

## Overview

The authors reviewed carbon biogeochemistry in the India Coast, presented its spatiotemporal variability and discussed the potential drivers. As a continuum between land and ocean, estuarine system is important while suffering a great spatiotemporal heterogeneity. Such study could help constrain this variation and better understand its essential role on global carbon budget. However, this manuscript of its current version still saves a large space for improvement, particularly from its data interpretation and content structure perspectives. I am afraid this manuscript will need a thorough revision to fit for the journal.

## Major comments

(1) Traceable data is vital for a review article. Unfortunately, there is no clear pathway(s) for data sources in this manuscript. For example, how many sampling stations, how many observations for each estuary, sampling time, etc.? We even do not know if the authors are presenting annual average data or just one-time surveyed data. Why there is no standard deviation for each estuary data in figures? A proper conclusive data table (can be supplementary material) is badly needed to show its rigor and reliability.

(2) Following the first major concern, then the data interpretation is problematic. First, the data visualization needs improvement, why only list Sundarbans and Hooghly Estuary sampling stations in Fig.1? Differences on estuaries or dry/wet cycle cannot be well distinguished in both figures and supplementary figures. Second, the way of data processing is also unclear, for example, how do the authors conduct statistical analysis, t-test? two-way ANOVA? any process to meet the assumptions? In supplementary figures, several estuaries are excluded to meet a high p-value relationship seems arbitrary and misleading, same as the threshold  $6800 \mu\text{atm}$  for  $p\text{CO}_2$ . Is there any reason/accordance to do so?

Also, I have a feeling that the authors messed with riverine and estuarine data. For example, in Line 815 the “ $\sim 10.30 \text{ Tg C yr}^{-1}$ ” belongs to riverine export fluxes (Krishna et al., 2019) rather than “export fluxes from Indian estuaries”, and the following discussion (Lines 819–831) is all about riverine C exports. Accordingly, in Fig. 7 export flux values may put in wrong place. Similarly, I do not think there are so many dams built in coastal estuaries list in Table 1. This is the reason why readers are curious about the data details, if so, I would suggest the authors clarify each estuary area/coordinates and further check about the data.

(3) The manuscript structure is organized in a research article format instead of a review. In addition, the separated discussions on DIC, DOC, POC,  $\text{CO}_2$ ,  $\text{CH}_4$  read super repetitive and distracting. In fact, carbon biogeochemistry is comprehensive and synthesized, drivers (e.g. hydrologic, biochemical, etc.) on any single carbon species would further impact on other carbon interactivities and then the entire carbon budget. Re-organization of manuscript structure to look at the drivers more synthetically is highly recommended.

(4) Many important information are missing, such as temperature gradient, wind speeds, net

ecosystem productions, submarine groundwater discharge rates, two end-members values, etc., these are decisive to estuarine carbon biogeochemistry. Also, I am curious about the anthropogenic impact on estuarine carbon biogeochemistry. It seems the anthropogenic discharges in this study are mostly referred as sewage discharges to upper rivers, then how to identify the anthropogenic carbon in lower estuarine area proportionally?

### **Line comments**

Line 210: more details on “statistical analysis”.

Line 347: references.

Line 348: “DIC addition/removal” details.

Line 399: should be “riverine DIC” instead of “estuarine DIC”

Line 483: the difference between “Terrestrial DOC” and “Riverine DOC” ?

Line 492: where is “Fig. 12A”?

Line 540-543: the purpose for comparing regional DOC/DON to POC/PON? or DOC fraction in global coastal ocean?

Line 552-553: you cannot say this unless the data about POC/DOC from two end-members.

Line 570: why 6800  $\mu\text{atm}$  threshold?

Line 579: further explain “a decrease of aerobic bacterial activity with increasing DOC”

Line 616: further explain “freshwater mixing is not the major driver of POC”, as it shows lower salinity with higher POC and  $\delta^{13}\text{C}$  values.

Line 668-671: more direct evidence is needed to evaluate anthropogenic impact on  $\text{pCO}_2$  rather than population density. For example, anthropogenic  $\text{pCO}_2$  is 100  $\mu\text{atm}$  out of total  $\text{pCO}_2$  400  $\mu\text{atm}$  in Estuary A, where as anthropogenic  $\text{pCO}_2$  is 200  $\mu\text{atm}$  out of total  $\text{pCO}_2$  1000  $\mu\text{atm}$  in Estuary B.

Line 686: where is “Fig. 21”?

Line 695-696: wrong statement “nitrification plays crucial role in increasing pH”

Line 699: “unlikely”

Line 712-713: details for  $\text{FCO}_2$

Line 862: where is “Table 6”

### **Tables and Figures**

Table 1: add coordinates, references

Table 2: are they annual averaged numbers? Standard deviation?

Table 5: confusing table, please improve

Fig. 1: why only zoom in two estuaries? Instead display  $\text{C}_3$  and  $\text{C}_4$  plants area, population density is more important to be visualized.

Fig. 2 - Fig. 6: cannot distinguish that data between dry and wet, standard deviation needed.

Fig. 6: estuarine export fluxes values should be river-borne C, the figure is unnecessary if only two

components are evaluated.

For all supplementary figures: there is no spatial information, reason why exclude several estuarine data, the number of observations are too small, standard deviations? Data interpretation seems unconvincing due to potential data manipulation.