

Overview comment (to all referees), followed by responses to individual referees.

We would like to acknowledge the helpful comments received by the referees. Here we address two of the main concerns expressed by the referees. We note that the referees expressed a recommendation that while the manuscript contained a large amount of valuable information, it should focus on the main factors influencing CO₂ efflux. In addition the referees asked for a more detailed description of the methods. We have addressed these concerns and suggestions by:

- Omitted the tidal flat data to concentrate on CO₂ efflux from intact and cleared mangrove forest sites and the main factors influencing the sediment CO₂ efflux.
- Removed the macrofaunal data
- We have reassessed the criteria for including flux data. In the revised version only fluxes where the r^2 of the linear regression (increase of CO₂ concentration vs time) exceeds 0.8. In general, r^2 values of less than 0.8 occurred at sites where there was minimal change in CO₂ efflux, typically less than $\pm 0.4 \mu\text{mol m}^{-2} \text{s}^{-1}$. While it is possible that the flux at these sites exhibits a non-linear trend, we have removed them to in order to strengthen the interpretation of the remaining dataset.
- This resulted in a decline in the number of clearance sites from 40 to 23, and intact mangrove forest sites from 18 to 13.
- While working on the calculations we identified an error in the CO₂ efflux calculation script (the chamber volume was overestimated by about 40 %) and we re-calculated all sediment CO₂ efflux values, re-did all related statistical tests, corrected the tables and figures.

The second point raised by referee#3 was in regards to the procedure of the CO₂ flux measurements, i.e. the possible continuation of photosynthesis if measurements were made immediately after the chamber deployment. Based on this we undertook additional measurements to test the impact of pre shading the sediment for > 30 minutes prior to dark CO₂ efflux measurements. We selected an existing location (Hatea 1) where CO₂ uptake had previously been measured. The manuscript has been modified to include the results of this experiment.

We compared control and biofilm removed measurements using identical methodology to that described in the manuscript. Relevant sections are included below:

2.3.1 Pre-shading the sediment

Frames (0.5 m²) were located approximately 20 cm above the sediment surface. The frame was completely covered by layered cloth to exclude light penetration. At site Hatea 1, three frames were deployed throughout the mangrove forest, at least 10 m from each other and the mangrove edge. After 30 minutes of shading, two CO₂ efflux measurements using a dark

respiration chamber were conducted at different locations within the 0.5 m² area, before and after the removal of the surface biofilm. The biofilm (top ~2 mm of surface sediment) was scraped off using a spatula. Biofilm removed measurements were collected immediately following biofilm intact measurements in the identical location. Corresponding dark CO₂ efflux measurements were also conducted at locations that had not been pre-shaded (control) adjacent to each shaded measurement, as well as corresponding biofilm removed measurements to account for heterogeneity in sediment conditions.

2.3.2 Sediment CO₂ efflux from intact and cleared temperate mangrove

Sediment CO₂ efflux was measured in the centre of the cleared sites at three randomly selected locations. Locations in the intact mangrove forest were > 10 m from the cleared areas. No pre-shading of the sediment was undertaken prior to measurements.

The sediment CO₂ efflux was measured at low tide, between 8 am and 6 pm local time, using an infrared CO₂ analyser (Environmental Gas Monitor (EGM-4) with a dark sediment respiration chamber (SRC-1, PP Systems Ltd., Amesbury, MA, USA). Using a dark chamber prevents the photosynthetic activity of benthic microbial communities which results in the uptake of CO₂. A PVC collar (10 cm height) was attached to the base of the respiration chamber to protect the chamber from potential flooding. The collar was inserted approximately 5 mm into the sediment, avoiding damage to surface roots. Sediment within the chamber included crab burrows and pneumatophores < 7 cm which fit within the respiration chamber. The sediment area covered by each chamber was 0.00785 m². Chamber height was measured during each measurement as collar insertion varied based on sediment characteristics. Total chamber volume varied between 1.72 and 1.98 l depending on the depth of collar insertion. The CO₂ concentration in the chamber was measured at 5 second intervals over a 90 second period. Air and sediment temperature (Novel Ways temperature probe) and moisture (CS620, Campbell Scientific, Logan, UT, USA) to a depth of 12 cm was measured with each CO₂ efflux measurement.

In addition to measuring CO₂ efflux in intact (undisturbed) sediment, sediment CO₂ efflux was re-measured at the same location after the removal of the surface biofilm. Measurements were made within 30 seconds following the removal of the surface biofilm.

Sediment CO₂ efflux was calculated from linear regression of the CO₂ concentration within the chamber over time. Only regressions with r² values ≥ 0.8 were used for flux calculations.

The sediment CO₂ efflux rate was calculated as follows.

$$\text{CO}_2 \text{ flux } (\mu\text{mol m}^{-2} \text{ s}^{-1}) = (\Delta\text{CO}_2/\Delta t) \times (P \times V/R \times T \times A) \quad (1)$$

Where $\Delta CO_2/\Delta t$ is the change in CO_2 concentration over time, based on the slope of the linear regression ($\mu mol mol^{-1}$), t is time (s), P is the atmospheric pressure (Pa), V is the volume of the chamber including collar (m^3), A is the surface area covered by each chamber ($0.007854 m^2$), T is the temperature (K), R is the ideal gas constant, $8.20528 m^3 PaK^{-1} mol^{-1}$).

We note that as part of a separate study we also undertook similar testing within intact mangrove at a new location (Whangateau 2), with similar results which we include in the response to referees but not the manuscript. A total of 18 measurements were collected for each treatment at Whangateau 2 (control biofilm intact, and control biofilm removed, shaded biofilm intact, shaded biofilm removed).

Statistical analysis used:

A Shapiro-Wilk test was used to test normality. As data conformed to normality, paired t-tests were used to determine significant differences ($p < 0.05$) in shaded and control measurements of sediment CO_2 efflux within intact mangrove at Hatea 1.

