

Interactive comment on “Post-depositional vivianite formation alters sediment phosphorus records” by Nikki Dijkstra et al.

Anonymous Referee #2

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This work investigates the potential role of vivianite (an iron II- phosphate mineral) precipitation in altering the trends of sedimentary P, a commonly used paleo-productivity proxy. The samples were collected in the Bornholm Basin (Baltic Sea) in 2013. A combination of modeling, biogeochemical and electron microscopy analyses and experiments shows that manganese and molybdenum rich vivianite precipitation under sulfidic conditions can strongly alter P sedimentary records after their deposition, especially when environmental perturbations such as primary productivity changes associated to marine-lake transitions occur. The authors nicely summarized an intensive laboratory work and they present a coherent manuscript. The results are relevant because they provide new insights to the use of sedimentary solid phase phosphorus analysis to reconstruct paleo environments. Vivianite in particular, seems a potentially

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useful proxy for the occurrence of freshwater – marine transitions in systems like fjords in other parts of the world during, for instance, the last glacial maximum. However, the authors showed that vivianite peaks in the sediment could be strongly affected by sulfidic conditions and the presence of Fe²⁺, resulting in concentration peaks not directly related to water column productivity but to diagenetic reactions. I only have a few comments after reading the manuscript. My concerns are mostly related to format and some passages that I found a little bit “obscure”. In methods I think the redaction of the paragraph referred to P XANES analysis could be improved (page 16, lines 10-14). I found hard to follow the procedure, probably because I am not familiar with this particular technique. In the text the authors mention a “white line” that I cannot see in Fig. 7. There is a problem citing first Fig. 11 (page 17, line 15) and then figure 10 (page 21, line 14). In addition, I think that Fig. 11 is complex compared to the rather brief references to it within the text. Moreover, where did ages the authors mention came up? (e.g. Page 18, lines 3-6). It is not clear if the authors used an age model and how they derived it. I guess they probably used ages estimated in the IODPreport by Andrén et al. (2015) or maybe, they used a constant sedimentation rate to estimate a composite core depth – age relationship. I think the authors should clarify this!

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