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Interactive comment on "Benthic alkalinity and DIC fluxes in the Rhône River prodelta generated by decoupled aerobic and anaerobic processes" *by* Jens Rassmann et al.

Anonymous Referee #2

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I read carefully the manuscript of Rassmann, Eitel, and collaborators and I recommend it for publication after revision. This paper presents an in-depth analysis of benthic biogeochemical processes and DIC/TA release in different stations in the Rhône River Delta area. I particularly like the multitude of measurements applied to improve understanding of processes driving anaerobic formation of TA and benthic fluxes; particularly, combining in situ incubations, potentiometric and voltammetric micro-profiles with more conventional pore water and sediment analyses. This combination of methods is rarely encountered in these types of investigations which often focus primarily on submillimetric processes at the SWI. Furthermore, the amount of data collected is significant, and has to be published, definitively.

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My main concern is related to the overall perspective of the research. The authors do not convey very clearly the scientific importance of their work. For instance, in the introduction they mention that the objective of the paper was to " investigate if sediments from deltaic regions exposed to large riverine inputs of carbon and minerals represent an alkalinity source to the bottom waters and identify the biogeochemical processes responsible for the net production of alkalinity in these sediments. ". There are several studies that have done this in coastal area as well, thus, they should portray how this work is different. And I would say that the combination of methods is unique. After reading the paper several time, I am still not sure to understand the take home message of this study. What is really new? I strongly appreciated the effort to present this very large set of data that includes Fe(III)-Lorg, sulfide species, and methane in addition to major (or classical) diagenetic species. However, I often lost myself in detail that ultimately brings little. For examples, the section on the role of nitrification / denitrification and the section on IAPs are long but their conclusions are not very relevant for the rest of discussion. Overall the discussion should be shortened.

My second concern is on the role of terrigenous organic matter in this type of sediment. The authors characterized the study site as “deltaic sediments exposed to large riverine inputs of inorganic and organic material”. In these sediments, coarse particulate organic matter is deposited during flood events and supports the establishment of sulfidic conditions and the precipitation of Fe-S phases (François et al., 2014; Fagervold et al., 2014; Rassmann et al., 2016). As POM, CPOM is probably a source of DOM and organic alkalinity in pore water. What is the role of organic alkalinity on TA in these sediments? Did you calculate the theoretical TA based on DIC and pH? The production of organic alkalinity should be discussed and the contribution to TA should be estimated (it’s rare to have enough data to do it). In addition, the accumulation of refractory organic carbon in sediments appears intimately associated with the sequestering of iron and sulfides in micro-environments (see the works of François). When the authors discuss about the aggregation of FeS, do they talk about microenvironments? I think the role of terrigenous organic matter on these biogeochemical processes should be clarified.

My third concern is on the time scale of the explored biogeochemical processes. The deltaic sediments cannot be considered at steady state, specifically in the proximal stations. However, the discussion is based on a steady state view of the different reactions. So, what is the impact of floods on the oxygen demand and DIC/TA release? Are these fluxes constant over the year, with no seasonal variations? “[. . .] their presence in zones of sulfate reduction suggest these sediments are highly dynamic with periods of intense sulfate reduction alternating with periods during which sulfate reduction is repressed and replaced by microbial iron reduction (line 458-460). Does this sentence mean that there are two different conditions depending of the flood conditions or seasons? What are the consequences on FeS precipitation and on TA release? This sentence is too vague and raises questions on the temporal representativeness of the data (episodic event? Seasonal variations?). I have the same questions on the spatial representativeness of the data. How do the authors explain the difference between the two replicates Z and Z’? Then, I encourage the authors to discuss about the spatio-temporal representativeness of observations.

My last issue is on the role of bioturbation. I think about bioturbation when I looked at the figure 10. According to the frequency of flood events and to the accumulation rates at the proximal stations, the diffusive transport of the anaerobically-produced alkalinity in the flood deposit to the SWI, takes time no? (see the work of Anschutz and collaborators in natural turbidites (Anschutz et al., 2002; Chaillou et al., 2006) and in experimental turbidites (Chaillou et al., 2007)). Bioturbation and biodiffusion could be an efficient mechanism to transport anaerobically-produced metabolites, as TA from the anaerobic zone to the surface. Did the authors measure the bioturbation coefficients in the incubations? Did they consider the macrofauna in the studied sediment? What about the difference between total fluxes and diffusive fluxes of DO, TA and DIC? Are they similar (same magnitude)?

The authors are kindly asked to see the attached annotated PDF with my suggestions

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and points. Finally, I encourage the author to shorten the result and discussion sections before to resubmit the paper. This study has definitively a strong potential and must be published.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-32/bg-2019-32-RC2-supplement.pdf

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