

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

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Dear Babette, dear Sebastian and co-authors,

let me first say that I don't tend to write reviews that I haven't been invited to. I do not mean to make the authors' lives harder than they already are. However, the topic your nice manuscript is about is quite close to my heart, and I have therefore decided to add a few comments that might help to widen the perspective of the manuscript and put into context of a few publications that the authors might have missed. As it happens, some of these publications are (co-) authored by me and my review could be understood as shameless self-promotion. This is not my intention, but the editor might have a different view on this and may therefore decide to ignore my comment.

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The manuscript prepared by Beil et al. is an impressive data set on an impressive number samples from two locations that resolve two OAEs (1a and 2) in very high temporal resolution. I have read the comment by Hugh Jenkyns, which focuses on the definition/duration/isotopic expression of the OAEs, and I will not go into any detail on those. Instead, my comment refers to the phosphorus side of the story.

I applaud the authors for having generated a very nice P speciation data set, for reporting the recovery of their extractions relative to total P, and for a very detailed method description in the appendix. In the broadest sense, I also agree with the interpretation of the authors that P recycling from the seafloor during much of OAE2 has potentially led to higher primary productivity, fueling an anoxia-productivity feedback loop that has been previously suggested to extend the "lifetime" of OAEs.

My comments, which are all included in the attached PDF as annotations, relate to the (a) a more precise distinction between different redox conditions (namely ferruginous versus euxinic) and (b) the weathering regime. The main reason for raising these issues is that Poulton et al. (2015) conducted a study on the onset of OAE2 from a different Tarfaya core, with a focus on the potential effects of weathering conditions on land on ocean redox, and the related response of the P cycle to these redox changes. Since this manuscript is using very similar methods and proxies on samples from effectively the same location, I think it would be an omission and a missed opportunity to not refer to the published manuscript, and put the new data into context. My comments in the PDF are hopefully self-explanatory, but please feel free to ask for clarification.

I hope the authors will take my relatively minor comments in the good spirit of scientific exchange, and I am looking forward to seeing the final version of the manuscript published in Climate of the Past.

Kind regards, Christian Maerz

Please also note the supplement to this comment:

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<https://www.clim-past-discuss.net/cp-2019-118/cp-2019-118-SC1-supplement.pdf>

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-118>, 2019.