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Altered Seasonal Sensitivity of Net Ecosystem Exchange to Controls Driven by Nutrient Balances in a Semi-arid Savanna	
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Response to Report from Reviewer #2

Reviewer comments are printed in black.
Answers are printed in blue below the respective comment.

This is a sound piece of work showcasing the use of eddy covariance measures of ecosystem fluxes as an experimental tool, with measures examining impacts of N and P additions to semi-arid savanna ecosystem. Tree-grass savannas occupy ~30% of the global land surface and quantifying fluxes from these systems is critical as they appear to be a source at present to the atmosphere and they drive global variability of global atmospheric CO2. This is a unique experiment, and the EC work described here is a great add on, highlighting the power of long-term studies - another 7 or 8 years we will start to pick up impacts of CO2 fertilisation, heatwaves, increasing variability of precip, as well as the +N and P effects reported. Given the novel statistical analytics described in this study, untangling all of these interacting drivers of C, water and nutrient dynamics may well be possible. I hope funding can be made available to continue this important work.
Methods used were well described for both the EC measures, the experiment and a statistical analysis linking Singular Spectrum Analysis, Pearsons coefficient and information theory to identify key drivers of fluxes, plus lags between biophysical drivers of NEE all as a function of time and across the + N, +P and additive N and P treatments effects, impressive.
Given the quality of site and data set, the performance of the CT site is interest as it was a significant source to the atmosphere of ~80-100 g m2- y-1 or almost 1 t C per year lost from the system. Mean and interannual variability is similar to other semi-arid savanna as reported by Archibald et al (2009) for the semi-arid savanna at Kruger NP and Ma (2007) over oak savanna in California - more needs to made of this in the Discussion. Where is this carbon coming from – grazing and net loss from the soil? Can't be fire here. The word 'fire' is never mentioned in the paper, an oversight given this is a major feature of savanna ecosystems.
Revisions are required, overly long ms, somewhat verbose and repetitive, the writing needs to be tightened considerably! With revisions post-reviews will make for an excellent paper.

Thank you very much for your kind assessment of our work, for suggesting improvements and proposing important additional literature. We have shortened the Methods and the Results section considerably to make the overall manuscript more concise, complemented the Discussion section as suggested and addressed all technical comments in detail. Your suggestions have helped us to improve the quality of this manuscript in various aspects.
Please find below a point-by-point reply to your comments and suggested changes in the revised manuscript.

We do not mention fire in the discussion as wildfires happen rarely in such managed ecosystems in Spain, and in the concrete case of our study site, no fire happened as far as we know and as far as local people remember, which mean at least during the last 50 years.
In the case of our site, the long-term NEE estimates from EC data (measurements since 2004) suggest the ecosystem acts a significant source of carbon, but they are challenged by the estimates of carbon stocks performed at the site (tree biomass stock changes from dendrometers and soil inventories performed in 2006, 2015 and 2019), which suggest that the carbon budget of the ecosystem is rather neutral. The analysis and discussion of long-term carbon budget at the site is another work in progress to be concluded and published in the near future. Therefore, despite the measured annual NEE for the analyzed period is about 90 $gCm^{-2}\ y^{-1}$, there is so far not undisputable evidence that the ecosystem acts as a significant source of carbon. As no clear statement about the mean carbon balance of the ecosystem can be made now, and as it is not in the scope of the presented study, we prefer not to speculate about possible carbon sources.

We added the following paragraph in the beginning of section 4.1 to the discussion, aligning with your suggestions to compare with mean and interannual variability of other semi-arid savannas:

"The annual NEE average from EC measurement is about 90 $gC\ m^{-2}\ y^{-1}$, suggesting the unfertilized site acts a carbon source with a high interannual variability. Similar mean and variability were found in other semi-arid savannas, such as in Kruger National Park in South Africa (75 ±105 $gC\ m^{-2}\ y^{-1}$) (Archibald et al., 2009). However, semi-arid savannas can also act as carbon sinks. In California, a similar oak savanna (i.e., Tonzi Ranch) was observed to be a carbon sink (values from -144 to -35 $gC\ m^{-2}\ y^{-1}$), while the neighboring grassland (i.e., Vaira Ranch) was found to be a carbon source (-88 to 189 $gC\ m^{-2}\ y^{-1}$)(Ma et al., 2007). In Dakar, Senegal, a Sahelian savanna ecosystem acted as a carbon sink with an average annual NEE budget of $-180 \pm 29\ g\ C\ m^{-2}\ y^{-1}$ (Wieckowski et al., 2024). Another natural West-African savanna in the South of Burkina Faso has been found to be a strong sink of CO_2 (-864 to $-1299\ g\ CO^2\ m^{-2}\ y^{-1}$) while two degraded sites nearby were CO_2 sources (118 to 605 $g\ CO^2\ m^{-2}\ y^{-1}$)(Berger et al., 2019)."

