Methane gas emissions from savanna fires: What analysis of local burning regimes in a working West African landscape tell us

Paul Laris et al.

You - as the contact author - are requested to individually respond to all referee comments (RCs) by posting final author comments on behalf of all co-authors no later than 12 Jun 2021 (final response phase). Please log in using your Copernicus Office user ID 590183 at: https://nam12.safelinks.protection.outlook.com/?url=https%3A%2F%2Feditor.copernicus.org%2Fbg-2020-476%2Ffinal-response&data=04%7C01%7Cpaul.laris%40csulb.edu%7C83b124bd0f144b144bb108d91d134bca%7Cd175679bacd34644be82af041982977a%7C0%7C0%7C637572790994410786%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzIiLCJBTiI6Ik1haWwiLCJXVCI6Mn0%3D%7C1000&sdata=6HbGR%2FaoJsIeorokKgUdc22LnIvWfr8z20kNc0Rmkw8%3D&reserved=0**Comment types**: **AC** – author | **RC** – referee | **CC** – community | **EC** – editor | **CEC** – chief editor | : Report abuse

We wish to thank all reviewers for their comments. Below we seek to address all major comments and suggested changes as best we can and we also made nearly all minor changes recommended (a few we could not decipher).

One important point is that our study was not originally designed to statistically test the factors that cause changes in methane emissions. It was designed to determine the emissions from fires set in accordance to local burning practices on "working" landscapes. As such, the kinds of grasses and the types of savanna vegetation that burn ***differ in important ways over the course of a dry season.*** *It is thus difficult to isolate factors such as fuel moisture or grass type. A couple of the reviewers questioned, how can fuel moisture rise from early to mid-dry season. The answer, as one reviewer later noted, is because they are not the same grasses—perennials are not burned in the early season, for example, thus we have no emission values for them during that season and this explains how fuel moisture values increase in mid-season (when perennials are burned), before declining in late-season.*

We did the following analysis on the 36 fire sample set:

*Bivariate statistical analyses were performed to test the significance of the difference of means (t-tests) in CH_4EF by season (EDS and MDS) and by fire direction (head fires and back fires) and in MCE by season and fire direction. F-tests established the similarity of variances, all t-tests were done with pooled estimates of variance. These were done in the OpenOffice Calc spreadsheet (Apache Software Foundation 2021) and PAleontological STatistics (Hammer et al. 2001), with effect sizes (Cohen's d) and post-hoc power calculated in G*Power (Faul et al. 2009). We used bivariate regression analysis to look for correlations between the two dependent variables—methane EF and density—and independent variables—Byram's fire intensity, percent grass biomass, total fuel moisture, and Viney fuel moisture (a function of ambient temperature and humidity). These were done in Calc and power was estimated in G*Power*

All detailed findings are in the revised manuscript.

- **RC1**: 'Comment on bg-2020-476', Sally Archibald, 10 May 2021 reply

  The paper: "Methane gas emissions from savanna fires: What analysis of local burning regimes in a working West African landscape tell us" presents some useful data on fire behaviour and methane emissions for a range of fires in West Africa. The authors aim to replicate the types of fires set by people in "working savanna landscapes" so as to provide data to test ideas around manipulating fire regimes in these landscapes to reduce GHG emissions.

  They demonstrate that because amount of biomass consumed, the moisture of the fuels, and the weather conditions all change through the course of a fire season, it is not simple to predict the emergent effects on MCE or EF. They also argue persuasively that any attempts to change the way that fires burn currently in these systems are unlikely to have the desired climate mitigation effects.

  I find huge value in the data they present, but it would have been even more useful if they could have moved beyond the crude discussions currently playing out in the literature about "early" "mid" and "late" season burning, to derive empirical relationships between the various factors affecting EF and methane emissions. If they could have presented regressions of methane emissions factors and combustion completeness against fuel moisture, rate of spread, and weather conditions then it would be a step forward in terms of understanding the trade offs between burning when fuels are moist but CC low, or when fuels are dry but CC high, and what the emergent methane emissions are under different conditions.

  We ran regressions for individual variables and found no correlations except for Byram's fire intensity. As we note in the text, we believe that the reason intensity correlates with methane emissions is that fires burning with higher intensity burn into the juvenile tree canopy where combustion releases methane. We observed this numerous times in the field. Methane emission spiked at over 5000 ppm when burning green leaves on small trees. Note that one issue is that we have gas samples for only 36 of our total sample of fires, so our "n" is low.