Results of the additional testing at Hatea 1:

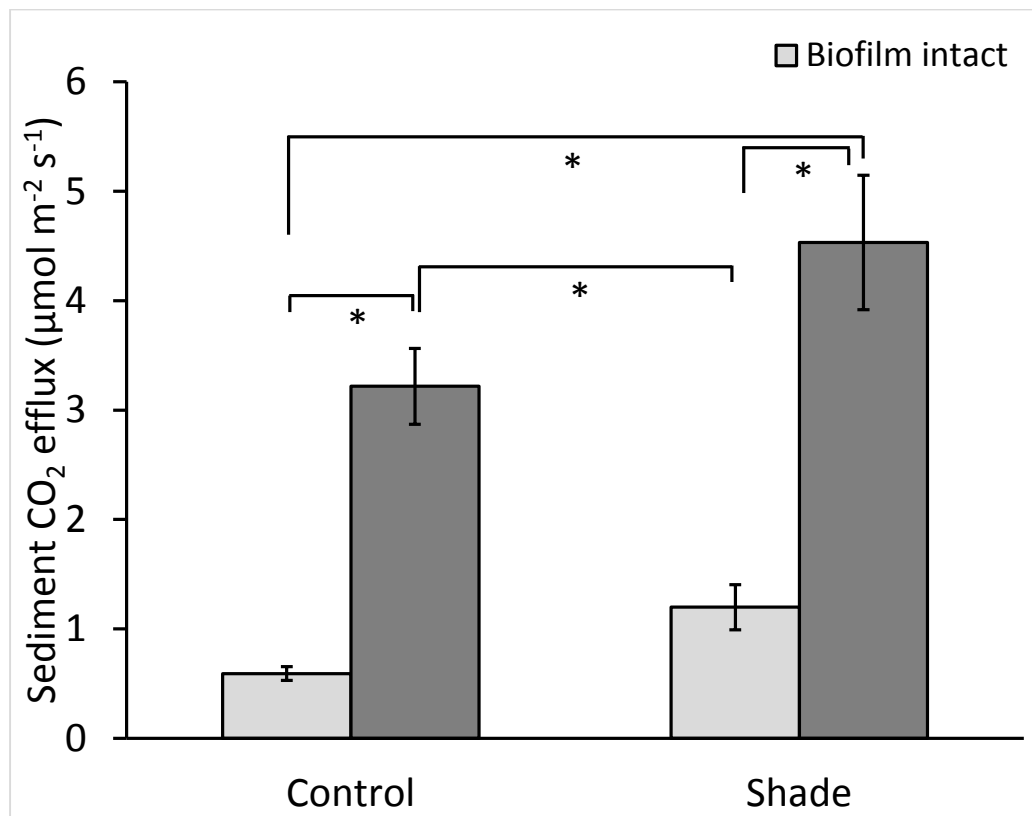


Figure 1. Mean sediment (\pm SE) CO_2 efflux ($\mu mol m^{-2} s^{-1}$) before and after surface biofilm was removed, from control ($n = 6$), and pre-shaded sediment ($n = 6$) at intact mangrove site Hatea 1. *significant difference ($p < 0.05$)

No significant difference ($p > 0.05$) was detected in mean CO₂ efflux between shaded and control treatments (Figure 2). Removing the surface biofilm resulted in significantly higher CO₂ efflux ($p < 0.05$) for both shaded and control treatments (Figure 2).

Results of the additional testing at Whangateau 2:

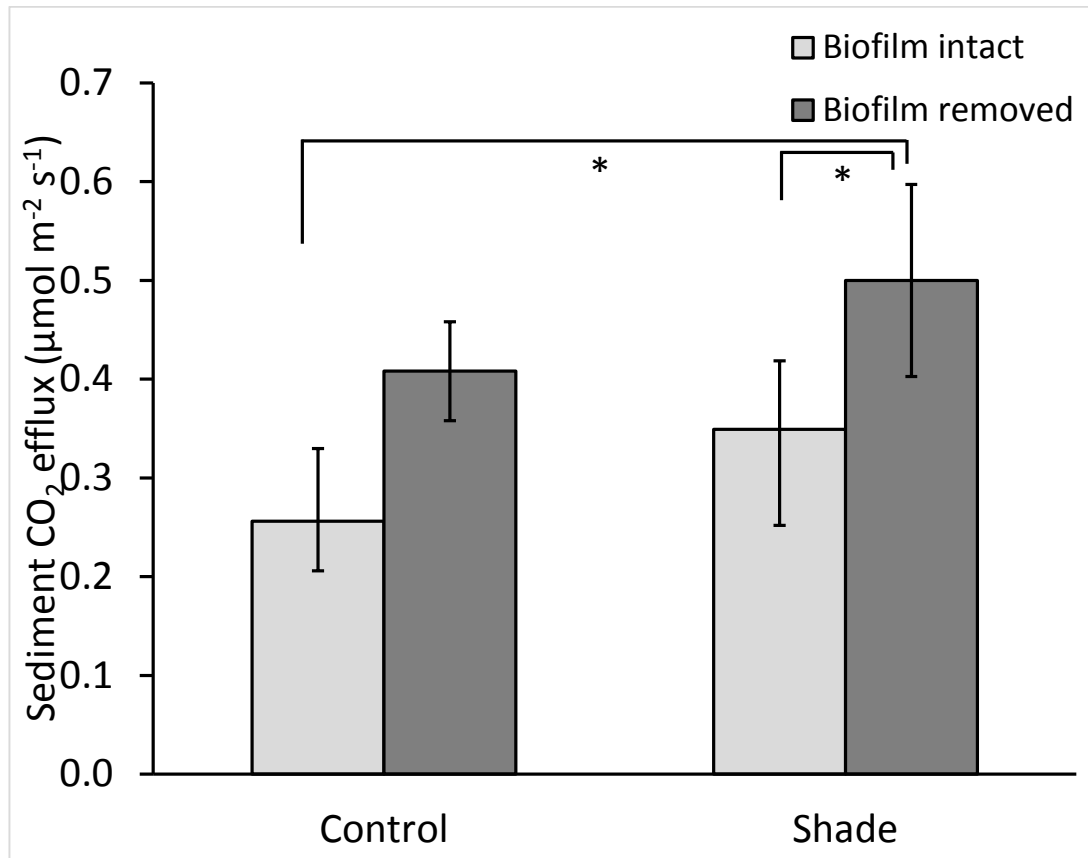


Figure 2. Mean sediment (\pm SE) CO₂ efflux ($\mu\text{mol m}^{-2} \text{s}^{-1}$) before and after surface biofilm was removed, from control ($n = 18$), and pre-shaded sediment ($n = 18$) at intact mangrove site Whangateau 2. *significant difference ($p < 0.05$)

No significant difference was detected in mean CO₂ efflux between shaded and control treatments at Whangateau 2 ($p > 0.05$). Removing the surface biofilm resulted in significantly higher CO₂ efflux for shaded treatments (Figure 2), ($p < 0.05$).

Based on these results we derive the following conclusions.

