

Anonymous reviewer 1:

The manuscript presents valuable data on forested wetlands in the Congo, addressing a significant gap in current knowledge and demonstrating how different flooding regimes impact surface CO₂ fluxes. I particularly appreciate the detailed descriptions in the Methods section. However, I believe the Results section would benefit from a more thorough discussion, and the Discussion section could be further strengthened. Additionally, there are several formatting issues, such as mismatches between in-text figure numbers and the actual figures, as well as inconsistent use of subscripts. Below are my detailed comments.

Dear Reviewer, thank you for your time and positive, valuable as well as detailed feedback on our manuscript. Please find below how each of your comments has been integrated into the new version of the manuscript.

Line 30: I'd suggest using the full term for pCO₂ before using the acronym.

The sentence has been changed as follows: due to very high partial pressure of CO₂ (pCO₂) values measured in the flooding waters.

Line 55-57: The authors mention that 'probably only one study' has looked into GHG emissions from Congo's forested wetlands, specifically methane (Tathy et al. 1992). However, it seems there is another study that also investigates GHG emissions, including both methane (CH₄) and nitrous oxide (N₂O), from forested wetland soils in the Congo Basin (Barthel et al., 2022, Nature Communications).

Thank you for pointing this out. The sentence has been modified:

'Despite its immense global importance, only three studies, to the best of our knowledge, have been looking into GHG emissions from Congo's wetlands (Tathy et al. 1992; Barthel et al. 2022; Daelman et al. 2025).

Line 71-73: I suggest moving the description of measurement variables to the methods section. This would help streamline the Introduction and ensure methodological details are presented in the appropriate context.

The description of the measurement variables has been removed from that paragraph and explained in detail in the methods section.

Line 94: The sentence suggests that the forest is flooded because of its proximity to the river, which may not fully capture the cause of the seasonal flooding. It would be more accurate to say that the forest is flooded due to increased river flow during the rainy season or similar hydrological events. I recommend rephrasing the sentence to reflect the influence of the rainy season on the river's water levels, which causes the flooding.

Indeed, the sentence does not consider the different possible causes of the seasonal flooding. As the different origins of the flooding water (rainfed or riverfed) are already discussed in section 2.6 Water level, we suggest simplifying the sentence as follows :

'The SFF site is seasonally flooded from about December to January (~ 2 months).'

Line 99: It seems that the sentence doesn't correspond to the content of Figure 2.

Thank you for pointing this out. The reference has been corrected to Figure 3. Our apologies for the issues with the referencing of the figures. There was a last-minute change that led to this mix-up of figure assignments.

Line 116-118: I'd suggest clarifying whether the reference to Drake et al. (2023) is meant to support the statement about the previous analyses showing TDN consistently comprising 90% of DON, or if it only pertains to the specific methods and calculations. It might be helpful to provide a reference for the previous analyses as well to avoid confusion.

We modified the sentence as follows to overcome confusion:

'Previous analyses (Drake et al. 2023) showed that TDN consistently comprised an average of 90% of DON and thus reflected well the relative changes of DON concentrations. The specific methods used for sample processing and analysis as well as the calculations are described in Drake et al., (2023).'

Line 126: Please clarify the abbreviations "h" and "ø" for better readability. It would be helpful to define these terms for readers who may not be familiar with them.

Modified as follows:

'A total of six polyvinylchloride soil flux chambers (height = 0.3m, diameter = 0.3m) were installed in November 2019'

Line 135: Please use the full term for "h" here.

Modified as follows: 'were taken at timesteps of 20 min throughout 1 hour.'

Line 144-145: Could you please clarify why data with such a low r^2 threshold were retained, despite the strong regression fit observed in most of the data?

Most fluxes with a low r^2 are related to low flux rates and not because of methodological or technical issues. Eliminating cases with low r^2 would bias the data to high fluxes. Therefore, we retained also fluxes with low r^2 values.

The following sentence was added to clarify this for the readers :

'Such a low r^2 threshold was maintained because fluxes with low r^2 values are typically related to low flux rates rather than due to methodological or technical issues. Increasing the threshold would introduce a bias toward higher fluxes in the data.'

