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

Anonymous Referee #2

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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 (8x 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 riverine 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

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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.)

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).

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 pCO2 in the main estuary (not river) channel and closeby 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 CO2 rather than a tidal trend. The authors identified correctly that the term "mangrove surrounding waters" can be ambiguous. Station

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MR1-3 might be better to compare to (previous) estuary CO2 emissions than mangrove CO2 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

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.

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, a title similar to this : "Low pCO2 in mangrove surrounding water in the Sundarbans".

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.

L498 "pCO2 concentration" is wrong. It is pCO2 or CO2 concentration (e.g. μM).

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?

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.

Yes, the term "pCO2-lean seawater" is awkward.

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