Anonymous Reviewer 2:

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.

Dear Reviewer, thank you for your valuable and positive comments and for taking the time to give detailed feedback. Please find below our clarifications and changes implemented following your inputs.

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.

This is a very important point mentioned by the reviewer. The combination of a lasting effect of the flooding, a relatively high-water table, and consistent rain events during the whole year could indeed lead to soil moisture contents remaining near optimal conditions for vegetation and soil microbes to thrive. Such stable environmental conditions may likely maintain autotrophic and heterotrophic respiration at a steady level despite undergoing a discernible dry and wet season cycle. However, we would like to acknowledge that our available dataset does not allow us to draw a definitive conclusion regarding this assumption.

In accordance with the present feedback as well as the one from Reviewer 1, this is explored in greater detail in the discussion of the paper and is now specifically mentioned in section 4.1.:

'The surface CO_2 flux dataset from the SFF site, measured for three consecutive years, showed intra-seasonal and interannual variability. However, no clear seasonal patterns were observed (Figure 1C). Baumgartner et al. (2020) showed a similar low seasonality in lowland forests of the Congo Basin, attributing it to the limited rainfall variation between dry and wet seasons. Similarly, the unclear seasonal pattern of the CO_2 fluxes at the SFF site could be attributed to the lasting effect of the flooding (Docherty and Thomas 2021) and/or consistent rain events during the whole year (Figure 1A-B). These factors, along with the brief duration of both dry seasons, may lead to soil moisture contents remaining near optimal conditions for vegetation and soil microbes to thrive. Such uniform environmental conditions may maintain autotrophic and heterotrophic respiration at a steady level despite undergoing a discernible dry and wet season cycle. '

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

We agree with the reviewer that the water table is a misleading term as it refers to the groundwater table. However, we also refrain from using ponding depth as we need to cover the range above-belowground.

For readability, we implemented the following approach:

- When referring to the water level at the SFF site (either above or below ground), we used the term 'water level'. We believe that it is a good term to cover the range of water height above and belowground.
- When referring specifically to the dataset of river gauge measurements or the level of the river (i.e. in section 3.2.1.), we used the term 'river level'.

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?

The absence of enrichment between SOC and respired CO_2 could be due either to a substrate limitation and closed system dynamics or to an important contribution of the autotrophic component of soil respiration (that would decrease the overall soil respired $\delta^{13}C$ value based on the relative contribution of autotrophic and heterotrophic component). However, the set-up of our study did not allow us to discriminate between both options.

This sentence was added to the abstract to further detail this:

'This lack of enrichment can be attributed to either a significant contribution from the autotrophic component of soil respiration or a result of closed system dynamics.'

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.

Bwangoy et al., (2010) estimated the wetland cover in the Congo Basin at 359,556 km^2 (36% of the total watershed) whereas Fatras et al., (2021) estimated a size of 332,620 km^2 with a mean flooded area of 89,408 km^2 in the Congo Basin. Furthermore, the Congo has been found to hold the largest peatland complex on Earth (167,600 km^2; Crezee et al. 2022), exemplifying the importance of the region for CO_2 emissions and the global carbon budget.

The second paragraph of the introduction now explicitly refers to the wetlands cover and their importance for global CO_2 emissions. It now reads as follows:

'Wetland cover in the tropical Congo Basin is estimated to range between 332,620 and 359,556 km^2 (Bwangoy et al. 2010; Fatras et al. 2021). This area includes the *Cuvette Centrale*, which spans approximately 167,600 km^2 and hosts lowland and swamp forests, including the largest peatland complex across the tropics (Crezee et al. 2022). With catchment drainage from north and south of the equator as well as sustained rainfall at the center of the basin (Runge 2007; Breitengroß 1972), the *Cuvette Centrale* shows near permanent inundation. Characterizing CO_2 fluxes in this extensive region is especially important since inland waters are increasingly recognized as significant sources of greenhouse gases (GHG) within the terrestrial landscape (Bastviken et al. 2011; Drake, Raymond, and Spencer 2018; Borges, Darchambeau, et al. 2015; Rosentreter et al. 2021) and notably in global carbon dioxide emissions (Raymond et al. 2013).'

