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Interactive comment on “Black shale deposition during Toarcian super-greenhouse driven by sea level” *by* M. Hermoso et al.

Anonymous Referee #2

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The study by Hermoso and colleagues addresses the question of Toarcian black shale deposition in shallow shelf-sea environments. In particular they intend to decipher principle mechanisms for the formation of oxygen deficiency in shelfal waters during and after the Toarcian oceanic anoxic event (T-OAE) as it is defined by the carbon isotope excursion (CIE). By using sedimentary, mineralogical, stable isotopic and elemental concentration proxy records from the Sancerre core in the Paris Basin they suggest relative sea level changes and consequently water depth as a primary prerequisite to establish anoxic conditions in deeper parts of the basin. One result of the study is the geochemical characterization of four distinct laminated organic-carbon rich horizons, with the lowest one (Sc1) corresponding to the negative CIE of the T-OAE. Furthermore, the authors reconstruct relative third-order sea-level by changes based on fluctuations of the Qz/Qz+Clay ratio. The interplay between climatic and sea-level

forcing on the formation of shelf-sea anoxia is still not fully understood. In this context this study provides interesting new aspects to the discussion. However, the data interpretation is too focused on sea level change alone and the applied proxies are not sufficiently discussed in detail. At first, one aspect that strikes me is the definition of the T-OAE which differs from that of other studies. I cannot see from the presented TOC and carbon isotope data, why the onset of the T-OAE is placed at the base of the tenuicostatum Zone and not at the base of the first black shales which appears to be synchrounous to the negative CIE in European basins. The authors argue with a first small organic rich bed of 3 % TOC which, however, is not visible in the lithological log, in the TOC or the δ13C data in Figures 2, 3 and 4. For consistency, it would be useful to adopt the definition of previous studies (e.g. Hesselbo et al. 2000 or Harazim et al. 2013) or to provide a robust discussion for such an early commencement of the T-OAE. A second issue is the application of the Qz/Qz+Clay ratio as proxy for relative sea-level change. 3rd order sequence stratigraphic boundaries are detected based on the assumption that the Qz/Qz+Clay ratio is a proxy for change in grain size change and that all of the Qz is of detrital origin. However, enrichment in Qz can also be related to siliceous bioproductivity. Without an independent calibration of the Qz/Qz+Clay ratio with grain size I would questioning the reliability of this proxy. My doubts are supported by the lack of correlation with the detrital element ratios Si/Al, Ti/Al and Zr/Al. While the Qz/Qz+Clay ratio shows its global maximum at 342 m, the Ti/Al and Zr/Al ratios are highest between 337-340 m. Furthermore, the Qz/Qz+Clay ratio strongly resembles the carbonate curve, which also supports a productivity component within the proxy. Since the sequence stratigraphic interpretation of the Sancerre core is the key issues of the paper, I would recommend a more rigorous discussion of the sequence stratigraphic model and the interpretation of applied proxy data. For instance, what is meant with maximum sediment argilosity? Why does this indicate a maximum flooding? Usually this transition marks the change from retrograde to prograde sediment stacking, which means the rate of sediment supply is higher than the rate of increase in accommodation. Why is such a transition associated with the lowest Qz, Si/Al, Ti/Al and Zr/Al ratios in the record? The MRS Pl8 boundary is the most prominent surface in the core, also evident from other sections elsewhere (hiatuses at the stage boundary). The applied proxy (Qz/Qz+Clay) shows only minor changes and not a global maximum at this boundary. How do the authors explain the fact that the highest Qz/Qz+Clay ratios are recorded during the early Toarcian transgression? Although I agree that a sufficient water depth is needed to establish shelf-sea anoxia, the authors should not forget to discuss this in the context of shelfal circulation, nutrient supply, productivity and climate change. Finally, many aspects of this study are already discussed in detail in the work of McArthur et al. (2008). The authors should spent more efforts to make it clear what are the new scientific results of their study.

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