Technical comments
Introduction

Background provided in the Introduction was good
Thank you.

L52-55 "Light absorption … Oritz (2024)" – consider delete these lines, your audience will be aware of this theory, basic plant physiology. There is a lot of these sort of statements in the Discussion as well.
Thanks for pointing this out, we have deleted the respective lines and also took your comment into account while revising the Discussion section.

L57 re-word – "Typical ecosystems in semi-arid regions are savannas where coexisting vegetation layers (e.g., tree and grass) interact in complex ways (Higgins *et al.,* 2000, House *et al.* 2003).
Cite some classic savanna ecology papers here to support this important claim, e.g.;
Higgins, S. I., Bond, W. J., & Trollope, W. S. W. (2000). Fire, resprouting and variability: A recipe for grass-tree coexistence in savanna. Journal of Ecology, 88(2), 213–229. doi.org/10.1046/j.1365-2745.2000.00435.x
House, J. I., Archer, S., Breshears, D. D., Scholes, R. J., & Participants, N. T. I. (2003). Conundrums in mixed woody–herbaceous plant systems. Journal of Biogeography, 30(11), 1763–1777. https://doi.org/10.1046/j.1365-2699.2003.00873.x

Thank you for your comment and for suggesting further references. We have added them to our bibliography and rephrased the sentence in the following way:

"In semi-arid regions, savannas are a typical ecosystem type. They comprise coexisting vegetation layers (e.g., tree and grass), which interact in complex ways (Higgins et al., 2000; House et al., 2003)."

L60 delete "Especially the …"
Thanks, we have deleted that.

L61 should read "On the Iberian Peninsula…"
Thank you, we have corrected that.

L66 "…), limited by water in the dry season and by nutrients and energy in the wet season (Moreno, *et al.* 2008… }. Add Whitley et al 2011 here, relevant paper on light limitation on GPP in savannas
Whitley, R., Macinnis-Ng, C., Hutley, L. B., Beringer, J., Zeppel, M., Williams, M., Taylor, D., & Eamus, D. (2011). Modelling productivity and water use across five years in a mixed C3 and C4 savanna using a soil-plant-atmosphere model: GPP is light limited not water limited. Global Change Biology, 17, 3130–3149. https://doi.org/doi.org/10.1111/j.1365-2486.2011.02425.x
Thank you for recommending this important reference, we have added it.

L84 delete "set up", replace with "established"
Thanks, we have replaced that.

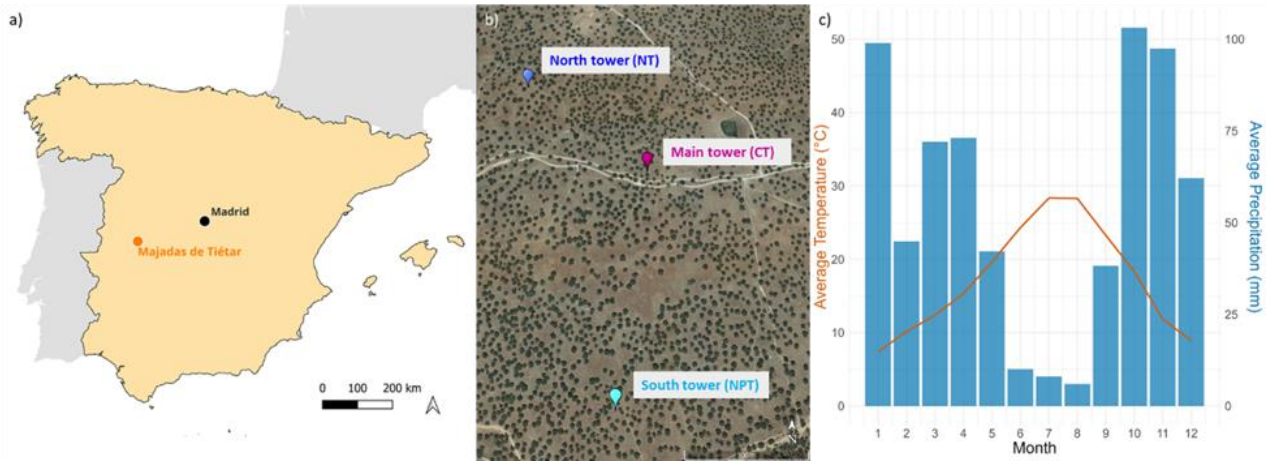
L110 "… 20-25 trees" a bit loose, do you have estimates of mean tree basal area in m2 ha-1 or similar tree size metric? Mean height also useful.
Thank you for pointing out that this information is missing. We have added it to the respective sentence as follows:
"The tree density is around 20-25 trees per hectare, with a mean diameter at breast height of 46 cm (El-Madany et al., 2018), the fractional canopy cover of trees is 23 % and the canopy height is on average 8.7 m (Bogdanovich et al., 2021)."

