

Interactive comment on “A large CO₂ sink enhanced by eutrophication in a tropical coastal embayment (Guanabara Bay, Rio de Janeiro, Brazil)” by L. C. Cotovicz Jr. et al.

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The authors presented a well conducted research in Guanabara Bay, Brazil. They suggested their “findings of a net annual CO₂ sink indicate that more field data are needed in particular in the highly productive tropical coastal ocean, in order to adequately integrate estuarine CO₂ fluxes at the global scale,” and I agree. The paper is also generally well-written and easy to follow. I recommend publication with a major revision regarding the few points I listed below. If the first author is writing (one of) his/her first research papers, I must congratulate him/her. Well done!

1) While the Results are very nice, I feel the Discussion lacks a rigorous analysis. The

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authors provided a statistical analysis of data (which environmental and biogeochemistry factors is in control of pCO₂ or CO₂ flux); that is very good. But can you move a step further by providing a more rigorous biogeochemical analysis. For example, a 1-D (seasonal) analysis on how pCO₂ changes with time (from temperature, air-sea flux, mixing, and biological production) at few sites. NEP (or NCP) would come up in this analysis. If the authors feel this is too much to ask, then, they should say why (such as this is good enough, or they need further information to do a more rigorous analysis, or it will be in their next paper, etc.).

2) This is a low wind regime. You have used two k600 models, one as the upper boundary and the other (RC) as the lower boundary, which is fine. But I thought RC method provided quite high fluxes. Could you at least make a comparison with Wanninkhof 1992 equation or his later revisions?

3) The carbon budget: p.4697, Is there a strong reason that sediment burial must equal to air-water gas flux of CO₂? I was expecting that this section would show how much of CO₂ is taken from surrounding mangrove and cities, how much is exported to the sea, how much is buried and how much is recycled, etc. I may have asked too much. So you may ignore me; but at least don't call this section carbon budget.

4) While EDIC to AOU relation is present in the last figure, DIC and TA are hiding somewhere. Why? TA and DIC to salinity plots may illustrate an idea whether all uptake CO₂ is buried in sediment or is recycled and exported to offshore.

5) pH measurement method is missing in the Method section. Since it is used to calculate DIC, it must be evaluated more rigorous. How much uncertainty is in the calculated DIC?

Other points I noted as I read through.

Title: I do not see the need of the word “large.” Better just say “A CO₂ sink enhanced by. . .” Abstract: a bit repeating, can be shortened. Also in the last line, not clear what

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you mean by “behave specifically.” uniquely (being a sink)?

Introduction

P. 4673, line 23, “suite a lot of. . .” don’t know what you mean.

P. 4674, “which”? “with”?

p. 4674, line 23, may replace “incipient” with a more commonly used word or term.

p.4674, line 27, I don’t think “Amazon River plume” is an appropriate example here. It is very different from what you are talking here.

p.4674, line 12, in this context, you may want to reference works from the Mississippi River plume

1. Guo, X., Cai, W.-J., Huang, W.-J., Wang, Y., Chen, F., Murrell, M.C., Lohrenz, S. Dai, M., Jiang, L.-Q. and Culp, R., 2012. CO₂ dynamics and community metabolism in the Mississippi River plume. *Limnology and Oceanography* 57(1):1-17. And/or

2. Huang, W.-J., Cai, W.-J., Wang, Y., Lohrenz, S.E., and Murrell, M.C. 2015. The carbon dioxide (CO₂) system on the Mississippi River–dominated continental shelf in the northern Gulf of Mexico – I: Distribution and air-sea CO₂ flux, *Journal of Geophysical Research-Ocean* (in press, paper #2014JC010498).

p.4675, line 25, extremely low (not extreme low)? 2.1 Reading figure 1, I can’t tell where is the sea? Does seawater come from S1 or S4? Mark it. Reading further to the 2nd paragraph and to line 26 of p. 4677, I guess then see S1 is near bay mouth. Better make it clear.

2.3.1 How was pH measured? Since it is a critical parameter that is used to calculate DIC (from pH and TA). You must document it in details.

2.3.3, I think it is better just use Merbach refitted by Dickson and Millero (1987), rather than the composite one with Hansson data. Since DIC is calculated, possible issues

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related to the calculation should be mentioned.

Fig. 2, make the label larger and shorter (just precipitation and temperature; leave other words such as atmosphere in figure caption). I can barely read them.

3.3 Spatial screening???

Fig. 4, caption. What is “superficial waters”? Does it tell a different meaning from the more commonly used term “surface or surficial waters”? also, p.4688, line 14.

p.4685, line 4 relatively stable

p.4685, line 8, I don’t know what is the meaning of the word “activation” here.

p.4689, line 1, here you may reference to low pCO₂ in the Mississippi plume (Huang et al. 2015, above).

p.4689 lines 7-21, and figure caption. What exactly is this 1:1? Need to say this in the figure caption and probably a bit more in the text.

p. 4694, line 17, (also line 1 the next page) while many carbonate chemists also make this mistake, you cannot say “the pCO₂ concentrations.” Here p, the partial pressure, already means concentration (in gas phase). I suggest “pCO₂ values.”

p.4697, Is there a strong reason that sediment burial must equal to air-water gas flux of CO₂? I was expecting that this section would show how much of CO₂ is taken from surrounding mangrove and cities, how much is exported to the sea, how much is buried and how much is recycled, etc. I may have asked too much. So you may ignore me; but at least don’t call this section carbon budget.

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