  I found the methods quite hard to follow and indicate where I was confused in my detailed comments below. I careful re-write aiming to help the reader through the experimental design would greatly improve the manuscript.

  Yes, we have revised methods to clarify per several reviewers.

  Detailed comments:

Abstract line 27-29: I thought you recommended using seasonally-varying EF for W. African fires? Not sure that your conclusions in the discussion and your conclusions here totally align. Perhaps have another look at what you want the key messages of this paper to be.

Yes, I agree, we will remove the single value and keep to the seasonally adjusted recommendation.

Line 107 - "emit less" *Done*

Line 109 - Reference Russell-Smith et al JEM 2021 - this paper works hard to suggest that a shift to EDS fires is an appropriate approch to reducing GHG emissions from fire in Africa, but ends up having to conclude that there is uncertainty and that fuel moisture (in particular vegetaiton greenness/curing) is key and has not properly been resolved.

Yes, we added this reference and a comment on it. I find it very odd that R-Smith argues that Methane EF did not vary over the dry season for "uncured" fuels. This is to be expected as it is fuel moisture that theoretically causes lower combustion efficiency.

We also added a section discussing how this relates to the issue of EDS/LDS burning. My conclusion is that the RS study compares MDS and LDS fires as they compared emissions values for cured grasses and leaves had already fallen from trees—this is not EDS burning in my view. I completely agree we need a new system of fire classification in savannas and commented on this in the text..

Line 112-114: I agree completely. However, I am still a bit vague about what the "key factors noted above" are. Can you make a table or a text box or something where you list the key factors affecitng emissions factors and give an indication of the level of knowledge/data around them? that would be very helpful.

Yes, I added two paragraphs with specifics on these factors, which are biophysical and human oriented and added a new figure. I created a "text box" table and placed this in the appendix. It can be moved into the main body if nec.

Line 136 over 750mm as you say before, or over 900mm?

Yes, we changed text to read 900 throughout. Mesic is defined as above 750, our sites are all above 900, clearly mesic.

Line 162. So you only account for curing in your moisture content measurements? What about variation due to weather conditions or position on the ground? ... Data to show that curing over rides these others?

We weighed uncured fuels and use Viney to estimate moisture in cured fuels.

164: more details on the Viney method? How did you dry it and did you determine fuel moisture content?

The Viney method is used to determine the fuel moisture of cured fuels as a function of weather conditions. I added the formula and explanation from previous pub.

198 It is usually better to give fires a chance to get some energy before measuring rate of spread. Did you always light in the direction of the wind?

Yes. As noted we burned in both directions (fire type). Measurements were taken once the flaming front developed, which we explain in text now

170 what 10m plot you haven't explained this yet?

OK, we changed order to describe plots first

200 repetition? *Corrected*

205. Oh, I see, here is wher you explain your calculation of fuel moisture. Can I suggest that you rework this section to make it a bit easier to follow?

Yes, we rearranged the text to make clear.

also, it seems a shame to have to use a model then you are out in the field with a scale and some real fuels....I am not sure leaf litter and grass litter respond to environmental conditions that same way as some are lying on the ground and others are standing up. Somewhere it will be nice if you present the range of fuel moisture from weather and the range of fuel moistures from curing that you calculated - I am in a discussion with global fire modellers about which of these sources of moisture is most important to get accurately and your data will be useful for this discussion

These values are in the table along with standard deviation to show the high level of variance due primarily to grass type (perennials vs. annuls). This is very complex and I wrote about this in the new section on complexity. The key in my view is: Critically, the seasonal timing of a fire not only impacts the fuel moisture of grasses, but also that of leafy biomass which burns green and standing on small shrubs and trees in the early fire season, but falls to create a bed of compact and less aerated fuels by the late dry season. **As such, the incomplete combustion of leafy biomass is a function of high fuel moisture in one season, and low oxygen conditions in another, with unknown implications for gas emissions**.

I think the latter point is understudied, yet important. It may explain why Methane EF rises with fire intensity but CO EF does not.

209 repetition again? *Corrected*

213 - it would be nice to have more detail on how the smoke was collected? Did you wait for the fire to be fully lit? How did you get the canisters into the smoke? Did you manage to do multiple samples per fire?

All fires were sampled a single time after the flaming front had developed (approximately two-thirds of the way through the burn plot). Note that we simultaneously collected data from a hand held device with an intake nozzle, but are still evaluating these results.

