

Dear Editor,

Dear Authors,

It has been a pleasure to review the work by Clippele and colleagues on “Aquatic and Soil CO₂ Emissions from forested wetlands of Congo’s Cuvette Centrale”. I congratulate the authors on collecting a CO₂ flux dataset from the Congo Basin spanning three years at the interface between aquatic and terrestrial landscapes in tropical forests. Such rich datasets remain scarce in the tropics. Thus, this manuscript and emerging insights from the presented data shed new insights on the contribution of these inadvertently under-sampled regions in the Congo Basin to the overall tropical forest CO₂ budget. The manuscript should be of interest to the readership community of Biogeosciences. The manuscript is well written, and I recommend its consideration for publication after integrating some minor suggestions and clarifications from the authors.

Specific comments

LN 23, why do you think there were no discernible seasonal differences in CO₂ fluxes across the two sites? For instance, could it be that in seasonally flooded forests, the effect of flooding on soil moisture during the wet season persists through the dry season, keeping soil moisture content near optimal for microbial and plant activity? This probably could dampen the effect of seasonality on the measured CO₂ fluxes at both sites. You need to shed more light on the lack of seasonality effect on CO₂ fluxes since you already mentioned the positive correlation between measured CO₂ fluxes and moisture content at the seasonally flooded sites. Please shed more light on this.

LN 25, can you talk of water table depth when the site is flooded? Maybe ponding depth is more appropriate

LN 26-28. What do we take away from the progressive enrichment of pools with ¹³C? How about the implication of no significant differences in the enrichment between ¹³C in SOC and respired CO₂ pools?

LN 29-30. What is the extent of tropical wetlands in the Congo Basin? I think it would be valuable if you could somehow estimate the area coverage of these wetlands and what not measuring from these areas would mean for the total ecosystem CO₂ flux budget.

LN 40. “up to 64% of global soil respiration” sounds better.

LN 49. Please provide the exact amount of C emitted by Congo Basin inland waters compared to the Amazon instead of just saying “more C.”

LN 91-92, I would also add information on base saturation to the brackets since it is the premise on which soil is classified as either eutric or dystic. Could you also reference the soil classification system you used, whether it is the WRB 2014 or 2022, etc.? I am also curious to know why there is no mention of stagnant properties in the prefix or suffix qualifiers of the main soil reference group, yet the study area is seasonally flooded. What is the depth of the water table? Please indicate it as well.

LN 95-96, I am also curious to know why the sensors that logged soil-environmental parameters were installed at a depth of 30 cm instead of close to the surface where you measure the soil-atmospheric CO₂ flux.

LN 126 Could you also mention the depth of installation of the chamber bases/collars? Were the chambers left in place for the entire measurement period?

LN 135: Longer chamber closure times are not recommended. Pavelka et al. (2018) (doi: 10.1515/intag-2017-0045) recommended a maximum chamber closure time of 45 minutes. Could you add a few details in the methods explaining why you chose the maximum closure time of 60 minutes?

LN 137-138: How long were samples stored in exetainers before being shipped back to Europe for analysis? Some literature suggests that more extended storage periods of gas-filled exetainers before analysis could lead to sample contamination/degradation. Besides the overpressure, what else did you do to lower the risk of sample degeneration/degradation during storage in DRC and during shipping to Europe?

LN 181-182. Could you add the flush time?

LN 229 What did you do to the data if the model assumptions did not conform to normality and homoscedasticity prescriptions?

LN 235—As Reviewer #1 indicated, please update the figure numbers accordingly. There is a mismatch between the figure numbers and the in-text citation.

LN 252 You measured the fluxes every other fortnight, but you report mean weekly fluxes. This is confusing. Clarify

LN 260: Could you add vertical lines to the graph showing the beginning and end of the dry season over the three years? I am also slightly confused about how you present weekly fluxes when measurements were done fortnightly. Did you interpolate fluxes for the weeks you did not measure? Could you also add a short note in the figure caption about why some time series are discontinuous?

LN 286: "Individual relationships between soil CO₂ fluxes and the environmental parameters (soil temperature (A), volumetric soil water content (VWC) (B) and river level (C))". Measures taken while the soil chamber was partially flooded are represented in green. Regression lines are displayed in brown. These sentences sound like figure captions and not results. Please reword.

LN287: CO₂ and not CO₂

LN 295: Could you display the r-coefficient values for Figures 4 a-b. The positive correlation displayed in Figure 4 a looks very weak, at least visually, and it would be misleading to add a regression line to this figure if $r < 0.5$. Also, could you briefly talk about the strength of these correlations when first introducing them in the results section? You should indicate whether the r values are less or greater than 0.5 in the text description. Additionally, for Figures 4 c-d, add the quadratic equations.

LN 314. Add a full stop at the end of the line.