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

The sentence has been modified as follows: Indeed, tropical regions are estimated to contribute up to 64% of global soil respiration,

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."

The authors of the referenced study (Alsdorf et al., 2016) based their assertion on a comparison between carbon emission estimates for the Amazon Basin (Melack, 2016) and those for the Congo Basin (Borges, 2015). They argue that while the estimates are similar (~480 Tg C/yr), the difference in basin size (6 vs. 3.6 million km^2) suggests that inland waters in the Congo Basin may emit more carbon per unit area than those in the Amazon Basin. However, the authors acknowledge that these comparisons are based on extrapolations from limited datasets, particularly for the Congo Basin, and should be considered as an indication of the potential importance of the Congo Basin regarding carbon dynamics rather than final estimates. Hence, we respectfully suggest refraining from including exact figures as these remain very speculative.

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 dystric. 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 stagnic 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.

Unfortunately, no water table measurements were available for the whole measurement period. However, a subsequent study at the same site measured a range of -74.5 to 113 cm from early November to March. Regarding the classification of the soil, we have no measures of base saturation, it is based on the EU Soil Atlas of Africa. Additionally, the classification is based on WRB 2014 and the qualifiers are not provided.

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.

Thank you for pointing this out. When setting up the field trial we installed a sensor profile at 5, 10, and 30cm from the surface to have a detailed measurement from the biologically most active layer. Unfortunately, this sensor setup was malfunctioning soon after installation due to the high humidity and termite activity. As the 5 cm sensor had missing data, the data from the other depth layers was used. These issues were resolved after using the TOMST sensors which integrate soil moisture measurements over 14cm topsoil and measure soil temperature at 8 cm depth.

We amended the text as follows:

'At the SFF site, combined soil moisture and temperature sensors (ECH$_2$O 5TM, Meter Group, Inc. USA) connected to loggers (Em50, Meter Group, Inc., USA) were installed at 10 and 30 cm depth, respectively. The data was recorded every 6 h. Unfortunately, one logger was stolen and the other logger stopped working during deployment; thus, data is only available from November 2019 to July 2020 (Figure 1). Afterward, TMS-4 dataloggers (TOMST, Czechia) were installed in December 2020 to record surface volumetric soil water content (0-14cm) and soil temperature at 8 cm depth in 15-minute intervals.'

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?

The chamber bases were installed about 5 cm deep and left in place for the entire measurement period with the chambers remaining open except during sampling. This information has been added to the method section. It reads now:

'A total of six polyvinylchloride soil flux chambers (height = 0.3m, diameter = 0.3m) were installed in November 2019 at the SFF site. The chamber bases were inserted approximately 5 cm into the ground and remained in place throughout the measurement period, with the chambers left open except during sampling.'

LN 135: Longer chamber closure times are not recommended. Pavelka et al. (2018) (doi: 10.1515/intag2017-0045) 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?

Thank you for pointing us to this nice publication. We decided to have such a long chamber closure time b/c we were also interested in measuring the isotopic composition of CO_2 released from the surface. Thus, to reach a good signal-to-noise ratio on our IRMS with this chamber size we opted for a longer closure time. We are aware that longer closure times lead to unwanted saturation effects but the near-perfect linear regressions of the concentration vs. time correlations gave us confidence that this was of minor importance. We added the following line to the methods:

"A longer chamber closure time than recommended (Pavelka et al., 2018) was used to obtain robust Keeling plots along with the flux measurements."

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?

For logistical reasons, it was not always possible to regularly ship the samples back to Europe due to the remote location of the sampling site. The time between collecting and analyzing was often several months. Besides storing the samples at overpressure and in a dark cool place an additional layer of silicone was applied on each vial septum before evacuation. This additional silicone layer helped preserve the vacuum as well as the sample. Given the high quality of our coefficient of determination of the linear relationship between the concentration increase over time we are confident that minimal sample degradation occurred. If that was the case more erratic concentration increases would have been observed frequently. We added the following line to the method section:

'To aid vacuum and sample preservation, each evacuated vial was sealed with an additional silicone layer (Dow Corning 734, Dow Silicones Corporation, USA).'