Line 166: Remove the space before the colon.

Corrected.

Line 172: Please add a space between "50" and "µL" for consistency with units.

Corrected.

Line 179: The abbreviation "GC" should be defined earlier as "gas chromatograph" in line 177.

Corrected as follows: 'Gas samples were analyzed at ETH Zurich using a gas chromatograph (GC, Bruker, 456-GC, Scion Instruments, Livingston, UK)'

Line 184: Remove the space after the dash.

Corrected

Line 189: Please use the full term "V-PDB" when first mentioned.

The sentence was corrected as follows: 'Post-run off-line calculation and drift correction for assigning the final $\delta^{13}\text{C}$ values on the Vienna Pee Dee Belemnite (V-PDB) scale was done following the 'IT principle' (R. A. Werner and Brand 2001).

Line 206-212: Water table already implies the level of water beneath the ground so "level" is redundant. Since you seem to be referring to the river, "water level" may be a more appropriate term.

This paragraph was revised: 1) The redundancy of the term "level" when referring to the water table was removed. In accordance with the feedback of Reviewer #2, to avoid confusion, the term "water level" rather than "water table" will be consistently used in the manuscript 2) The level of the river is referred to as "river level".

Line 208: Could you clarify the distance between the Congo River and the study site, or explain how it is hydrologically connected to the Ruki River?

The Ruki river at the point of sampling is directly hydrologically connected to the Congo River with a distance of approximately 4000m from the Ruki River gauge to the confluence with the Congo River. This is exemplified by the direct relationship between Congo River and Ruki River water levels (Supplementary Fig. 3). The distance from the SFF study site itself to the Ruki River is 755m.

The following sentence was modified to clarify this :

'This data was extracted from an almost continuous record of water gauge readings, collected in vicinity of the SFF site (~ 4 km) by the Congolese public institution, *Régie des Voies Fluviales*, since 1913.'

Line 210: DRC is not defined earlier in the text.

Corrected as follows: '...water levels of riverine locations in the Democratic Republic of the Congo (DRC) correlate...'

Line 215: I'd suggest changing the heading from "Statistics" to "Statistical Analyses" for better clarity and accuracy.

Changed as suggested.

Line 218: "Added" might be a better word than "tested" as the quadratic term was included in the model to account for the non-linear effect.

Corrected as follows: 'Hence, a quadratic term was added to account for the non-linear effect.'

Line 232: Remove the space after 'tydr v1.3.0' and before the comma.

Space removed.

Line 236-248: For this section, I'd suggest first describing the general climate characteristics at the study site, such as the timing of the dry and wet seasons, to better contextualize the variables. Additionally, I recommend incorporating the water level into this section, potentially alongside Figure 3. Although you mentioned that in-situ measurements were not available and the Congo River water level was used as a proxy, you also noted in Section 2.6 that riverine water levels are more influenced by river hydrological dynamics than rainfall. Therefore, you might consider using a more general heading, like "Environmental Conditions". For line 238, should it refer to Figure 3A instead of 2A? Lastly, in lines 247-248, please combine the sentences for better continuity instead of starting a new line.

The references were corrected to Figure 3. The sentences were combined in lines 247-248. The section heading has been changed to “Environmental Conditions”.

