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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 2's comments:

Reviewer 2's comment:

The present study investigates water pCO2 at 8 different stations of mangrove surrounding waters (creek, island boundary, mid-river) at Dhanchi Island in the Sundarbans, India. The authors present an interesting high resolution data set (8 x 24h time-series, diurnal, tidal) at 1 min interval of pCO2 and find mangrove surrounding waters to be a weak source or sink of atmospheric CO2. The authors aim to reveal and identify why the here studied mangrove waters act as a net sink compared to previous studies, that are commonly found to be a source of CO2. They conclude that the reduced river-

ine input and increased buffering capacity from oceanic water is responsible for the low pCO2 in the mangrove waters. Although the data set is impressive and worthwhile publication, I am not convinced that the authors have sufficiently identified and discussed the low pCO2 at the different study locations based on their data. The discussion is very speculative (see reviewer#1, I mostly agree with reviewer #1: regarding the low TAlk of the marine end-member. This value is questionable. I also agree, that the Revelle cannot be used to explain the CO2 sink. I further agree with reviewer #1 that some of the data (optical, NEP, NEC) seem out of context and do not provide relevant information to explain the low pCO2.NEP and NEC calculations need to be included in the methods section. High salinity combined with high abundance of phytoplankton or benthic micro-algae could be an explanation for the low pCO2.)

Authors' Response:

Thank you very much Reviewer 2 for the constructive comments on the improvement of our manuscript.

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

We agree with Reviewer 1 and Reviewer 2 that TAlk value is unusually low in marine end member (MEM). 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explained the background of MEM in details in the methodology section (from Line 300 to 312) using published and our unpublished data.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 the 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.

(2) Use of Revelle factor:

We did not focus on the ‘CO2 sink character’ in our manuscript, because, 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 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’. 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 Reviewer 1 and Reviewer 2, that biological uptake can also an important mechanism explaining for low pCO2. 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 ΔpCO2 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 reviewers’ 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.

(3) Optical signature, NEP and NEC data:

According to the suggestion of Reviewer 1 and Reviewer 2, we’ll eliminate NEP, NEC and optical signature, i.e. CDOM part from the whole manuscript.

Authors’ changes in the 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.

We’ll add 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 eliminate optical signatures (i.e. CDOM), NEP and NEC part from the whole manuscript.

Reviewer 2's comment:

The authors mention that there is no (or almost none) riverine connection. Yet, they use a freshwater end-member upstream to estimate the conservative mixing lines, which does not makes sense if there is no riverine connection. Similarly, the marine end member seems questionable with a salinity of 26, which is very close to the mangrove waters (salinity 25-26).

Authors' Response: Considering Reviewer 2's comments and rechecking previous studies (for example Ray et al. 2018; Dutta et al. 2019) in Sundarban (Indian part) on CO2/ carbon dynamics, we decided to eliminate all the wordings and sentences, which seems there is no riverine connection (or almost none), and replace the confusing phrases like "negligible riverine freshwater input" to "indirect and lesser riverine freshwater input". This is because the quantification of riverine freshwater input from Hooghly River to the Sundarban (Indian part) was not done previously using hydrological modelling and out of scope for the present study, It is well established fact, that Indian part of Sundarbans are not getting riverine freshwater directly from its upstream rivers (for example Matla, saptamukhi, Thakuran etc.), but there are waterways from where the riverine freshwater is entering to the Indian part of Sundarbans, as shown in high resolution maps (for example google earth). Namely, Hatania Doania canal

which connects Hooghly River with Saptamukhi Estuary (Sundarban) (Ray et al. 2018) through Muriganga River. Riverine freshwater from Hooghly River enters through this canal and then spreads to Sundarban by different waterways. We'll show the Hatania Doania canal, which connects Sundarban (Indian part) with Hooghly River, in the Fig.1 of the revised manuscript. Hence, we think "indirect riverine freshwater input lesser than other river dominated estuary" will be more appropriate phrase and will clarify the ambiguity. It is also a well-established fact by previous studies that more riverine freshwater input causes higher pCO2(water) and subsequent air-water CO2 efflux in comparison with lesser input (Jiang et al. 2008; Maher and Eyre 2012; Akhand et al. 2016). We'll add supporting reference(s) and add sentence(s) to 'Introduction' section to clarify the ambiguity and to describe the exact conditions.

The salinity data of the marine end member is comparable with previously published data (Akhand et al. 2012, 2013) in the same region during the same post-monsoon season. The salinity data of the marine end member during the same month (January and February) of sampling is also in good agreement with the data archive of salinity (https://www.nodc.noaa.gov/OC5/woa18/). We anticipate that the salinity at the site is mainly affected by the 'Bengal Fan'. As Hooghly-Bhramhaputra Rivers are huge source of freshwater to the northern Bay of Bengal and because of Bengal Fan, the salinity of 60 km off the coast was found 26.9 in the same month (reported in this manuscript), which is a unique feature in this region. As mainly such a saline water mass enters and recedes within Matla Estuary, the salinity range of 25-26 which observed in the study area during post-monsoon is quite natural.