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

The flushing time is dependent on the gas flow rate. As a rule of thumb, we flushed each vial with at least ten times its own volume of gas. With our standard operational procedure, we usually N_2 flush 30 vials (12ml) at a flow rate of 5 L/min for about 3 mins which is sufficiently long to replace residual air in the vial with N_2. Flush quality was frequently checked using gas chromatography.

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

The model presented in the manuscript conformed to the assumptions; hence no transformation (except standardization on the predictors) was applied to the data.

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.

Mismatched figure numbers and references in the text have been corrected. Our apologies for this issue with updating properly the references.

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

Thank you for highlighting the confusion regarding the data displayed. To ensure clarity, we have addressed your comments on LN 252 and LN 260 together, providing our response below the comment on LN 260.

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?

For better contextualization, vertical bands were added to the graph to represent the typical flooding period (Jan-Dec). We focus on the flooding period (Dec-Jan) rather than the wet/dry season. Defining a wet/dry season is difficult as there are frequent rain events throughout the year (see Figure 3). However, we added in the text which months are considered to be the dry and wet seasons as follows:

'The long dry season in Mbandaka is considered from July to August whereas the short dry season spans between January and February. However, frequent rainfall as shown in Figure 3 renders the region as relatively wet throughout the entire year. Annual precipitation was the highest in 2020 with 1855 mm and lowest in 2022 with 1417 mm (self-measured; Figure 1). The flooding period at the study site is typically centered around December and January.'

Thank you for pointing out the confusion regarding the data displayed. It is currently poorly described. We decided to display a weekly over a fortnightly resolution to maintain a higher resolution of the environmental parameters and to better streamline the data between the two sites which were not always measured during the same week. Moreover, for some periods, more intensive measurements were available (daily, weekly). All CO_2 flux data points (cross) presented in Figure 3C correspond to individual fluxes measured, while only the line displayed corresponds to the weekly means linearly connected. To clarify this, as well as the discontinuity, the legend has been modified as follows:

Figure 1. Weekly precipitation, volumetric soil water content, temperature, and CO_2 fluxes. (A) The sum of the weekly precipitation [mm] (blue) obtained from the Trans-African Hydro-Meteorological Monitoring Observatory and mean volumetric soil water content [m^3 m^{-3}] measured with soil moisture sensors (ECH₂O 5TM = solid line, TMS-4 dataloggers = dotted line). (B) Distribution of volumetric soil water content [m^3 m^{-3}], both sensor types combined. (C) Mean weekly air temperature [°C] (gold) was obtained from the Trans-African Hydro-Meteorological Monitoring Observatory. The mean weekly soil temperature [°C] was measured with soil temperature sensors (ECH₂O 5TM = grey solid line, TMS-4 dataloggers = grey dotted line). (D) Distribution of air and soil temperatures [°C], both sensor types combined. (E) Measured surface CO_2 fluxes (cross) [μmol m^{-2} s^{-1}] from the SSF site (brown) and calculated CO_2 fluxes from the PFF site with a K of 3.5 cm h^{-1}(pink). Calculated weekly means (line) and the standard error of the mean are displayed. Blue shading represents river levels (see Section 2.6), while grey bands indicate flooding periods (Dec-Jan) at the SFF site. The displayed time series are discontinuous due to fieldwork constraints (see Section 2.2.) (F) Distribution of surface CO_2 fluxes at the PFF and SFF sites.

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.

Indeed, our apologies for this mistake. These sentences are the figure caption that had been added by mistake with the Figure cross-reference. This was corrected: the caption has been removed from the main text.

LN287: CO_2 and not CO2

The format of CO_2 has been corrected.

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.

