Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-388-RC1, 2019

Interactive comment on "Low CO_2 evasion rate from the mangrove surrounding waters of Sundarban" *by* Anirban Akhand et al.

Anonymous Referee #1

Received and published: 5 November 2019

Review of Akhand et al submitted to BG

This paper reports some original new data in the water surrounding the Sundarban, the largest mangrove forest in the world. The major result obtained is the low pCO2 in these waters, which, contrarily to previous reports elsewhere, act as a CO2 sink to a moderate CO2 source. Although the pCO2 data presented here are apparently of the required quality, some other parameters (for instance alkalinity) have very unusual values, which makes publication hazardous. Most importantly, the paper fails at explaining why the mangrove waters in the Sundarban emit little CO2, and why these mangroves behave differently from the others elsewhere. The paper is very speculative and not based on adequate knowledge and appropriate reference to the literature, although the reference list is well updated. Some of the presented data are almost out of scope and do not provide any relevant information to explain the observed low pCO2; some parameters such as NEP and NEC (Fig.4) are not described in the methods. Discussion and conclusion are very speculative and not supported by the data, nor by a correct analysis of the literature. For all these reasons, I recommend rejection.

Major comment #1. Data quality and data relevancy. In fig.2, the temporal evolution of parameters during 24h cycles show minimum pH (when available) and maximum pCO2 that are not in phase. Minimum pH always occurs about 2-3 hours before the maximum pCO2. The quality of these data is highly questionable. In addition, the authors report a total alkalinity (TA) value of 1.646 mmol kg-1 in the marine end-member (P14 L301), which is very strange. TA is very relatively constant in the surface ocean with values around 2.2 mmol kg-1 (same values as those reported by Goyer et al.). Even if the salinity of this marine end-member is only 26, if the freshwater TA is 2.977 (P17 L 361), it is hard to believe that TA at salinity 26 can be so low… The authors present data of Net ecosystem Production and Net Ecosystem Calcification, but the methods are not described in the main text. In addition, they present the result of these parameters ONLY FOR ONE STATION, because "measured NEP and NEC yielded statistically significant relationships only at C2 among the eight stations" (P18L382). If I well understood, the method gave exploitable results only at one station and incoherent or not significant results at the other 7 stations. If this is the case, I suggest the authors question the reliability of their method and simply do not publish the results of the unique case were it apparently worked before checking what was wrong with the other 7 stations.

Major comment #2. The authors attribute low pCO2 to the buffer capacity of the carbonate system in these waters. However, the buffer capacity alone will never make seawater change from CO2 source to CO2 sink. Buffer capacity (high revelle factor) will make the pCO2 lower for a same CO2 input, but it will never make the water a sink. Biological uptake is necessary (in the case here of a high alkalinity in the freshwater end-member, thermodynamical mixing will not generate pCO2 values below the atmospheric equilibrium). The authors also attribute low pCO2 to strong “dilution” of mangrove soil porewater with estuarine surface waters. However, they do not provide quantitative evidence that dilution could be more important in the Sundarban than in others mangrove waters elsewhere in the world. All the discussion is extremely speculative and finally it does not explain why pCO2 is low at the study site. A detailed analysis of pCO2 variations as a function of salinity may have made the paper less speculative.

Detailed comments P11: it is not clear what is the interest of these CDOM and SUVA data for the main message of the paper (same for NEP and NEC) P13 L 268: why choosing the k600 of Ho et al. 2011 and not other parameterizations from other authors? P13L281 NEP and NEC are not easy to measured and it is strange to describe these methods in the supplementary material P14L286 how is Xmix calculated? Provide a formula L294 what is a “near-zero salinity regime”? L292-299 this looks like discussion and not material and method L300-312 this looks like results or discussion but not material and method P18 section 3.4 these parameters are not defined and explained in the text. The fact that only one value could be obtained among the 8 experiment make the quality of these data questionable. Why are NEP and NEC negative? This is not understandable. Section 3.5. the problem with these parameters is that they do not provide relevant information that could help the interpretation of the low pCO2 values. ARE THESE WATERS RICH IN PHYTOPLANKTON? Uptake of CO2 by phytoplankton could be the reason of low pCO2 L398-404: only truisms here, you say basically nothing Section 4.2: the term “diel” is confusing here, as most of the pCO2 variations are driven by tidal movements, but “diel” generally refer to differences between night and day. The term “CO2-lean seawater” you use all through the MS is awkward

L437 this is speculation, not supported by any data L439 “as the consumption of bicarbonate by phytoplankton during photosynthesis activity tends to convert pCO2(water) to bicarbonate ions (ie a decrease in pCO2)” please write statements in accordance with classical handbooks on ocean carbonate chemistry. L447-448 “the photosynthetic potential and composition of phytoplankton would not vary widely between these stations because they are close to each other” this is just speculation, no data on phytoplankton are available The all section L446-453 is pure speculation L462 “the unit increase in pCO2 with respect to the unit input of DIC was much higher at the creek stations than at the IB station” not understandable L465 “greater water turnover enhances the solute fraction” any quantitative evidence for that? L467-468 high speculatives statements L474 last sentence “pore water is generated exclusively within the mangrove environment” awkward formulation L474 groundwater seepage would increase pCO2 not decrease End of page 22 until centre of page 23 is pure speculation, the revelle Factor will not explain explain low pCO2 in this cases

L505: buffering capacity cannot generate a CO2 sink alone... L513 high d13C-DIC values are not necessarily due to carbonate dissolution, it can be fractionation by phytoplankton L508-520 the potential impact of phytoplankton and gas exchange on d13C-DIC is missing Section 4.4 is highly speculative and not connected to the question of low pCO2

Fig3 : use squares triangles and circles as symbols; an explanation for the positive TA anomalies is missing in the text

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-388, 2019.