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Interactive comment on "Sediment CO_2 efflux from cleared and intact temperate mangroves and tidal flats" *by* R. H. Bulmer et al.

Anonymous Referee #3

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This manuscript presents a study on CO2 emissions from exposed mangrove and tidal sediments with emphasis on the role of mangrove clearing. Measurements of CO2 emissions were done at low tide during daytime at 18 to 40 sites depending on the environment. The fluxes were then correlated with a variety of sediment, flora and fauna parameters. Based on these correlations, it was concluded that sediment organic content, chlorophyll, grain size, mangrove height, macrofaunal abundance, temperature and sediment water content controlled the emissions. It was also concluded that stored organic carbon in the sediment is released within a few years, and that the surface biofilm of the sediment prevents release of CO2.

The study is in principle very interesting and relevant, but the approach is not so good. Many of the methods used are not described adequately and some of them appear

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flawed (see below). I wonder why so much effort is put into the analysis of fauna communities, while the results on these are not used very much. The results section is poor as it only describes a wealth of correlations. Correlations can of course be an important tool to see if various parameters show the same trend, but they are not a proof for any causal relationship. Many of the correlations found here may very well be spurious.

I feel that the authors are benthic ecologists trying to do biogeochemistry. Some of the biogeochemical arguments are simply wrong. For example line 64-65, where it is stated that "CO2 efflux originates from photosynthetic and chemoautotrophic microbial degradation of organic matter within the sediment". This is simply nonsense as all autotrophic processes fix CO2 into organic carbon and not the other way around. Another example is line 211-213, where it is stated that the oxic layer in sediments is defined as the depth of the upper tan colored sediment and the anoxic zone is the black sediment below. This is not true. The tan colored sediment is oxidized and show where oxidized iron dominates. The oxic zone in mangrove sediments is only 2-3 mm deep and cannot be determined visually.

The authors have also difficulties with the terminology. They use both sediment and soil to denote the substratum. They must be consistent, and I prefer sediment. They should also use the term "mangrove" to denote the trees in a "mangrove forest". Thus use the latter term to describe the environment.

Another major (the most important) concern is the reliability of CO2 flux measurements. I don't trust the obtained rates and believe that they are flawed. When CO2 flux measurements are made on intertidal sediments at low tide in the middle of the day, it is required that the sediment must be pre-darkened for at least 30 minutes before initiating measurements. Otherwise, the benthic microalgae present may still assimilate CO2 from the energy gained in light before the incubation. They can in fact continue with that for some time. As I understand the approach used here, the darkened chambers were placed on the sediment and fluxes were measured during a 90 second period

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right after. This will certainly lead to an underestimate and explain the uptake of CO2 in the intertidal flats, which cannot occur in darkness. Chemoautotrophic carbon fixation is much too slow to account for such uptake. This flaw can certainly also explain the difference in fluxes found after removing the biofilm. Then the benthic microalgae are removed and no such delayed CO2 assimilation occurs.

Based on the above, I cannot recommend publication of this manuscript.

Detailed comments:

Abstract. Line 7: Here and throughout the MS, I recommend denoting the environment "mangrove forest" as "mangrove" refers to the trees only. Lin16-17: Here and throughout the MS, I recommend using the standard biogeochemical units for fluxes "mmol m-2 d-1". At least, it must be "m-2" and not "m2".

Introduction. Line 34: Change to "Temperate mangrove forests are subject to harsh climatic condition leading to a lower. . .." Line 46-48: Isn't vertical accretion and sea level rise of opposite direction and the latter will most likely not lead to mangrove expansion. Line 75: Here and throughout the MS the authors focus very much on organic carbon concentration in sediments. They should also consider the quality – i.e. the composition and lability of the organic matter. Line 88: The reference here is old and not related to mangrove environments. Please use one of the several publications on the issue by Alongi or Kristensen.

