## Reviewer 2

The authors have conducted an experiment in Gulu, Uganda in which the e9ects of various conventional and conservation agricultural treatments on crop productivity, soil N dynamics, and N2O emissions are analyzed. The study is conducted over two cropping seasons within a single year – April – October 2023 and October-Jan 2024. Plots that had been fallow for the previous 3 years were cleared and prepared in an RCB design with four treatments: conventional maize monocrop (ConMM), conventional pigeon pea/maize (Con) rotation, conservation agriculture (CA), and conservation agriculture with biochar amendments (CA+bc). Some di9erences were observed in NH4 but not in NO3. Maize grain yield in season 1 was lower in conMM than pigeon pea grain yield in the other treatments; there was no di9erence in grain yields in the second season. Yield-scaled N2O emissions were higher from the conventional than the CA treatments in the first season, but there were no di9erences in the second season.

This study adds much-needed measurements of N cycling and N2O emissions from low/no-input cropping systems in Uganda, and I look forward to seeing them in the peer reviewed literature. I do have several general comments.

Major comments:

A major limitation of this study is that data are only presented from a single year (which encompasses a long-rain and a short-rain cropping season). Interannual variability can play a big role in crop productivity and N cycling (highlighted by the statement in lines 548550 that drought drastically reduced maize grain yield in the first season), and this type of study would benefit from more than one year of measurements. It is true that two cropping seasons are included, but these are di9erent cropping seasons – long rains and short rains—and cannot be said to represent the same thing as multiple years of measurements. The fact that there are only one year of data presented here and, as a result, it is not really possible to generalize really needs to be emphasized front-and-center in the text. (There are so many examples in ecological studies of a second year of data drastically changing results that it's become cliché to joke that "I should have quite collecting data after one year!") That said, 1 year of data can still be quite useful, and I think it's particularly important to have these data published because there are so few data from the region, and particularly few data from pigeon pea systems. And I am especially sympathetic to the tremendous amount of e9ort required to establish these experimental plots and to collect and analyze these data. I leave it to the editor to decide whether a revised manuscript would meet biogeosciences' editorial threshold for publication.

Response: We thank the reviewer for the positive appraisal and agree that data from several subsequent seasons would have been valuable. However, due to budget and time constraints, this was not feasible. Nevertheless, we believe that presenting effects of different management practices on $N_2O$ emissions from an unfertilized system in SSA for two growing seasons is of interest to both the scientific community and stakeholders. Since the effects of biochar on soil chemical properties are expected to diminish over time (e.g., Cornelissen et al. 2018), measuring emissions immediately after biochar application for one full year provides some insight.

On a somewhat related note, the authors' suggestion that treatment differences were not expected in this first year (e.g., Line 540-542) would seem to raise a fundamental question: if treatment differences were not expected, then why were measurements conducted in the first year, and why is it interesting to publish them? A way to address this concern would be to cite examples from the literature where treatment differences (when similar treatments were compared) *were* found in the first year of establishment.

Response: We agree that the sentence about expected effects did invalidate our study to some extend. We have therefore removed the sentence. We have now cited other studies with treatments effects in the first year.

