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Biogeosciences Discussions

# *Interactive comment on* "The postmonsoon carbon biogeochemistry of estuaries under different levels of anthropogenic impacts" *by* Manab Kumar Dutta et al.

**Anonymous Referee #1**

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Review of Dutta et al., The authors made measurements of organic and inorganic carbon parameters, along with isotopes and other ancillary measurements in an attempt to determine the sources and distribution of DIC, DOC, and POC in an estuary in the Hooghly-Sundarbans system (shortly written as C biogeochemistry by the authors). Although the ms falls within the scopes of BG and covers a good data range from various sites of the estuarine system but finally it ends up in a disappointment because of poor writing and hesitations of choosing a concrete aim. Unfortunately, the manuscript reads like a data dump, with incomplete descriptions of the methods, presentation of the data, and some speculation about processes but with major processes left out; nothing seems conclusive. The manuscript is still in quite a rough stage, as detailed



with a non-exhaustive list of examples below, and does not seem ready for publication. Specific comments: The problem lies within the title. It seems the authors are in serous dilemma to show the data what actual basis: on C dynamics in polluted vs non-polluted system or only focus on mangroves and compare with sidechain Hooghly in a specific season or discuss on DIC mainly and less focus on DOC and POC or avoid already published articles on the same systems on same parameters on same season ! (e.g. Samanta 2015, Ray 2018, 2015) Unfortunately nothing was clear due to poor writing and unclear intention.

Other major comments I would suggest authors to give details of the sampling stations e.g. how or what type of anthropogenic input is there in the Hooghly? From where it is more coming from (upstream?). Its better to segment the study sites of Hooghly as upper/mid/lower stretch and Sundarbans as west/central and east. I anticipate the upper and mid stretches are human or industrial impacted compared to lower, so one of ideas in designing the story would be to explain variations of results within Hooghly first between e.g. H1-6 and H6-11 and then compare with S,T,M series. That would read the paper interesting otherwise its just mimicking the findings already shown by Samanta 2015, Ray 2018. Authors argued on C- data limitation of previous reports but it is found that Samanata'15 covered even much higher sites from Hooghly than the present report (c.a 35 vs 13 surface water and 8 vs 8 ground water) and Ray '18 was also not far (>10 in S series vs 10 S,T,M). So this argument on data imitation does not hold true ! Result section is only meant for results and it should be avoded to define data set and add citations in Results that fully present in the paper. It is proposed to move those parts of the Result section to discussion (LN 229-234, 248-49, 257-59, 267-71) This is over-speculative to argue on contributions of pore water on the overlying DIC concentrations based on only one measurement (Tab 3, Lothian PW). LN342-345: This is unclear why $\Delta$DICM2 is shown as micromole instead of permil. Authors should better calculate the amount of DOC and POC added or subtracted from the system applying conservative mixing (same way they did for DIC) and explain in-depth details of their mixing pattern (same applies to DIC). LN349 Are the ground and pore

water discharge not being considered as 'biogeochemical' process? Section 4.3. This part is weakly written and over-speculative without supporting any evidence .e.g. the argument of DOC photooxidation or conversion of DOC to POC as removal process. While it requires suitable ambient condition for DOC photooxidation such as high water residence time, stable environmental condition (not expected in mangroves), the same applies to adsorption/desorption of DOC-POC. Part of that exchange is mediated by charged complexes, repulsion - attraction interactions, and therefore subject to salinity effects. So, when river water rich in DOC first mixes with saline water, at least a portion of DOC is lost from solution (removed) and incorporated into POC (Fe-oxide colloids usually are extracted at the same time). Once the salinity exceeds 2 - 3, however, the effect of salinity on coagulation behavior is largely complete. Another point is no detailed explanation on distribution pattern with salinity was given, authors should highlight the reasons of the mild upward gradient along Hooghly and steep downward trend along the Sundarban. Section 4.4 LN410 only freshwater runoff, no surface run off that adds POC too in upstream? LN440-446 this part is totally redundant as there was not an iota of signal of CH4 from the observed d13 POC (13CH4 is $\sim$ 55-60 permil) Does the author have Chl-a or nutrient data (even from literature) to support higher marine input in POC in Sundarban and 13C values of mangrove leaf, and soil from Hooghly to denote higher terrigenous contribution to the POC pool? Authors are suggested to read carefully the works of Samanta'15 and Ray'18 and use their values to support some of the arguments.

points of concerns

terminology > I counted 'biogeochemistry' was used over 25 times in the 16 pages ms ! too much. Additionally, this is not clear to me what does it actually mean by C biogeochemistry? Is it C-components distributions in different phases (solid suspended and dissolved) under varying biogeochemical processes? If so please specify at least once > d13C values are not 'depleted' or 'enriched' (LN256, 428..). When referring to d13C values, they can be described as higher or lower when comparing different



samples, or one could describe differences as e.g. a certain C pool is enriched or depleted in 13C versus another C pool or sample. > r2 not R2 Inconsistent use of [POC] in the discussion, if the bracket is used for POC then it should also appear for DIC and DOC

unit Random use of units : DOC in mg/L, DIC in mM, POC in uM. These should be harmonized. Use DOC in uM for better compare with other studies

Sampling Define sampling strategy neatly, Its written postmosoon was chosen due to high litterfall, but there is no account of litter source identified for DOC or POC or any impact positive or negative on estuarine C biogeochemistry authors assumed. That is to be addressed in the discussion. Mention the H, S, T, M series in the text Mention general tidal nature while sampling (height, HT/LT, depth)

methods specify> pore size of filters used for DOC, SPM relative uncertainty in POC methods; technique of pore water collection; ground water (from tube pump?)

Figs Again weak representation: font sizes of x, y axis digits (and titles) to be increased much (too much stress to eyes now !); use box to cover legends, its confusing with data points and legends, remove break in y axis in Fug 3e and 4a), black star coding was used both for sundarban and observed d13DIC and grey round coding was used for Hooghly and observed DIC, these symbols must be changed to give separate identity of them in all figs <overall IMPROVE CLARITY of ALL FIGURES>

Data use a consistent number of decimals (1) to report d13C data, and Salinity considering the analytical error on the measurements.

Minor comments First sentence of abstract is redundant LN65 Use current reference for the riverine export flux (works of Pete Raymond, Huang) Many references are out of place e.g. the comparison of present data with Khura (LN 231, 249 Miyajima paper) was unlikely as two environments are totally different even if compared authors should mention conservative data like S in Khura estuary for better comparison. LN234: Pro-

vide values of Samanta et al 2015

Finally, I think it is necessary to stand back and consider how to best weave the entire story together in the discussion more efficiently and succinctly

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