

## ***Interactive comment on “Precessional and half-precessional climate forcing of Mid-Devonian monsoon-like dynamics” by D. De Vleeschouwer et al.***

### **Anonymous Referee #1**

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Review of the paper entitled "Precessional and half-precessional climate forcing of Mid-Devonian monsoon-like dynamics" by D. De Vleeschouwer, A. C. da Silva, F. Boulvain, M. Crucifix, and Ph. Claeys.

This paper deals with a very original question: to derive from high accumulation sediments cores, high frequency climate variation. The main idea is to analyze, using different tools, the magnetic susceptibility (MS) and the microfacies curve, the high frequency signal which is attributed to precessional and half-precessional frequencies. The authors, by analogy to present day precessional and half-precessional equatorial observation, try to establish a link between these high frequency signals and monsoon

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dynamics. Whereas, the paper is original and brings interesting information, there is some room for improvement I'll describe below. The authors should answer clearly to the points raised in the following part of the review to resubmit the paper.

## Abstract

A very important point in this study is related to tropical / equatorial climate dynamics. It is therefore a key issue to give the paleolatitude with associated error bars of the core. It should appear clearly in the abstract and should be discussed in the manuscript (with more accuracy than in figure 1). The analogy for precessional and half-precessional frequencies should be discussed in line with paleogeography of the tropical area in Devonian compared to Present Day.

1. Introduction We expect a bit more on what are the specificities of Mid Devonian climate than very general considerations. Especially the link between paleogeography CO<sub>2</sub> and climate in the tropical area should be described in the framework of monsoon system.

2. Geological setting This is really a very poor section for a key issue. On figure 1 from Ron Blakey, it is just impossible to have an idea of the paleolatitude no scale just qualitative stuff. May this core be referenced in paleolatitude through paleomagnetism? If yes, the authors should clearly explain and discuss the uncertainties. If not then it is a real problem because all the discussion is based on tropical equatorial dynamics and monsoon.

3. Materials and methods 3.1 Magnetic susceptibility Fine to me

3.2 Time-series analysis procedure Line 13, page 1431: estimates instead of estimate

3.3 Possible distortions of the astronomical frequencies Line 5 page 1433: What is the meaning of "semi- constant"? The authors should comment more on the hypothesis on sedimentary accumulation rates and associated error bars invoked.

4. Results and discussion 4.1 Recognition and identification of astronomical cycles

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Figure 2 and 3 are quite puzzling. On marine environment, the two proxies MS and microfacies do not show the same pattern. At 0.55, MS shows a peak which is different from the 3 peaks shown by Fore-reef record. Therefore, this identification remains a bit speculative as the authors themselves claim page 1433, line 5 to 8. Then page 1434 is even more puzzling to me. With only one dated point “ensensis biozone” ~300 kyr. (no error bar provided), the authors derived a constant accumulation rate. Then to account for precession cycle derived from Berger 1992 at 18 Kyr (no error bars provided), they “recalibrate” their accumulation rate to fit this value. They describe this process very honestly (page 1434), but we may conclude that the cycle they observed are just tuned to astronomical value. This “normalisation” reduces from 11.3 to 8.8 cm kyr<sup>-1</sup> so not negligible.

This section 4.1 is indeed pivotal for the paper, but it is unclear to me to which extent in figure 2 to 4, the precessional and half-precessional peaks represent a large part of the power spectrum. The spread in frequency is very and large the percentage of the signal corresponding to these peaks has to be properly quoted. After numerical treatment precessional cycle are shown to be representative, but part of that is due to the procedure of time-axis computation as written by the authors (page 1435). Next page (1436), the existence or not, in the signal analysis, of eccentricity modulation is confusing. Why is it absent / present? As a whole this section 4.1 should be rephrased more clearly and convincingly.

4.2 Palaeoclimatological interpretation At the bottom of page 1439, the authors argue that precession will drive strong monsoon that will produce large erosion. I agree on these processes. Nevertheless, there is no discussion of paleogeography impact, of what may be recorded by the site. Is this location consistent with monsoon area at Devonian? What about literature on climate modelling for Devonian? Any idea of extent of monsoon? Such a discussion in terms of paleoclimate a bit more quantitative than very general ideas would be required. Concerning the half-precessional periodicity, this part is very unclear. Because it is a crucial point of the paper, the authors should:

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a. Explain the causes of such a frequency in more details for PD (not only through references but with mechanism). b. Then it should be possible to investigate why the Devonian also allows such oscillations.

### General Conclusion

Despite dealing with interesting questions, the manuscript shows major caveats: Lack of information on the paleolocation of the record, no reference / discussion of Devonian climate simulation for the monsoon extent and intensity, no real discussion on error of dating and accumulation rate, no clear discussion on tuning the peak on precession cycle, no clear discussion of why the analogy with PD monsoon and half precession could be robust and appropriate.

For all these reasons, I suggest that the authors account for these criticisms suggestions to resubmit an improved manuscript.

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