

Interactive comment on “Earth as diode: monsoon source of the orbital \sim 100 ka climate cycle” by R. Y. Anderson

Anonymous Referee #2

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This manuscript presents a very interesting climatic record from the late Permian, with a remarkable resolution, non trivial frequencies and some "rectifying" properties. This record should be published since it provides very valuable information on the climatic variations in the remote past of our Earth. But I believe the author is trying too hard to provide mechanistic explanations that are often confusing and not very convincing, while he should spend more time describing the record, its context and its climatic significance. I believe the author should make some major changes in the manuscript so that its message can be valuable for the reader. Some major comments are listed below.

- the focus of the paper should not be the 100-ka cyclicity. First, it is very difficult from the data (Fig. 2) or Fig.4, with only 1.5 cycle, to relate this "long-term" wave to a

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periodicity. Second, the discussion on glaciations and Milankovitch is completely out of the subject. The paper's subject is not about glacial cycles and I do not see any connection with the Quaternary time period. Astronomical frequencies are of course present all the time during Earth's history. The fact that the same frequency arise at different time periods in very different context (and probably for very different physical reasons) is in itself not very surprising. Discussions and speculations along these lines are only distracting and do not help to focus the manuscript on its main topic: monsoons in the Permian context. The title and the abstract should be changed accordingly since they are very misleading: It is only in the introduction that the author explains that the paper is about the Permian time period. A rapid look at the title and figures could lead the reader to think it deals about Quaternary glaciations. This is not the case.

- the same comment applies to Fig.2 and 4. Harmonics of precession signals, or amplitude modulation of CP, are indeed found in many records. There is no need to plot other records of precession or 100-ka signals from very different time periods and climatic context. This is only confusing. The idea that all the 100-ka oscillations during the Earth history should obey the same climatic processes is a bit naïve. There are many possible mechanisms (climate, ice sheets, geochemistry, monsoon, biology, ...), these mechanisms are not exclusive, and they have certainly different contributions at different geological time periods.

- the manuscript spends a lot of time trying to identify some "rectifying mechanism" in the climate system. The discussion is difficult to follow and I believe many parts of it are either inaccurate or, again, misleading. First, the author explains that the recording process is linear, and therefore the "rectifying effect" should be in the climate system itself. I believe this assertion is the center of the problem. If I understand well, the data is recording the maximum temperature (Tmax). But the author should be much more explicit on this point... Does the precipitation occurs at specific times during the year (max temperature) or all year round ? If the record is indeed about maximum temperatures, then this is in itself a strongly non-linear procedure, and it is not clear to

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me that the author needs to look much further than that to explain most of his findings. Indeed, taking the maximum of an annual T series is what the author describes as a "clipping" process in the intertropical zone. This is not a non linear climate process, but a non-linear recording process. So there is no need to find complex climatic explanations for that ?... But if the precipitation occurs all year round (as suggested in parag. 5.1) then why the author is linking the varve thickness to the maximum temperature ? If mineral precipitation increases linearly with temperature, the varve thickness should be linked to the integral over the year of the temperature, not its maximum value. Then I do not understand why the author is always mentioning the maximum temperature in the manuscript... I think the manuscript should be re-written in order to explain in much more details what the record actually means, before jumping to wild speculations on the climate system. Without a clear idea of the climatic variable which is recorded, I have difficulties to follow the discussion exposed here. If it is Tmax, then the "recording" itself is generating a "clipping" effect (and not the climate system, as suggested in 6.1). If "recording" is a linear system, then I do not understand the discussion about Tmax.

- the question of what is actually recorded is in particular critical when introducing the "upper" and "lower" boundaries called Tmaxu and Tmaxl. According to Fig.5, these two numbers seem to be related to two different seasons ? But without information on the temporal succession of these values (is it annual? is it linked to the QBMO? is it random?) it is not possible to follow the author discussion. Since the record is "annual", how can it be influenced sometimes by one season, and some other times by the other ? Or is the record semi-annual ?

- EBM models are mostly linear. They cannot say much about the monsoon dynamics. The EBM models are mainly a translation of the insolation signal in terms of temperatures. I would again not call this a "non-linear" system. But clearly the choice of the output of the model, "Tmax", is a "non-linear" one (though I do not like this term very much). Again, the author is misrepresenting the system. The climate can be linear, but

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taking the max of some variable is obviously some non-linear data processing...

- the author is very elusive about the chronology. Are the varves really annual or is it possible that this system could be (at least some times) semi-annual, as could be expected from the temperature at the equator ? Are alternative hypotheses possible ? Is there any other chronological control beyond varve counting ? Again, I would have like a deeper discussion of the record, which is indeed very impressive.

- the QBMO is a very interesting feature. Again, I would have like more information on the data, its spectrum, the stability of the frequency over the record, etc... Again, the comparison with the Quaternary is probably not appropriate and only distracting here. But I do not understand why the author raises questions like "the QBMO... should be amplified at EC, CP and SP frequencies" but does not gives any clue to the answer, by showing for instance the filtered signal at 2.3k, or its amplitude modulation. This is quite frustrating.

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