- Our procedure to measure dark CO₂ efflux (which do not include > 30 minutes of pre shading) are valid.
- Lagged photosynthetic processes within the sediment of the dark incubation chamber are unlikely to be resulting in the CO₂ uptake observed at certain sites, or the significant increase in CO₂ efflux following biofilm removal.

We have included the following in the discussion as a potential explanation of the CO₂ uptake observed at certain sites in our study.

Sediment CO₂ uptake (negative flux) was observed at one intact (Hatea 1) and three cleared (Tairua 3, Whangamata 1, Hatea 1) mangrove forest sites. CO₂ uptake has also been reported in other mangrove efflux studies (Lovelock, 2008; Leopold et al., 2015; Lovelock et al., 2014). CO₂ uptake has been explained by the presence of biofilm microbial communities, as CO₂ uptake changed to efflux following biofilm removal (Leopold et al. (2015). In other habitats, CO₂ uptake from terrestrial shrub sediment has been attributed to sediment effusion-dissolution processes driven by sediment pH and moisture (Ma et al., 2013). CO₂ uptake from wetland sediment has been attributed to the drawdown of CO₂ into the sediment during large ebbing or very low tides (Krauss and Whitbeck, 2012).

Microphytobenthos have been shown to be significant contributors to benthic primary productivity (Oakes and Eyre, 2014; Kristensen and Alongi, 2006; Bouillon et al., 2008). Due to the short duration of our measurements (90 seconds), CO₂ uptake might be explained by the continuation of photosynthetic activity by surface biofilm communities at the onset of dark measurements until coenzymes were depleted (NADPH, ATP) (Leopold et al. (2015). However, the results from our shading results suggest that this was not the case, as we did not see significantly higher CO₂ efflux from sediment that was pre-shaded compared to sediment which had not been pre shaded.

Another possibility is that the decrease in CO₂ concentration within the chamber observed at these sites is driven by the leakage of CO₂ from dark chamber measurements, via cracks, fissures or burrows in the surface sediment. The removal of the surface biofilm resulted at CO₂ emission even at the sites where CO₂ uptake was previously observed. This is possibly related to homogenising the sediment surface following biofilm removal, with cracks or burrows covered by scraped sediment, minimising CO₂ leakage to adjacent non-shaded microphytobenthos. Other studies have suggested that the biofilm may also act as a barrier to the flow of CO₂ from deeper sediment, which when removed results in a rapid increase in CO₂ efflux (Leopold et al., 2015; Leopold et al., 2013).

Chemoautotrophs have also been shown to fix carbon in intertidal sediment under dark conditions (Boschker et al., 2014; Lenk et al., 2011). In particularly at the interface of aerobic and anaerobic zones where large amounts of reduced compounds, such as sulphur, accumulate (Santoro et al., 2013; Boschker et al., 2014; Thomsen and Kristensen, 1997; Lenk et al., 2011)). This is consistent with what is observed in mangrove sediment, where aerobic to anaerobic transitions typically occur close to the sediment surface, with sulphur driven processes likely to dominate in anaerobic conditions (Kristensen et al., 2008).

Below is the response to individual referee's feedback.

Referee #1

Comment from referee: This manuscript investigates the spatial variability of CO₂ fluxes from three different intertidal systems in New Zealand: a tidal flat, an *Avicennia* mangrove stand and a cleared mangrove stand. These mangroves are the southernmost ones in the IWP area and only *Avicennia marina* can grow in this temperate climate. Opposite to what is happening in the tropics, mangroves in New Zealand are expanding mainly because of increased sedimentation as a result of increased agricultural activities in water-sheds. However, numerous clearings occurred recently notably in order to “recover recreational values of estuaries”. The main objectives of the authors were to understand the effect of mangrove clearance on sediment biogeochemistry and specifically on CO₂ fluxes from mangrove soils. To reach their goals, they measured CO₂ fluxes and collected 2-cm deep cores in numerous mangroves, cleared areas, and tidal flats at one season (late spring and summer). CO₂ fluxes were determined on the field using dark incubation chambers connected to infra-red gas analyzer before and after having removed the biofilm from sediment surface. On sediment samples, grain size, TOC, and Chl *a* content were measured. In addition, forest biomass and macrofauna distribution were determined. Methods seem to have been conducted with care and references are up to date. The main results of the authors are: i) lower CO₂ fluxes in cleared mangroves compared to *Avicennia* stand, ii) after clearance, a decrease in CO₂ fluxes with time, iii) a strong effect of biofilm on CO₂ fluxes, with increased values after biofilm removal. Mangrove forests are among the most productive terrestrial ecosystem, with high rates of carbon sequestration, both in their biomass and in their soil. Unfortunately and although there is an increasing number of studies working on it, there is still a need of data to constrain the becoming of mangrove primary productivity, notably carbon mineralization with the sedimentary column and the export of CO₂ from mangrove sediments to the atmosphere, which are underestimated and understudied, even more in temperate mangroves (e.g. see papers of Leopold et al., 2013, 2015, Lovelock et al., 2014; Chen et al., 2012, 2014). The topic is thus relevant and the references are up to date; however the ms. is characterized by flaws that do not allow its publication in its present form. Usually, I find that ms. are too long for what the authors have to say, which is the opposite with the present ms. The authors did not present enough their data, and do not discuss them enough. As a result, I believe that this paper does not have the necessary breadth and depth in terms of providing fundamental new understanding in mangrove geochemistry and ecology for a publication in Biogeosciences.

Author's response: We thank the referee for the helpful suggestions. Based on the referee's comments we modified the manuscript substantially. In particular, we re-analysed our data and re-wrote the discussion to provide an in-depth interpretation of our findings.

Comment from referee: Additionally, I have listed some points that have to be explained or modified in the ms. concerning the sampling strategy, the methods, and the presentation of their results in figures or tables. I'm not sure that it was relevant to study so many sites (40 mangrove clearance, 18 mangroves, 30 tidal flats). The authors should better describe the sites and their complementarity. With such a number of sites, the reader is expecting some figures or tables to present statistical analyses between sites, as well as for the relationships between CO₂ fluxes and the parameters that can drive them. The authors may have chosen some specific areas, where they were able to have the 3 stands together (having the same sediment characteristics, hydrology, activities in watershed, etc), and to do more analyses on these specific sites. In the same way, the authors have a lot of data, including macrofauna characteristics, but since they are not well discussed, I would suggest the authors to focus, and deeply discussed the main parameters that can explain CO₂ fluxes variability in their 3 strata. Another option would be to analyse the influence of mangrove clearings on sediment biogeochemistry and biology, not only focusing on CO₂, and to present them in a more applied journal. For instance, the authors can discuss the evolution of grain size, of the TOC content, of the macrofauna density, etc., before and after clearing.