Materials and methods. Line 104: Change to ". . .from the top to the central North Island. . .." Line 113: Change to ". . .we sampled at cleared (40 sites) and adjacent intact (18 sites) mangrove locations, as well as tidal flats (30 sites) where existing." Line 132: What was the area covered by the chamber? This is important information. Line 139: Here we have one of the places where sediment and soil terms are mixed. Please delete "soil" here. Line 146: It is quite late to inform about the darkened chambers here. It must be done earlier. Line 148-150: I disagree that the approach used excludes photoautotrophic contribution. I have found that CO2 fixation occurs during the first 30

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minutes after darkening. It is a very serious flaw. Line 152: Change “years” to “time” here and throughout. Line 154-155: This statement is not clear to readers because it refers to one of the conclusions of the paper. Please omit. Line 155-165: I am not sure that I trust this proportion of difference adjustment – and I certainly don’t like it. It seems to be a kind of data manipulation to obtain the expected results. It is also weird to have output values between 0 and 1 – and then the explanation of what they mean includes an option to have values below 0 and above 1. Line 176: What do you use the inorganic carbon concentration for? Line 179: This hydrogen peroxide approach is used very much by geologists. However, it removes the biologically important particles. Biogeochemists usually include these particles in their grain size distribution. Line 186-187: How were the samples for chlorophyll stored during the month before analysis? This is important. Line 202-223: There seems to be two methods to obtain infauna by either raking the quadrat or by sampling cores and sieving them. It is unclear how these two approaches differ and how the results from each are used. Line 224-237: This section is very unclear. Actually I don’t know what was done. Is it really necessary to go into this kind of detail? The fauna data are not used for much. Line 245-248: Delete these lines. They repeat what is stated just above.

Results. Line 264-266: Please correct the units as described by me above. I still don’t believe a CO2 uptake by the tidal flats in darkness. Line 269: Table 2 is not the correct table to refer to here. Line 275-284: Scale down this description of fauna – and scale up your description of CO2 fluxes above. The study focuses on emissions and not fauna. Line 285-315: These lines are just a long list of correlations. Please rewrite this in a meaningful way and include all correlation values in a table. Is the first value in all parentheses r2? This is not mentioned. Line 316-319: This biofilm effect is not true. It is simply because carbon fixation by benthic microalgae is missing after removing the upper 2 mm of the sediment.

Discussion. Line 321-322: Again, the units are wrong. Line 322-323: This is a contradiction. First it is stated that the results are within the range of those previously

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reported, then they are suddenly higher than previously reported!!!! What about the results obtained by Alongi and/or Kristensen. They are not mentioned. Line 323: Change to ". . .tropical mangrove forests. . .." Line 333-334: It seems that everything is affecting CO2 emission. The list mentioned covers almost everything. Line 337: Now the unit becomes even more strange "m2 s-1". Line 344-349: These lines are nonsense. The efflux in darkness is not driven by autotrophic communities, but rather the heterotrophic degraders. These lines must be deleted. Line 355-356: How can sediment characteristics play any role? Please clarify. Line 358-363: I still don't believe the biofilm story. However, the sites that are referred to here have apparently dense algal mats. They will then be assimilating CO2 long time after darkening. So, the studies cited here must have the same flaw as the present study. Line 373-375: Did you consider the burrows as chimneys of CO2 release as found in other studies. Also pneumatophores act as conduits for CO2 transported from deep in the sediment. Line 393-395: I don't understand this sentence. Line 396-397: This effect must be short-term. Line 407-411: I don't believe in this adjustment. Line 434: We have not heard that crab burrows were counted. These burrows are important conduits for CO2 release. Line 424-440: There is no explanation for the uptake of CO2 in tidal flats. Again, I believe that it is a flaw. The correlations can therefore not be fully trusted. Line 442-446: This statement supports my argument for continued assimilation of CO2 by microalgae right after darkening. These biofilms are important for the benthic primary production in the light, but they are part of the heterotrophic community during night (hours after sunset). Line 447-449: No, such polymeric surface film cannot be a strong barrier. This has been shown by others. Line 450-452: Such aeration will not result in instant oxidation by microorganisms. Furthermore, labile organic fractions are degraded at the same speed irrespective of the presence of oxygen. It is degradation of refractory organic matter that is speeded up by the presence of oxygen.

Table 1: The chlorophyll and phaeophytin units are weird. They must be wrong.

Interactive comment on Biogeosciences Discuss., 12, 3547, 2015.