

Interactive comment on “Bioturbation has a limited effect on phosphorus burial in salt marsh sediments” by Sebastiaan J. van de Velde et al.

Peter Kraal (Referee)

peter.kraal@nioz.nl

Received and published: 9 December 2020

Review of bg-2020-340

With interest I have read this paper, in which the authors explore the impact of bioturbation on OM and P burial in salt marsh sediments. The findings are presented within the context of the rise of burrowing animals and the biogeochemical impact of burrowing on the ocean-climate system. The paper is well-written (I have almost no technical comments on spelling/grammar) and presents an interesting, well-constrained test case to evaluate assumptions regarding causal relationships between ecology and biogeochemistry, which are also important to parametrize regional and global biogeochemical models.

C1

The manuscript suffers from a rather limited exploration of the data, shackled by a strict context of two “hypotheses” which are presented as key foundations of our understanding of (the evolution of) P cycling, while a broader context and more in-depth analysis of the presented data and the processes that are discussed would be beneficial. The focus on the validation of ancient (open) ocean scenarios with results from terrestrially-impacted salt marsh sediments is a bit of a stretch to me.

I have a few considerations for the authors, with which I hope to provide fair and constructive feedback to improve the manuscript.

Bioturbation| To my understanding, bioturbation has a rather complex impact on surface sediments bringing oxidants into reducing deeper sediment and the other way around, explored in some detail by modellers (e.g. Boudreau and others) and experimenters (Aller and others). The authors start the paper by only in very general terms mentioning that bioturbation impacts biogeochemical processes without a mechanistic backdrop, and I do not see further process-based explanation later in the introduction. The ‘burrowing revolution’ and ‘bioturbation’ are not explicitly linked in the text, so bioturbation is not really defined. Starting like this, the paper is only accessible to people already intimately familiar with processes and effects of bioturbation. I think the authors move to fast here and should reconsider how to establish the general context of their study, and in particular inform the reader at a mechanistic level what it is that bioturbation does to the sediment to impact the “geochemical cycling and burial of elements”.

Ordering of information | The manuscript, especially the introduction, seems to have a style in which cause-effect relationships are mentioned, but the underlying mechanisms are only mentioned later. For instance, how do C and P burial control atmospheric O₂ and CO₂ (p2. L3-4), primary productivity should probably be mentioned here already, instead of later (L11-14). And how does bioturbation decrease the (C/P)org ratio of buried organic material (p2. L11-16, L22-23)? I find the phrasing somewhat confusing, because from the context (also from the refs) I gather that bioturbation contributes additional organic P from diagenesis of polyphosphates, which

C2

I not apparent from the text “The key assumption is that bioturbation decreases the carbon-to-phosphorus ratio of the organic matter (Corg : Porg) that is buried in marine sediments”. The authors should critically assess whether they provide the required mechanistic backdrop (in a timely manner) in these and other cases. The reader should not be left with questions of why lingering in their mind, only to find answers later on (or not at all).

The hypotheses on P burial and bioturbation | The manuscript sets out to test two supposed key assumptions on the links between P burial and bioturbation: (1) bioturbation boosts Porg burial (initially polyphosphates) and thus results in lower overall (C/P)org and (ii) inorganic P burial is not affected by bioturbation. The salt marshes offer a unique system where bottom waters are oxygenated and thus the isolated effect of bioturbation can be studied, rather than having to untangle anoxia and burrowing intensity. In setting this scene of key assumptions, the authors lean pretty heavily on one publication, Boyle et al., 2014. In this publication, a highly simplified P cycle is used in a general model with a simplistic mathematical representation of the effect of bioturbation, namely by making the total organic P burial flux dependent on a bioturbation factor that apportions P burial between (prescribed) high-C/P (laminated) and low-C/P (bioturbated) sediment. I find the current balance of the manuscript, where a local P study in salt marshes is used mostly to challenge some broad assumptions in a global modelling study not very strong. Added to this, the current manuscript incorrectly implies that the Boyle “assumptions” represent our collective understanding of P burial and bioturbation. I have noticed that the other reviewer was also struck by this, and I agree that the findings can be much better placed within the broader context of factors controlling P burial in sediments. Furthermore, I wonder about the representation of the “assumptions”: the authors mention that Boyle et al. assume that bioturbation has no effect on Pinorg burial. However, the model adaptation by Boyle et al. includes redox-driven FeOx - P coupling that shows a strong increase in sedimentary FeOx-P content upon bioturbation (and subsequent counter-intuitive effects on ocean oxygenation). This suggests that bioturbation boosts Porg formation and burial but also FeOx-P

C3

(Pinorg) burial? This study complements that understanding nicely by showing that this effect seems short-lived. All in all, the study would profit from a shift away from rebutting “assumptions” in a simplistic P cycling model, and the authors should carefully evaluate their representation of the “assumptions” in Boyle’s et al. 2014.