Author's response: Based on these recommendations we have made the following modifications.

- 1. We focus on sediment CO₂ efflux and sediment characteristics from intact and cleared mangrove forest sites*
- 2. We have removed macrofaunal data from the manuscript.*
- 3. We have re-written the discussion focusing on the main factors which could influence CO₂ efflux within intact and cleared mangrove forest.*

Based on the recommendations from referee 3 we also conducted a shading experiment to investigate the impact of pre-shading the sediment. The findings of this experiment are included in the revised version of the manuscript and described above.

Comment from referee: Do the authors think that cores of 2 cm are adequate for their topic? CO₂ fluxes may be influenced by physico-chemical conditions (TOC, root respiration, redox, etc.) that are developing deeper than 2 cm. What was the limit between the saturated and the unsaturated zones at low tide during their measurements?"

Author's response: We acknowledge that deeper cores would provide a better assessment of the sediment characteristics influencing sediment CO₂ efflux. However, cores to 2 cm reflect the surface sediment conditions which are likely to be a significant driver of sediment CO₂ efflux. Additional cores were also collected to 15 cm depth and used to measure remaining root mass within cleared mangrove forest sediment. As part of a separate unpublished study conducted at four temperate mangrove sites in New Zealand, we also observed a significant positive correlation between TOC at

0-2cm and at depths from 2-4cm (r_s 0.93, $p < 0.01$), 4-6cm (r_s 0.92, $p < 0.01$), and 6-8cm (r_s 0.58, $p = 0.048$).

The limit between the saturated and unsaturated zones at low tide was not directly measured. However, tides for the sites are semi diurnal with a range of 1.3 – 4.1 m, with mangrove forest sediment typically not inundated for at least half of the tidal cycle. We have included a paragraph discussing the potential implications of measuring sediment CO₂ efflux at different time of the tidal cycle in the discussion of the manuscript.

Changes to manuscript:

We note that all sediment CO₂ efflux measurements in this study were made at low to mid-tide. The efflux of CO₂ from mangrove sediment during low tide can be up to 40% greater than during tidal immersion as molecular diffusion of CO₂ is faster when sediments are aerated and the surface area for aerobic respiration and chemical oxidation increases (Alongi, 2009). However, benthic light availability is also reduced during tidal immersion, which may result in increased respiration by the microphytobenthos (Billerbeck et al., 2007).

Comment from referee: Chla concentrations are usually highly variable at sediment surface in mangroves, thus I'm not sure that one measurement per site is enough.

Author's response: At each site three sediment samples were collected using two small sediment cores (2 cm deep, 2 cm in diameter). Chlorophyll a concentration and sediment grain size were initially measured in all three samples, however as variation between samples was small only one sample was analysed for the majority of sites. For example, sediment chlorophyll a concentration ranged from 18.92 to 22.87 $\mu\text{g}^{-1} \text{g}^{-1}$ sediment at mechanically cleared mangrove forest site Whangamata E.

Comment from referee: The authors did not measure CO₂ fluxes at light, and mentioned that their measurements exclude the uptake of CO₂ by photoautotrophic process. I agree, however they mentioned that Leopold et al did not observed any differences between light and dark measurement in Avicennia stand. I have read this paper again, and it seems that it is not directly linked to mangrove species, but rather to the position in the intertidal zone and canopy closure, that will lead to specific development of the biofilm. I do not know if the length of tidal immersion in New Zealand and canopy closure are the same that in New Caledonia for the Avicennia stands. In addition, Leopold et al. did not measure CO₂ fluxes from tidal flats, but from salt flats (so not in front of mangroves, but in the back, at higher elevation, it means different conditions of sediment oxygenation).

Author's response: We modified the discussion accordingly. Please refer to the earlier section regarding the uptake of CO₂ observed at some of our sites.

Thank you for your valuable suggestions on this manuscript.

Referee #2

Comment from referee: 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.

Author's response: We thank the referee for the detailed and constructive comments. We agree that research investigating the fate of carbon when mangrove forests are cleared is valuable, as well as the relative contribution to the atmosphere.

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 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.

Author's response: Based on the referee's comment we removed the macrofaunal data. We kept the tree biomass, root mass, and pneumatophore abundance data and described their role in influencing sediment CO₂ efflux in more detail in the discussion.

Changes to manuscript:

Higher sediment CO₂ efflux observed within our study may partly be explained by the inclusion of crab burrows and short pneumatophores within flux measurements. The omission of crab burrows and pneumatophores has previously been proposed as a potential explanation of why global estimates may be underestimated (Bouillon et al., 2008). Crab burrows have been shown to increase CO₂ efflux by increasing the surface area for sediment-air exchange of CO₂ (Kristensen et al., 2008) and enhancing carbon decomposition processes (Pülmanns et al., 2014). Pneumatophores have been associated with increased CO₂ emissions by efficient translocation of CO₂ exchange from deeper sediments (Bouillon et al., 2008; Kristensen et al., 2008).

Comment from referee: 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.”

Author’s response: We substantially revised the methods section. Equations for CO₂ flux measurements, criteria for inclusion of fluxes, and the equations used to determine biomass have been included. No changes in the significance of the relationship between biomass and CO₂ efflux was observed using diameter rather than height for the two sites where height exceeded the range of the allometric equation.

Please see the earlier comments regarding changes to the fluxes.

