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

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Response to reviewer 1's comments:

Reviewer 1's comments: 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 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.

Authors' Response: Thank you very much Reviewer 1 for the constructive comments on the improvement of our manuscript.

(1) Quality of the data:

We believe that the quality of the data was thoroughly maintained for all the parameters. However, thanks to Reviewer 1's comments, we found an unintentional error in our present version of the manuscript. Almost all the sensors used in this study were set according to Japan Standard Time. We prepared the other graphs by correctly adjusting the data to Indian Standard Time, but unfortunately, we wrongly adjusted the time stamp for our pH sensor by mistake. As Indian Standard Time and Japan Standard Time has a time difference of three and a half hours, this disparity between pH and pCO2(water) data occurred. We'll correct the time stamp for our pH sensor in the revised manuscript. Nevertheless, any interpretation has not been affected for this unintentional error.

(2) Low TA in MEM during the post-monsoon season:

We agree that the TA value in the marine end member (MEM) is unusually low. That is why we have already explained the background of MEM in details in the methodology section (from Line 300 to 312) using published and our unpublished data. Briefly, the TA data of the present study are also in good agreement with a few available published work in this transition zone conducted during the post-monsoon season of 2010-2011 and 2011-2012 (January and February) (Akhand et al. 2012; 2013), showing low values during post monsoon season (the season of the present study). From the unpublished data, the TA of MEM was found to be higher during pre-monsoon (1932 μmol kg-1) and monsoon (1879 μmol kg-1), than post-monsoon (1646 μmol kg-1). Indeed, the sampling and cruise of Goyet et al. (1999) was conducted between 29 August 1995 and 16 October 1995, which was of different season and far offshore (approx. 10o N latitude). Furthermore, the present study used the standard preservation method for TA samples and used the gran plot method with an automatic titrator (batch sample analyser). Moreover, a certified reference material was procured and used to maintain the company specific accuracy of the batch sample analyser. Therefore, we believe that the present work is reporting sufficient precision to be published. To further examine the interesting phenomena of low TA in MEM during the post-monsoon season, we

also examined the modelled data archive (https://esgf-node.llnl.gov/projects/esgf-llnl/) (Dunne et al. 2012). The spatial resolution of the data is coarse, i.e. 1o; hence, not exactly comparable with our field observation data because these data covers whole area of the coastline to the offshore transition zone, but we believe that it can be used as a supporting data to understand the carbonate chemistry of MEM. We used the spatial extent of 20o to 21oN and 87oto 91oE to extract the data which covers a substantial area of the northern Bay of Bengal and for the years 1995-1996 to 2004-2005 (10 consecutive years). The extracted data showed the mean TA value being 2.11 mmol kg-1(pre-monsoon, from February to May), 2.04 mmol kg-1(monsoon, from June to September) and 1.98 mmol kg-1(post-monsoon, from October to January), indicating the same pattern as our field observation data, i.e., TA never reaches 2.2 mmol kg-1 in the transition zone, and both TA and DIC decrease during post-monsoon season. The decrease in both TA and DIC during post-monsoon seasons might be because of phytoplanktonic calcifies (foraminifera and coccolithophores) blooming during post-monsoon season (Biswas et al. 2004) in the northern Bay of Bengal (Stoll et al. 2007; Mergulhao et al. 2013).

Overall, the phenomenon of low TA in MEM during the post-monsoon season is indeed a very interesting topic of research. However, addressing further this phenomenon is out of scope for our present work.

(3) NEP and NEC:

We agree with the comments of both Reviewer 1 and 2. We'll eliminate all the part of NEP and NEC in the revised manuscript, according to your suggestion.

Authors' changes in the manuscript: We'll correct the time stamp of the pH data in Fig. 2 according to Indian Standard Time and will eliminate NEP and NEC part from the whole manuscript. We'll add sentence(s) and citation(s) to describe the probable reason behind such low magnitudes of TAlk and DIC during post-monsoon in the marine end member in 'Data Analysis' (section 2.6) under 'Materials and methods' while describing the carbonate chemistry of the MEM.

Comment of the Reviewer 1:

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 sea water 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.

