

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

### **Anonymous Referee #2**

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Review of Dutta et al “The postmonsoon carbon biogeochemistry of estuaries under different levels of anthropogenic impact”. Submitted to Biogeosciences. This study presents data from a single cruise in 2 Indian estuaries to try and decipher differences in carbon cycling between a 2 Indian estuaries with differing levels of anthropogenic influence. After reading and rereading this paper several times, it is unclear what the purpose of this study is. There is no defined hypothesis to be tested, and while the title suggests there will be some kind of comparative analysis to look at anthropogenic impact on carbon cycling (an interesting and important topic), I am left a little underwhelmed with the analysis undertaken. The entire manuscript is based single sampling campaigns, which while not ideal is not the main issue. The main area of concern is the

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lack of any direction in the paper, and the somewhat descriptive and qualitative nature. I suggest that the authors define their hypothesis more clearly, and use the data to test this hypothesis.

I have a series of comments below, some minor some major that may help.

Abstract I am not convinced that the data as presented can be used to draw such strong conclusions as to the drivers of carbon dynamics in the studied estuaries. For example Ln 35-38 The evidence supporting these processes is weak at best – no measurements of production, carbonate dissolution nor porewater exchange were measured, and the spatial trends in concentrations and isotopes (and relationships between carbon variables and DO etc.) were not strong enough to draw any distinct conclusions on the importance of these mechanisms.

Same goes for lines 45-47.

Line 49 – 52 I am unconvinced that the observed trends are shown to be directly linked to anthropogenic influence. Yes the estuaries appear to differ, but what else might be driving this. For example looking at salinity and pCO<sub>2</sub> in the 2 different estuaries – the highest salinity in the “anthropogenically” impacted estuary is lower than the lowest salinity in the “undisturbed” estuaries. Could the observed differences simply be related to freshwater input? What are the nutrient concentrations in the 2 estuaries? How different are they in hydrodynamics (looks like the geomorphology is distinctly different between the 2 estuary types from Fig 1). These are just a few of the alternative reasons to look at for explaining the differences observed

Introduction

Ln 59 – 60 What is meant by “record biogeochemical and hydrological processes”?

Ln 67 – Richey is not correct ref for this statement (Richey paper is on Amazon)

Ln 68 – 70 – Still large uncertainties on estuarine CO<sub>2</sub> flux – look at error bars on Cai 2011 estimate

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Ln 76 What is meant by “biogeochemical characteristic”?

Ln 78 – 79 Not always – see Cotovicz Jr, L. C., Knoppers, B. A., Brandini, N., Costa Santos, S. J., & Abril, G. (2015). A strong co<sub>2</sub> sink enhanced by eutrophication in a tropical coastal embayment (guanabara bay, rio de janeiro, brazil). Biogeosciences, 12(20), 6125-6146. doi:10.5194/bg-12-6125-2015

Among others

Ln 81 – 84 There has been a lot of work on mangrove carbon cycling work done since Dittmar and Larra’s work in the early 2000’s. Might be worth looking at more recent papers to see how far our understanding has come since then.

Ln 104- 106 Give some quantitative data to support your “anthropogenically influenced” argument. What are nutrient concentrations like? Population density? Land use? Freshwater inflow? Etc etc. A table compiling this data would give the reader an instant understanding of the differences.

Line 117 What is meant by positive and negative feedback here? These terms are not really applicable to biogeochemistry as a whole, but may be related to specific mechanisms/cycles.

Ln 137-140 Clearly there is freshwater input – the salinities are very low. In fact my thoughts are that these freshwater inputs are a main driver of the observed differences.

Ln 159 Assume the filters were GFF filters – add these details.

Ln 161 Accuracy of TAlk measurements. Were CRMs measured (hope so!). Also add accuracy/precision etc of all other parameters.

Ln 196 – 198 What were the input parameters for measuring pCO<sub>2</sub>? What disassociation constants were used etc?

Ln 205 – 208 Why use L&M relationship? Need some kind of justification here other than saying it is conservative.

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## Results

Do not compare and contrast your data with previous studies in the results. Just report your data.

## Discussion

Ln 289 – 293 What are the implications for these findings? Need to dig deeper or remove.

Ln 306-311 (and Fig 3b) How was the conservative d13C-DIC mixing line calculated? Looks like you have simply added a linear relationship between the 2 endmembers, the relationship is generally not linear (See Fry, B. (2002). Conservative mixing of stable isotopes across estuarine salinity gradients: A conceptual framework for monitoring watershed influences on downstream fisheries production. Estuaries, 25(2), 264-271. Also as you do not have any mineralogy of carbonates – I would avoid using the term “calcite” precipitation, change to “carbonate” precipitation

Ln 323-325 – What does DO tell you about primary production? Looks like DO is generally undersaturated?

Ln 335-338 Describe all the terms in this equation in the following text

Ln 359 Where do the TAlk/DIC numbers come from? The stoichiometric relationship should be based on the slope of the line over the whole estuary, rather than individual data points – therefore not sure how you have a range here.

Ln 364 – 368 Give details on this calculation. Just using the discharge rate and pore-water DIC concentration I get a different value.

Ln 383-390 – Not sure that looking at pCO<sub>2</sub> VS DOC gives any indication as to the importance of porewater exchange! Could also simply be freshwater input from upstream, surface water runoff, or simply leaching/respiration.

Ln 412 Give details about the “jute” industry.

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Ln 424-426 The POC isotopes could simply be related to the relative amount of fresh-water inputs in each system (this can also be applied to most of the other differences observed)

Ln 431-446 I am unsure why anaerobic respiration (which is energetically less favourable than aerobic respiration) would be more important in a well oxygenated estuary. The authors should expand this to explain things more clearly or remove.

Ln 447-451 What is the importance/implications of this – expand or remove.

Ln 455 – 460 These sections seem to contradict each other. Initially it is stated mangrove inputs are insignificant – then porewater exchange of mangrove derived CO<sub>2</sub> is highlighted as important?

Ln 463 – Cai ref – there are plenty of mangrove references for this process, might be more appropriate to use some of those here

Ln 463 – 466 How about plotting ECO<sub>2</sub> vs AOU (in molar units). Look at the slope of the line. This will give a better indication of the importance or aerobic vs anaerobic R.

Ln 470 – 473 How was gas exchange and the differences between CO<sub>2</sub> and O<sub>2</sub> coupled into this calculation? Also how does this value compare to your air-water CO<sub>2</sub> fluxes (you will need to normalize your volumetric rates to surface area for comparison)

Ln 480 – I think your global value for mangrove systems (63  $\mu\text{mol}/\text{m}^2/\text{d}$ ) should be 63  $\text{mmol}/\text{m}^2/\text{d}$  – which is much higher than the fluxes measured in this study.

Conclusions:

Point 1 – this variability is likely simply linked to the variability in salinity (and therefore freshwater inputs) between the studied estuaries.

Point 2 – Unconvinced that primary production has been shown to be the main controlling factor on DIC. Without any measurements of PP or some more thorough analysis of other potential mechanisms, this statement is far too strong

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Point 3 – I see no strong conclusive evidence of either of these points. Again statement is too strong without measurements of DOC flocculation or porewater exchange of DOC

Point 4 Assume this is based on isotopes? Again this could simply be related to the marked differences in freshwater content within each of the estuaries.

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