Title: *Drivers and CO_2 flux budgets in a Sahelian Faidherbia albida agro-silvo-pastoral parkland: Insights from continuous high-frequency soil chamber measurements and Eddy Covariance*.

Author (s): Seydina Mohamad Ba et al.

MS type: Original research article

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Dear Dr Jim Boonman,

We were delighted to receive your detailed and highly constructive comments on our manuscript entitled "Drivers and CO_2 flux budgets in a Sahelian Faidherbia albida agro-silvo-pastoral parkland: Insights from continuous high-frequency soil chamber measurements and eddy covariance".

We sincerely thank you for agreeing to review our work and for the considerable time and effort you have invested in it. We have carefully considered every one of your suggestions and have implemented the recommended changes, which have substantially improved the clarity and overall quality of the paper.
Please note that the line numbers mentioned in the responses are provided on a provisional basis, as further corrections and revisions of the manuscript at the next step may result in changes to the line numbering. Consequently, this will be taken into account in the final revised version.

Please find below our point-by-point responses to your comments.
With best regards,

On behalf of all the co-authors,

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Response guidelines:
The comments from Reviewer RC2 are reproduced in *black and in italic*. The authors' responses are provided in black and in regular (non-italic) text. The revised text or lines added/changed lines from the manuscript are shown in **blue and in bold**.

Authors' response to reviewer RC2

Reviewer's report:

The research of Ba et al. focuses on CO_2 flux dynamics of an increasingly popular agro-forestry land use in Africa, featuring Faidherbia albida trees, groundnut plants and livestock grazing. All chapters are neatly written which gives the reader a full and clear picture of the research that has been done and the results that were collected. Overall, the research seems to be conducted well and features interesting findings. Most of all, the authors have quantified GPP and Rh of various ecosystem elements and showed how these elements fit within the bigger picture of the complete ecosystem. This increases our general understanding of these systems, which is needed to enable improvements of land-use in the longer term. Moreover, the results that are presented can be of high value for ecosystem and/or climate models as the (Sahel) region seems to be, as the authors state, particularly underrepresented in global carbon flux research. Nevertheless, I do have three major concerns or questions that I would like to mention below.

Comment #1

First, I am concerned about the methodology to partition and extrapolate CO2 fluxes discussed in section 2.3.3. The authors discuss the assumptions on which the Arrhenius-type function from Lloyd & Taylor (1994) relies that was used for extrapolating ecosystem respiration. The first assumption features an exponential response between soil temperature and respiration. However, the authors also describe that high (soil) temperatures suppress daily respiration (discussion section 4.1). This is attributed to a decreased microbial activity which suppressed soil respiration and has been described more often in literature. The authors not only found that respiration is suppressed at higher temperatures, but they also even mention a (weak) negative correlation between respiration and soil temperatures (section 3.4). Figure S2.6 shows, besides a suppression of Rch due to high temperatures, that there does not seem to be a clear exponential temperature relation. This raises questions about the validity of the assumption on which *the partitioning, extrapolation and gap-filling of CO2 fluxes were based. The authors show that the nocturnal respiration can be modelled quite well in Fig. 3, but how does this translate to daytime when the temperatures are higher? Given the observed negative correlation, the authors should justify their approach. If the model is inappropriate under high temperatures, a different approach might be needed.*

Response #1

The reviewer questions the validity of estimating daytime soil respiration from nighttime values modelled using the Lloyd and Taylor (1994) function. This is a highly pertinent and constructive comment.

In the present study, soil temperature ranges during day and night are remarkably similar. For instance, in full-sun conditions (away from trees), daytime soil temperature varies from a minimum of 20.7 °C (shortly after sunrise) to a maximum of 45.8 °C (around 17:00), whereas nighttime temperature reaches a minimum of 22.1 °C (just before dawn) and a near-maximum of ∼45 °C immediately after sunset. Consequently, nocturnal and diurnal respiratory processes can reasonably be assumed to occur, a priori, under very similar thermal conditions. The model therefore remains applicable provided that the temperature range used for calibration encompasses the full thermal regime experienced over the diel cycle.

Furthermore, we emphasise that the Lloyd and Taylor (1994) model employed here is purely empirical, yet has become a widely adopted standard in the literature, making it particularly suitable for large-scale comparative studies and meta-analyses. We therefore consider the use of this model to be fully justified in the present context.

To address the reviewer's concern and to enhance clarity for readers, a concise introductory statement has been added at the start of Section 4 ("Discussion"):

"Prior to the discussion, it is important to recall the methodological basis underlying the modelling of soil respiration.

An empirical Arrhenius-type equation, as proposed by Lloyd and Taylor (1994) and widely adopted in the literature, was used to model soil respiration as a function of soil temperature. In the present study, daytime soil respiration was calculated from the modeled values of nighttime soil respiration, assuming comparable thermal conditions between day and night. Indeed, measured soil temperature is remarkably similar (20.7—

45.8°C during the day versus 22.1— 45.0°C at night; data not shown), thereby ensuring that the model calibration encompassed the full diurnal thermal cycle.