Authors' Response: The focus of the present work is not 'CO2 sink character'. It is well established fact, as a whole, mangrove surrounding water of Sundarban (Indian part) is net source of CO2 covering all seasons throughout the year and considering upper to lower estuary. The fact is evidenced from a number of previous studies and in parity with the upscaled data of the present study (section 4.5). The tittle of the present manuscript also represents CO2 'evasion', not 'sink'. Hence, we explained in the present version of the manuscript that the CO2 efflux / evasion rate of Sundarban is much lesser than the recently estimated world average due to high buffer capacity (low revelle factor).We also agree with that biological uptake can also an important mechanism explaining for low pCO2.

However, we did not state about biological uptake in the present manuscript because biological uptake seems to be minor. We showed using Fig. 5a that most of the $\triangle$pCO2

value was positive, which indicates the study area exhibited heterotrophy. We'll add sentence(s) for better clarification of this issue. Furthermore, we collected time series phytoplankton standing stock (chlorophyll-a fluorescence) data using a fluorometer but did not get any significant correlation with pCO2 (water). However, we agree with the reviewer's concern and will present the time series data of chlorophyll-a in the supplementary material (to be included in figure S1). We'll also add discussion about less correlation with pCO2 (water) and chlorophyll-a to show the less significant effect of phytoplankton productivity in pCO2(water) and CO2 flux, especially in the stations showing CO2 sink.

Nevertheless, we believe the main reason for the low pCO2 character is mainly twofold: 1. Special character of MEM i.e. Bay of Bengal and subsequent predominance of low pCO2 marine water. Bay of Bengal is specially having low pCO2 and considered as a sink for CO2 (Kumar et al. 1996; Goyet et al. 1999; Akhand et al. 2013). In contrast, western part of Indian Ocean, i.e. Arabian sea is considered as a strong source of CO2 (Kortzinger and Duinker 1997; Sarma et al. 1998; Sarma 2003). 2. Getting lesser amount of riverine freshwater, which are well discussed.

In turn, Reviewer 1's notion that we are stating that "quantitative evidence that dilution could be more important in the Sundarban than in others mangrove waters elsewhere in the world" is a misunderstanding as we have not stated such an assertion. We believe, dilution is equally important in all mangrove surrounding water, that is why tidal variability i.e. high tide / low tide shifting is so important explaining CO2 dynamics in mangrove surrounding water (Ovalle et al., 1990; Maher et al., 2013; Akhand et al. 2016; Yang et al. 2017). Hence, the tidally driven estuaries of central Sundarban (Indian part) are affected by the low pCO2 water of Bay of Bengal. This effect of Bay of Bengal, is further accentuated for rapid transport of material due to estuarine geometry, as discussed in L 530 to L 535 of the present version of the manuscript. However, according to Reviewer 1 and Reviewer 2's concern and to avoid ambiguity, we'll delete the word 'rapid' from L 436 and L 437 in the revised manuscript.

Authors' changes in the manuscript: We'll add / edit sentence(s) for better understanding of the Revelle factor issue. We'll add a separate paragraph to clarify about the 'Biological factor' in relation with low pCO2. We'll add sentence(s) on the heterotrophic nature of the study area showing Fig. 5a, regarding ΔpCO2 value. We'll add chlorophyll-a time series data in the Fig. S1 and will add sentence(s) on the relation between pCO2 and Chlorophyll-a in the 'Discussion' section along with necessary sentence(s) in the 'Materials and methods' and 'Results' section. We'll add sentence(s) and references(s) on the special low pCO2 character of the Bay of Bengal in the 'Introduction' section. We'll delete the word 'rapid' from L 436 and L 437.

Comment of the Reviewer 1:

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)

Authors' Response: According to the suggestions of Reviewer 1 and Reviewer 2, we'll eliminate the NEP, NEC and optical signature, i.e. CDOM part from the whole manuscript.

Authors' changes in the manuscript: We'll eliminate the NEP, NEC and optical signature, i.e. CDOM part from the whole manuscript.

Detailed comments: L 268: why choosing the k600 of Ho et al. 2011 and not other parameterizations from other authors?

Authors' Response:

The justification is there in L 267 to L 271. However, according to the concern of Reviewer 1 and suggestion of Reviewer 2, we'll add other parameterisations for flux calculation.

Authors' changes in the manuscript: We'll add other parameterisations for CO2 flux calculation.