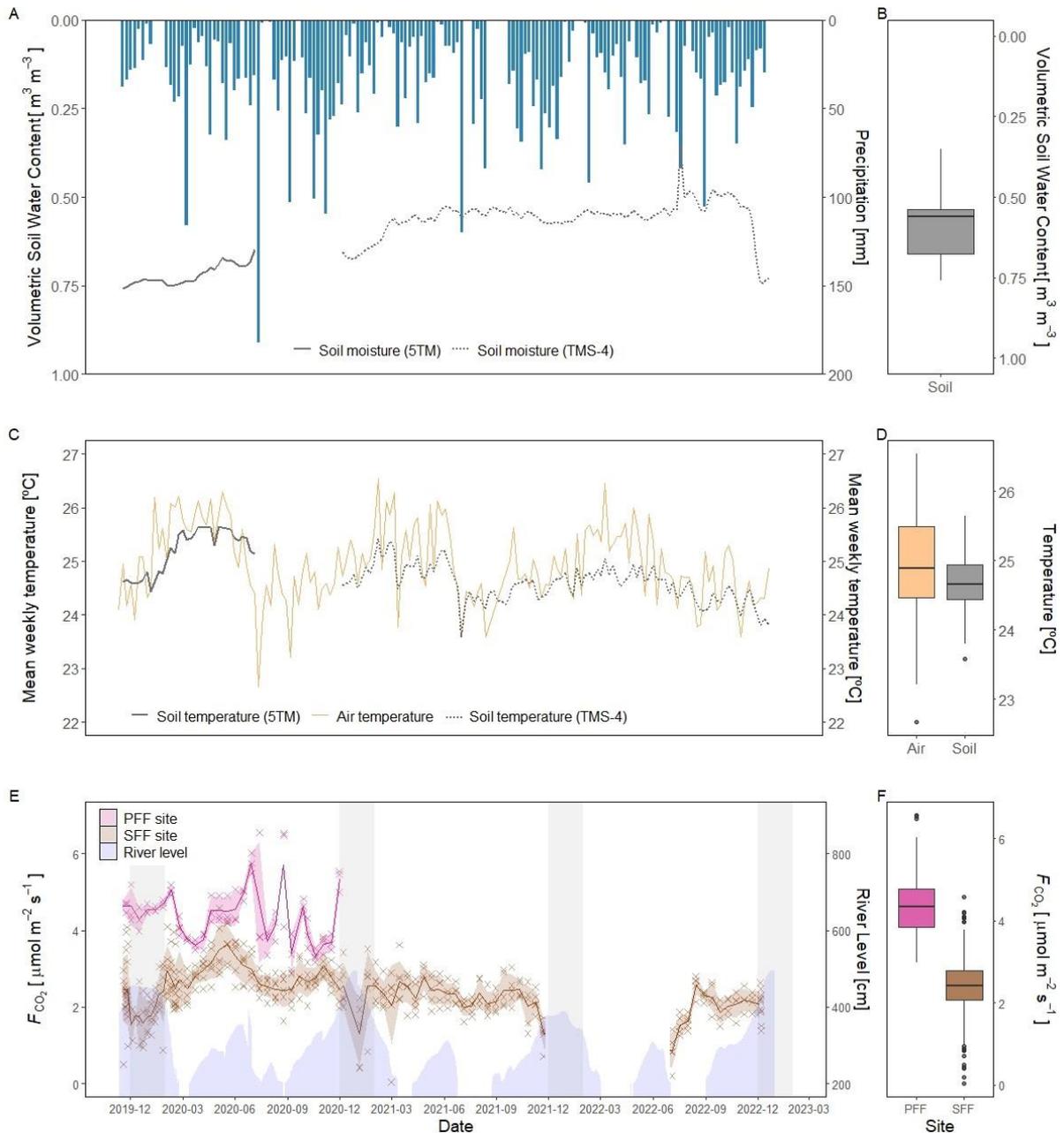
The first sentences (line 236) were modified to contextualize the information. We focus on the flooding period (Dec-Jan) rather than the wet/dry season. Frequent rain events (see Figure 3) render the region as relatively wet throughout the year. We mention now in the text which months are considered 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 3 The flooding period at the study site is typically centered around December and January. ‘

Finally, Figure 3E (numbering in the updated version, previously 3C) was updated to include the river level as suggested. After further modifications of the figure to integrate the feedback from Reviewer 2, the caption now reads:

Figure 1. Weekly precipitation, volumetric soil water content, temperature, and CO₂ 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 [m³ m⁻³] measured with soil moisture sensors (ECH₂O 5TM = solid line, TMS-4 dataloggers = dotted line). (B) Distribution of volumetric soil water content [m³ 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 CO₂ fluxes (cross) [μmol m⁻² s⁻¹] from the SSF site (brown) and calculated CO₂ 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 CO₂ fluxes at the PFF and SFF sites.

