

Interactive comment on “Sediment CO₂ efflux from cleared and intact temperate mangroves and tidal flats” by R. H. Bulmer et al.

Anonymous Referee #2

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This paper measured sediment to air CO₂ fluxes from a large number of mangrove dominated, cleared mangrove, and intertidal sites in New Zealand. Mangrove coverage is increasing in temperate areas, and the importance of these mangroves in carbon cycling is well known. Therefore the research question of what happens to this carbon when the mangroves are cleared is a valuable one to explore.

My general feeling with this paper is that it suffering a little bit from an identity crisis, is it an ecology or biogeochemistry paper. For example the inclusion of macrofauna data seems to have no relevance, particularly in light of the fact that this parameter was not measured in the “control” treatments (i.e. the undisturbed mangrove sites). While the importance of macrofauna in sediment respiration rates is well established in previous studies, in this paper there is really no exploration of the relationship between

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macrofauna and CO₂ fluxes. For example – were any of the flux incubations carried out over crab burrows? If so was there a relationship between burrow size/density with the flux rate (for example see Kristensen et al. 2008)? Does the loss of crab burrows = lower CO₂ fluxes? On the same note, what about pneumatophores? Similarly, tree biomass, root mass etc are not really adequately explored to warrant inclusion. There is a lot of data that is just thrown into the manuscript with little consideration as to how it fits into the CO₂ flux story.

There needs to be a greater detailing of methodology. For example there needs to be the inclusion of equations for CO₂ flux measurements, criteria for inclusion/exclusion of fluxes (i.e. the linearity of the fluxes), what are the empirical equations used to determine biomass, did the use of different equations for biomass depending on tree height induce any differences. Further, some of the geochemical interpretations are a little bit too qualitative to be included in any kind of analysis (e.g. redox depth characterization and compaction). For example, looking at the redox depth by change of sediment color is fine in a 2 dimensional system, however when you have biogenic structures such as crab burrows, roots, pneumatophores etc. this analysis is not appropriate. Looking at the influence of biofilm removal on CO₂ fluxes is an interesting aspect, however without undertaking “light incubations” the interpretation is limited. Most of the CO₂ uptake is likely to be by photosynthetic organisms, rather than chemosynthetic. While the reference of Leopold et al 2013 is used to justify the lack of light incubations, I would like to see a better explanation considering the Leopold study was in New Caledonia (Latitude 20 S with a very high mangrove density and therefore low light penetration to the sediments), as opposed to this study at 35 S with low mangrove density (and presumably higher light penetration). Also, considering that 2 of the treatments are free of mangroves (i.e. cleared and tidal flats), one would assume that the importance of photosynthetic organisms in these sites would be even higher. Some more details on the biofilm removal procedure would also assist the reader, for example how long after the removal was the incubation started (i.e. was time given for the sediment to reach a steady state). I am not convinced the normalization procedure used (i.e. the

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calculation of the CO₂prop value) is suitable for such small sample sizes (i.e. n=3 for each of the paired sites). For example do all the “cleared” sites have similar vegetation, are all the tidal flats, mangrove sites and cleared sites at the same height, and experience the same hydrodynamics? This is important because you are using the fluxes from these sites to normalise your data, therefore there needs to be some consistency there. Also it is unclear whether the n=3 relates to 3 incubations over the same sediment, or 3 separate incubations. Either way there is not enough replication there, I would think at each site a bare minimum would be triplicate incubations at 3 sub-sites (n=9). My experience with these incubations is that the spatial variability is quite large, and therefore replication is important. Particularly when looking at the mangrove sites where biogenic structures (e.g. crab burrows and pneumatophores) play such a large role. My feeling is that the authors have focused too much on sampling as many sites as possible, at the expense of adequate within site replication (spatial and temporal). I would like to see more figures to illustrate your key points, for example a few simple plots of CO₂ flux rates vs drivers (e.g. sediment organic C, chlorophyll a, temperature etc.) would add significant value to this paper. It would be also good to put some of these fluxes into context with other mangrove carbon cycling processes, such as NPP, burial and lateral tidal export. While not specifically measured in this study, these factors are key components and should at least rate a mention in the intro.