Geochemical context | Wording on p8. L3 that “Phosphorus adsorbs onto iron oxide minerals, which are introduced at depth by the downward mixing of benthic fauna” is slightly ambiguous as it could mean P-enriched FeOx being mixed into the sediment, or P adsorbing onto FeOx that were mixed deeper into the sediment. Perhaps seems trivial, but good to clarify. This brings me to a more general point: identification of geochemical processes (at depth). The authors mention reductive dissolution, release of P, precipitation of Fe-P, but all without showing any data for pertinent dissolved species such as Ca, Fe and P. As such, the actual (redox) P cycle remains speculative, while the availability of these cores from ponds offers the possibility to address a lot of these questions by measuring pore-water chemistry. The authors refer to data in Van der Velde et al. 2020 (e.g. dissolved Fe), it seems that the detailed geochemical functioning of these sediments was evaluated in that paper and a small side study on P was taken from the dataset for this paper. As a result, the current manuscript is very light on geochemical context. This does not only apply to the pore-water, but also the sediment chemistry: the authors mention limited Ca-P formation in the cores and contrast this with previous studies (p9. L4-16). Solid-phase chemistry (e.g. sediment composition) could further support statements on the likely fate of P released from organic material.

Comparing across timescales and depositional systems | The authors compare C/P ratios of salt marsh sediment that, according to the text, probably receive mostly terrestrial organic matter with C/P ratios of ancient sediments undergoing (tens to hundreds of) millions of years of diagenesis. Having studied such ancient sediments myself, I doubt how informative the (C/P)org ratios in such materials really are (e.g. Kraal et al., BG, 2012), which is in fact the reason why Corg/Preactive ratios are commonly used for ancient sediments. The transformation of Porg or PFe to stable authigenic

C4

phases over geological timescales is very well established for a range of marine settings. A complete lack of such signals in the current data (at least on the very short timescales considered) may say something rather specific for the type of sediment under consideration. Similarly, polyphosphates are becoming recognized more and more as an important (intermediate) P sink in many marine systems, and in fact are the key intermediate for the bioturbation-Porg burial link considered here, but seem to play no role whatsoever in the studied sediment? For me, this truly raises questions about the validity of comparing across such disparate timescales and depositional conditions. Currently, the authors deal with this by making bold claims in the abstract and the discussion, followed by listing all kinds of caveats. The central role of polyphosphates in booting Porg burial is mentioned in the intro but not really discussed in much detail at all. This does not make the manuscript particularly strong. I think it would be better to discuss the data for what they are, an interesting look into P cycling (assuming supporting data such as dissolved Fe and P will be added) in salt marsh sediments which offer a chance at investigating the role of bioturbation there. A section of the discussion could then be spent on how this may impact our more general understanding of the impact of bioturbation on P burial, but the system is too specific and the dataset too limited to support the current setup.

P profiles | The authors present the conclusions from the POP data and (C/P)org ratios with no caution in the abstract but with great caution in the discussion: the organic matter is probably mostly terrestrial ($C/P > 500$ under oxic conditions) and may behave differently than "normal" (algal) organic material in marine sediments. There is no characterization of the OM. This puts quite some strain on the validity of linking the salt marsh results to global ocean (modelling) studies. Having said that, the POP profiles shown in Fig. 2 provide more insight. The non-bioturbated POP profile shows a steady decrease ($\sim 50\%$) with depth that follows TOC, which suggests that OM is being degraded at appreciable rates (can this be ascertained for instance from NH_4^+ profiles (Van der Velde, 2020), or are other processes responsible for the down-core decrease?). If the authors can evaluate OM degradation rates from the profiles and

C5

show that these are comparable to down-core TOC and POP profiles in more marine settings, the results presented here regain validity as testing ground for ocean processes. In short, I would recommend to: (i) include the disclaimer about different types of organic matter in appropriate places such as the abstract and (ii) dig a little deeper into the salt-marsh POP profiles and their relation to more generic marine settings. A second point is the P budget. The authors present Porg and Pinorg in different figures and do not show the total P profiles; this makes it a bit difficult to assess down-core trends in P, it would be nice to see all P profiles and dissolved P) together, followed by the boxplots that assess the differences between sites. I am also curious to know whether these sediments are a net source or sink of P based on diffusive exchange between the bottom water and sediment.

Specific p6. L10. non-bioturbated sediments have strongest gradients in Fig. 2 p8. L7-17. Discussion starts to infiltrate the Results section at this point. Also creating unnecessary overlap with first section of Discussion.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-340>, 2020.

C6