And here is the updated figure 3 :



Line 249-256: This section presents CO₂ flux data from two sites (PFF and SFF), showing variations over time. However, it doesn't explicitly explain the drivers behind these fluctuations. It would be useful to explore how these CO₂ flux variations correlate with specific environmental variables (e.g., temperature, precipitation, soil moisture, water level) or if they follow consistent seasonal patterns.

The text was modified to describe more explicitly the variation of the CO₂ flux and acknowledge the significant correlations with environmental variables. For readability, we suggest exploring further the correlation between drivers and flux fluctuations in the following results section 3.2.1. This section has also been revised to not only describe the signs and relative importance of the main environmental drivers but also to examine the relative contributions of each variable in explaining flux variations.

The start of the section (LN 249) now reads:

‘Over the observation period, CO₂ fluxes from the PFF site were higher than from the SFF site (Figure 3E). At both sites, CO₂ fluxes exhibited intra-annual variability. However, distinct seasonal patterns were not clear. Notably, at the SFF site, the onset of flooding appeared to induce a decline in fluxes. Furthermore, among the environmental variables, CO₂ fluxes exhibited significant correlations with soil moisture, soil temperature, and river level (‘Table 1).’

And the section 3.2.1. has been completed with:

‘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 river level are visualized in Figure 3. The inclusive R² (IR²) 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, soil temperature (IR² = 0.225) soil moisture (IR² = 0.126), and the quadratic component of the river level (IR² = 0.097) appear as the primary factors explaining the variance of surface CO₂ fluxes, whereas the interaction between soil temperature and river level (IR² < 0.001), along with the linear component of the river level (IR² = 0.001), make no meaningful contribution (Figure 3).’

To account for the estimation of the inclusive R², the statistical methods were also completed as follows :

‘Marginal and conditional R² values for mixed effects were calculated using Nakagawa et al., (2017), inclusive R² estimated with *partR2* package (Stoffel et al., 2021) and p-values using Satterthwaite’s approximation with the *lmerTest* package (Kuznetsova et al., 2017).’

Line 250, 254 and 256: Same comment as Line 238—should it be Figure 3C instead of 2C?

Thank you for pointing this out, the references were corrected to Figure 3.

Line 260: Figure 3 presents important data, but the current color scheme and legend structure could be improved for better clarity:

- Subplot A: The colours for the three lines (precipitation and two soil moisture sensors) are too similar, making it difficult to distinguish between them.
- Subplot B: The two lines representing soil temperature from different sensors (ECH2O 5TM and TMS-4 dataloggers) also have very similar colours, which may confuse readers.
- To enhance readability, it would be beneficial to include legends for each subplot.

As shown earlier, Figure 3 has been modified: colours and style are now more distinct and harmonized between subplots, and legends are included for each subplot. In response to the comment on Lines 236-248, the water level was also integrated into the figure.

Line 279: Are the fixed effect estimates presented in Table 1 standardized or are they unstandardized? Clarifying this would help in interpreting the relative importance of the predictors and their effects on soil CO₂ fluxes.

The estimates presented are standardized. The legend of the table was modified as follows:

‘Table 1. Fixed effect estimates for surface CO₂ fluxes at SFF site including river level, soil temperature, and soil moisture as standardized predictors, allowing comparison of their relative importance. For each effect, standard error

and p-values (Satterthwaite's method) are estimated, as well as the marginal (m) and conditional (c) R^2_{adj} (Nakagawa, Johnson, and Schielzeth 2017).

Line 287-290: The text appears to be the caption for Figure 4, please remove it from the text.

The legend text was removed from the main text.

Line 294-295: There appears to be a mismatch between the subplot labels (A and B) in the Figure 4 caption and the content. Also, please use the correct subscript for "CO₂".

Mismatched labels and subscripts were corrected.

