Biogeosciences Discussions

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Comment on bg-2021-100

Anonymous Referee #1

Referee comment on "Phosphorus natural background estimation in the Scheldt river using tidal marsh sediment cores" by Florian Lauryssen et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-100-RC1, 2021

Comments –

The manuscript titled "Identification of the natural background of phosphorus in the Scheldt river using tidal marsh sediment cores" by Lauryssen et al., is a good piece of work to justify the possible sedimentary records of dissolved phosphate-P in river water in the past. Authors have analysed two types (old and young) of sediment cores from the river flood-land to establish the fact that bottom sediment retains the signature of phosphate concentration in overlying water without much alteration over the period of time. And based on those sedimentary records authors have tried to predict the "natural background" value of phosphate-P prevailed during the age of pre-industrialisation and/or any major human intervention. For this objective, one should be very careful about the possible artifacts in prediction of such background value; because that may cause unexpected errors.

No doubt it is an innovative attempt; but the present manuscript has some shortfalls and queries. The text language is fine but some longer statements can be avoided.

In present study it was assumed that the immobile nature of P helps in retaining water column signature in sediment. From this basic idea it is expected that the past time-zones which correspond the phosphate peaks in water should precisely match with the peaks of P_{ox} or DPS found in core sediment. In Figure 2, the maximum dissolved phosphate peak appears immediately after 1980s but the most of the DPS profiles (except Young-1) in Figure 3 have peaks before 1980 (between 1920-70). In this regards some suitable explanations have to be included in the discussion section.

Authors have estimated the level of P-saturation in sediment samples following a relation (as mentioned in the Equation 1); actually verified for the porewater-soil system of typical agricultural fields. This similar relation could be applicable to the estuarine sediment also?? Specially, I have doubt about the factor of 0.5 in this Equation 1. It may differs for sediments in aquatic environments; characterised with different geochemical settings as compared to those from agricultural lands.

Ln 79-80: "In lowland rivers with tidal influence, also called estuaries such as the Scheldt estuary in Flanders, tidal marshes….." The statement is not clear; use simpler sentences.

Ln 106-07: Author has mentioned that they restrict their observation within the freshwater areas of the Scheldt estuary. Is there any specific reason for selection of only freshwater segments of that estuary?? -Here they should cite those actual reasons for such site selection in context to the aims of the present study.

Ln 120-121: Authors should mention about the site specific sedimentation rates (as obtained from radiometric dates) at four sampling locations. As the sediment accretion rates in this estuarine system are quite variable (e.g. 0.3-3.2 cm/yr); this information would provide better perception regarding the 'young" and "old" settings.

Ln 150-155: This section of sediment analyses needs more details. It has "…..solid-liquid ratio of 1 g in 50 ml….". What is that solid-liquid ?? Here, it looks solid-liquid refers sediment-oxalate solution. Provide the details about the liquid and its purity, concentration etc used for this leaching experiment.

Ln 250: In the Table 1, authors should provide the estimated Fe_{ox} and Al_{ox} values for each samples. This would help reader to verify the DPS values presented in the table.

Ln 295: In Table 2, there are two background values (for 1800 and 1930) have been presented. Its bit confusing; which one represents the natural background value. Is 1930s value is really essential??

Secondly, in the Table 2 caption, it has mentioned that the profiles of only one site (Old-1) has used for prediction of background phosphate concentration. However, in same table, the model 3b is based on Young-1 and Old-1 data set). This is also confusing.

Ln 309-310: The models using mean DPS and individual PO4 are considered more useful for evaluating the factor "K". Here authors should mention the proper reasons for this choice.

Ln 356-357: The summer anoxia induced oxide dissolution has projected for excess P loads in river water and thus explained for higher projected background value of 62 microgm/l. Summer anoxia is generally short-term seasonal process and therefore is it really could be effective to maintain higher background over the long run?? Or, it is some other processes responsible for such projected higher values in river water relative pristine lakes. Similar effects of seasonal anoxia are not evident in those lake waters??

Ln 400: "Our data estimated that the pre-industrial background concentration is about half of the….." In this conclusionary statement, mention the actual predicted background value i.e., 62microgm PO4-P/l along with confi. Intervals.

The plots of Figure S9 are easy to visualise as compared to those in Figure S7 and S8. Therefore, S7 and S8 can be presented in similar format of S9. Furthermore, in these depth profiles, the extremes (Highs and lows) should marked with corresponding ages. In my opinion instead of supplementary documents, these depth profiles would be good to present as the part of the main text.