

Review comments on “Drivers and CO₂ flux budgets in a Sahelian *Faidherbia albida* agro-silvo-pastoral parkland: Insights from continuous high-frequency soil chamber measurements and Eddy Covariance” – Ba et al. (EGUsphere)

The research of Ba et al. focuses on CO₂ 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.

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 R_{ch} 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.

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 $R^2 < 0.8$? Would that be the right choice?

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.

Overall, the manuscript is valuable and presents interesting findings. Addressing the issues outlined above would strengthen it further. Below, I list several other minor issues.

Minor comments:

Highlights

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

Introduction

Line 99. Please check the usage of present time.

Line 225. Please remove the repetition.

Results

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

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).

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

Discussion

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.

Section 4.6. Please see third point above.

Conclusion

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