Thank you for pointing out this lack of clarity. Figures 4a-d intend to illustrate the relationship between the different explaining variables and the dependent variable of the LMER model presented in Table 1. In that sense, the R^2 value to consider is the one related to the complete LMER model (0.354/0.430). However, we agree with the reviewer that this might be misleading. To address this issue, we have modified the plots, so that the line appears as a suggested grey dashed line, clarified the figure caption as written below, and added the inclusive R^2 on each plot. The inclusive R^2 provides a measure of the proportion of the variance explained by each predictor, including both its direct effects and its interactions with other predictors, whereas part R^2 quantifies the unique contribution of each predictor, considering only its direct effects (i.e., without interactions; Stoffel et al., 2021). We suggest this approach as it can provide the reader with a clear way to understand the overall contribution of each predictor and its relationship with the dependent variable. Finally, the method section was modified accordingly to explain how the inclusive R^2 was calculated (R package partR2)

The method section for the statistical analyses now reads as follows:

'Marginal and conditional R^2 values for mixed effects were calculated using Nakagawa et al., (2017), inclusive R^2 estimated with *partR2* package (Stoffel et al., 2021) and p-values using Satterthwaite's approximation with the *lmerTest* package (Kuznetsova et al., 2017).'

The line 286 was also modified to clarify the link between Table 1 and Figure 4, as follows:

'For a more complete understanding of the LMER model (Table 1), the individual relationships between surface fluxes and the different predictors (soil temperature, river level, and soil moisture) as well as the effect of the interaction between the soil temperature and rivel level are visualized in Figure 2. The inclusive R^2 (IR^2) of each predictor is also presented, offering a measure of the proportion of variance explained by each predictor, including both its direct effects and interactions with other predictors (Stoffel et al., 2021). In this context, the soil temperature (IR^2 = 0.225), soil moisture (IR^2 = 0.126), and the quadratic component of the river level (IR^2 = 0.097) appear as the primary factors explaining the variance in surface CO_2 fluxes, whereas the interaction between soil temperature and river level (IR^2 < 0.001), along with the linear component of the river level (IR^2 = 0.001), make no meaningful contribution (Figure 2).'

The figure caption was modified as follows:

Figure 2. Individual relationships between soil CO₂ fluxes and the environmental parameters (volumetric soil water content (VWC) (A), soil temperature (B), and river level (C)). Measures taken while the soil chamber was partially flooded are represented in green. Regression lines are displayed as dashed black lines. The interaction between soil temperature and river level is illustrated. Values were predicted based on the LMER model (Table 1) (D). Inclusive R² (A, B, C) was estimated based on the LMER model (Error! Reference source not found.**; Stoffel et al., 2021).**

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?

We added a full stop at the end of the line. Linear models were tested between the aquatic fluxes (PFF) and explaining variables (pH. C:N ratio, river level etc.). None of the linear relationships tested were statistically significant. The dataset, however, was small (n = 28) with high variability in the response. This has been clarified in the text by explicitly mentioning that the relationship was not statistically significant and/or adding the p-value in brackets if applicable. Furthermore, on second thought and based on the reviewers' feedback, we decided to remove Figure 5 from the manuscript as it does not add much value.

The corresponding result section (line 299) has been modified as follows:

'At the PFF site, surface CO₂ fluxes did not exhibit statistically significant relationships with pH, river level, carbon to nitrogen ratio (C:N), dissolved organic carbon, or biodegradable dissolved organic carbon. Trends were observed, such as an increase in CO₂ fluxes with rising river levels and opposing trends with pH, CO₂ fluxes appeared to decrease as pH increased. However, these are just visual tendencies and not statistically significant.'

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

This is indeed a very interesting question. Our data falls within the lower range measured across the pan-tropical realm which could have been caused by site-specific characteristics such as local climate, soil type, and/or vegetation. It is challenging to offer a definitive explanation. Among the literature, only Baumgartner et al., (2020) and Daelman et al., (2025) measured CO2 fluxes in the Congo Basin. When comparing the lower fluxes observed at our SFF sites with those reported by Baumgartner et al. (2020) in the Congo Basin lowland forests, one plausible explanation is that the flooding period at the SFF site significantly influences the annual mean flux.

