

The paper is well written and fluid. The topic is interesting and deserves to be published. 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 cyclostratigraphy. The paleomagnetic work is reliable and solid. Nevertheless, the cyclostratigraphy is not ready yet.

There are two major lacks in this study:

- The pooriness 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 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.

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?

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”.

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.

Results

Line 22: how MS can facilitate the characterization of sedimentary rhythms?

Here they need spectral analyses to proof it!

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

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

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

Line 2: subtle compositional of what?

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 section, which is supposed to be deeper water, how can express paleosols?

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

Line 11: rewrite the sentence: "massive sandstone cycles vary"

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

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?

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!

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 of the samples..."

It does not seems to me they spell NRM.

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.

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.

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!

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.

Line 23: avoid using "+" sign

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. 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)

Page 2827 line 1: something wrong with the "by".

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