Other changes to manuscript:

*Within intact mangrove forests the tree height of the closest 5 mangrove trees to each measurement/sampling point and the density (number of mangroves within a 2 m x 2 m area) was recorded. Above ground biomass was estimated using the allometric equations developed for *Avicennia marina* in New Zealand (Woodroffe, 1985):*

$$\text{Total above ground biomass}^{-1/3} \text{ (g dry weight)} = -4.215 + 0.121 \times \text{Height (cm)} \quad (3)$$

At two sites, Mangere 1 (Auckland) and Hatea 1 (Northland) mangrove height exceeded the range the allometric equation was designed for (determined from trees ranging in height from 40 to 248 cm) and measures of trunk diameter were instead used to estimate biomass (based on the trunk diameter at 30 cm height of the closest 5 mangrove trees to each sampling point):

$$\text{Total above ground biomass}^{-1/3} \text{ (g dry weight)} = 0.264 + 2.597 \times \text{Diameter (cm)} \quad (4)$$

At each clearance site a quadrat (0.5 m x 0.5 m) was sampled at three haphazardly placed locations (within a 10 m radius). The following metrics were recorded within each quadrat, the proportion of surface covered by mangrove leaf litter, proportion of surface covered by macroalgae, number of mangrove seeds and seedlings, and number of pneumatophores. Further, three randomly located root biomass cores (13 cm diameter, 15 cm depth) were collected at each clearance site. After sorting, all vegetative material was air dried for one week on aluminium trays, and then oven dried at 70 °C for approximately 4 days until dry weight stabilised. Weights for each mangrove constituent were then recorded (fine root mass = root diameter ≤ 2 mm, thick root and pneumatophore mass > 2 mm, and total root mass). No cores were collected from intact mangrove forest sites.

Comment from referee: 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.

Author's response: Measures of oxic depth and sediment compaction have been removed from the manuscript. We note that no significant correlation or regression was observed between these sediment properties and sediment CO₂ efflux.

Comment from referee: 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.

Author's response: We modified the discussion to address these aspects, including a considerable expansion to potential causes of CO₂ uptake. Please refer to earlier comments for further details.

Comment from referee: 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).

Author's response: Further information on the biofilm removal procedure has been included in the manuscript. Measurements were made within 30 seconds following the removal of the surface biofilm on the identical location to the corresponding biofilm intact measurement. Only flux measures with an r^2 greater than 0.8 were included. Typically the r^2 values of the biofilm removed flux values exceeded 0.95.

Changes to manuscript: In addition to measuring CO₂ efflux in intact (undisturbed) sediment, sediment CO₂ efflux was re-measured at the same location after the removal of the surface biofilm. Measurements were made within 30 seconds following the removal of the surface biofilm.

Comment from referee: I am not convinced the normalization procedure used (i.e. the 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).

Author's response: The CO_2 proportion value was used in an attempt to control for the factors which may confound the relationship between CO_2 efflux and time since clearing. However, we acknowledge that a number of assumptions are made in this calculation. We removed the CO_2 prop calculation from the analysis and focused the discussion on factors which may be influencing efflux within intact and cleared mangrove forest, rather than the relationship between CO_2 efflux and time since mangrove forests were cleared.

Additional information regarding where the three CO_2 efflux measures were collected at each site has been included in the manuscript. Measurements were haphazardly located at least 1 m apart. We acknowledge that spatial variation in measurements is quite large, both within and between sites. However, the mean variability within a site ($CV = 0.55$ for intact mangrove and 1.1 for cleared mangroves) was lower than among sites ($CV = 0.99$ for intact mangroves and $CV = 1.34$ for cleared mangroves). While increased replication in fewer sites would improve individual site estimates, we feel there is benefit to demonstrating that CO_2 efflux also varies significantly between different sites. By sampling at a large number of sites we are able to provide an overall estimate of CO_2 efflux from intact and cleared mangrove forest that better accounts for this difference than if fewer sites were included.

Comment from referee: I would like to see more figures to illustrate your key points, for example a few simple plots of CO_2 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.

Author's response: Figures of significant linear relationships between CO_2 efflux and mangrove biomass, and CO_2 efflux and sediment organic carbon concentration have been included in the

manuscript. A section where the fluxes are put into the context of other mangrove carbon cycling processes has been included in the introduction.

Changes to manuscript:

Included in the introduction:

Carbon (C) cycling and storage are important ecosystem services provided by mangrove forests (Twilley et al., 1992; Bouillon et al., 2008; Kristensen et al., 2008; Alongi, 2014). Global net primary productivity in mangrove forest has been estimated at $218 \pm 72 \text{ Tg C a}^{-1}$, which includes the rate of litterfall, above- and below-ground biomass production (Bouillon et al., 2008). An important component of the carbon cycle is the efflux of carbon dioxide (CO_2) from the sediment into the atmosphere (Raich and Schlesinger, 1992). Sediment CO_2 efflux (also called soil/sediment respiration) is the total of CO_2 released through root/mycorrhizae respiration (autotrophic respiration) and microbial respiration (heterotrophic respiration) associated with the decomposition of organic matter (Bouillon et al., 2008).

Included in the results:

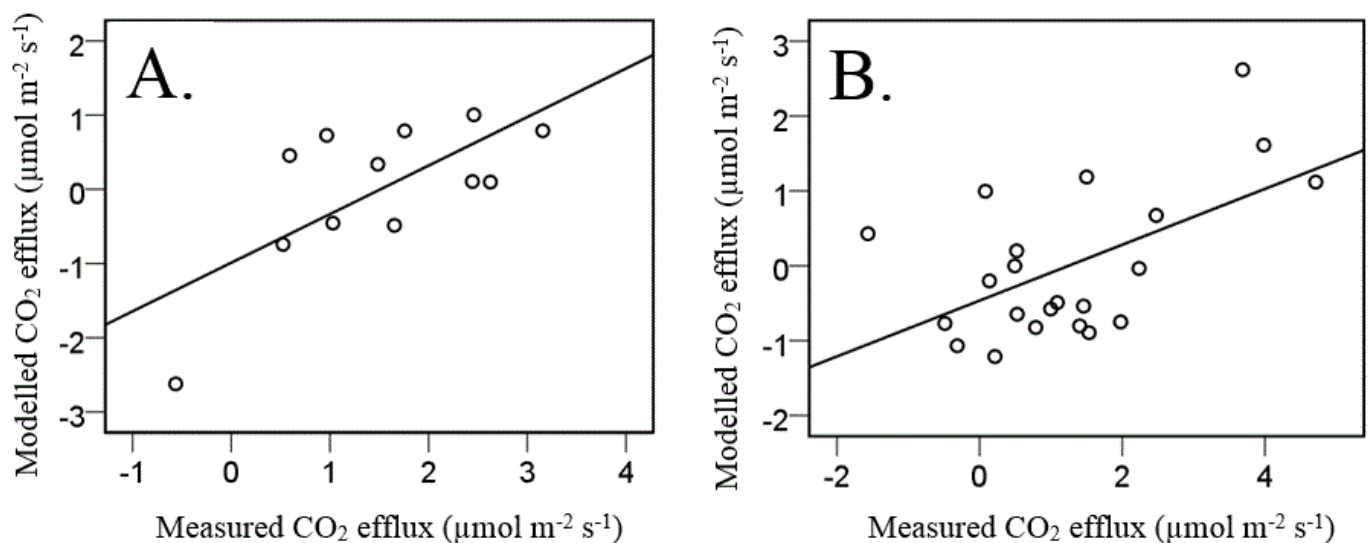


Figure 4: Model A. Modelled values of mangrove forest CO_2 efflux (based on mangrove biomass) compared to measured CO_2 efflux ($y = -0.73 + 0.59x$, $r^2 = 0.49$, $p < 0.01$). Model B. Modelled values of cleared mangrove forest CO_2 efflux (based on sediment organic carbon concentration) compared to measured CO_2 efflux ($y = -0.47 + 0.37x$, $r^2 = 0.32$, $p < 0.01$).