**Other general comments:**

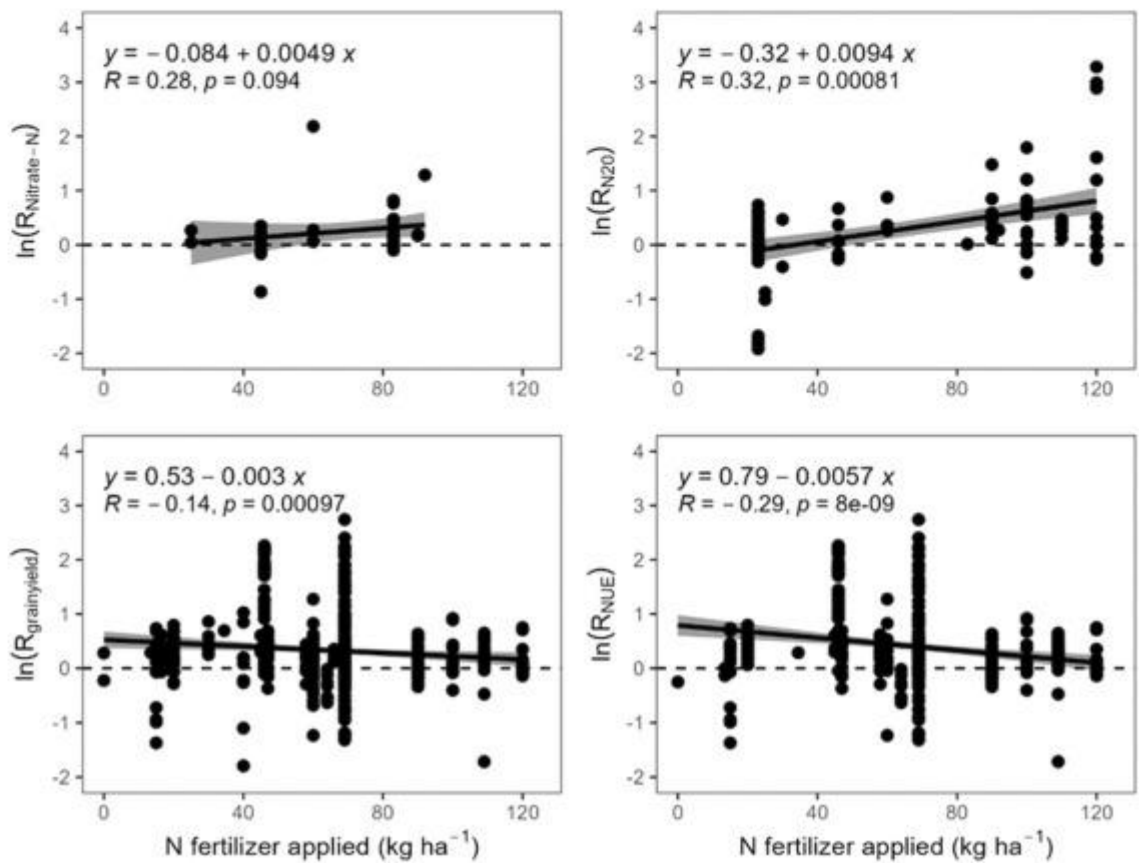
I don't understand why chamber position is included as a treatment in statistical analyses. If there were some targeted analyses into mechanism then it might make sense, but in this manuscript the central questions are how the agricultural management treatments differ, and how soils change over time within an agricultural management treatment. To answer this question, the weighted mean of the within row and interrow chambers would be the appropriate response variable—i.e., an estimate of the flux from each plot.

Response: Chamber position was included as a factor in the statistical analysis to assess potential differences between basins/inrow where maize or pigeon pea was planted) vs. interrow (no planting, no soil disturbance under CA, but overall digging under conventional treatment). A distinction between basin and inrow was also needed to discern biochar effects, as biochar was only added to the basins and not to inrow soil. Abiven et al. (2015) found root biomass inside planting basins to be twice as large in biochar amended plots compared to regular CA in Zambia. In this unfertilized system, the main difference between basins and interrow location is that basins are re-opened every season under CA and that biochar is added to the basins only. Effects of biochar addition on soil properties were expected in the basins where biochar was applied, not the interrows.

In addition to reporting differences between basins/inrow and interrow positions, we now include area-weighted cumulative N2O emissions in Fig 4 and 5, as well as yield- and yield-N scaled cumulative N2O emissions (Table 3) to show estimated total fluxes for the different management practices, area-weighted for basins/inrow and interrow positions.

I think there's a missed opportunity for more discussion of what makes this study unique-the focus on unfertilized systems. Currently this is not discussed much at all. There are actually quite a lot of N2O data from unfertilized maize in tropical African systems that could provide context for understanding the emissions in this study– the control plots from fertilizer trials. Off the top of my head, there are studies in East Africa with lead authors that include Baggs, Hickman, Millar, Pelster, Rosenstock, Tully, Zheng, and studies in Zimbabwe from Mapanda. Typically there are multiple studies from each author, and at least some of them include explicit calculations of yield-scaled emissions.

Response: We agree with the reviewer that the uniqueness of our study is the absence of fertilization, next to the use of legumes and biochar for CA. However, based on a recent review of 26 published studies dealing specifically with N2O emissions in SSA (Namatsheve et al., 2024; see figure below), at least 20 kg N $ha^{-1}$ was applied.




*Write a figure caption here*

We now include this information in the manuscript as follows (Ln 104 – 106): *In our recent meta-analysis of CA and biochar effects on N cycling, we found that residue retention increased soil* $NO_3^-$*, leading to higher* $N_2O$ *emissions; this finding was based on 26 published studies focusing specifically on* $N_2O$ *emissions, and applying at least 23 kg N* $ha^{-1}$.

