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Interactive comment on "Cretaceous Oceanic Anoxic Events prolonged by phosphorus cycle feedbacks" *by* Sebastian Beil et al.

Sebastian Beil et al.

sebastian.beil@ifg.uni-kiel.de

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Reply to Interactive comment on "Cretaceous Oceanic Anoxic Events prolonged by phosphorus cycle feedbacks" by Sebastian Beil et al.

Matthew Clarkson (Referee) RC2

matthew.clarkson@erdw.ethz.ch

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The manuscript presents an impressive dataset of P-speciation data and high resolution XRF core scans, which help build on earlier works regarding i) the duration of OAEs, and ii) the hypothesis for P-cycling as an important feedback mechanism for

OAE development. The work involved in this manuscript could feasibly represent two papers, if the authors saw fit, as the cyclo-stratigraphy aspect over-shadows the P spe- ciation and the data presentation become very lengthy. I have read through the detailed comments from Hugh Jenkyns and Cristian Maerz and agree with their inputs. I will try to give additional contributions, rather than repeating their observations. Generally, this is an impressive dataset and it shouldn't take much work to address these comments.
We would like to thank Matthew Clarkson for his helpful, constructive comments and suggestions, which we have followed as much as possible.

General Comments:

I think there is a missed opportunity here in that one of these cores has been extensively studied previously by the authors (Scholz et al., 2019), with Fe-speciation, redox sensitive metals (Mo, V) and N isotopes. The new P-speciation data would complement this previous study very nicely and more could be made of integrating the two datasets. This could be valuable for the discussion of redox and P cycling through OAE2 and would help give more contextual information, particularly with reference to the evolving nature of redox conditions through the core. I think it would be very useful to the community to examine P-speciation results within the context of the established redox framework that varies locally from nitrogenous to euxinic, and compare this to intervals of ferruginous and euxinic deposition elsewhere (Poulton et al., 2015), and so I am somewhat mirroring a comment made by Dr. Maerz.
A preliminary discussion of the long-term redox change in the Tarfaya Basin was included in Beil et al. (2018). We feel that a more detailed discussion of redox-conditions is beyond the scope of the present paper, which focuses on a synthetic comparison of OAE1a and OAE2. However, a detailed discussion of redox-changes is in preparation (Scholz et al., in prep.) with principal aim to reconstruct redox-variability at high

resolution in the Tarfaya Basin.

Minor comments:

Line 93: 'oceanic anoxia'
Changed

Line 114: The MCE is referred to frequently as it appears in the records, however not much background is given on the significance of this event. Please detail if this is a local feature or a global event comparable to the other OAEs studied.
We propose to add the following text to the first paragraph of the introduction:
The mid-Cenomanian event (MCE; Coccioni and Galeotti, 2003) appears to represent a less intense precursor event of OAE2. However, detailed records of the MCE are still sparse with most studies focusing on the higher amplitude events (OAE1a, b, c, d and OAE2). Records until now are predominantly from the North Atlantic and Tethys region (e.g., Umbria Marche Basin (Coccioni and Galeotti, 2003); English Chalk (Gale, 1989; Jenkyns et al., 1994); Western Interior Seaway (Keller et al., 2004); Blake Nose (Ando et al., 2009))., which display a positive isotope excursion during the Thalmanninella reicheli foraminiferal zone. In shelf areas of the global ocean, major sea level changes associated with the cycles Ce2.1 and Ce3 of Gale et al. (2002) may have caused long lasting hiatuses obliterating evidence of the MCE.

Line 160: As a disclaimer, I am not so familiar with XRF core scanning techniques, but I would be suspicious of using Fe as a terrestrial element, included in the logTerr/Ca proxy, as there is likely redox-dependent behaviour in these settings that would obscure or bias trends in terrestrial elements if Fe is included. It might be that Fe is lost from the sediment due to reduction in the pore-waters (thereby removing any Fe

cycles), or that Fe has been enriched through Fe-shuttling across the basin. It would be possible that the stepped increase in Terr/Ca could be caused by an increase in Fe, due to enrichment of highly reactive Fe-phases (e.g. at the onset of OAE1a). It is also possible that this could create apparent cyclicity, analogous to the cyclicity in FeHR/FeT in other Tarfaya data (Poutlon et al, 2015). If Fe is plotted separately or removed from this measure, do you see any behaviour that might be indicative of local redox changes dominating the record?
This could be an opportunity to add additional information on redox systematics. Can you pull out Fe/Al from the XRF data to aid comparison to the Fe-speciation cyclicity ob- served by Poulton et al., 2015 in the other Tarfaya core and the previous Fe-speciation data of Scholz et al., 2019?
We added the following figure to the supplementary material showing Log(Terr/Ca) calculated with (red) and without (black) iron for OAE1a and OAE2. This figure reveals no major deviations between datasets.