Comment from referee: 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

Author's response: This sentence has been modified.

Changes to manuscript:

Sediment CO₂ efflux (also called soil/sediment respiration) is the total of CO₂ released through root/mycorrhizae respiration (autotrophic respiration) and microbial respiration (heterotrophic respiration) associated with the decomposition of organic matter (Bouillon et al., 2008).

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

Author's response: The supplementary table has been modified to include whether the site is exposed or sheltered.

Comment from referee:

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

Author's response: The methods section was revised. Please refer to earlier comments.

Comment from referee: 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 sediment.

Author's response: Our aim was to investigate the losses of CO₂ from the sediment following clearing of temperate mangroves. Thus we measured CO₂ efflux using dark respiration chambers. We acknowledge that transparent chambers are critical to study the difference between photosynthesis and respiration (ecosystem) CO₂ fluxes.

Comment from referee: 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

Author's response: The normalization procedure has been removed.

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

Author's response: The wavelengths and equation for the chlorophyll a analysis procedure has been included in the methodology.

The following modifications were made to the manuscript:

Chlorophyll a concentration was calculated based on the following equation:

$$\text{Chlorophyll } \alpha \text{ (}\mu\text{g g}^{-1} \text{ sediment)} = ((750a - 665a) - (750 - 665)) \times \text{Abs} \times \frac{\text{Ethanol in extraction (l)}}{\text{Sediment analysed (ug)}} \quad (2)$$

Where 750 and 665 is the absorption at wavelengths 750 and 665 nm, 750a and 665a is the absorption at wavelengths 750 and 665 nm after acidification with 0.05 mL 1 mol HCl and Abs is the absorbance correction for chlorophyll in ethanol (28.66)

Comment from referee: 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.

Author's response: The allometric equations for the tree biomass equations and differences between the diameter vs height equations have been included in the methodology. We acknowledge the referees comment that the measures used to measure C stocks in mangrove sites were based on small sample sizes. Still, we feel the tree based information provides useful information to understanding the processes influencing sediment CO₂ efflux

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

Author's response: The measurements of oxic depth and sediment sink have been removed from the manuscript.

Comment from referee: 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.

Author's response: The macrofauna data has been removed from the manuscript as suggested by the referee.

Comment from referee: Page 3557 Line 16 – This analysis does not add anything to the CO₂ flux story.

Author's response: The manuscript has been modified with this line removed.

Comment from referee: 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.

Author's response: The manuscript has been modified to focus on CO₂ efflux from intact and cleared sediment, with tidal flat data removed. We note that the r^2 of the linear regression of the change in CO₂ efflux at many of the tidal flat sites was less than 0.8, originally included due to minimal change in flux at many of locations leading to poor r^2 values. An expansion of the possible reasons for CO₂ uptake has been included in the manuscript discussion as well as additional testing of pre-shaded sediments.

Comment from referee: 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.

Author's response: We have removed the data/discussion on macrofauna abundance

Comment from referee: 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 this 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.

Author's response: The manuscript has been modified to include figures of the significant linear regressions. The influence of site hydrodynamics and organic matter supply has been expanded on within the discussion.

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

Author's response: These values have been updated

Comment from referee: Page 3560 Line 2 and 4 – the Figure states p

Author's response: This values has been updated

Comment from referee: Page 3560 Line 17 What about mangrove NPP and hydrology?

Author's response: The manuscript has been modified to include consideration of mangrove NPP and hydrology. Please refer to earlier response

Comment from referee: Page 3561 Line 6 to 12 No light incubations to test this!

Author's response: We acknowledge that no light chamber measurements were collected, however we infer that higher chlorophyll a concentrations measured in the sediment may be used as a proxy for increased photosynthetic activity (Bishop, 2007).

Comment from referee: 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.

Author's response: The manuscript has been revised.

Changed to the discussion:

We did not find a significant difference in sediment CO₂ efflux between intact and cleared mangrove forest sites. Further, we did not find a relationship between time since clearing and sediment CO₂ efflux. In contrast, sediment CO₂ efflux from cleared peat mangrove forests in Belize declined logarithmically over a 20 year period (Lovelock et al., 2011). Two months after the clearing of mangroves in Kenya, sediment CO₂ efflux increased approximately two fold before returning to comparable levels to adjacent intact mangrove forests approximately five months after clearance (Lang'at et al., 2014). It is likely that a number of factors (such as differences in site sediment characteristics, size, exposure, and method of clearance) are confounding the effect of time since clearing on sediment CO₂ efflux in our study.

Comment from referee: Page 3563 Line 2 - 9 Elaborate on this some more

Author response: The manuscript has been revised. Macrofaunal data has been removed from the analysis based on referee recommendations.

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

Author response: Yes, however no significant relationship was observed between crab burrow abundance and CO₂ efflux. However, we have expanded on the potential impact of including crab burrows and pneumatophores within flux chambers in the discussion.

Comment from referee: 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.

*The manuscript has been modified to include greater consideration of phototrophic CO₂ uptake.
Please refer to earlier comments.*

Thank you for your valuable suggestions on this manuscript.

Referee #3

Comment from referee: 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 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.

Author's response: We appreciate the referee's helpful comments. We agree that greater consideration should be made regarding the rate of carbon released from mangrove forest following clearance and the relative contribution to the atmosphere. The manuscript has been modified to include a more detailed explanation of the methods, as well as the inclusion of significant linear regressions. We also modified the discussion substantially.

Comment from referee: 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.

Author's response: We revised this statement

Changes in manuscript: An important component of the carbon cycle is the efflux of carbon dioxide (CO₂) from the sediment into the atmosphere (Raich and Schlesinger, 1992). Sediment CO₂ efflux (also called soil/sediment respiration) is the total of CO₂ released through root/mycorrhizae respiration (autotrophic respiration) and microbial respiration (heterotrophic respiration) associated with the decomposition of organic matter (Bouillon et al., 2008).