Initial soil sampling for NO3- and NH4 analyses was conducted after treatment application. Since there were no NO3- and NH4+ measurements before treatment, these measurements are able to answer one question: how treatment affects changes in NO3- and NH4+ snap shots when comparing the start of the first crop season to the end of the second crop season of a given year. I think it would be very useful to include some discussion/contextualization as to why this is an important question—it's not immediately clear to me.

Response: We do not have the background values of mineral N from before the experiment. The absolute amounts especially for the first season were mainly influenced by the mineralisation of organic matter, built up during the 3-year fallow period before establishing the experiment. The amounts of NH4+ and NO3- in the soil were investigated as potentially important explanatory variables for N2O emission and to explore the effect of different soil managements in soil mineral N. Relative differences should be valis.

**Specific comments:**

Line 40: delete "tropospheric"

Response: deleted

Line 42: N2O is actually the largest anthropogenic driver of stratospheric ozone depletion (see Ravishankara 2009 in Science DOI: 10.1126/science.1176985)

Response: Thank you for the comment, we have now included the provided reference.

Line 44: replace 'atmospheric' with 'anthropogenic' and change to "60% to global **anthropogenic** N2O emissions"

Response: done

Line 60: you can delete 'or by-product'

Response: done; however, in nitrification, N2O is a by-product, not an intermediate.

Line 64: also labile carbon availability ('substrate availability' would most likely be interpreted to refer to nitrogen compounds)

Response: We thank the reviewer fir this comment and have changed the sentence accordingly, which now reads (Ln xxx-yyy): *The biogeochemistry of $N_2O$ in soil is to a large extent regulated by complex interactions between environmental and biogeochemical factors such as temperature, water, labile carbon availability, oxygen levels, acidity and substrate availability (Case et al., 2015; Tian et al., 2020)*.

Line 137: indicate in the text when specifically the clearing was conducted and when glyphosate was applied.

Response: We now give this information in (Ln 138 – 141): *Prior to the establishment of the experiment, on 15 March 2023, a dense, naturally grown vegetation of grasses and shrubs was removed by slashing and chemical weeding using glyphosate [N-(phosphonomethyl) glycine]*.

Line 138: I'm not an expert in herbicides, but I think glyphosate can potentially have a range of impacts on crop production (via uptake by crops either from glyphosate that has persisted in soil or is released from decomposing weed biomass; it also has potential interactions with mineral nutrition: see https://pmc.ncbi.nlm.nih.gov/articles/PMC6918143/), which makes the timing of its use useful to know, and which might have implications for season 1 production.

Response: Thank the reviewer for this comment and appreciate the reference to relevant literature. In our study, glyphosphate was applied on 15 March 2023, 19 days prior to sowing in the season 1 (4 May). We agree that the short period between spraying and sowing might not have been enough to fully degrade the chemical. The glyphosate remaining in soil may therefore have had potential impacts on crop production and mineral nutrition, but there was no possibility to determine its residual concentration. Given the high temperatures, however, we believe that residual glyphosate concencentrations decayed quickly. Despite these potential challenges, glyphosate is promoted by the mainstream of the conservation agriculture community where controlling perennial weeds with mechanical and biological methods is a challenge, especially in early years of adoption.

Line 139: indicate in the text specifically when the plots were tilled

Response: Date for tillage is now given in Ln 142 – 145: *On 27 March 2023, plots under conventional management were prepared by overall digging using hand hoeing (100% tillage) and plots of the same size under CA by manually digging 10-L planting basins (35cm long × 15cm wide × 20cm deep) spaced 70 cm × 35 cm (interrow × within row spacing).*

Line 144: ConventMM has not been defined at this point. The CA+BC abbreviation has also not been introduced—it was introduced as CA+biochar.

Response: Thank you for this comment. We have now consistently changed 'CA+biochar' to 'CA+BC' throughout the manuscript, and introduce 'ConventMM' in abstract and introduction.

Line 157-158: I'm a little confused by plant spacing. In line 141, it sounded like the spacing was 70cm x 35 cm, which would yield a planting density of 40,898. Here it says 10cm; if that's 10cm x 70cm, that would be a planting density of 143,143. And were the same planting densities really used for maize and pigeon pea?

Response: Plant densities for both maize and pigeon pea were the same. We accounted for the size of the basins which were 35 cm long, 15 cm wide and 20 cm deep. Therefore, if the spacing for the basins is 70 x 35 cm. With 3 seeds in each basin, one ha has 21 000 basins and a planting population of 63 000 plants ha$^{-1}$.