General comment on the results. I miss the discernment between the results that were statistically significant and those that were not. The Authors use phrases like slightly increased, strongly enriched, etcetera, but do not specify whether this was a statistically significant change. Could you add p-values in brackets where you describe changes in measured fluxes and environmental controls?

LN330, why are your fluxes on the lower end? Please substantiate.

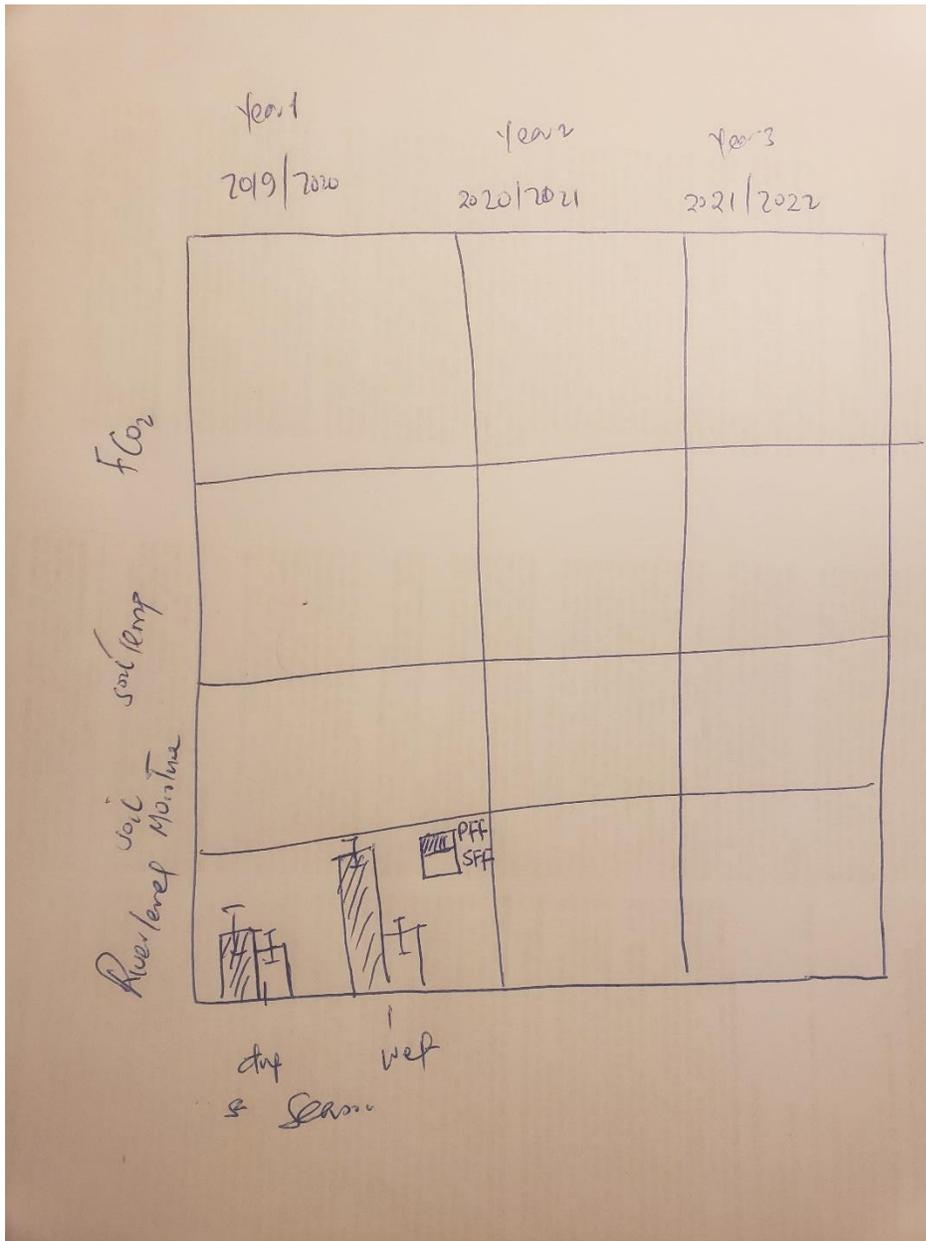
LN 332 Please reference the other flooded forests you are referring to here and give the magnitude of fluxes measured at those tropical flooded forest sites.

LN 336. You need a reference here.

LN 341 Please add a reference (s)

LN 325-355: Your arguments need elaboration and should be backed by literature.

General comment on the results. Please add a bar graph of the magnitude of the measured fluxes and all the controls showing the differences in these variables between the SFF and PFF sites across the wet and dry seasons. See suggestion below



LN 378. It is interesting to note the increasing evidence of the lack of seasonality in CO₂ fluxes. There have been a couple of studies in the region that have found similar results. It might be necessary to cite them here to back up your findings.

LN 397. There is a typo. The new sentence should read, "in other words"

LN 430-434, Be consistent in the way you abbreviate the author names Mbartel versus TWD