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Discussions

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 comments

Comment 1: The manuscript is generally well written and comprehensible, with few language lapses. There are anyhow severe methodological problems with the measurement of gas fluxes. Response: The language edit has been done for the manuscript.

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On the other hand, detailed illustrations of the methods were also added in the revised manuscript; please refer to the following responses to detailed comments.

Comment 2: Also, it is not clearly discussed and reasoned exactly what NPP – soil gas emissions measures and how does it describe the “mitigating effect of mangrove wetland on atmospheric warming”. Response: The present study quantified NPP to estimate the CO2 sequestration by mangrove plants. Soil release greenhouse gases and contribute to the atmospheric radiative forcing. This study balanced the CO2 sequestration rate of mangrove plant with the warming effect of soil greenhouse gas emissions (indicated by CO2-equivelent flux). Such balance is based on the greenhouse gas exchange between mangrove ecosystem and atmosphere, which indicates the reduction of or contribution to the atmospheric radiative forcing (Chmura et al., 2011), and therefore reflects the role of mangrove wetland in mitigating atmospheric warming. This is stated in the revised manuscript; please refer to Page 3 Lines 10-13 and Page 16 Lines 15-17.

Comment 3: Further, at least one of the sites (CPT) is “rehabilitated”, but there is no description of the site history: when and how was it rehabilitated, what happened before the rehabilitation? One site (HMI) is “natural”. It is not revealed if the third site is natural, rehabilitated, or something else. Response: The detailed information of the three sites, including the site history, area, is added in the Study area section in the revised manuscript.

Comment 4: The measured C and GHG cycle components are useful and necessary for the estimation of climate effect of mangroves. But the validity of the gas measurement methodology is highly questionable especially for CO2 and it is not clearly interpreted what the result really indicate (what is the effect of NPP on climate). Response: NPP relates to the capability of plants removing CO2 from atmosphere and thus indicates their capability of mitigating climate warming; this was stated in manuscript; please refer to Page 2 Line 25 to Page 3 Line 2. In addition, the validities of the gas measurements, such as the flux measurement and the estimation of annual flux from limited measurements were discussed in the revised manuscript. Please refer to the response to the detailed essential comments (Comment 5-8).

Detailed essential comments

Comment 1: The sites should be well described. At least land use history and possible human interventions (must exist at rehabilitated sites as least) should be announced. Otherwise the reader cannot know what kind of ecosystems the manuscript deals with. Also, canopy heights and stand densities should be announced for all the sites (now they are only partly announced). It is told that the sites are located at different tidal zones, but this is very superficial. Give some exact numbers/descriptions of the tides (lengths of inundation/exposure times, graphs describing daily water table dynamics etc.). Response: Thanks for the suggestions. In the revised manuscript, more detailed information, such as the land use change, plantation of mangrove, canopy height, tree density for the three sites were provided. As the information of the tides and flooding days for the sites are unavailable, we are sorry that it was not provided in the revised manuscript. The present study described the tidal zonation of the mangrove sites based on their intertidal elevations and the intertidal zonation scheme in Jiulong River Estuary, as described by Chen et al. (2006). This was stated in the revised manuscript; please refer to Page 5 Lines 18-20.

Comment 2: NPP is estimated by estimating annual aboveground litter production and multiplied by 2.75 to arrive at total NPP of K. obovata. This is a reasonable way to roughly estimate the magnitude of biomass production. But what happens to this biomass? At least in the case of the natural site, biomass cannot just increase but it should be more or less at steady state. Response: Agree that mangrove plants have variable growths and biomass accumulation at different age state. However, the global ratio of litter fall production to NPP was mainly derived from natural forest. In the present study, two mangrove sites are natural plants (XG and HMI) and the CPT mangrove (~40 years old) has also been considered as mature (Chen et al., 2007). Therefore, we though this estimation was reasonable. In another study by Lee (1990),

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the author also measured a biomass accumulation in a natural K. obovata forest.

Comment 3: The soil CO2 emissions estimated in this study counterbalance only a very small fraction of the estimated NPP. Would not the dead parts of trunks and branches decompose releasing CO2 back to the atmosphere? Is CO2 lost with tides as DIC and released then back to the atmosphere? etc. (see for example Bouillon et al. 2008). Response: Agree with the reviewer that the decomposition of dead organic matter, like the trunks, branches and leaf fall releases CO2 back to the atmosphere, which also contribute to the greenhouse gas emissions and their global warming effect. This was discussed in the revised manuscript. CO2 released from mangrove soils could be dissolved in tides during the flood. Partial of the dissolved CO2 is released to atmosphere from the seawater in the mangrove, while partial is exported with tides and further released to atmosphere. In the present study, the assessment was based on the greenhouse gas exchange between mangrove wetland and atmosphere, which was estimated within the mangrove area. The release of GHGs out of the mangrove area was not considered in this study. However, we considering this lack of consideration would not affect the finding of this study as the same flux of soil-atmosphere during exposure as that of water-atmosphere during flooding was assumed based on previous studies. This has been clarified in the revised manuscript; please refer to Page 9 Lines 12-19.