Line 175-176: two different biochar treatments are described here, including one that isn't defined or mentioned anywhere else in the manuscript (CA+BC+BC).

Response: Thank you for noting this. We have now deleted CA+BC+BC, as it is not part of this paper.

Line 213: Be specific about when the collars were installed (you can say something like ‘at least xx days before the first measurement).

Response: Thank you for this comment. We have rephrased the sentence as follows (Ln 203 - 205): *Permanent gas sampling plots were established by inserting 17 cm diameter PVC rings (the base) to a depth of 7 cm into the soil on 19 April 2023, 3 weeks before first sampling on 10 May 2023*.

Line 210-211: I don’t think I quite have a complete description of the chamber top—it sounds like it should be a pvc pipe – is it a pipe that was manufactured with a top, or did you have to add and seal a top to the pipe?

Response: We used prefabricated lids as top on sewer pipe lid (figure below and Fig. S2). On top of the lids, we drilled holes and fitted it to a self-sealing rubber septum for gas sampling.

Line 218: citing a reference here for using petroleum jelly would be great. It sounds like it would work, but a reference would be helpful.

Response: We have included a citation and the sentence now reads (Ln 209 - 210) : *To facilitate chamber deployment, the contact area between the collar and chamber was sealed with a thin layer of petroleum jelly as described by Shumba et al., (2023)*.

Line 220: were the initial measurements really taken at 1 minute? (i.e. sampling was delayed until the chamber had been sealed for a full 60 seconds)

Response: Yes, the first sample was taken during the first minute; in addition to sealing the chamber, the plunger of the syringe was pumped three times in and out before a representative sample was obtained. This took 0.5 – 1 minute.

Line 220: 60 minutes is a fairly long period. Can you establish that N2O did not reach or approach equilibrium in the chamber?

Response: We measured concentration in the chambers after 1, 15, 30 and 60 minutes. The 60-minute period was chosen because we anticipated relatively low $N_2O$ emission rates and also determined CH4 uptake (which will be reported in another study). In some cases, chamber N2O kinetics indicated that equilibrium was approached, i.e. concentration raise slowed down. In these cases we applied quadratic fits to emphasize the “time-zero” rate as given by the coefficient of the non-quadratic term, which comes close to estimating the flux from the first two measurement points.

We established that in most cases N2O did not reach equilibrium in the chamber. This is stated in the manuscript (Ln 230 – 231) as follows: *Plotting measured $CO_2$ and $N_2O$ concentrations over time, revealed linear increase in most cases with little saturation observed*. In some cases, the $N_2O$ concentration in the sample taken right after chamber deployment was substantially higher than 0.336 ppm (ambient $N_2O$ concentration), pointing at residual $N_2O$ in the chamber. The exalted $N_2O$ concentration after chamber deployment usually decreased until the second measurements (15 min) and to avoid fitting negative fluxes, the first sampling point was discarded. *Flux rates were estimated by fitting a linear or second order (polynomial) function to the concentration change over time. A quadratic fit was only used in few cases in which $N_2O$ accumulation in the chamber showed a convex downwards trend, i.e., decreasing emissions* (Ln 230 – 238).

Line 228: Did you have any checks to make sure there wasn't any sample contamination/leakage during shipping? (e.g., having vials filled with a standard gas in Uganda and then measured in Norway.)

Response: He-filled vials were included as blanks to check for contamination during storage and shipment of the vials. Detected concentrations of $CO_2$ and $N_2O$ were <5% of ambient. We used slightly over-pressured, crimp sealed glass vials with thick butyl septa, which have been shown previously to maintain pressure and mixing ratios during air transport and storage (Raji and Dörsch, 2020). (Ln 220 - 224).

Line 235: how was it determined when a linear or when a polynomial fit should be used? (i.e., it would be good to make sure overfitting was avoided; the additional parameter in the polynomial model can provide a closer fit to the data even if it is inferior to the linear model)

Response: see Ln 235 – 238: *Flux rates were estimated by fitting a linear or second order (polynomial) function to the concentration change over time. A quadratic fit was only used in few cases in which $N_2O$ accumulation in the chamber showed a convex downwards trend, i.e., decreasing emissions*.

Line 242: a quadratic fit does not seem like an appropriate way to calculate a flux here, and makes me a bit worried about the polynomial fits in general. If concentrations are convex downward, that suggests to me that concentrations in the chamber have equilibrated with soil over the 60-minute period, and the resulting flux calculated would not be a good estimate of soil fluxes in the absence of a sealed chamber. If polynomials are fit to data in for which the slope between 30 and 60 minutes is smaller than the slope between 1 and 15 minutes, again it sounds like you're seeing a chamber artifact where concentrations in the chamber are approaching equilibrium with concentrations in the soil, and you might be better o9 using data from 1 to 30 minutes.