Line 309: Figure 3 doesn't appear to show $\delta^{13}C$ values.

The reference was corrected to Figure 5.

Line 325-334: The authors compare CO₂ fluxes from both seasonal and perennial flooded forest sites with those from other reported sites. However, the reasons for the higher emissions at the PFF compared to the SFF are unclear. It would be beneficial to discuss this, as it could provide further insight into the differences between the two forest types.

This is a good point raised by the Reviewer. In order to discuss it, this section now reads as follows:

‘The perennially flooded forest site (PFF), located at the interface between terrestrial (forest) and aquatic (stream) ecosystems showed relatively high emissions ($4.38 \pm 0.64 \mu\text{mol m}^{-2} \text{s}^{-1}$) 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). 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 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).’

Line 340-341: It would be helpful to provide references for these factors influencing $p\text{CO}_2$ concentrations.

References were added as follows:

‘Generally, the $p\text{CO}_2$ 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).’

Line 346: I did not see the plot of water level and $p\text{CO}_2$ in Figure 5. Could you please clarify if this plot is included or if it is shown in a different figure?

We apologize for this confusion. The statement refers to the plot depicting CO₂ fluxes and water levels (Figure 5A), and by extension, to $p\text{CO}_2$ values. The use of a constant gas transfer velocity to calculate F_{CO_2} in the study implies that variations in CO₂ fluxes correspond to variations in $p\text{CO}_2$.

The sentence was revised for clarity and moved to line 337 to improve readability and text logic:

‘A non-significant positive trend between water level and the aquatic CO₂ fluxes was visually discernible which is in line with a positive relationship between $p\text{CO}_2$ 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 CO₂ fluxes reflect the variations in carbon dioxide concentrations (*p*CO₂) in the water. Moreover, the generally low gas transfer velocity (3.5 cm h⁻¹) reflects further the very high *p*CO₂ concentrations (10197 – 17260 ppm) measured at the PFF site.’

Line 350: Please use a subscript for “CO₂” in the table caption.

Corrected.

Line 379: Supp. Fig. 6 does not show seasonal CO₂ fluxes.

Corrected to Supp. Fig. 5B

Line 379-380: The authors suggest that factors like aboveground inputs, deposition, and rain-induced events may influence soil CO₂ fluxes. While a linear relationship between the Congo and Ruki river levels is noted, and previous research indicates that water levels in the Cuvette Centrale correlate more with river dynamics than rainfall, there are still uncertainties to consider:

- The study site is approximately 1 km from the Ruki River, and local topography and soil characteristics could affect how well river levels represent the actual water table at the forest site.
- Short-term variations in soil CO₂ fluxes may not be fully captured by this proxy, as local hydrological dynamics might differ from those of the broader river system.
- The lack of correlation between water table changes and CO₂ fluxes may reflect limitations of the proxy measurement rather than a true absence of a relationship.

While you made a reasonable methodological choice using available data, it might be possible to acknowledge that the study site's water level could differ from the river proxy. This limitation should be considered when interpreting the results, especially regarding the influence of water table dynamics on soil CO₂ fluxes.

Thank you for pointing out this methodological limitation. The mentioned limitations were more explicitly acknowledged as follows:

‘Nevertheless, it is important to note that both the water level and soil moisture measurements exhibit seasonal patterns but do not capture well the short-term changes of the surface CO₂ fluxes at SFF site. Furthermore, the CO₂ fluxes exhibit unclear seasonal pattern (Supp. Fig. 5B). This suggests that other factors, such as aboveground inputs from vegetation, river sediment deposition, and rain-induced events, may significantly influence surface CO₂ fluxes, both in the short term and at seasonal timescales. Additionally, it is important to stress that using river level as a proxy for water level at the SFF site presents limitations such as neglecting local topography or soil characteristics. Thus, fortnightly variations in surface CO₂ fluxes may not be fully captured by this proxy, as local hydrological dynamics might differ from those of the broader river system. Hence, this method may not fully capture the dynamics of the water level and its influence on surface CO₂ fluxes.’