Some specific comments are listed below: Page 3550 Line 7 – CO₂ efflux is due to heterotrophic processes (in both autotrophs and heterotrophs), CO₂ uptake is due to chemoautotrophic and photosynthetic processes. This sentence needs a rewrite

Page 3552 Line 10 Nothing in the supplementary table about hydrodynamics – this would be welcomed though

Page 3552 Line 24 What are the dimensions of the chamber

Page 3553 Need to include the equations used for CO₂ flux calculations along with acceptance/rejection criteria for fluxes.

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Page 3553 Line 15 There is a big difference between mangroves and climate in New Caledonia and New Zealand, can the authors justify the use of dark chambers only based on some of their own data? Looking at those high chlorophyll a concentrations I would expect a lot of photosynthetic activity in these sediments

Page 3554 Line 2 As mentioned above, I am not convinced you can use this normalization procedure with such a small sample size in the paired sites

Page 3555 Chl a analysis needs a few more refs – what equations wavelengths etc were used.

Page 3555 Tree biomass section needs some fleshing out, what were the allometric equations, was there a difference between the diameter vs height equations etc. Also only one 2 x 2m quadrant per site for density and only 5 trees per site for biomass seems too small a sample size. There are a number of protocols out there for measuring C stocks in mangroves (e.g. see the blue carbon initiative) I would recommend that the authors look closely and refer to these resources.

Page 3556 Line 1 – See comment above re. redox depth in 3D sediments structure.

Page 3556 Line 10 If no macrofauna were collected analysed at “mangrove” sites then I feel it is not worth including as no cross comparisons can be made. The macrofauna data is not explored in any detail so I would recommend removal.

Page 3557 Line 16 – This analysis does not add anything to the CO₂ flux story,

Page 3558 Line 4 – Hard to believe that chemosynthetic CO₂ uptake exceeded all respiratory processes in the tidal flats! No light therefore no photosynthesis, but plenty of OC and Chl a therefore one would expect in the dark that respiration would exceed fixation.

Page 3558 Line 13. The whole paragraph bares little relevance to the CO₂ story. Perhaps a separate paper on changes in macrofauna abundance could be written, but in its current form it seems this data is just an added extra with no relevance.

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Page 3558 line 24 – Would be good to have some figures showing these relationships, and those on the next page. One thing to consider is that a lot of these factors are likely covariates, e.g. OC, N and sediment composition are likely all driven by hydrodynamics and organic matter supply. Therefore teasing apart what is actually driving the CO₂ flux story is a little more complicated than simple correlation analysis.

Page 3560 Line 2 and 4 – the Figure states $p < 0.05$, need to be consistent

Page 3560 Line 17 What about mangrove NPP and hydrology?

Page 3561 Line 6 to 12 No light incubations to test this!

Page 3562 Line 1 and 2 – no statistical difference between mangroves and cleared (previous sentence and Figure 2), yet talk about why the flux is lower in cleared in these sentences.

Page 3563 Line 2 - 9 Elaborate on this some more

Page 3564 Line 11 – 20 Did you do incubations over crab burrows? If so is there a relationship between flux and burrow size/density?

Page 3565 Line 1 – 9 I think the biofilm discussion is a little weak without accounting for the influence of phototrophic CO₂ uptake (i.e. light incubations). I would like to see some discussion about this, or at least an acknowledgement.

Kristensen, E., M. R. Flindt, S. Ulomi, A. V. Borges, G. Abril, and S. Bouillon. 2008. Emission of CO₂ and CH₄ to the atmosphere by sediments and open waters in two Tanzanian mangrove forests. *Mar. Ecol. Prog. Ser.* 370: 53-67.

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