Response: Using the coefficient of the non-quadratic term in a second order polynomial is equivalent to estimating the flux from the initial slope of the curve

Line 249: Instead of 'scaling up' I think it would be more precise to say that you're estimating a flux that is representative of the entire plot, and it's being done by calculating a weighted mean of fluxes from the basin and interrow chambers. You could write something like "We estimated a flux representative flux for each plot ($N_2O_{plot}$) by calculating a weighted mean of fluxes from the basin and interrow chambers. We used weighting factors of 0.12 for basin . . .".

Response: We thank the reviewer for this constructive suggestion and have rephrased as follows (Ln 249 – 253): *We estimated a representative flux for each plot (area-weighted cumulative $N_2O$ emission) by calculating a weighted mean of fluxes from the basin and interrow positions. Weighing factors of 0.12 and 0.88 were used for basin and interrow areas, respectively, in CA treatments (CA and CA+BC), while a factor of 0.50 was applied to both inrow and interrow areas in conventional treatments (Conventional and ConventMM)*.

Line 259: indicate when extractions were conducted relative to sampling (e.g., x hours, the next day, etc)

Response: We have added the following sentence (Ln 259 - 260): *The soil samples were extracted the same day, within 5 hours after sampling*.

Line 280: I would change "dividing the scaled cumulative N2O emissions . . ." to something like "dividing the weighted mean N2O emissions" or "dividing $N_2O_{plot}$ emissions. . ."

Response: Since we earlier mentioned how scaled cumulative N2O emissions were calculated (Ln 249 - 253), we have rephrased the sentence as follows (Ln 281 - 284): *Yield-scaled $N_2O$ emissions (kg $N_2O$-N $kg^{-1}$ grain yield) and N-yield scaled emissions (kg $N_2O$-N $kg^{-1}$ grain N) were estimated for each season by dividing the area-weighted cumulative $N_2O$ emissions with grain yield or N content of the grain (N concentration × grain yield)*.

Line 281: I would change "scaling factor" to "weight"
Response: done

Line 283: I would change "scaling factor" to "weight"
Response: done

line 293, you indicate that random effects were introduced to account for repeated measurements. I need more justification for this: typically, one assumes that repeated measures of the same plot will be correlated to some extent. And since statistics are only conducted on cumulative variables (which is perfectly reasonable, and a good way to avoid issues of autocorrelation in repeated measures), I'm not sure why time is discussed as an effect at all.

Response: Repeated measurements (frequency) was used as a random effect on hourly N2O fluxes and mineral N, not cumulative N2O emissions (see Tables S1 and S2). N2O flux data were used for regression analyses with WFPS and mineral N.

Line 296-298: I find it rather remarkable that no data transformations were required (I've never seen field data like this that didn't).

Response: For cumulative $N_2O$ emissions, we checked the normality of residuals. Visual inspection of the QQ plots showed that residuals were near normal without transformation as shown in fig 1. See also our response to reviewer #1, who raised similar concerns.

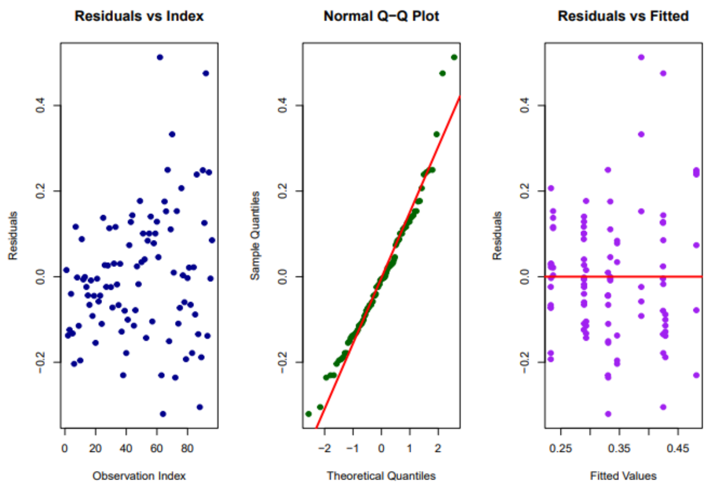



Fig 1: Residuals of cumulative $N_2O$ emissions

For N2O fluxes, The QQ plot shows deviations from normality, which is common for $N_2O$ flux data. However, mixed-effects models are robust to violations of normality in residuals (Zuur et al., 2009), especially with large sample sizes. We tested log transformation, but it did not improve model fit (AIC/BIC remained similar), so we retained the original scale for interpretability.