To further discuss our results and shed light on the distinctions between sites, we have focused our comparison on the PFF and SFF sites—using other studies primarily to provide context. The following text was added in section 4.1. to compare both sites:

' The elevated CO₂ fluxes at the PFF site resulted in higher fluxes relative to the SFF site. Further research is needed to determine whether a greater water depth-integrated respiration (Amaral et al. 2020), a positive correlation with a larger inundated area (Amaral et al. 2020), prolonged river interactions or other factors could explain such difference. In contrast, the SFF site presented reduced CO₂ fluxes during the onset of flooding, speculatively due to the inhibitory effect of excessive soil moisture on soil respiration (Courtois et al. 2018; Nissan et al. 2023).'

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.

The sentence was modified as follows:

'The perennially flooded forest site (PFF), located at the interface between terrestrial (forest) and aquatic (stream) ecosystems showed relatively high emissions (4.38 ± 0.64 µmol m-2 s-1) when compared to other tropical flooded forests (Scofield et al. 2016;Table 2) or those streams draining catchments dominated by seasonally or continually inundated swamp forests (Mann et al., 2014; Alin et al. 2011; Table 2).'

LN 336. You need a reference here.

In response to Reviewer 1's comments, this paragraph was modified as follows:

'A non-significant positive trend between water level and the aquatic CO_2 fluxes was visually discernible which is in line with a positive relationship between pCO_2 and discharge measured on the adjacent Ruki (Drake et al., 2023). As a constant gas transfer velocity was used in the present study, short-term changes in aquatic CO_2 fluxes reflect the variations in carbon dioxide concentrations (pCO_2) in the water.'

The statement regarding the direct relationship between aquatic CO_2 fluxes and dissolved pCO_2 is based on the calculation method of this paper and not on an external study. That is, the use of a constant gas transfer velocity in the study to calculate F_{CO_2} implies that variations in CO_2 fluxes are directly linked to variations in pCO_2.
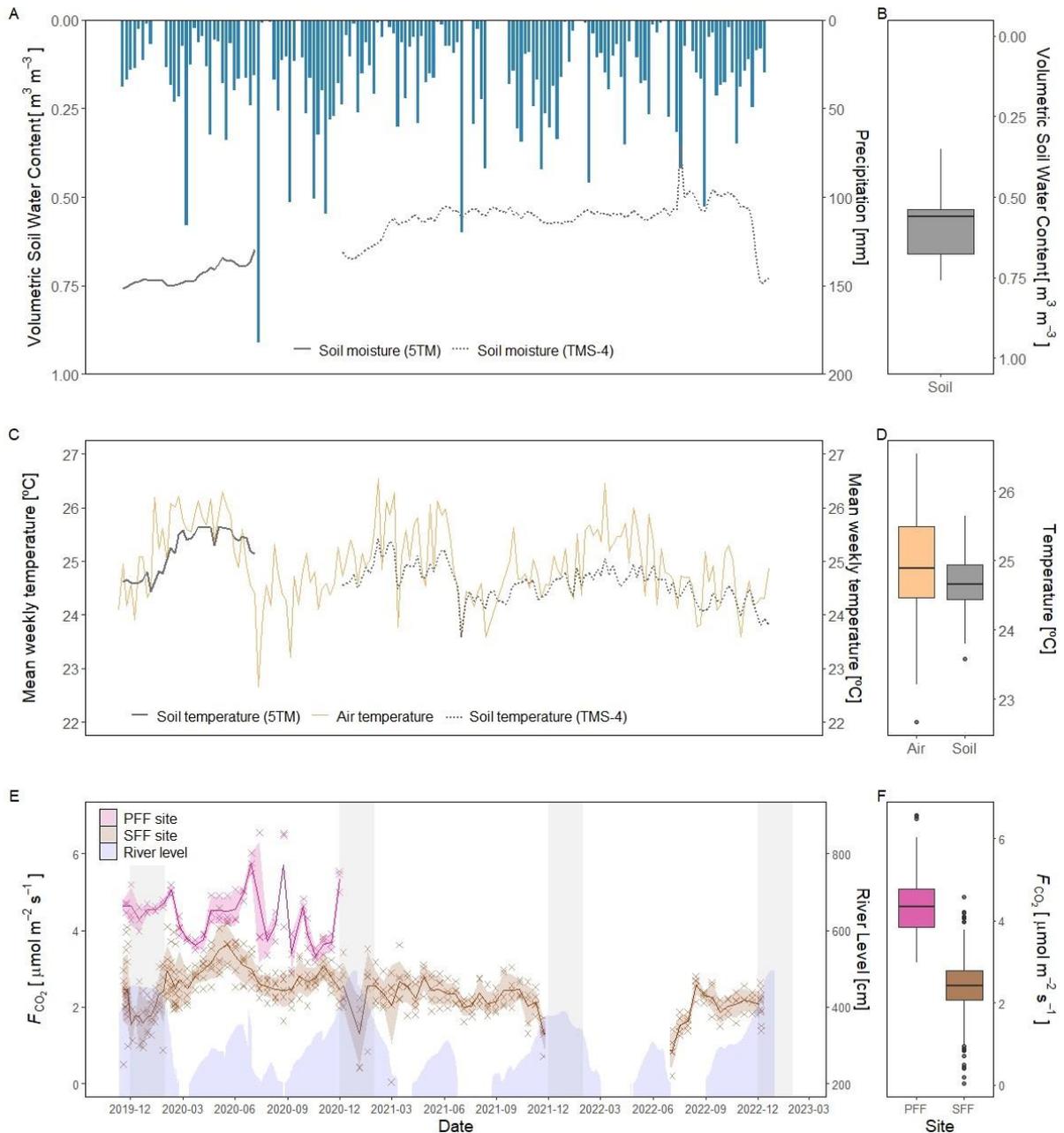
LN 341 Please add a reference (s)