L112 spatially variability of grass - here and comment on significant seasonal temporal variability of grass growth that you describe L309 add text "described in detail below".
Giving an LAI range a single value not useful in this context
Thank you for your comment, we have revised the text as follows:

"The tree leaf area index (LAI) is around 0.35 m^2 m^{-2}, the grass layer has a peak LAI in spring but is quite spatially variable with values between 0.50 and 2.50 m^2 m^{-2} due to the seasonal temporal variability of grass growth (described in detail below) (Migliavacca et al., 2017)."

move Figure 1 here which is in the methods as more site information here would be useful.
You could also Fig 1 more comprehensive by adding 2 panels - Fig 1a) add a site map, this is lacking, showing location within country and the treatment locations, plus a second panel b) mean monthly precip and mean monthly Tair, and c) the current Fig of GCC of the grass layer over the growing season. This will highlight the seasonality of this savanna climate system and the dynamic phenology, largely driven by the grass phenology, with presumably tree cover relatively constant.

Thank you for the suggestions on the figures. We have added a site map (which was also suggested by reviewer #1), indicating the location of the sites on the Iberian Peninsula as well as the location of the three eddy covariance towers on an airborne image. Following your suggestion, we have further added a figure depicting the monthly mean temperature and precipitation sums over the study period (2016-2023):



"Fig.1: a) site location on the Iberian Peninsula. b) location of the three eddy covariance towers. Nitrogen added tower (NT) in blue, control tower (CT) in purple, nitrogen + phosphorous added tower (NPT) in light blue. The tower locations were chosen in a way that during dominant wind directions their footprints don't overlap. Footprint climatologies can be found in El-Madany et al. (2018), Fig.1. c) average monthly precipitation sums and temperature (measured at 15m) across 2016-2023."

We added it in the suggested position, at the end of the section "Site description". As this figure already comprises three panels, we decided to keep the figure with GCC of the grass layer separate and in the "Phenological Seasons"-section.

L167 define the standard NDVI acronym "… and satellite data (normalized difference vegetation index, NDVI)".
Thank you, we have defined the acronym here.

L192 consider using Sentinal-2 EVI as well, or both indices. I have found with flux data that EVI covaries more closely than NDVI.
Thank you for the suggestion, you make a valuable point and we discussed including EVI from the FluxnetEO dataset (MODIS) as an additional variable. However, NDVI is already the most important driver, showing the highest mutual information and Pearson correlation values. Therefore, after some consideration we decided to not include EVI, as it would probably show a similar pattern as NDVI with higher interaction values and it will likely not provide additional insights. Nonetheless we agree that EVI is an important vegetation index and we mention its potential in the discussion (4.3) (former line 629):

"Alternatively to NDVI, the Enhanced Vegetation Index (EVI) could be considered as a representation of vegetation greenness, as it is found to covary closely with the carbon flux in semi-arid ecosystems (Maluleke et al., 2024)."

L311 "spring, dry down, summer, autumn and winter, as described above in Chapter 2.1.). Delete "as described above in Chapter 2.1), clearly a left over from your PhD thesis?!
Thank you for pointing this out, we have deleted it.

L348 delete ".and".

Thank you, we have corrected that and revised the sentence according to the comments of reviewer #1 in the following way so that everything is grammatically correct:

"Pearson correlation coefficient *r* considers only linear relationships between variables; Mutual Information (MI), accounts for collinear relationships. MI_{sync} and *r* values show synchronous relationships, MI_{max} values can account for leading and lagging interactions by identifying the day of the highest interaction between the potential driver and NEE within a 60-day window."

L351 should read "For all plots, …"
Thank you, we have corrected that.