The model parameters were recalibrated every five days, which represents a methodological compromise between the temporal resolution and the robustness of data. This rolling calibration allowed us to capture the seasonal variability of soil respiration while maintaining sufficient stability for reliable model parameters estimation.

Overall, this approach provides a consistent and empirically grounded framework for estimating diel CO2 exchange dynamics within the system; data not shown), thereby ensuring that the model calibration encompassed the full diurnal thermal cycle.

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Overall, this approach provides a consistent and empirically grounded framework for estimating diel CO_2 exchange dynamics within the system".

Line 556 to 570 (Revised version)

Comment #2

Second, I have a question about non-linearity which could affect chamber flux results. When working with the chambers the authors noted fogging and decided to shorten the flux-analysis from 15 to 5 minutes during the groundnut growing season. Other causes may still lead to a non-linear measurement of CO2 concentrations after chamber closure. For example, a high plant uptake of CO2 could diminish CO2 concentrations substantially, eventually slowing down plant uptake. When a flux is calculated using a fitting period that is too long, the slope of the CO2 uptake will be lower than the initial slope, misrepresenting the actual initial CO2 uptake and affecting the total CO2 balance. How did the authors make sure this non-linearity was minimized during the flux calculations? Did some 5-minute flux measurements turn out to be non-linear? If so, how were these cases handled? Were any non-linear fluxes excluded by filtering fluxes that had a R2 < 0.8? Would that be the right choice?

Response #2

We thank the reviewer for this pertinent comment regarding the potential non-linearity of CO_2 concentration increase following chamber closure.

In the present study, we observed no substantial deviation from linearity, even when measurement duration was extended from 5 to 15 minutes. Moreover, the relatively large chamber headspace height (0.50 m) helps maintain a stable concentration gradient over time, in contrast to the much smaller chambers sometimes used in other studies. These design features ensure that measured fluxes remain linear throughout the entire recording period.

We are willing to provide, as an appendix if desired, the raw recordings of the temporal evolution of CO_2 in the chambers over several cycles, showing that the slope does not change when the chambers remain closed for extended periods (15 minutes).

Regarding data filtering, an $R^2 > 0.8$ threshold was applied to exclude measurements that could be compromised by water ingress into the tubing during the rainy season, by water-vapour condensation, or by any other artefacts likely to bias the flux estimates (e.g., incomplete chamber closure).

Comment #3

Third, the authors present an annual carbon budget of the ecosystem that was measured but did not include harvest and livestock manure C-terms. Even though the authors clearly mention and discuss this problem in the methods and discussion sections, I have my doubts about the usage of the term carbon budget. When the livestock was not fed externally, and manure is not exported from the system, we could assume that the presence and grazing would have a marginal impact on the carbon budget. However, in the discussion it is mentioned that faeces are collected from the field. Furthermore, biomass harvest C-export normally represents a substantial term within a carbon budget of an agricultural system. I do understand that a carbon budget is a valuable result. However, ignoring these C-terms and then comparing the carbon budget to literature seems incorrect and may lead to misleading comparisons. Would it be possible to roughly estimate the missing components to construct an actual carbon budget? The estimates could feature substantial errors that can be propagated. Such an approach may provide a more complete carbon budget and facilitate a fair comparison with other studies.

Response #3

We thank the reviewer for this highly pertinent comment. We acknowledge that neither carbon exports associated with biomass harvesting nor carbon inputs from animal excreta were quantified in the present study. We agree that the use of the term "carbon budget" may be misleading, and we have therefore clarified at line 297 to 299 (revised version) that the budget calculated here is only apparent. This means that the budget calculated here represents only **the balance of vertical CO2 fluxes between the soil, the vegetation and the atmosphere**, excluding lateral C fluxes such as biomass export/import and free manure return from animals.

Our objective was to provide a first integrated estimate of the major vertical CO_2 fluxes (photosynthesis, respiration, and net ecosystem exchange) based on two complementary approaches (chamber-based vs. eddy-covariance), rather than to deliver a complete carbon budget. This is now explicitly stated at lines 301–303 (revised version). Accordingly, throughout the revised version, we have systematically replaced "annual C budget" with **"annual vertical CO_2 balance".**

Lines 1, 52, 293, 297, 549, 555; 798 and 828 (Revised version)

We have also added the following to Section 4.7 (Limitations of the Study) for further clarification:

"Furthermore, the present study constitutes only an intermediate step delivering a first integrated estimate of the main vertical CO_2 exchanges (photosynthesis, respiration, and net ecosystem exchange) as a base for a forthcoming paper that will present a more comprehensive carbon budget of the ecosystem. Establishing such a carbon budget would require substantial additional data acquisition and poses considerable methodological challenges. In particular, quantifying carbon inputs/outputs associated with free-ranging livestock grazing would be difficult to achieve with acceptable accuracy. It must also be recognised that the system is in a dynamic, non-steady state, characterised by marked inter-annual variability as well as periods of carbon storage and release, which are difficult to constrain empirically except through modeling".

Line 827 to 835 (Revised version).