213 - also mention here that you couldn't collect LDS EF

Done, we couldn't collect with canisters (too expensive to ship them to the US) but did with the analyzer to be forthcoming…

252 I think you have your units confused in this equation or in the text below. Is it BA percent or Ha?

Correct, percent does not belong in sentence

Overall however I support your proposal to alter the equation. I would argue that the weather variables are accounted for by seasonal variation in CC however so it is only the variation in fuel curing/moisture that needs to be additionally included as an adjustment for EF.

Not sure I agree, weather, type of grass, mix of fuels all vary by season. I have made this more clear in the new section in the intro.

249 - I think you will have to be more clear about how you distinguish between BA (burned area) and BE (burn efficiency). As someone who has watched a lot of savanna fires I am assuming that your BE term refers to the "patchiness" of the fire - i.e. how much of the area that the fire spread through actually encountered a flame. Often in cool early-season burns there are patches of unburned fuel inside the burn scar. If this is what you mean, then I think most people account for this by taking more measures of biomass before and after the burn, and assuming that some of these biomass measures will not be burned - i.e. some of their measures of combustion completeness will be 0.

I think the distinction is important and is a scale issue as well. We changed BE to Burn Patchiness (BP) throughout the text. BA is ideally determined from imagery and it needs to be adjusted for patchiness, which is a fine scale phenomenon.

258. I don't understand....see comments above about clarity and distinguishing between BE and BA. But now you say this can't be determined? Can you explain your logic more clearly? (PS Some people use burn severity to indicate how patchy a fire is - either way, it might be good to suggest a way for people to measure this or calculate it).

I agree, but patchiness is scale dependent and our approach considers burned area will be estimated using Landsat or a like imagery, as such, patchiness on the order of meters burned in a pixel size plot is critical.

272.I don't see how moisture content can go up when plants are curing and weather driven moisture is going down?

Type of grass is key here…the reality is that people often burn dryer grasses first! And, more critically, perennials will not burn in early season and folks would not do this ;)

280. Where is table 1?

I placed tables in the text and at end for readability

291 - aha! so the moisture content goes up in the Mid dry season (see question above) because it is perennial grasses that burn (presumably annual burn in the early dry season)? This is very important information, so needs to be clarified. I would like to develop some more processed-based way of assessing curing and moisture content that could easily be integrated by remote sensing and modelling products to account for the variability you describe here, so being clear about these processes is important if your data are to be useful

Yes, this too is in the new section.

Line 306 - this is the first time you mention MCE - can you define and give units please? (you could do this in line 64-66 where you define emissions factor.

Done

Table 3: indicate that MCE is a ratio so no units

It is a commonly used ratio in emissions studies.

Results: I don't see anywhere where you report on the curing of the grasses in the different seasons. There are two sources of fuel moisture, and your study is able to distinguish between these. It would be good to know which of these is driving the changes in CC and BE through the season so is it possible to present these data?

*Interestingly we did not find any correlation between fuel moisture and CC or Patchiness. I am sure there is some relationship, but the variation in fuel moisture of cured grasses is very small as are variations in wind speed (another factor that did not have dramatic impacts. Please see Laris et al 2020 (based on the 97 fires); we found "*Calculated fuel moisture content declined over the course of the dry season from a mean of 4.81% in the EDS, to 4.59% and 3.65% in the MDS and LDS respectively; however, when combined with the measured mean cure rates for wet grasses, the total fuel moisture was 8.40% in the EDS,12.04% in the MDS, and 3.65% in the LDS." *We Note these are mean values with high variation for the uncured grasses (not cured ones). For cured grasses the small drop of 4.81% to 4.59% from early to midseason is relatively insignificant and reflects the drop in T and H during this period. We also concluded:* Mean intensity was lowest for the middle season fires and highest for the late season fires. Minimum fire intensity increased over the fire season except for a sharp drop mid season, while maximum intensity progressively decreased. Seasonal values were highly variable. Fire intensity was moderately positively correlated with scorch height and more modestly correlated with visual efficiency, but only marginally correlated with combustion completeness. Average combustion completeness increased weakly as the dry season progressed.

Results: as a way to move this debate forwards would it be possible to try to develop some predictive models of emission factors in relation to some of the key drivers you mention here? i.e. plot regressions of EF vs biomass, fuel moisture, and rate of spread for example. Then we can start to get at the processes driving the patterns that you find.

We found that methane emission EF (and emissions density) were a function of fire intensity and % grass biomass in total biomass. Theoretically, we argue that this is due to the fact that more intense fires (especially head fires) burn fresh leaves in the tree canopy releasing methane. Since green leaves fall from trees in mid-season, theoretically more green leaves burn in the EDS.