The text was amended as follows:

'Generally, the pCO_2 concentration itself is driven by factors such as terrestrial inputs, gas exchange with the atmosphere, water temperature (gas solubility), water chemistry (pH, alkalinity), and in-stream metabolism (Rocher-Ros et al. 2019; Hotchkiss et al. 2015; Battin et al. 2023).'

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

Section 4.1. was further elaborated and referenced thanks to the previous comments and the feedback from Reviewer 1. The main text additions were written in response to the comments on LN 331 and LN 23. Thank you for detailing your explanations with a drawing. Based on your comment, Figure 3 has been revised to include boxplots alongside each graph (A, B, C). We suggest this approach to keep the different graphs in the same panel, allowing direct comparison of the time series and the fluxes distribution. These boxplots illustrate the distribution and magnitude of the measured fluxes at the SFF and PFF sites, as well as key environmental variables such as precipitation, soil moisture, soil temperature, and air temperature. However, we recommend avoiding the distinction between dry and wet seasons, as frequent rainfall throughout the year makes it challenging to define clear seasonal boundaries and rather suggest to indicate on the plot the typical flooding periods (see Figure 3).

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.

Thank you for pointing this out. We added references to Baumgartner et al. (2020) whose study showed weak seasonality in CO_2 fluxes in the lowlands forests of the Congo Basin.

The start of the section 4.1 now reads as follows:

'The surface CO_2 flux dataset from the SFF site, measured for three consecutive years, showed intra-seasonal and interannual variability. However, no clear seasonal patterns were observed (Figure 1C). Baumgartner et al. (2020) showed a similar low seasonality in lowland forests of the Congo Basin, attributing it to the limited rainfall variation between dry and wet seasons.'

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

The typo has been corrected.

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

As co-authors Marijn Bauters and Matti Barthel shared the same initials, it was decided to add a complete name in their case but stay concise and keep the abbreviations for the rest of the authors.