Comment 4: It should be well discussed, what exactly is the effect of NPP on climate. NPP converted to CO2 equivalents will definitely result in a huge climate cooling effect, but it is only the part of NPP that is permanently stored somewhere (in soils, biomass, oceans. . ..) that really has a climate cooling effect. Response: In this study, we estimated the NPP to indicate the CO2 sequestration rate of mangrove plants. The CO2 sequestration by plants i.e. the CO2 removal from atmosphere reflects their cooling effect (Page 2 Line 25 to Page 3 Line 2). The CO2 uptake by plants is then transferred to their biomass, partial of which is loss as litter fall or dead roots, which are further cycled within or outside the wetlands. Agree with the reviewer that the NPP that is permanently stored in mangrove soils, biomass and oceans reflects the climate cooling effect. This is due to that partial of the NPP is directly or indirectly returned into atmosphere as carbon gases. In this study, the assessment was based on ecosystem gaseous exchanges. The lack of consideration of the fate of NPP and its importance in the mitigation potential of mangrove wetlands were discussed in the revised manuscript; please refer to Page 16 Lines 3-20.

Comment 5: Gas fluxes are measured with small closed chambers, which is a widely used method. But it is told that there was no gas mixing (fan) inside the chamber during the closure. The very idea of calculating gas flux based on concentration change in a chamber applying its volume and area is based on the assumption of constant gas concentration in the chamber head space. The reported linear concentration change during the closure is good and necessary, but it does not imply that air mixing would not be needed. The gas concentration may increase linearly at the sampling valve and any other single point in the chamber, but there likely develops a vertical concentration gradient inside the chamber during the closure. Thus this method is biased to unknown magnitude (depending on chamber shape, flux rate etc.) to start with. Response: Agree with the reviewer that the gases develop a vertical concentration gradient inside the chamber during the closure. It is generally assumed that molecular diffusion is sufficiently rapid within the chamber headspace such that homogeneous gas concentrations exist when sampling, unless large amounts of vegetation are present or the chamber volume/basal area is large (Livingston and Hutchinson, 1995). A comparison study by Moore and Roulet (1991) showed that the fluxes measured using static chambers (0.053 m2 basal area and 40cm height) has no difference from those using dynamic chamber (the insider air was circulated) with similar height. In this study, the chambers, with a volume of 1.25 l and an area of 0.025 m2, had similar volume/basal area ratio to that (1l vs.0.02 m2) used by Corredor et al. (1999), which is sufficiently small for rapid gases accumulation but large enough to minimize disturbance of the enclosed sediment surface. The height of the head space inside the chamber is around 5 cm in the present study, much lower than that used by Moore and Roulet (1991).

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Therefore, we consider that such gradient would not affect the flux measurements and the static chamber in this study is suitable for the flux measurement. These have been stated in the revised manuscript; please refer to Page 6 Lines 5-16.

Comment 6: Also, the small chamber size (V/A) is a bad problem with CO_2. For N_2O and CH_4, it and the long closure time are needed to capture the very small flux. But with CO_2, the same chamber design leads to an extreme concentration increase during the closure (even thousands of ppm during the 1/2 hour closure). This likely very badly underestimates the CO_2 flux. Typically CO_2 flux is measured with only a few minutes chamber closure by portable CO_2 analyzers and even nonlinear regressions are applied to realistically estimate CO_2 flux after chamber closure. Response: Agree with the reviewer that CO_2 has more intensive concentration increase in the chamber. The present study selected the sampling time at 10-min intervals based on the finding of previous study (Chen et al., 2010). The results showed that the linear regression relationships between the deployment time (with regular sampling at 15-20 minutes' intervals) and the greenhouse gas concentrations were highly significant (the regression coefficient values R^2 were 0.963-0.999, 0.824-0.999, and 0.870-0.994 for N_2O, CH_4 and CO_2, respectively), even in event that the gas concentrations in the chamber were very high. These indicated that gases continuously released from the sediment and accumulated in the chamber during their sampling. In this study, the CO_2 flux was much lower than those reported by Chen et al. (2010) in Maipo and Futian mangroves in South China, and the dimension of sampling chamber was the same as that used by Chen et al. (2010); therefore, the build-up concentration of CO_2 in the chamber would not result in significant underestimation of gas flux. The explanation of sampling interval has been included in the revised manuscript. Please refer to Page 6 Lines 18-27.

Comment 7: Further, it is not well justified, how the CO_2 flux measurement from bare soil without pneumatophores equals heterotrophic respiration. Would not pneumatophores act as pathways for also CO_2 from heterotrophic respiration, and would not there also be K. obovata roots respiring in the soil even when pneumatophores are not present? Or if the measurements were conducted very far away from the vegetation, would not the measurement points have unrealistically low litter input and thus unrealistically low heterotrophic respiration? It is not clearly described how the measurement points were placed in relation to the vegetation. Response: In the present study, K. obovate develops no pneumatophore. The samplings were conducted in the forest interior, so the measurement points have no unrealistically low litter input (stated in Page 9 Line 28 to Page 10 Line 2). 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. The sampling points located between the trees, and the no obvious root biomass was observed in the soil cores under the chambers. Thus, the respiration of below-ground roots didn't make relevant contribution to the CO_2 flux measure. These have been clarified in the revised manuscript; please refer to Page 9 Lines 20-28.

