

Interactive comment on “Two distinct decadal and centennial cyclicities forced marine upwelling intensity and precipitation during the late Early Miocene in Central Europe” by G. Auer et al.

A. Sluijs (Editor)

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Dear Dr. Auer,

Thank you for responding to the two reviews. Please allow me to briefly advise how your manuscript could be optimally revised. The most notable comment made by Reviewer #1 regards the actual constraint on the sedimentation rates in the studied section. In the first version, this was essentially treated by one sentence at the start of the discussion: “Using the average sedimentation rate calculated for the basinal parts of the Central Paratethys based on the cross-correlation of insolation cycles by Hohenegger and Wagleich (2011) as a rough baseline for this hypothesis, a sedimentation rate

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of 512 mm kyr⁻¹ was assumed.” Considering that this is a crucial piece of information for the interpretation of your cycles, I would recommend making a separate section “Stratigraphy” as section 3.1 of the Material and methods section that discusses previous stratigraphic work in some detail. It must describe the basic biostratigraphic framework, including a discussion on the uncertainties involved in correlating the section to the GPTS as done in Figure 2. It should also treat the Hohenegger and Wagleich (2011) to some extent to indicate how significant the cyclicity is and how applicable their findings are to the interval studied here. Also the points made by Reviewer 2 can be addressed here. Essentially, the above should lead to an estimate of uncertainty regarding the assumption you make regarding sedimentation rates. In the discussion section (probably), there should be a brief discussion on how the uncertainty affects your identification of the cycles, essentially as a brief sensitivity study to slightly different assumptions within the uncertainty of the larger age model.

I think the way you reply to reviewer #2 is generally constructive but I cannot always see how you will change the manuscript accordingly. Some of the remarks of the reviewer lead you to explain your methodology a bit better than was done in the original version of the manuscript. I would recommend including such statements in the revised manuscript so that it will also be optimally readable by the sedimentologists, micropaleontologists and geochemists. Moreover, I agree with reviewer 2 that the paleoecological preferences of the various nannofossil taxa is not very well discussed in the original manuscript; what are the inferences based on? Do various authors agree with this or does discussion remain? I would recommend this to be improved in the revised version.

Finally, a couple small things: P3. Line 19: ‘warved’ should be ‘varved’ and Fig. 6 needs clarification for red, orange and black lines.

I’m looking forward to receiving your revised manuscript. Sincerely,

Appy Sluijs, Editor

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