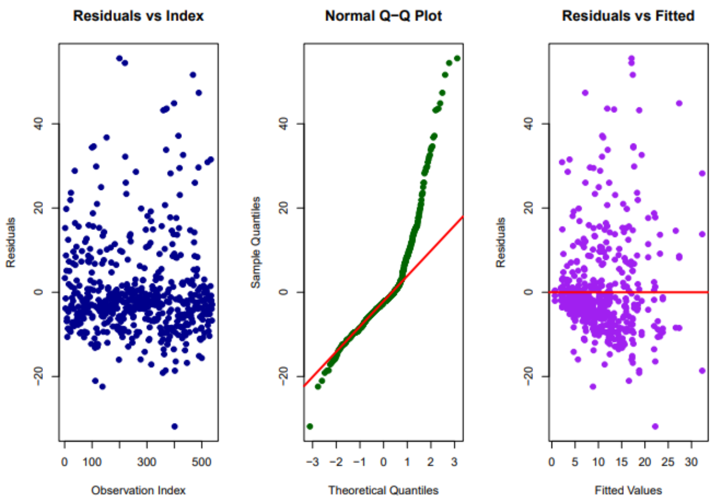



Fig 2: $N_2O$ fluxes

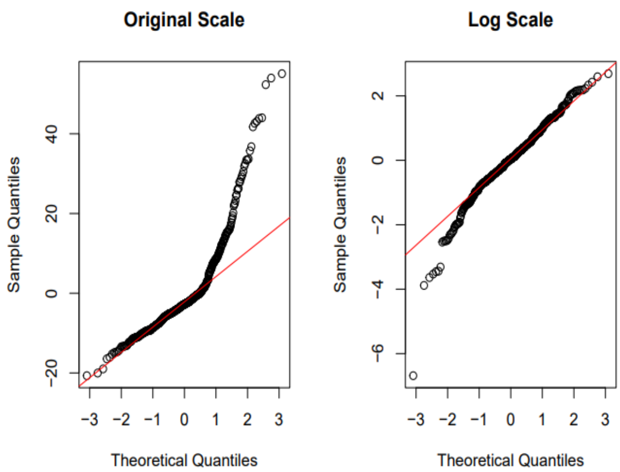



Fig 3: Original $N_2O$ fluxes vs log transformed $N_2O$ fluxes

We have rephrased and included this information in the manuscript as follows (Ln 296 - 301): We validated model assumptions by checking quantile plots of residuals against fitted values. Visual inspection of QQ plots showed that residuals were approximately normally distributed for cumulative $N_2O$ data. However, hourly $N_2O$ flux data were transformed and did not substantially improve model fit, so we retained the original scale of interpretability. Mixed-effects models are robust to mild non-normality (Zuur et al., 2009).

Line 333: change "were" to "when"

Response: done

Line 349: change "second respectively" to "second season, respectively"

Response: done, thank you.

Figure 2 & 3: I think you want to combine these into a single figure—I don't think it's necessary to compare Conv to convMM in one figure and then compare Conv to the CA treatments in a separate figure, especially since you're presenting post-hoc contrasts.

Response: In this study we tested the effect of (1) rotation and (2) tillage. This is stated in Ln 116 – 119 as follows: *Specifically, we compared crop rotation (pigeon pea – maize) with maize monocropping under conventional tillage (ConventMM). In addition, we compared pigeon pea – maize rotation under three practices, i.e. conventional tillage, CA (reduced tillage), and CA in combination with biochar (CA+BC).*

We therefore think it is relevant to present the data in two separate figures (Fig. 2 and 3). Figure 2 compares the effect of rotation under conventional tillage and figure 3 the effect of different tillage practices and biochar on mineral N.

The higher NH4+ in convMM than all other treatments in season 1 seems unexpected, and (maybe) higher or equivalent to the CA pigeon pea treatments in both years? Something that may be worth discussing.