Comment 8: Finally, it is not revealed how many measurements were conducted at each campaign or altogether. Measurements only once per three months at a certain time of the day/tide does sounds sparse and un-presentative. And how were the annual fluxes calculated? There is no mention on that. Response: The annual fluxes were calculated as the means of fluxes obtained from the four seasonal samplings. This was stated in the revised manuscript. The fluxes were calculated from limited measurements (once per three month at a certain time during the daytime) and the diurnal variation was not considered. Although tidal effect on gas flux was observed during the exposure in previous study (Chang and Yang, 2003), the results were inconsistent. The study showed that CH_4 fluxes in a K. obovate dominated wetland had significantly more emission after ebb tide than before flooding in August 1996; while in

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June 1997, the fluxes were similar between these two measurement campaigns, but the fluxes before flood were significantly higher than that after ebb tide in May and August 1998. Our preliminary study also showed that the temporal variation in gas flux was insignificant during the daytime (unpublished data). In addition, the fluxes might be similar between exposure and flooded periods (see the discussion in the first version of manuscript). Based on these considerations, the limitation of measurement number would not affect the finding of this study. This limitation and its impact have been discussed in the revised manuscript; please refer to Page 9 Lines 1-19.

Minor comments

Comment 1: In many places, expressions “gases fluxes” and “gases emissions” are used. Proper ones would be “gas fluxes” and “gas emissions”. Response: Accepted. In the revised manuscript, ‘gases’ was corrected to ‘gas’, and some ‘gas emissions’ were replaced by ‘gas fluxes’.

Comment 2: p. 2 r 32-33: “characterized, and few studies have considered the warming effect of the simultaneous emissions” Which studies? Response: According to our knowledge, there is no such quantitative study at this moment. To avoid the bias of such conclusion, we changed the sentences to “However, the greenhouse gas emissions from mangrove soils remain poorly characterized, and to what extend the gas emissions would offset the benefit of plant carbon sequestration is still unclear’ in the revised manuscript.

Comment 3: p. 3 r. 17-18: Also tell the length of the sampling area. In Results, means of the three sites are given in many places. How were the three sites selected? Is their mean representative of the area? Why give the mean values for three different sites? Response: The length of the three sampling area were added in the revised manuscript, please refer to Lines 139-141 in the revised manuscript. Some other information, such as the lengths of the mangrove sites and their widths were indicated in Fig. 1. As some mangrove dominated shores in Jiulong River Estuary were subjected

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to erosion, Spartina alterniflora invasion or garbage from upstream, we chose the three mangrove sites in good conditions so as to eliminate such exogenous impacts. The three sites locate at different areas (north-shore mangrove and island mangrove) in the Jiulong River Estuary, and cover both the rehabilitated and natural sites in this region. We therefore considered they are representative the situation of the Jiulong River Estuary. In this study, we meant to compare the fluxes and the roles of mangrove wetland in mitigating global warming. In the revised manuscript, their mean values were given but some discussions on their spatial variations were also added due to the significant variations.

Comment 4: p. 8 r, 30: "The contrarily spatial variation" What does this mean? Response: Sorry for the confusion. Here we meant that the variation pattern of CH_4 flux (in order of XG=CPT>HMI) in summer was contrary to that of porewater salinity (XG=CPT<HMI). In the revised manuscript, this has been revised to "Lower soil CH_4 flux and higher porewater salinity in HMI was also consistent with such inhibition effect".

Comment 5: Figs 2 and 3: What are the error bars in these figures? Response: the error bars in these figures mean the standard deviation. The figure legends were revised to indicate the data.

Comment 6: Based on the volume and area of the measurement chamber, and assuming a tube as in the referred articles, the height of the chamber would be only 5 cm. This doesn't make sense, especially when the chamber is inserted 3 cm to the soil. Volumes, areas and heights don't go together even in the referred articles. State clearly the dimensions and shape of the chamber. Response: Sorry for the confusion. The volume 1.25l was interior volume after the chamber was pushed into soil, and the height was 5-6 cm over the soil surface. The dimension and shape of the chamber were stated in the revised manuscript. In addition, the volume and the basal area of the chamber used by Corredor et al. (1999) were also stated in Page 6 Line 11.

Comment 7: How could the chamber pushed into the wet soil without creating a pressure disturbance? Response: The chambers used in this study have a hole at their top. When pushing the chambers into soils, these holes were left open to eliminate the pressure disturbance, and were closed using rubber stoppers for first gas collection. This has been stated in the Method section in the revised manuscript (Page 6 Lines 16-18).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-662/bg-2015-662-AC3-supplement.pdf

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