Detailed comments: NEP and NEC are not easy to measured and it is strange to describe these methods in the supplementary material

Authors' Response: We'll eliminate NEP and NEC part from the whole manuscript.

Authors' changes in the manuscript: We'll eliminate NEP and NEC part from the whole manuscript.

Detailed comments: how is Xmix calculated? Provide a formula

Authors' Response: For calculation of Xmix, linear and non-linear two end member mixing model have been implemented, which are in common use and the formula proposed by Mook and Tan (1991) have already been cited in the present version of the manuscript, we believe that no more details are needed.

Authors' changes in the manuscript: No change.

Detailed comments: what is a "near-zero salinity regime"?

Authors' Response: We'll replace the phrase "near-zero salinity regime" by "near-zero salinity region" in the revised manuscript.

Authors' changes in the manuscript: We'll replace the phrase "near-zero salinity regime" by "near-zero salinity region" in the revised manuscript.

Detailed comments: L292-299 this looks like discussion and not material and method L300-312 this looks like results or discussion but not material and method

Authors' Response: L 292 to L 299 of the present version of the manuscript is the justifications for choosing the freshwater end member and marine end member sites. Further, L300-312 is justifications to explain the characteristics of carbonate chemistry parameters of the marine end member and the Bay of Bengal, as the Reviewer 1 questioned. We believe that it is better remained this part in 'Materials and methods' section rather than 'Results' or 'Discussion'.

Authors' changes in the manuscript: No change.

Detailed comments: 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 experiment make the quality of these data questionable. Why are NEP and NEC negative? This is not understandable.

Authors' Response: We'll eliminate NEP and NEC part from the whole manuscript.

Authors' changes in the manuscript: We'll eliminate NEP and NEC part from the whole manuscript.

Detailed comments: 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

Authors' Response: Despite of not being directly related, why OM related parameters are also important in relation to pCO2(water) and air-water CO2 flux in mangrove environment, have already explained thoroughly in L 575 to L 583.We'll add a separate paragraph and discuss about 'biological uptake' and phytoplankton standing stock (chlorophyll-a) in relation to pCO2(water) and could show the effect of 'biological uptake' and phytoplankton productivity on low pCO2 and CO2 uptake in this case.

Authors' changes in the manuscript: We'll add a separate paragraph and discuss about 'biological uptake'. We'll add chlorophyll-a time series data in the Fig. S1 and will add sentence(s) on the less correlation between pCO2 and Chlorophyll-a in the 'Discussion' section along with necessary sentence(s) in the 'Materials and methods' and 'Results' section.

Detailed comments: L398-404: only truisms here, you say basically nothing

Authors' Response: This part will be eliminated, as we'll eliminate the optical signature, i.e. CDOM part.

Authors' changes in the manuscript: We'll eliminate optical signature, i.e. CDOM part from the whole manuscript.

Detailed comments: 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.

Authors' Response: As per our knowledge, "diel variability" refers to variability within "24 hours cycles which includes day and night shifts". We think, though the term 'diel variability' generally used to "refer the differences between day and night", but it can be also used for other differences within 24 hrs cycle (for example, due to high tide / low tide). However, for better clarification, we'll use 'diel and tidal variability' according to the purpose of use. "pCO2 lean" will be replaced by "low pCO2" in this section and throughout the manuscript.

Authors' changes in the manuscript: We'll use 'diel and tidal variability' according to the purpose of use in the revised manuscript. We'll replace the term "pCO2 lean" by "low pCO2" throughout the manuscript.

Detailed comments: L437 this is speculation, not supported by any data

Authors' Response: We believe that this is not speculation because it has been supported by DIC and δ13CDIC data. Please see figures 3b, 3i and 5a. However, we'll delete the word 'rapid' from this sentence.

Authors' changes in the manuscript: We'll cite figures 3b, 3i and 5a in L437 and will delete the word 'rapid' from this sentence.

Detailed comments: 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.

Authors' Response: We agree with the reviewer, that this statement was a bit ambiguous and meaning was not clear. We'll revise the sentence in the revised manuscript.

Authors' changes in the manuscript: We'll revise the sentence in the revised manuscript.

Detailed comments: 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

Authors' Response: We'll provide chlorophyll-a data as well as the relationship with pCO2 in the revised manuscript.