Response: Thank you for the comment, we now discuss this finding as follows (Ln 489 – 499): *Generally, NH4+ and NO3- contents were more variable in the first season (May – October) than the second season (October – January) (Fig 1b, 1c, 3). At the onset of the experiment, mineral N was most likely from mineralisation of chemically mulched grasses having grown on the fallow for 3 years prior to the experiment. Higher NH4+ in maize than pigeon pea in the first season under conventional tillage might be attributed to differences in crop phenology. Pigeon pea is a slow starter; its nodulation and peak biological $N_2$-fixation typically occur around 80 days after sowing. Therefore, during the early establishment phase, April – June, pigeon pea likely relied on native soil NH4+. Legumes generally show strong affinity for soil $NH_4^+$ in their early growth stages reflecting the lower energy cost for its assimilation. Consequently, efficient soil N uptake by pigeon pea in the early stages reduces soil $NH_4^+$ levels compared to maize, which generally exhibits slower early-season N uptake.*

Line 388: Delete the text "Fig. 4" -- it does not present cumulative emissions

Response: deleted

Line 392: Interpreting the treatment main effect in the presence of a significant treatment x position interaction (which says that the treatment effect depends on the chamber position) is complicated—another reason to use the weighted mean flux for each plot as the response variable. But I do believe interpretation of a significant main e9ect in the presence of an interaction in an RCB design does indicate a main e9ect that is over and above the treatment x block interaction.

Response: We agree, in addition to the position effect, we have now added area-weighted cumulative N2O emissions as a response variable (Table 3).

Figures 5 and 6: I think you want to combine these into a single figure—I don't think it's necessary to compare Conv to convMM in one figure and then compare Conv to the CA treatments in a separate figure, especially since you're presenting post-hoc contrasts.

Response: In this study we tested the effect of (1) rotation and (2) tillage. This is stated in Ln 116 – 119 as follows: Specifically, we compared crop rotation (pigeon pea – maize) with maize monocropping under conventional tillage (ConventMM). In addition, we compared pigeon pea – maize rotation under three practices, i.e. conventional tillage, CA (reduced tillage), and CA in combination with biochar (CA+BC).

We therefore think it is relevant to present the data in two separate figures. Figure 5 compares the effect of rotation under conventional tillage and figure 6 the effect of different tillage practices and biochar on $N_2O$ emissions.

Table 3:

- It would be helpful to have a reminder in the Table caption that the weighted mean fluxes were used to calculate yield-scaled emissions.
- I would add something to the discussion explaining why we would care about N yield scaled emissions.
- Why are the season comparisons (capital letters) only included for ConventMM and not the other treatments?
- I would explain in the table caption why ConventMM is not included in the treatment tests (lower case letters) in Season 1 (presumably because comparing maize yields to pigeon pea yield isn't a useful comparison).
- I think it would be useful to include the P values rather than "ns." It provides the reader with more information and context for interpreting the results. A P value of 0.08 is very different from a P value of 0.5

Response: We have now updated Table 3 caption as follows (Ln 417 – 426): *Grain yield, grain N yield, weighted N2O, yield scaled N2O emissions and N yield scaled N2O emissions during the first and second rain season, in northern Uganda. When* *calculating yield-scaled N2O emissions, a weighing factor of 0.12 in planting basins and 0.88 in interrows was used in CA treatments (CA and CA+BC), and a weighing factor of 0.50 was used for both inrows and interrows in conventional treatments (ConventMM and Conventional). Means are shown with standard errors of means (N=4). Uppercase letters compare seasons specifically for a monocrop treatment (ConventMM), maize was grown in both seasons. Lowercase letters compare treatments with rotation (Conventional, CA, CA+BC) within a season. Different letters represent significant differences ($p < 0.05$), determined at 5% level using Tukey test.*

Line 434: Again, and throughout the manuscript, I would report the actual P value rather than $P>0.05$ or $P<0.05$

Response: We have now reported the actual p values

Line 434-443: I would just make it explicit here that you are not including ConvMM in treatment comparisons for season 1 since maize was grown in that treatment rather than pigeon pea, as in all the other treatments.

Response: done

Line 442: "significantly higher" needs to be changed: Maize yield was higher, but yield-scaled emissions were lower

Response: Thank you for noticing this. We have corrected the phrase, and it now reads as follows (Ln 432 - 438*): Maize yield in ConventMM were significantly higher in the second season compared to the first season, while yield scaled emissions were greater in the first season*.

Line 461: I think it would be useful to compare emissions to conventional agricultural settings as well—control plots from other experiments in East Africa. It may also be useful to provide emissions—including yield-scaled emissions--from conventional management using fertilizer as context for the conventional and CA plots in this study, while also discussing issues associated with no-input agriculture such as nutrient depletion—how long could these different practices remain sustainable?

