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Comment on bg-2021-5

Tom Jilbert (Referee)

Referee comment on "Deep-water inflow event increases sedimentary phosphorus release on a multi-year scale" by Astrid Hylén et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-5-RC2, 2021

The study "Deep-water inflow event increases sedimentary phosphorus release on a multi-year scale" presents a thought-provoking hypothesis concerning the effect of deep water oxygenation on DIP fluxes from the sediments of the Eastern Gotland Basin (EGB), Baltic Sea. In a nutshell, the authors propose that the repeated inflows of 2014-2017, which led to persistently oxygenated bottom water conditions at their study sites, promoted organic matter remineralization in the surface sediments, leading to an overall enhancement in the flux of DIP to the water column relative to that observed during the typical anoxic conditions. According to the authors' calculations, the post-oxygenation flux of DIP exceeded that predicted from the re-release of P trapped immediately following the initial oxygenation, hence leading to the conclusion that an additional P source (OM remineralization) is required to explain the results. The observations run contrary to a long-held paradigm concerning retention of DIP by sediments following oxygenation. Several mechanisms are proposed to explain the results, including changes in organic matter supply and enhanced rates of remineralization under oxic conditions.

The main body of data supporting the core hypothesis is derived from lander-based flux measurements of DIP, DIC, DIN and DSi. This dataset is a very high-value component of the paper. The level of replication is good, and the parallel trends in the fluxes of all biogenic parameters during the period 2015-2018 appear robust. The more problematic aspects of the study concern the attempted closed-sum budgets of P cycling, including the upscaling of the data to larger areas of the EGB. Some of the interpretations and assumptions here need to be better elucidated. In general I am supportive of the study and especially the robust flux, porewater and sediment data, but I feel that the further treatment of that data needs improvement to be ready for Biogeosciences.

Major comments:

As raised by the first reviewer, there is an issue concerning the closed-sum budget of in-

situ P pools in the sediments and the estimated DIP fluxes, that requires further elaboration in the text. The range of measured DIP fluxes at station F during 2016-2018 is 2-4 mmol/m2/d (Fig. 3a, b). The first reviewer calculates that a 2 mmol/m2/d flux would exhaust the P-org supply of the upper 5 cm of the sediment column in 200 days, assuming porosity 0.8 and P-org = 15 umol/g. I repeated this calculation with a porosity of 0.9 (probably closer to the true value in the EGB) and of course the duration is even shorter due to the lower volumetric P-org inventory in the sediments (less than 100 days, exact value depends on assumed sediment density). The flux data are correct, and so are the sediment profiles, so to me the only explanation can be that the turnover of biogenic material at the very surface of the sediments (e.g. in the "fluffy layer") must be extremely rapid, therefore dictating the high fluxes. This is very important because it has an implication for the further interpretations. If the observed DIP flux cannot be sustained by the in-situ pool of P-org (as measured in the sediment profiles), it must be sustained by an incoming flux of organic material to the sediment surface. The higher fluxes in 2016-2018 therefore indicate first and foremost that the input of organic material has increased at station F. I suggest that the authors use their own porosity data, if available, to carry out this exercise for themselves and present the results and implications in a revised version.

Following on from this line of thought, I am concerned that the study interprets the enhanced fluxes (and especially DIP fluxes) at these study locations too simply. For example, section 3.4 is devoted to implications of DIP release for coastal deoxygenation and is set up in a way that extrapolates the observed fluxes to a larger area, implying overall enhancement of DIP fluxes and therefore a possible feedback to eutrophication/hypoxia. On the contrary, I would challenge the authors that what is being observed here is an internal rearrangement of the distribution of organic material in the deep areas of the EGB after the inflow, leading to changes in remineralization rates at different locations. Therefore the enhanced fluxes at one location are presumably matched by lower fluxes at another location where the same organic matter would be sedimented under normal conditions. The authors refer to several recent papers describing possible physical mechanisms such as gravity currents and particle aggregation following oxygenation, which could modify the spatial distribution of sedimentation of organic material. I think these are critical to understanding the results. For example, I would ask the question whether we are looking primarily at an enhancement in particle shuttling into deeper areas following the inflow? (it is interesting to note that the deeper site F shows the strongest effects on fluxes, see e.g. Fig. A2, despite a similar oxygenation timeline to the shallower site E, see e.g. Fig. A1).

I do not deny that remineralization of OM under oxic conditions can be more rapid (at least for certain compounds; I refer to the cited Arndt et al. 2013 ESR paper for the concept) and hence that the observed effects on fluxes can be influenced directly by the presence of oxygen. However the fact remains that the in-situ pool of OM is insufficient to support the temporally-extrapolated fluxes as presented in Fig. 5, under either oxic or anoxic conditions. So the authors must emphasize the role of variable carbon inputs *(donor control* in the terminology of Arndt et al.) in their interpretation.

It is of course a fascinating result that there is a genuine increase in the flux of DIP from sediments to the water column in 2016-2018, despite the oxic bottom water conditions. This does indeed run contrary to the often-cited paradigm of P retention with Fe oxides under oxic conditions (e.g. early Einsele and Mortimer papers). This begs the question,

why should the P not be retained after release from OM during remineralization? Is it simply a question of the relative availability of Fe-Mn oxides with respect to P, or do the authors have an alternative hypothesis? Note that Fe:P ratios are often used in limnological studies to estimate potential P retention in sediments (Jensen et al Hydrobiol. 235, 1992). If the authors have bulk sediment Fe and Mn contents (or even e.g. CDB-Fe, CDB-Mn data) these could be added to aid this investigation. At the very least, further discussion is required on this important topic.

Minor comments:

Line 61, 340: The reference to Jilbert and Slomp (2013) is not ideal for this concept. That paper focused more on the role of particle shuttling in stimulating authigenic mineral formation. I suggest to find an alternative.

Line 195-200 and Fig 3: It took me a while to understand the message here, which is that an impact of Fe-P dissolution on the flux data would be seen as a DIC:DIP ratio even lower than the mean value of 40 observed in 2016-2018. I think the figure can be annotated to improve this concept. Perhaps add another line for the ratio of 40, or a segment for the ratio 20-70 as referred to in the text, and modify the text to link better to the figure. Also label the lines in the figure (including the existing Redfield line) with the corresponding ratios.

Line 231: It is not clear what is meant by “change over time in response to external factors”. Please clarify.

Line 233: I think that the SMHI data referred to here is presented in Fig. A1. If so please cite the figure.

Line 318-320: Should the units in the text be the same as those in Fig. 5 (mmol/m2/d)?