however are usually short in time, i.e., fragmentary, and it is often difficult to establish a high-resolution timescale for this type of terrestrial records. Here we demonstrate that it is possible to establish a refined timescale for the fragmentary terrestrial records when different types of datasets of the terrestrial records are integrated synergistically.

However, I do not agree with their conclusion. It seems to me that the strongest model is the ensemble 5. All their thoughts on the changes in sedimentary rate are wrong because N2 and N3 can be C16N or 16N and 17N since there might be a gap which is very common also at sea in this period. So, I think they have to reconsider most of their discussions and conclusions. They must also consider that main lithological changes in the Tethys realm occur before that the EO boundary (namely Scaglia variegata FM-scaglia cinerea FM)

Magnetozone N3 cannot be used for correlation to the standard GPTS because its lower bound is not well defined. Its low bound is "0"m in our study because our studied section starts from "0" m. Its real lower bound could well be below the studied section. This is why we did not use the magnetozone N3, but only used the well-defined N1, R2, and N2 zones for correlations (Table 1).

As to the comment about "a gap", the studied Youganwo Fm including the N2-N3 portion show predominately oil shale that was deposited in semi-deep to deep lake environments (Guo, 2006). Previous workers did not recognize sedimentary hiatus in

the Youganwo Fm and we did not see any sedimentary hiatus in the studied Youganwo oil shale either. The “gap” was probably common in marine records from Tethys realm and was probably associated with oceanographic changes such as circulation and/or carbonate compensation depth (CCD) that can cause sedimentary “gap”. We can mention this in the discussion, but we do not think that the occurrence of a sedimentary “gap” in marine records from Tethys realm would necessarily also warrant a sedimentary hiatus in a lacustrine record from an intramontane basin in low-latitude Asia. As such, we believe Ensemble 6 is the best among these six possible correlations and is used to establish the refined timescale.

Page 2827 line 1: something wrong with the “by”.

It is a part of the phrase “by and large”.

Page 2829 line 13-23: This part should be completely cancelled. The do not show any spectral analyses and they end up building a astronomical calibration… this is completely wrong. Worst… they are so many uncertainties in they work starting from the lack of the phase analyses based on any environmental assumption and, them, serious mistakes in the magnatostratigraphy interpretation. So, all the rest of the text need to revised after having solved those two major issues first.

Lines 13-23 will be deleted in the revision. As mentioned above, spectral analyses of the depth series of the MS data will be performed in the revision. Other comments regarding calibration and phase relation have been replied at the beginning of this response.

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