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Interactive comment on "Soil greenhouse gases emissions reduce the benefit of mangrove plant to mitigating atmospheric warming effect" *by* Guangcheng Chen et al.

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Dear Reviewer,

Thank you very much for your comments and suggestions. The followings are our responses to your comments. We hope the following responses and revisions made are satisfactory.

General comment

Comment 1: Why were the three sites that were studied chosen? Response: As some mangrove dominated shores in Jiulong River Estuary were subjected to erosion, Spartina alterniflora invasion or garbage from upstream, we chose the three mangrove

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sites in good conditions so as to eliminate such exogenous impacts. The reason for the site choosing was added the in revised manuscript. Please refer to Page 5 Lines 12-14.

Comment 2: The IPCC Wetland supplement (2013) is an important contextual document that could be cited. The Supplement suggests that CH_4 emissions occur with salinity < 15 ppt, as seems to be the case in this study. Response: Thanks for the suggestion. The IPCC Wetland Supplement was cited in the revised manuscript.

Comment 3: There are recent papers on GHG emissions not cited by the authors including the work by Maher et al. Response: Some recent studies on greenhouse emissions relevant to our study, such as Alongi (2014ab), Lewis et al. (2014), Bulmer et al. (2015), Nóbrega et al. (2016) andLeopold et al. (2015) have been cited in the revised manuscript. The work by Maher et al. (2013), suggesting DIC as majority of dissolved carbon exportation in mangrove forest was also cited in the revised manuscript.

Comment 4: The hypotheses could be improved. In the paper the relationship between GHG emissions and soil parameters are reported (as correlation coefficients) but there are no explicit hypotheses stated. Response: Accepted. The hypotheses have been revised and the relationship between GHG fluxes and soil parameters are also stated. Please refer to Page 4 Lines 26-30.

Comment 5: Table 3 is the center piece of the study, yet there is a lot of information missing of how the authors arrived at these values. The authors use measures of NPP derived from litter fall (2.75 x) and an annual value of soil respiration to calculate Net Ecosystem Production (NPP - SR). An annual plant CO_2 sequestration rate is calculated as NPP x 44/12 and then compared against the annualized GHG emissions to provide an Ecosystem mitigation potential; and the GHG emissions are assessed as a proportion of annual NPP, although the % values are not provided in Table 3. Response: Thanks for the suggestion. The detailed information for the data calculations were added in the Method section and in the table title in the revised manuscript.

Comment 6: The authors need to explicitly indicate in the methods how they scaled up their point measurements to annual values. How was tidal variation and seasonal variation incorporated into the scaling up? The calculations do not include C inputs due to allochthonous C sources trapped in sediments which may be large at this site? Response: In this study, the annual GHGs emission rates were the averages of the fluxes measured in the four seasonal sampling campaigns. This calculation and whether the estimation based on these limited measurements would affect the finding of present study were stated and discussed in the revised manuscript (refer to Page 9 Lines 1-19). Agree that allochthonous C sources trapped in mangrove may be significant but the present study didn't measure the allochthonous C buried in mangrove soils. However, result of carbon burial rate from Alongi et al. (2005) was cited in the manuscript to further discuss the relevance of mangrove wetland (Page 15 Line 22-24), and a discussion how results of present study could be incorporated with the global carbon dynamics (including the C burial) was also added in the revised manuscript (Page 16 Lines 3-20).

Comment 7: In the Methods section the authors claim that all CO_2 fluxes from the soils are derived from heterotrophic metabolism as chambers were not deployed over aboveground roots. But below ground roots are dense in mangrove forests and thus the claim needs to be better substantiated. The citation to Tomlinson is not appropriate as this is a botanic text with no reference to gas fluxes from heterotrophic vs. autotrophic sources. Response: Sorry for the in-appropriate citation of the Tomlinson's literature. Tomlinson (1986) suggested that the aerenchyma tissues of below-ground roots are connected with lenticels on pneumatophores, prop roots, and buttresses above the ground. Komiyama et al. (2008) further concluded that most metabolic respiration from mangrove roots is considered to be released through the lenticels and suggested that below-ground roots of mangroves may make a small contribution to the soil respiration when soil respiration chambers are placed so as to avoid peumatophores. This has been corrected in the revised manuscript. Please refer to P9 Lines 9-28. In our study, the Kandelia obvata trees develop buttresses and have no pneumatophores (shown in Fig. 1); the gas samplings were done at the interior of vegetation and the roots in the surface sediment under the chambers are not dense and obvious. Therefore, the respiration of below-ground roots didn't make relevant contribution to the CO2 flux. This was also stated in the revised manuscript (Page 9 Lines 28 to Page 10 Line 2).

Comment 8: There are lapses in English expression that need to be corrected. Response: Thanks for the suggestion. An improvement in English has been done in the revised manuscript.

Comment 9: Providing the dry bulk density of sediments would also be useful. Response: Soil bulk densities in the three sites were added in the revised manuscript and the results showed no significant difference among the three sites.

Comment 10: Table 3 is confusing. More information needs to be provided in the caption. Response: Detailed information was added in Table 3 in the revised manuscript.

Minor comments

Comment 1: Title: mangrove plants (plural) or mangrove forests L17 – plural for plants. Forests maybe more suitable word. Response: Considering that the word "forest" is close to "ecosystem" and the present study is to quantify the carbon sequestration by mangrove plants, "plants" was thus used in the title.