Response: We thank the reviewer for this suggestion, and now compare our results with those reported for conventional treatments in East Africa where no fertilizer was applied. The main challenge here is to find adequate literature reporting studies that did not use inorganic fertilizers. We found only two studies which are now included the discussion (Ln 511 – 514): *Area-weighted cumulative $N_2O$ emission in the conventional treatments ranged from 0.3 to 0.6 kg N $ha^{-1}$ in the $1^{st}$ and $2^{nd}$ season, respectively. These results are consistent with Baggs et al., 2006 and Bwana et al., 2021 who recorded $N_2O$ emissions of 0.2 and 0.5 kg $N_2O$-N $ha^{-1}$ in conventional treatments without fertilization*.

To address the issue of no-input agriculture, we now include the following paragraph (Ln 577 – 590): *A key challenge for the sustainability of unfertilized agroecosystems is the management of soil nutrient balances. While biochar amendments in CA systems can effectively reduce $N_2O$ emissions and maintain crop productivity, these systems gradually deplete soil nutrient reserves. Although $N_2O$ is a powerful GHG, it represents a relatively small component of the total annual N budget, often less than 1 kg N $ha^{-1}$. The primary pathway of nutrient removal is the export of grain, which removes a significant amount of N from the system. While biological $N_2$-fixation from pigeon pea can fix up to 110 kg N $ha^{-1}$, some of this input can as well be exported from the field in the stalks, and the remaining N in the form of decaying roots, rhizodeposits and leaf litter is often insufficient to fully compensate for the N removed by the exported grain. Furthermore, the biologically fixed N is prone to rapid mineralization and subsequent loss through leaching or other pathways before the next crop can utilize it. To achieve long term sustainability, an intergrated approach is required, including targeted fertilization to prevent continuous nutrient mining and to ensure the long-term viability of the agroecosystem*.

Line 472: https://doi.org/10.1029/2020JG005742 could be helpful for the rewetting discussion.

Response: We greatly appreciate the provided literature and have rephrased as follows (Ln 465 - 468): *A similar, though smaller emission peak was observed in June when abundant rainfall terminated a dry spell. Rewetting of dry soil triggers $N_2O$ emissions likely due to increased nitrification and denitrification fueled by release of readily available N and C from dead microbial biomass (Namoi et al., 2019), and it is an important source of $N_2O$ emissions in seasonally dry ecosystems (Hickman et al., 2020)*.

Line 487: much of the fixed N may be in the harvested biomass, limiting the amount returned to soil.

Response: We have now included this in our discussion as follows:

*A key challenge for the sustainability of unfertilized agroecosystems is the management of soil nutrient balances. While biochar amendments in CA systems can effectively reduce $N_2O$ emissions and maintain crop productivity, these systems gradually deplete soil nutrient reserves. Although $N_2O$ is a powerful GHG, it represents a relatively small component of the total annual N budget, often less* *than 1 kg N ha $^{-1}$. The primary source of nutrient removal is the export of grain, which removes a significant amount of N from the system. While biological $N_2$-fixation from pigeon pea can fix up to 110 kg N ha$^{-1}$, some of this input can as well be exported from the field in the stalks, and the remaining N in the form of decaying roots, rhizodeposits and leaf fall is often insufficient to fully compensate for the N removed by the exported grain. Furthermore, the biologically fixed N is prone to rapid mineralization and subsequent loss through leaching or other pathways before the next crop can fully utilize it. To achieve long term sustainability, an integrated approach is required, including targeted fertilization to prevent continuous nutrient mining and to ensure the long-term viability of the agroecosystem (Ln 575 – 588).*

Line 494: I don't think NH4 is higher in CA systems than conventional systems—isn't the NH4 in ConvMM no different than the CA treatments? And if ConvMM is higher than conv, it doesn't follow that the mineralization of pigeon pea residues is responsible for the difference. It also conflicts with the statement in line 486.

Response: Thank you for the comment, we have deleted the sentence.

Line 514-516: this sentence is missing something, or 'nutrient cycling' should be deleted.

Response: 'nutrient cycling' was deleted

Line 540-542: You may want to rephrase this. If no treatment effects were expected, it would seem to undermine a justification for the entire experiment (i.e., readers may ask why measurements were conducted in a year when no treatment e9ect was expected, instead of at a time when the treatments would be expected to have an e9ect).

Response: Thank you, we have deleted the statement. See also our response to reviewer #1.

Line 566: delete "We established that"

Response: deleted