L401 "NT (12 days) compared to 15 and 16 days at NPT and CT,…" Would this be a significant difference? How would you test this?
In fact all the these rather modest differences in lags i.e. 2-4 days in this paragraph, are they significant or simply error / variability in the data?
Thank you very much for pointing this out, this is a valuable point. While we tested and know that the results of each tower are significant, the significance of the tower differences in MI values and lag times is not proved. As our paper aims to show general patterns of the effect of nutrient addition on NEE driver importance, we decided to not put the focus on the differences in the specific values and revised section 3.3 in a way that it highlights the overall patterns:

"N fertilization appeared to shorten the reaction time of NEE to changes in NDVI. GCC at the grass level showed higher explanatory value for NEE at NPT and NT (MI_{max} = 0.37) compared to CT (MI_{max} = 0.33). EF showed only slight differences in interaction strengths among the sites (Fig. 4, S3). Relative humidity at two heights showed the lowest interaction with NEE at CT (MI_{max} = 0.24), while the fertilized sites had slightly higher values (0.26-0.27). The reaction time of NEE to relative humidity appeared to decrease with fertilization. VPD appeared to have the higher explanatory value for NEE at NT and NPT, and slightly less and CT (MI_{max} = 0.27). The interaction between NEE and air temperatures was slightly higher at the fertilized plots compared to the control. Soil temperatures showed similar interaction strength with NEE across treatments, with MI_{max} ranging from 0.31 to 0.33 (Fig. 4, S3).
Regarding radiation variables, PAR seemed to have a slightly higher interaction with NEE at NT, than at NPT and CT. Similarly, SWDR showed slighlty higher interaction with NEE at NT, while at NPT and CT it was equally strong.
In terms of soil variables, soil temperatures exhibited the strongest interaction with NEE. While soil temperatures below the canopy (T_{soil}_bc) were almost the same across sites (MI_{max} = 0.33), the importance of soil temperatures under open air were lower at CT compared to the fertilized plots. SWCn showed the highest explanatory value for NEE at NT (Fig. 4, S3). An overview plot with all variables including the ones with MI_{max} < 0.2 is provided in the Supplementary Material (S4)."

L406-408 delete this text, focus on VPD as a driver.
Thank you, we considered deleting this part and have edited it within the revision of chapter 3.3 as shown in the previous answer.

L431 re-word "… phenological seasons based on the grass layer GCC derived from PhenoCam "
Thanks for your suggestion, we have revised the sentence in the following way:

"We split the 7-year dataset into five different phenological seasons based on grass layer GCC, and calculated MI_{sync} between NEE and each of the drivers."

We have removed “derived from PhenoCam photos” as this was already explained above and redundant at this point.

L435 re-word “as well as radiation parameters PAR and SWDR …”
Thank you, we have rephrased that sentence as suggested.

L443 re-word “Additionally SWDR, PAR were important in …”. There are numerous examples like this, a bit repetitive, inefficient writing.
Thank you, we have edited this part as suggested.

L454 No need for this sub-heading, delete “3.5 Changes in NEE Sensitivity over Time” seemed to me like this text is continuing description of Table 2.
Thanks a lot for your comment. We understand your point but after some consideration we would prefer to keep the sub-heading here, as the previous chapter targets a different analysis. While section 3.4 shows the results of the analysis of different NEE controls in different phenological seasons, section 3.5 deals with the development of their importance over time. To make this distinction clearer we think it is advantageous for a facilitated comprehension to keep the sub-heading here. But we have added a short sentence in the beginning to emphasize the difference as follows:
“Using yearly MIsync for each single season, we observed that with N addition, NEE became less sensitive to certain variables during autumn (i.e., the regreening phase), the drydown phase, and winter over time (Fig.5).”

L476 “Fig. 6”, add a space
Thank you, we have added a space here.

L485 delete “amount”
Thanks, we have deleted that.

L515 see also Moore et al 2016 Biogeosciences 13: 5085-5102, doi:10.5194/bg-13-5085-2016.
Thank you, we have added that reference.

L544 re-word “do not appear to play a crucial role at the seasonal scale.”
Thanks, we have corrected that.

L560 “growing season, spring, NEE is dominated by GPP.” Careful making statements like this as these two variables are not independent of each other ie GPP is derived from NEE observations. You would have to be very confident of your Reco model used in this system to estimate GPP.
Thank you for pointing this out. We have rephrased the sentence in the following way:
“In the primary growing season, spring, NEE is typically dominated by GPP.”

L592 delete “made”
Thanks, we have deleted that.