Comment from referee: 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.

Author's response: We removed the oxic depth measurements from the manuscript.

Comment from referee: 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.

Author's response: The manuscript has been modified to consistently use the terms sediment and mangrove forest.

Comment from referee: 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 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.

Author's response: Please refer to the earlier sections where this point is addressed.

Comment from referee: Abstract. Line 7: Here and throughout the MS, I recommend denoting the environment “mangrove forest” as “mangrove” refers to the trees only.

Author's response: The manuscript has been modified as recommended.

Comment from referee: Line 16-17: 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.

Author's response: Different units are used to express CO₂ efflux in terrestrial and coastal soil/sediment studies. We feel that the units $\mu\text{mol m}^{-2} \text{s}^{-1}$ are more appropriate given that the measurements were conducted over short period of time (90 sec) during low tide. However, we converted $\mu\text{mol m}^{-2} \text{s}^{-1}$ into $\text{mmol m}^{-2} \text{d}^{-1}$ to compare with other studies.

Table 2: Comparison of mean estimates of sediment CO₂ efflux from a range of intact and cleared mangrove forests, \pm SE. * indicates no overall mean values provided

Intact Mangrove Forests				
Species	Location, number of sites	Overall mean CO ₂ efflux \pm SE (mmol CO ₂ m ⁻² d ⁻¹)	Overall mean CO ₂ efflux \pm SE ($\mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$)	Reference
<i>Avicennia marina</i>	New Zealand , 13	168.4 \pm 45.8	1.95 \pm 0.53	This study
<i>Avicennia marina</i>	New Zealand , 4	114.0 \pm 19.9	1.32 \pm 0.23	Lovelock et al, (2014)
<i>Avicennia marina</i>	South and North Australia , 4	107.1 \pm 45.8	1.24 \pm 0.53	Lovelock et al, (2014)
<i>Avicennia marina</i>	New Caledonia, 1	88.2 \pm 23.7	1.02 \pm 0.27	Leopold et al., (2013)
<i>Avicennia marina</i>	South Australia, 3	*Ranging from 73.73 to 117.89 throughout the year	*Ranging from 0.85 to 1.36 throughout the year	Livesley and Andrusiak (2012)
	Global estimate, 82	61 \pm 56	0.71 \pm 0.65	Bouillon et al., (2008)
	Global estimate, 140	69 \pm 8	0.80 \pm 0.09	Alongi, (2014)
Cleared Mangrove Forests				
Species	Location, number of sites	Overall mean CO ₂ efflux \pm SE (mmol CO ₂ m ⁻² d ⁻¹)	Overall mean CO ₂ efflux \pm SE ($\mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$)	Source
<i>Avicennia marina</i>	New Zealand , 23	133.9 \pm 37.2 (0 - 8 years since clearing)	1.55 \pm 0.43 (0 - 8 years since clearing)	This study
<i>Rhizophora mangle</i> – peat soils	Twin Cays, Belize, 5	*Declining from 658.3 to 181.4 over 20 years	*Declining from 2.10 to 7.72 over 20 years	Lovelock et al., (2011)
Tropical mangrove	Bali, Indonesia, 1	*Shrimp pond floors: 99.4; Shrimp pond walls: 272.2	*Shrimp pond floors: 1.15; Shrimp pond walls: 3.15	Sidik and Lovelock, (2013)
Tropical mangrove	Gazi Bay, Mombasa, Kenya	88.62 (343 days since clearing)	1.03 (343 days since clearing)	Lang'at et al., (2014)

Comments from referee: Line 34: Change to “Temperate mangrove forests are subject to harsh climatic condition leading to a lower. . .”

Author’s response: The sentence has been modified. We note that while tropical mangrove habitat is subject to cyclones and typhoons, temperate mangroves are subject to frosts, which by comparison may not be considered harsh.

Changes to the manuscript: Temperate mangrove forests are subject to colder and generally more variable climatic conditions, and are typically associated with lower diversity of tree species and lower faunal abundance and diversity than in the tropics (Alfaro, 2006;Morrissey et al., 2010).

Comment from referee: 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.

Author’s response: We modified this paragraph.

Changes to the manuscript: A landward expansion of mangroves into salt marsh is observed in Australia and USA (Cavanaugh et al., 2014; Saintilan et al., 2014) while mangrove expansion into tidal flats is typically observed in New Zealand (Stokes et al., 2009;Lundquist et al., 2014). In addition, there is evidence of mangroves expansion from tropical areas northwards and southwards to temperate regions (Saintilan et al., 2014). The expansion of mangroves in New Zealand have been linked to increased sedimentation leading to vertical accretion of tidal flats (Swales et al., 2007;Stokes, 2010), increased nutrient inputs (Saintilan and Williams, 1999), and climatic factors (Burns and Ogden, 1985).

Comment from referee: 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.

Author’s response: The manuscript has been modified as recommended.

Changes to manuscript introduction:

Studies from tropical mangrove forests have shown that sediment CO₂ efflux is influenced by sediment carbon and nutrient quantity (Leopold et al., 2013;Chen et al., 2010;Chen et al., 2012;Chen et al., 2014) and quality (Kristensen, 2000), sediment grain size ((Chanda et al., 2014;Chen et al., 2010), sediment water content (Alongi, 2009), redox potential (Leopold et al., 2013;Chen et al., 2010;Chanda et al., 2014;Chen et al., 2012), and sediment temperature (Chen et al., 2012).

Comment from referee: 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

Author's response: We revised this section

Changes to manuscript:

Chemoautotrophs have also been shown to fix carbon in intertidal sediment under dark conditions (Boschker et al., 2014; Lenk et al., 2011). In particular at the interface of aerobic and anaerobic zones where large amounts of reduced compounds, such as sulphur, accumulate (Santoro et al., 2013; Boschker et al., 2014; Thomsen and Kristensen, 1997; Lenk et al., 2011)). This is consistent with what is observed in mangrove sediment, where aerobic to anaerobic transitions typically occur close to the sediment surface, with sulphur driven processes likely to dominate in anaerobic conditions (Kristensen et al., 2008).