As a reminder, the study site has been equipped since 2018 with an eddy-covariance flux tower installed above the tree canopy, providing a continuous multi-year time series of ecosystem-scale CO_2 exchange. However, as is also the case for other carbon-budget studies conducted in the Sahel (Tagesson et al., 2015; Wieckowski et al., 2024), these data represent the balance of vertical CO_2 fluxes only.

In parallel, several complementary agronomic studies, particularly those quantifying harvested biomass, are currently underway. These ongoing efforts will enable us, in a forthcoming and more comprehensive article, to substantially refine the budget presented here and to construct a more realistic and complete carbon budget.

Comment #4

The highlights include abbreviations (Sh, FS) that are unknown to readers.

Response #4

The necessary clarifications have been added to ensure that these acronyms are readily understandable to readers.

Line 34 to 35 (Revised version)

Comment #5

Line 99. Please check the usage of present time.

Response #5

The use of the present indicative has been reviewed, and the sentence has been rephrased as follows: **"Specifically, the study aims to (1) conduct year-round, high-frequency in situ CO_2 flux measurements from soil and crops using automated static chambers; (2) partition the net CO_2 fluxes (FCO_2ch) into respiration (Rch) and photosynthesis (GPPch); (3) investigate the environmental drivers of fluxes and the spatial variability linked to tree presence; and (4) compare chamber-based flux estimates with ecosystem-scale measurements derived from the EC method".**

Line 102 to 107 (Revised version)

Comment #6

Line 225. Please remove the repetition.

Response #6

Repetition has been removed.

Line 231 to 234 (Revised version)

Comment #7

Line 443. Table 1 results for daily FCO2 are negative, while numbers here appear positive.

Response #7

We thank the reviewer for this comment. Indeed, in the table, FCO_2ch values (annual sum and mean values) are reported as negative (Line 481, revised version). However, when comparing in mean magnitudes between full-sun (FS) and shaded (Sh) conditions in the main text, FCO_2ch was expressed as an absolute value.

To eliminate this potential source of confusion, we have explicitly stated throughout the manuscript, where relevant, that FCO_2ch values are reported "in absolute terms" when presented as mean values in the main text.

Lines 466 and 468 (Revised version)

Comment #8

Table 2. It is a choice to not denote non-significant correlations. However, a p-value of 0.05 is arbitrary. There might be different visions on this matter, but I would not 'hide' non-significant correlations and show each p-value (or p-value category).

Response #8

The p-values have been added to Table 2.

Line 499 (revised version)

Comment #9

Table 4. How was the std error that is shown calculated?

Response #9

We thank the reviewer for pointing this out. An error had indeed been made in the uncertainty estimation. The reported standard error was initially based on the daily mean standard deviation, implicitly assuming that the uncertainty remained constant regardless of the number of measurement days. This approach is only valid when calculating the uncertainty of an annual mean flux, not when estimating the uncertainty of an annual cumulative flux.

We have therefore corrected this and added the appropriate clarification at the end of Section 2.4 ("Statistical analyses").

"The standard error of the total annual flux was estimated using the error propagation method. This calculation considered the mean standard deviation of daily fluxes (g C–CO_2 d^{-1}) and the effective number of measurement days (365). For each FS and Sh condition, the mean daily standard deviation was multiplied by the square root of 365 to obtain the annual standard error. The resulting values were then weighted by 90% for FS and 10% for Sh to derive the overall standard error of the annual flux sum, which was subsequently converted to Mg C–CO_2 ha^{-1}".

Line 345 to 350 (Revised version)

The necessary corrections for standard error values have also been applied throughout the entire manuscript.

Lines 52, 53, 551 to 556, 559, 779 to 780, 786, 804, (Revised version)

Comment #10

Section 4.5. Sometimes it is hard to follow which periods are being discussed. In general, it could help to specifically mention the months that are being discussed.

Line 704. The authors mention that chamber and EC GPP measurements agree closely. I do agree that this is the case in August, but after the beginning of September the two seem to start deviating remarkably. As mentioned above, please clarify which months are under discussion.

Response #10

When referring to the agreement between chamber-derived GPP and eddy-covariance (EC) estimates, we meant concordance at two distinct levels: 1) temporal dynamics (restricted to the rainy season), which exhibit highly similar patterns between the two methods until peanut harvest in the chambers, and 2) flux magnitude, with particularly strong agreement during the month of August, as the reviewer rightly highlighted.

Accordingly, we have added the corresponding clarification in Section 4.

Line 734 to 736 (Revised version)

Comment #11

Section 4.6. Please see the third point above.

Response #11

Checked.

Comment #12

Line 794. Since the actual carbon balance is unknown, it cannot be stated that the agroforestry systems that were studied are 'effective carbon sinks'.

Response #12

We have added the necessary clarifications in this regard. The sentence now reads:

« Sustainable management practices, particularly regarding C inputs/outputs from the system regarding crop harvest, residues exporting, and cattle free manuring must be taken into account to confirm the system capacity to act as a carbon sink".

Line 809 to 812 (Revised version)