Authors' changes in the manuscript: We'll add chlorophyll-a time series data in the Fig. S1 and will add sentence(s) on the less correlation between pCO2 and Chlorophyll-a in the 'Discussion' section along with necessary sentence(s) in the 'Materials and methods' and 'Results' section.

Detailed comments: The all section L446-453 is pure speculation

Authors' Response: We'll delete this part from the revised manuscript as it is not directly related to the scope of the study.

Authors' changes in the manuscript: We'll delete this part from the revised manuscript.

Detailed comments: 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

Authors' Response: We wanted to convey the exact meaning of higher Revelle factor. Revelle factor actually tells us the unit increase in pCO2 when unit DIC is introduced to the aquatic system. However, we'll simplify the sentence and will re-write it in the revised manuscript.

Authors' changes in the manuscript: The sentence mentioned by the reviewer will be recast in the revised manuscript.

Detailed comments: L465 "greater water turnover enhances the solute fraction" any quantitative evidence for that?

Authors' Response: No, we do not have any quantitative evidence. We'll change the sentence and citation to explain the role of pore water on pCO2 dynamics in mangrove surrounding water, as there is no quantitative evidence.

Authors' changes in the manuscript: We'll change the sentence and citation.

Detailed comments: L467-468 high speculatives statement

Authors' Response: The speculative part will be deleted.

Authors' changes in the manuscript: We'll delete the speculative part of the sentence.

Detailed comments: L474 last sentence "pore water is generated exclusively within the mangrove environment" awkward formulation

Authors' Response: We'll revise the sentence in the revised manuscript.

Authors' changes in the manuscript: We'll revise the sentence in the revised manuscript.

Detailed comments: L474 groundwater seepage would increase pCO2 not decrease

Authors' Response: We have never asserted it.

Authors' changes in the manuscript: No change.

Detailed comments: 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.

Authors' Response: We agree with Reviewer 1 at this point that 'biological uptake' should be discussed for better clarification. We'll add this part and we'll add phytoplankton standing stock data, which can provide further explanation about low pCO2.

However, we disagree with "the revelle Factor will not explain low pCO2" as we replied above both buffering capacity (Revelle Factor here) and biological uptake are important mechanisms explaining low pCO2. Authors' changes in the manuscript: We'll add sentence(s) to better explain revelle factor. We'll add a separate paragraph about the role of 'biological uptake' in low pCO2. We'll add chlorophyll-a time series data in the Fig. S1 and will add sentence(s) on the less correlation between pCO2 and Chlorophyll-a in the 'Discussion' section along with necessary sentence(s) in the 'Materials and methods' and 'Results' section.

Detailed comments: L513 high d13C-DIC values are not necessarily due to carbonate dissolution, it can be fractionation by phytoplankton

Authors' Response: In this line, we explained the deviation plot. Here, the determining factor is not only 'high d13C-DICvalues', but the combination of positive ΔDIC with positive Δd13C-DIC with respect to their conservative mixing concentration. We'll add sentence on the effect of isotopic fractionation by phytoplankton in appropriate place. However, in L513, 'fractionation by phytoplankton' is not the dominant regulating factor.

Authors' changes in the manuscript: We'll add sentence on the effect of isotopic fractionation by phytoplankton to appropriate place.

Detailed comments: L508-520 the potential impact of phytoplankton and gas exchange on d13CDIC is missing

Authors' Response: The potential impact of phytoplankton is mentioned in L 518. We'll add a sentence about the negligible effect of gas exchange, as the CO2 exchange from the water was nearly in equilibrium.

Authors' changes in the manuscript: We'll add a sentence about the negligible effect of gas exchange, as the CO2 exchange from the water was nearly equilibrium.

Detailed comments: Section 4.4 is highly speculative and not connected to the question of low pCO2

Authors' Response: The optical signature part, i.e. CDOM will be eliminated according to the suggestion of Reviewer 1 and Reviewer 2 . Authors' changes in the manuscript: We'll eliminate optical signature, i.e. CDOM part from the whole manuscript.

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

Authors' Response: We'll edit Fig.3 according to the suggestion of Reviewer 1 and add sentence(s) on positive TA anomaly.

Authors' changes in the manuscript: We'll edit Fig.3 according to the suggestion of Reviewer 1 and add sentence (s) on positive TA.

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