Comments from referee/author's response: Material and Methods

Line 104: Change to “. . .from the top to the central North Island. . .” ; *changed accordingly*

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.”; *section was revised*

Line 132: What was the area covered by the chamber? This is important information. ; *We revised this section “The sediment area covered by each chamber was 0.00785 m². Total chamber volume varied between 1.72 and 1.98 l depending on the depth of collar insertion.”*

Line 139: Here we have one of the places where sediment and soil terms are mixed. Please delete “soil” here.; *soil was replaced by sediment throughout the manuscript*

Line 146: It is quite late to inform about the darkened chambers here. It must be done earlier.; *modified accordingly*

Line 148-150: I disagree that the approach used excludes photoautotrophic contribution. I have found that CO₂ fixation occurs during the first 30 minutes after darkening. It is a very serious flaw. *Please refer to the comments earlier in regards to the impact of pre-shading the sediment.*

Line 152: Change “years” to “time” here and throughout.” ; *changed accordingly*

Line 154-155: This statement is not clear to readers because it refers to one of the conclusions of the paper. Please omit.; *statement was deleted*

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.” ; *we removed the CO₂prop calculation*

Line 176: What do you use the inorganic carbon concentration for?; *The reference to inorganic carbon concentration has been removed*

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; *we will modify the methods in future studies.*

Line 186- 187: How were the samples for chlorophyll stored during the month before analysis? This is important.; *samples were frozen and stored in the dark prior to analysis. We modified the methods accordingly*

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. *The macrofaunal data has been removed.*

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. *The macrofaunal data has been removed.*

Line 245-248: Delete these lines. They repeat what is stated just above.” ; *we deleted these sentences*

Comments from referee/author's response: 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.; *Please see the comment above regarding the units used.*

Line 269: Table 2 is not the correct table to refer to here.; *changed accordingly*

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.; *the fauna have been removed*

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.; *the result section was modified*

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.; *Please refer to the comments earlier in regards to the impact of pre-shading the sediment.*

Comments from referee/author's response: 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 reported, then they are suddenly higher than

previously reported!!!! What about the results obtained by Alongi and/or Kristensen. They are not mentioned.” ; *we revised this section*

Changes to manuscript discussion:

Dark sediment CO₂ efflux in intact Avicennia marina forests across its distribution range in New Zealand (1.95 ± 0.53 μmol m⁻² s⁻¹ which equals 168.4 ± 45.8 mmol CO₂ m⁻² d⁻¹) is similar to values reported for intact Avicennia marina forests in other temperate (New Zealand, Australia; Lovelock (2008), Lovelock et al. (2014), Livesley and Andrusiak (2012)) and tropical locations (New Caledonia; Leopold et al. (2013); Leopold et al. (2015)) (Table 2). In contrast, our values are higher than the global estimates of sediment CO₂ efflux from intact mangrove forests including a number of other tropical mangrove species (0.71 ± 0.65 μmol m⁻² s⁻¹ (Bouillon et al., 2008); 0.80 ± 0.09 μmol m⁻² s⁻¹ (Alongi, 2014)) (Table 2). Higher sediment CO₂ efflux observed within our study may partly be explained by the inclusion of crab burrows and short pneumatophores within flux measurements. The omission of crab burrows and pneumatophores has previously been proposed as a potential explanation of why global estimates may be underestimated (Bouillon et al., 2008). Crab burrows have been shown to increase CO₂ efflux by increasing the surface area for sediment-air exchange of CO₂ (Kristensen et al., 2008) and enhancing carbon decomposition processes (Pülmanns et al., 2014). Pneumatophores have been associated with increased CO₂ emissions by efficient translocation of CO₂ exchange from deeper sediments (Bouillon et al., 2008; Kristensen et al., 2008).

Line 323: Change to “. . .tropical mangrove forests. . .” , *changed accordingly*

Line 333-334: It seems that everything is affecting CO₂ emission. The list mentioned covers almost everything. *The manuscript has been modified.*

Line 337: Now the unit becomes even more strange “m² s⁻¹.” , *units were corrected (μmol m⁻² 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. ; *we re-wrote this section.*

Line 355-356: How can sediment characteristics play any role? Please clarify.

The manuscript has been modified to include the following:

High clay which was found at these sites may have also contributed to the accumulation of sediment carbon which has been shown to be associated with higher CO₂ efflux in tropical mangrove forests (Leopold et al., 2013; Chen et al., 2010; Chen et al., 2012; Chen et al., 2014).

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. ; *Please refer to the comments earlier in regards to the impact of pre-shading the sediment.*

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.

We have looked into the relationship between crab burrow/pneumatophore abundance and CO₂ efflux, but were not able to draw any significant conclusions from the dataset, likely due to other site characteristics confounding expected relationships. We have expanded on this as a potential explanation for the increased flux we observed.

Line 393-395: I don't understand this sentence. *This section has been revised*

Changes to manuscript:

Higher sediment carbon concentrations have been measured in older mangrove forests, growing further inland compared to younger mangrove forests, growing at the expanding seaward edge (Lovelock et al., 2010). This may also be related to the protection offered by seaward mangroves, enabling greater accumulation of carbon enriched mangrove detritus within the centre of the stand (Yang et al., 2013).

Line 396-397: This effect must be short-term.

Changes to manuscript:

Increased sediment CO₂ efflux has been observed within intact mangrove forest following disturbance of the top 30 cm of the sediment, however the effect was transitory, returning to pre disturbed levels within two days (Lovelock et al., 2011).

Line 407- 411: I don't believe in this adjustment.; *The manuscript has been modified and the CO₂prop calculation removed.*

Line 434: We have not heard that crab burrows were counted. These burrows are important conduits for CO₂ release.; *Crab burrows were counted at each site but not within individual chamber incubations. As no significant relationship between crab burrow abundance and CO₂ efflux was observed we have removed crab burrow abundance data from the manuscript. However, we have expanded on the importance of crab burrows within our discussion.*

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; *The tidal flat data has been removed from the*

manuscript.. Please see above for further information on the additional testing conducted on CO₂ efflux data.

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). *Please refer to the comments earlier in regards to the impact of pre-shading the sediment.*

Line 447-449: No, such polymeric surface film cannot be a strong barrier. This has been shown by others. *This statement has been modified.*

Changes to manuscript: Other studies have suggested that the biofilm may also act as a barrier to the flow of CO₂ from deeper sediment, which when removed results in a rapid increase in CO₂ efflux (Leopold et al., 2015; Leopold et al., 2013).

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.

This sentence has been removed

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

This was an oversight. The manuscript has been modified to include the correct unit ($\mu\text{g}^{-1} \text{g}^{-1}$ sediment)

Thank you for your valuable suggestions on this manuscript.

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