Authors' changes in the manuscript:

All the ambiguous words, phrases and sentences, like "negligible riverine freshwater input" will be replaced by "indirect and lesser riverine freshwater input". We'll add supporting sentence(s) to the 'Introduction' section to clarify the ambiguity and to describe the exact conditions of both indirect riverine freshwater inputs and the characteristics of the marine end member, i.e. northern Bay of Bengal. We'll show the Hatania Doania canal, which is the main waterway of entering riverine freshwater from the Hooghly River to the Sundarban (Indian part), in the Fig.1 of the revised manuscript and edit the Fig.1 caption accordingly with the citation of Ray et al. (2018).

Reviewer 2's comment:

Secondly, the station C1 and C2 are substantially different and should not be treated as one group. To me, station C2 seems like the only "real" mangrove site. As in several other previously studied mangrove surrounding water locations cited in this manuscript, a single creek ending in a mangrove forest is the ideal location to study tidal and temporal variability and fluxes of inorganic carbon and dissolved gases (tidal pumping). C2 has no connection other than to the estuary. In contrast, C1 is not a "creek" but more a branch or tributary of the main estuary channel that connects the left and right (Thakun) estuary channels, therefore is influenced by biogeochemical processes of both channels. I disagree that 20 meter width is indicative of a "very narrow creek". I am not surprised to see the very low pCO_2 in the main estuary (not river) channel and close by island boundary. These study sites (MR, IB, C1) seem more indicative of a marine environment with low change in salinity (salinity 24-27).

What is the effect from macro-tides compared to meso or micro-tides? Fig.2 MR3 shows a typical diurnal trend of CO_2 rather than a tidal trend. The authors identified correctly that the term "mangrove surrounding waters" can be ambiguous. StationMR1-3 might be better to compare to (previous) estuary CO_2 emissions than mangrove CO_2 emissions?

The latest global mangrove forest distribution is Bunting et al. (2018) Bunting P, Rosenqvist A, Lucas RM, Rebelo LM, Hilarides L, Thomas N, Hardy A, Itoh T, Shimada M,Finlayson CM. 2018. The global mangrove watch - a new 2010 global baseline of mangrove extent. Remote Sensing 10 (10) DOI: 10.3390/rs10101669

Authors' Response: We agree with the concern of Reviewer 2, that categorisation of Creek, Island Boundary and Main River are somewhat ambiguous. Also, thanks to

Reviewer 2, we found that the location of sampling station C1 was wrongly placed in the Fig.1. We'll rectify this unintentional error in our revised manuscript. However, the C1 is also a dead-end creek with a width of 20 m and becoming narrower with more inside, which is very common in Sundarbans. Therefore, we'll delete the word 'narrow', before 'creek'. We'll only categorise our sampling stations as Creek (C1 and C2) and River (R1 to R6) in the revised manuscript. We anticipate that, the less variability of salinity is due to the dominance by the marine water of the Bay of Bengal. Namely, the salinity of the northern Bay of Bengal is much lesser than the open ocean as northern Bay of Bengal receives huge riverine fresh water from Hooghly and Bramhaputra Rivers and because of 'Bengal Fan'. Moreover, freshwater input from the upstream, especially during study period (dry season i.e. post-monsoon), is lesser as discussed in the response of the previous comments.

In turn, we think that IB and MR stations (R stations in the revised manuscript) should also be considered as 'mangrove surrounding water'. The contribution of mangrove derived organic matter is higher in the POM of the creek stations than that of the IB and MR stations, but there are some contributions of mangrove derived organic matter in IB and MR stations (will be categorised as R stations in the revised manuscript) as evidenced from three end-member mixing model (L603-616 of the present version of the manuscript; Fig. S2 and Tables S3 and S4 of supplementary material). Moreover, IB and MR stations (R stations in the revised manuscript) are the only possible way to supply of mangrove derived dissolved and particulate carbon to the coastal sea i.e. Bay of Bengal.

Nevertheless, as we wrote in the 'Introduction' section, there is no well-established demarcation of the term 'mangrove surrounding water', and it might be misleading to demark it with certainty. Similarly, no ideal definition of the 'mangrove surrounding water' is possible. Only way is to consider the waters situated in the vicinity of mangrove forests / stretch / patches. Previously, researchers used the term 'mangrove surrounding water', 'mangrove water' or 'mangrove waterway' alternatively for creek, river or marine water situated in the vicinity of mangroves (for example, Borges et al. 2003; Bouillon et al. 2007; Sippo et al. 2016; Rosentreter et al. 2018; in Sundarban: Biswas et al. 2004). We'll add sentence(s) for better clarification of this point in 'Introduction' section.

Regarding the query 'What is the effect from macro-tides compared to meso or micro-tides?', we just wanted to emphasize on the quick rate of dilution, we used this term in the manuscript. We anticipate that the quick dilution is facilitated by the meso to macro-tidal nature of the study cite as stated in L532 ("large tidal amplitude") of the present version of the manuscript. We'll add the phrase 'meso to marco-tidal estuary' in this line for better clarification.

