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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.

Anonymous Referee #3

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General comments

In the manuscript, net primary production (NPP) and annual soil-atmosphere fluxes of CO_2, N_2O and CH_4 from bare soil at three mangrove sites is estimated. Further, NPP and gas fluxes are converted to CO_2 equivalents to estimate the "mitigating effect of mangrove wetland on atmospheric warming". The effect of mangroves on climate is apparently poorly known but may be considerable. Thus, the topic is interesting and suitable for Biogeosciences. The manuscript is generally well written and comprehensible, with few language lapses. There are anyhow severe methodological problems with the measurement of gas fluxes. 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". 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.

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 CO_2 and it is not clearly interpreted what the result really indicate (what is the effect of NPP on climate).

Detailed essential comments

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.).

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. The soil CO_2 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 CO_2 back to the atmosphere? Is CO_2 lost with tides as DIC and released then back to the atmosphere? etc. (see for example Bouillon et al. 2008) It should be well discussed, what exactly is the effect of NPP on climate. NPP converted to CO_2 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.

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.

Also, the small chamber size (V/A) is a bad problem with CO2. For N2O and CH4, it and the long closure time are needed to capture the very small flux. But with CO2, the same chamber design leads to an extreme concentration increase during the closure (even thousands of ppm during the $\frac{1}{2}$ hour closure). This likely very badly underestimates the CO2 flux. Typically CO2 flux is measured with only a few minutes chamber closure by portable CO2 analyzers and even nonlinear regressions are applied to realistically estimate CO2 flux after chamber closure.

Further, it is not well justified, how the CO2 flux measurement from bare soil without pneumatophores equals heterotrophic respiration. Would not pneumatophores act as pathways for also CO2 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.

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 unpresentative. And how were the annual fluxes calculated? There is no mention on that.

Minor comments

In many places, expressions “gases fluxes” and “gases emissions” are used. Proper ones would be “gas fluxes” and “gas emissions”.

p. 2 r 32-33: “characterized, and few studies have considered the warming effect of the simultaneous emissions” Which studies?

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 where the three sites selected? Is their mean representative of the area? Why give the mean values for three different sites?

p. 8 r, 30: “The contrarily spatial variation” What does this mean?

Figs 2 and 3: What are the error bars in these figures?

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.

How could the chamber pushed into the wet soil without creating a pressure disturbance?

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