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Interactive comment on "Orbital control on the timing of oceanic anoxia in the Late Cretaceous" *by* S. J. Batenburg et al.

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Dear authors,

congratulations on excellent data and an interesting paper. This study is an important contribution, although it might benefit from a better explanation of your approach to astronomical tuning. Could you please comment on the following points?

Published studies (Mitchell et al. 2008; Lanci et al. 2010) suggest relatively uniform sedimentation rates throughout the Furlo section (except of the Bonarelli L.). Your tuning options 1 and 2 imply markedly increased sedimentation rates (or reduced compaction) in the uppermost ∼3 m beneath the Bonarelli Level (from ∼1 cm/kyr to approximately 1.5 cm/kyr) and results in a ∼100 kyr difference relative to the published age models. I realize that this part of the Furlo section is particularly difficult to inter-

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pret. Your L* data look great, and after examining your figures in detail I believe your age model might be correct (the apparent increase in both spacing and thicknesses of organic-rich beds in this interval are consistent with your interpretation). As it is, however, your tuning in this interval does not look very convincing. In section 3.3, lines 20-21 you explain that the identification of 405-kyr maxima and minima is based on a 3-5 m bandpass of L* data at Furlo. In both tuning options, however, the uppermost bandpassed maximum below the Bonarelli Level is out-of-phase relative to the 405-kyr maximum in La2011 to which it is correlated. You are apparently using other criteria, but they are not explained. I assume the correlation is based on the bundling of organic-rich beds. This aspect is, however, also problematic, because your lithological log for this interval shows important differences from L*, and it is not clear which of these two is used to define the bundles. For example, the circumflex that should mark the uppermost organic-rich bundle beneath the Bonarelli Level is centered at an exceptionally thick limestone in the lithological log (Fig. 3); this seems to contradict the definition of organic-rich bundles. It would be very helpful if you could show the detail of this part of the section and comment on the differences between your lithological log and color reflectance data. This is particularly important considering the disagreement between your interpretation and published age models.

Could you please explain why do you prefer tuning option #1 over tuning #2? I believe you have good reasons. Without an explanation (which I cannot find in your manuscript), however, the reader is puzzled especially when considering that your tuning #1 appears incompatible with some of the published radioisotopic/astrochronological estimates for the age of the C/T boundary (cf. Eldrett et al. 2015).

Your argument for a Myr eccentricity node prior to OAE II is based on the observed gap in the black shale occurrence at 483-485 m (page 8, lines 30-31). According to your tuning options, however, this interval experienced a 50-60% increase in sedimentation rates (or decrease in compaction) compared to the rest of the section beneath BL. If

you apply correction for this change in sedimentation rate, then the thickness of the shale-free interval decreases by c. 35 %. Such a correction would make this interval comparable to other 405-kyr minima in this section (e.g., ~471-472 m) and disqualify the argument for a Myr node. The exceptional thickness of dark levels above this interval (page 8, lines 31-32) can be attributed to the overall increase in (compacted) sedimentation rates as well.

Recent papers (Jenkyns et al. 2007; Gambacorta et al. 2015) reinterpreted the timing of Bonarelli Level at Furlo and Bottaccione relative to the phases of OAEII. Osmium-isotope excursion marking the onset of the event starts immediately beneath the Bonarelli Level at Furlo (du Vivier et al. 2014). Thus, the possibility that Bonarelli Level represents only the second buildup phase and plateau (page 7, lines 30-31) seems to be outdated (see, for example, figure 12 in Gambacorta et al. 2015). Does this change affect your estimate of the OAE II duration?

Gambacorta et al. (2015) interpret hiatuses in the upper part of the Bonarelli Level at Furlo and other sites in the Umbria-Marche Basin. Could you please indicate how are these hiatuses considered in your age model?

Let me add a note on the paper by Lanci et al. (2010), which is criticized in your text. The phase calibration in this paper was based on a previous astronomical solution (La2004), and is probably incorrect as you noted. The change of interpretation is, however, not due to an incorrect sampling strategy by Lanci et al. (2010). We recently revisited the topic using the same data and simple numerical models. The results suggest that the omission of precession-paced organic layers in Lanci et al. (2010) does not distort the 100-kyr and 400-kyr eccentricity signatures to a degree that would prevent detection of 405-kyr eccentricity phases (Fig. S1.5 in the supporting information of Laurin et al., in press). I would not say that the sampling in Lanci et al. (2010) was “incorrect” (page 6, line 23 in your paper). It was correct considering that the authors needed to avoid lithological bias to focus on the record of changing bottom-water oxygenation in rock-magnetic properties. They just could not have assessed

precession-scale variability, which is a major advantage of your color reflectance data.

I believe the above issues can be fixed. Your paper includes important data and interpretations, and I am hoping to see the final version published soon.

Yours sincerely, Jiří Laurin (Institute of Geophysics ASCR, Prague; laurin@ig.cas.cz)

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