Comment 2: L16 – what direction of gas fluxes? Add a statement about the direction (uptake; losses?) Response: Accepted. "release" of greenhouse gases was stated in the revised Abstract (Line 24).

Comment 3: L20 – is 22% a large proportion? The authors are overstating the case. A large proportion would be most of the GHG gains being lost because of simultaneous methane emissions. Response: The statement was corrected to "partially offset" throughout the revised manuscript.

Comment 4: L26 – remove word "problems" Response: Accepted.

Comment 5: L28 – The CO_2. . .. . . Response: Accepted.

Comment 6: P2L9 – and as detritus in the sediment Response: Accepted.

Comment 7: P2L11 rewrite Response: Accepted. This sentence was rewritten to "indicating that the CO_2 sequestration capability of global mangrove is equal to 4996 g CO_2 m^{-2} yr^{-1}".

Comment 8: P2L12– alternating Response: Accepted.

Comment 9: P2L22 place the citations at the end of sentences Response: Accepted.

Comment 10: P2L26 Alongi indicates lower proportional loss of CO_2 due to soil respiration. However, losses through tidal exchange may also be high $\sim$30% (Maher et al.; Boullion; Alongi). Response: The finding of Alongi (2014) that soil respiration accounts for partial of the CO_2 loss from mangrove soil and other sources may also be significant was stated in the revised manuscript (Page 13 Lines 11-16). The significant loss of inorganic carbon by tidal water was also stated in the discussion section; please refer to Page 16 Lines 7-12.

Comment 11: P3L 30 – high levels of spatial variation Response: Accepted.

Comment 12: P4 first paragraph. Needs some English editing for clarity Response: This paragraph was removed from the manuscript according to the comments from other reviewers.

Comment 13: P4L18 - inserting chambers 3 cm. Would this result in severed roots? Response: In this study, the sampling points located between the trees, and the no obvious root biomass was observed in the soil under the chambers. This was stated in Line 260 in the revised manuscript.

Comment 14: P6 throughout the results P values are listed as 0.000 – this is not correct for the reported F statistic. Response: Thanks for the suggestion. '$P=0.000$' was corrected to '$P<0.001$' throughout the text in the revised manuscript.

Comment 15: P7L1 Relationships between GHG fluxes and soil characteristics need to be shown (plotted) rather than just present Pearson correlation coefficients. Pearson correlation coefficient can be very misleading when data is not normally distributed. Response: Thanks for the suggestion. Figures showing the relationship between soil parameters and gas fluxes were prepared. Considering that the numbers of the panels were up to 28 (4 gases and 7 soil parameters), these figures were provided as supplementary files. Please refer to Supplementary figures 1-4.

Comment 16: P7L11 Litterfall production Response: Accepted.

Comment 17: P8L8 Is 22% 'large'? I think 22% as a large proportion is overstating. How does this compare with values provided in the IPCC Supplement? Response: Agree with the reviewer that 22% as a large proportion is overstating. The conclusion was corrected in the revised manuscript. A comparison of our finding with IPCC Supplement was also added in the revised manuscript. Please refer to Page 12 Lines 11-12, Page 13 Lines 11-14.

Comment 18: P9L19 Does this suggest an autotrophic source for soil respiration? Response: In Lovelock's study, the author didn't attribute this relationship between litter fall production and CO2 to autotrophic root respiration. However, she suggested that the live roots contribute to mangrove soil respiration as her in-situ fluxes were higher than those reported from mangrove soil cores in other studies. Moreover, the relationship between leaf area index (LAI) and CO2 flux also indicated the contribution of autotrophic source of microalgae, as at low LAI light penetrates the canopy stimulating growth of microalgae. In our study, root respiration didn't contribute to the soil respiration (as explained above), but the CO2 flux measurements also included the metabolism microalgae. This is also stated in the revised manuscript; please refer to from Page 9 Line 20 to Page 10 Line2.

Comment 19: P9L29 – 'unelectable' this is not the correct word to use here Response: 'unelectable' was corrected to 'small' in the revised manuscript.

Comment 20: P9L33-34 a statement provided without a reference. This is a part of the need for the authors to more clearly articulate the scaling up approach in the methods section. Response: Thanks for the suggestion. The relevant description on the scaling up of gas fluxes were stated in the Methods section in the revised manuscript, and the sentence was revised so that the references are listed following the assumption.

Comment 21: P10L1-6 this discussion is related to assumptions made when scaling up measurements. But the approach is not described. Response: Thanks for the suggestion. The method for the estimations of annual gas fluxes was stated in the Method section.

Comment 22: P10L20 stored in biomass and as detritus? Response: Accept and revision was done.

Comment 23: P10L23 omit "in the mangrove wetland" Response: Accepted.

Comment 24: P11L5-9 English needs attention Response: Sorry for the confusion. This paragraph was rewritten accordingly. Refer to .

Comment 25: P11L11 – largely offset is overstating for a 22% offset. Partially offset would be more appropriate. Response: Accepted.

Comment 26: Table 1 Reproduction (remove hyphen) Response: Accepted.

Comment 27: Table 2 – caption needs to include abbreviations e.g. OC, TKN Response: Accepted.

Comment 28: Fig 1 – could this be improved to add more information? It locates us in the estuary, but does not do much else. Response: Thanks for the suggestion. An improved Fig. 1 was provided in the revised manuscript, with the mangrove areas and scenes of the canopy and the interior forest shown.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-662/bg-2015-662-AC5-

supplement.zip

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