No significant influence of redox-variability implies a predominantly detritic reservoir for iron. Fe/Al (not shown) is therefore controlled by the composition of deposited terrigenous material and cannot be used as a proxy for redox changes in both basins. A detailed discussion of redox-conditions is beyond the scope of the present paper, but a detailed discussion of redox-changes is in preparation with principal aim to reconstruct redox-variability at high resolution in the Tarfaya Basin.

Also, what about the dilution effect of Ca from high organic carbon production, would this potentially create cycles or stepped changes through the OAEs. There seems to cycles in TOC from just looking at the linescan photograph, so how much of the cyclicity in logTerr/Ca can be explained by simply changing CaCO3 concentration?
Yes, Log(Terr/Ca) is a proxy for carbonate content and shows a good correlation with carbonate content as shown in Beil et al. (2018). We assume that most of

this variability is rooted in the fluctuating terrigenous input associated with changing conditions on land.

Could you also please clarify what NGR represents in terms of sedimentary components that drives the cyclicity, and how this links to the orbital pacing mechanisms.
We expanded section 2.2 as follows:
The intensity of natural gamma radiation is predominantly influenced by the concentration of three different elements: Potassium (K), Uranium (U) and Thorium (Th). All three elements are bound to clay minerals with uranium also adhesively enriched in organic matter. In environments with low terrigenous input and high organic matter deposition as at the palaeo-position of Core SN°4, NGR is predominantly controlled by the concentration of organic matter. Predominant sedimentary control can be assumed for Cores LB3/LB1 characterized by low organic matter content.

Line 196: is smoked the correct term for this? ashed?
Smoked is the correct terminology: the liquid was minimized by evaporation.

Line 280: the PCE is often associated with faunal changes that represent different water mass movements or local re-oxygenation. I think it is a bit misleading to focus on the extinction aspect. More could be done to reference other studies here.
We expanded chapter 3.2 to include the most important of the environmental changes registered worldwide as follows:
The Plenus Cold Event is globally recorded (Forster et al., 2007; Sinninghe Damsté et al., 2010; Jarvis et al., 2011; Jenkyns et al., 2017) and coincided with invasion of boreal species in the European Chalk Sea (Gale and Christensen, 1996; Voigt et al., 2003), extinction of the planktic foraminifer Rotalipora cushmani (e.g., Kuhnt et al., 2017) and re-oxygenation of bottom water masses (e.g., Eicher and Worstell, 1970; Kuhnt et al., 2005; Friedrich et al., 2006).

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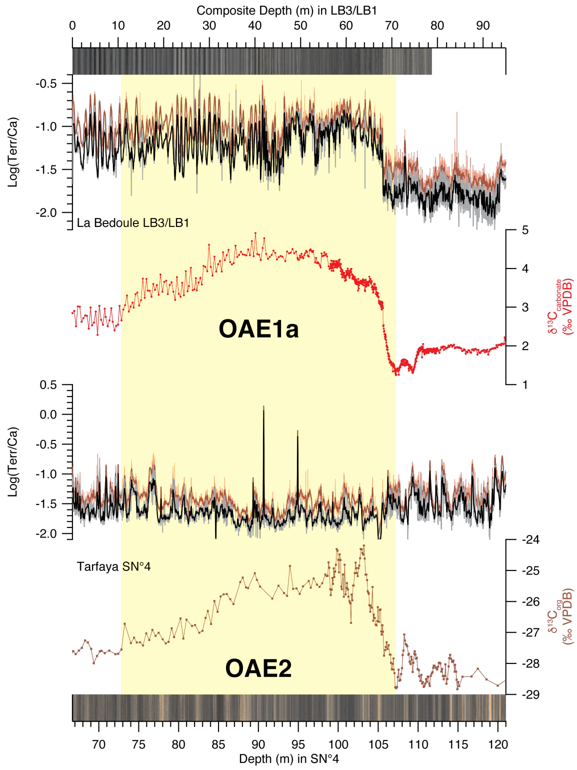

Fig. 1. Log(Terr/Ca) calculated with (red) and without (black) iron