

## ***Interactive comment on “Warming and ocean acidification may decrease estuarine dissolved organic carbon export to the ocean” by Michelle N. Simone et al.***

### **Anonymous Referee #1**

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Review of bg-2020-335 Warming and ocean acidification may decrease estuarine dissolved organic carbon export to the ocean Michelle N. Simone, Kai G. Schulz, Joanne M. Oakes, and Bradley D. Eyre

This contribution studies the effect of increased pCO<sub>2</sub> and temperature on the fate of DOC in photic sediments. There are two autochthonous sources for DOC in sediments: degradation of detrital POC and release from microphytobenthos. Diffusive fluxes between the overlying water and sediment pore water depend on the concentration gradient (excluding bioturbation in more permeable sediments). Increases in pCO<sub>2</sub> will be expected to enhance benthic primary production (and associated DOC produc-

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tion) while increases in temperatures will increase carbon mineralisation rates. The net effect of these combined is difficult to assess and hence the focus of this experimental study. The experiment is very well designed and carried out, and the results are clearly condensed and presented. The results and discussion sections are, however, difficult reading, and I had to re-read many times to follow. I wonder if the carbon budget/fluxes can be summarised in a figure or table so it is easier for the reader to follow the net result of the treatments. I found myself doing this while reading the discussion, gathering numbers from different figures. This would greatly increase the impact of the paper. I found it misleading to always refer to the high pCO<sub>2</sub> scenario as ocean acidification OA. It is the increased DIC availability that is fuelling higher primary production which seems to be the major driver, rather than acidification influencing a rate as such. I recommend that this is rectified. It is also unclear what the nutrient levels were during the experiment. The results and discussion are focused solely on carbon limitation and assume adequate nutrient supply. That said the system the sediment cores were sampled from appears to be low nutrient. It is worth addressing this at some point. What effect would N limitation have on the result. Competition between MPB and heterotrophs for available nutrients for example. Finally, I do not see the value in scaling the data up to global estimates of sediment estuarine DOC uptake (4.3.3). It is not necessary and is fraught with very large assumptions. Similar scale ups have been done in the cited literature (Duarte papers), arrive at questionable results and conflict with current understanding of the global ocean DOC budget. The findings of this present study are relevant, intriguing and warrant publication without this final section.

Specific comments Introduction Important to distinguish between photic and aphotic sediments. They differ greatly in their role and contribution to the larger net effects of coastal waters, which are outlined at the start of the introduction. The last part of the introduction could be rephrased to be clearer. Lines 54-78. First formulate what the dominating mechanisms acting on DOC uptake/release from photic sediments are. Then address how these mechanisms can be influenced by warmer temperatures, high CO<sub>2</sub>, and lowered pH, respectively. Then clearly state the hypothesis you had as the

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basis of your experimental design.

What influence would variable light conditions have on your findings? The cores are taken from a shallow estuarine site where one can expect considerable resuspension from tides, currents and winds. The light intensities used here are likely representative of best case. So one can maybe amplify the dark scenario?

Line 7. " Estuaries make a disproportionately". What do you mean here? With respect to what?

Line 19. DOC is smaller than that retained in soils and also in fossil fuels.

Line 28. And line 32-35. Here you state that 33% of the NPP in coastal waters is exported to the oceans and stored in the ocean interior. I question the validity of this statement/citation. Is there evidence that the interior ocean is increasing in DOC? Why the large difference between mineralisation efficiency of DOC produced in surface water of the ocean to that produced in coastal waters?

Line 43. Delete extra "lability"

First three paragraphs contradict. You start by arguing that coastal waters are an important source of DOC to the open ocean but then finish by stating that coastal sediments are an important sink for DOC.

Line 48. Check referencing. Fischot and Benner paper does not address the processing of DOC by estuarine sediments.

Line 55-60. The increased DOC production in the Engel et al 2013 study was due to nutrient limitation. When they added nutrients it was rapidly removed again. So no net accumulation of DOC.

Line 287-289. This can be deleted.

Line 340-343. Check phrasing and possible break into two sentences to make easier reading.

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Line 350-359. Here the authors begin to speculate about the lability of DOC without any measurements to support it. I am not sure it is necessary.

Line 395. DOC is also produced continually from the detrital sediment POC. This contributes to dark DOC production.

Lin 398-399. Are you inferring nutrient limitation in your set up? For now I have assumed you had adequate nutrients.

Line 401. A very bold statement and the reference (Costanza) does not seem to support it. Please check.

Figures Error bars in the figure should go both plus and minus. Check text in figure 4. Do you not mean aerobic respiration (with arrow pointing upwards).

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