Authors' changes in the manuscript: We'll categorise our sampling stations as Creek (C1 and C2) and River (R1 to R6), instead of Creek (C1 and C2), Island Boundary (IB1, IB2 and IB3) and Main River (MR1, MR2 and MR3). We'll delete the word 'narrow' before the word 'creek'. We'll add sentence(s) to clarify about the term 'mangrove surrounding water' in the 'Introduction' section. We'll add the phrase 'meso to marco-tidal estuary' in L532 for better clarification.

Reviewer 2's comment: L97-98 what is the difference between "mangrove surrounding waters" and "mangrove waters" in this context here? I would suggest to define what you mean with "mangrove surrounding waters" at the beginning of the manuscript and then use this term consistently throughout the manuscript.

Authors' Response: Same to our response to the previous comment, we want to convey, that 'to define' 'mangrove surrounding water', might be misleading. However, we'll add sentence(s) to clarify about the term 'mangrove surrounding water' in the 'Introduction' section. We'll change the sentence (L97-98) and will use the term 'mangrove surrounding water' throughout the manuscript.

Authors' changes in the manuscript: We'll add sentence(s) to clarify about the term 'mangrove surrounding water' in the 'Introduction' section. We'll change the sen-

tence (L97-98) and will use the term 'mangrove surrounding water' throughout the manuscript.

Reviewer 2's comment: I would suggest to change the title. The term "evasion rate" implies an efflux of CO2 from water to the atmosphere while the authors aim to highlight the influx. Alternatively title similar to this: "Low pCO2 in mangrove surrounding water in the Sundarbans".

Authors' Response: We believe that the title is leaved unchanged as our study did not highlight "CO2 influx", because, as a whole (throughout the year and including upper to lower part of the estuary), mangrove surrounding water of Sundarban (Indian part) acts as a source of CO2. This fact is well established from the previous works and from the upscaled data of the present study (sees section 4.5). We wanted to convey, that the CO2 efflux rate (evasion rate) is much lesser than the recently estimated world average.

Authors' changes in the manuscript: No change.

Reviewer 2's comment: The gas transfer velocity is the highest uncertainty in the gas flux computation; therefore k parameterisations should be chosen carefully. It is advisable to compare fluxes based on several different k parameterisations (not just one) in dynamic tidal ecosystems such as mangrove estuaries. It would be interesting to see how much this would change the average influx/efflux.

Authors' Response: We fully agree with Reviewer 2 at this point. We'll calculate the flux using different k parameterisation and add sentence(s) regarding that calculation.

Authors' changes in the manuscript: We'll calculate the flux using different k parameterisation and add sentence(s) regarding that calculation.

Reviewer 2's comment: L498 "pCO2 concentration" is wrong. It is pCO2 or CO2 concentration (e.g. μM).

Authors' Response: We agree and will change accordingly.

Authors' changes in the manuscript: The word 'concentration' will be deleted in L498.

Reviewer 2's comment: L521-529: This is unclear. Do the authors suggest that the source of DIC is a mix of all the possible sources listed in this paragraph?

Authors' Response: Probably, Reviewer 2 indicated about the paragraph of L508-520. L521-529 is supporting paragraph for the previous one. Yes, this part (L508-520) is a scientific assumption of the mixing of possible sources. We'll add sentence(s) for better clarification.

Authors' changes in the manuscript: We'll add sentence(s) for better clarification in the revised manuscript.

Reviewer 2's comment: L436-439, L601 The authors suggest "rapid transport to the coastal ocean". Do they mean rapid flushing of pore water? Or tidal pumping? Why rapid dilution? This is unclear. It might be helpful to calculate the freshwater flushing times for the estuary to support this hypothesis. Although with no or very little riverine input I assume very low flushing.

Authors' Response: We agree with Reviewer 2, that the word 'rapid' is ambiguous in L436-439. We'll delete the word 'rapid' from L436-439. Ray et al. (2018a) extensively studied the lateral transport of mangrove derived carbon in the adjacent coastal sea, i.e. Bay of Bengal. They stated the reason of "rapid transport to the coastal ocean" as shorter residence time due to large tidal amplitudes and estuarine geometry ("funnelling effect"). We cited their work (L530-535) and references therein to support our hypothesis. These reasons include both 'tidal flushing' and 'tidal pumping'. These reasons have been discussed in L530-535. The present study has not dealt with lateral flux and it is out of ambit to calculate 'freshwater flushing times', because of the unavailability of the residence time and other necessary data in such micro level.

We think, the term 'rapid transport' is appropriate in L601, because explained well previously in L530-535.

Authors' changes in the manuscript: We'll delete the word 'rapid' from L436-439. We'll replace the citation "Ray et al. 2018a" by "Ray et al. 2018a and the references therein". The word 'rapid' remained unchanged in L601.

Reviewer 2's comment: Yes, the term "pCO2-lean seawater" is awkward.

Authors' Response: We'll replace the term 'pCO2-lean' by 'low pCO2' throughout the manuscript.

Authors' changes in the manuscript: We'll replace the term 'pCO2-lean' by 'low pCO2' throughout the manuscript.

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