Line 393 - well, it is not that different from your results - you show that EDS produce slightly lower methane emissions? But I think this entire debate is confused by different definitions of "early" "mid" and "late" dry seasons. I would advocate for using a more empirically-based approach -see general comments above.

We completely agree on this! We added a section in the discussion explaining the many issues with EDS/LDS. We feel it helps to add MDS, but without definitions for these different terms there is no similarity across savannas.

I would argue that leaf fall might be a useful determinant of mid-season for savannas.

> Line 365 - agreed: your data show a very small change in methane emissions over the year and you can explain this by contrasting patterns of fuel amounts vs fuel moisture. How about testing whether you can explain this?
>
> *Methane EF is more a function of fire type than season or % cure rate according to our results, but they are incomplete.*
>
> Line 374 - agreed - as someone who has tried for years to get managers to burn savannas during conditions when fire is unlikely to spread I know that people on the ground know when fires will spread, and are reluctant to try to burn when conditions are not suitable. Theoretically, however, it would be good to have a model which predicts how CC vs fuel moisture interact to drive total emissions over the entire range of CC values.

I do not think you can do this without accounting for other factors such as grass type, savanna type and fire type.

> Line 382 - ah, finally you define BE and link it to burn patchiness. I would like to see this further up (or alternativley I suggest replacing the term BE with patchiness to avoid confusion with other terms which include the word "efficiency" in this paper.
>
> *Changed to Burn Patchiness (BP) and defined earlier*

Line 385 - it is still not clear to me why BE and CC cant be subsumed into one measurement. Combined they represent the proportion of biomass consumed in the area of the fire scar. Even if you measure them separetly in the field, there is not reason to have two parameters in your equation (for example, when it comes to fuel moisture you just have one value, not the two that you measured in the field). So I am interested to see how they change independently over time, but no need to have two terms in your equation (I am also interested to see how curing and dead fuel moisture change independently over time)

Here I draw upon the work of R-Smith who points out that emissions estimates use RS imagery and thus we need a measure of "patchiness" I agree as I explained above.

Line 392 - very important point: this is probably why you had less large increases in methane emissions over time.

Line 414 - 416: agreed. I think there is no one size fits all with these policy recommendations: they need to be made with reference to the particular ecological and social conditions and you demonstrate this very clearly here.

Line 420 - good point: increasing the area burned in the EDS requires that people burn fuels that are not yet fully cured, so this will immediately increase EFs. So in the W-African situation you describe there are no social, ecological, or biogeochemical reasons to do so.

No, I would disagree as I have argued elsewhere. If you look at how people burn most fires are early by definition of fuel moisture (recall Le Page et al 2010 find that West African's burn prior to the peak in fire index by about 30 days…by definition, this is early). The question is whether they can or should burn even earlier and what impacts this would have.

438-440: lovely - it would be nice to bring this mechanistic explanation into the abstract more clearly

Sally Archibald

General comments #2

This paper is potentially an important field-based study focusing on methane gas emissions that can provide useful savanna fire characteristics values suitable for West Africa. The authors quantified, compared, and analyzed these values by fire type and by seasons. Finally, the authors linked their findings to some practical fire management implications. I have a few specific comments that can help to improve the paper. I also suggest the authors carefully check if the reported values in the text are consistent with those in the tables.

Specific comments

Line 28: the value of 0.862 should be better clarified in the text, how is it calculated?

We deleted reference to this average value from the text.

Line 80: From a fire modeling perspective, emission factor was usually simplified defined in most global fire models(1, 2), I am thinking if you can add some "discussion" about the possible implications of your study to the fire modeling field to broaden the interests of this paper.

Good, importance here is change in emissions over time and link to policy

Line 138: what is the relationship between cool, hot dry season and early, middle, later dry season? *We clarified importance of this in text. During the cool dry season, the Harmattan wind blows strongest. It is desiccating and thus has strong impact of fuel moisture drop as well as fire weather. Note that the winds are strongest in mornings and weak in afternoon when local people tend to set fires.*

Line 157: clarify what is "time of day", ignition time? *Clarified in text.*

Line 175: more description of "burn efficiency" is needed *Clarified in text see above, changed to "patchiness"*

Line 226: delete a replicated "in" *done*

Lines 243, 252: The same symbols and/or units (for example, "Area" in equation 5 but "BA" in equation 6) should be used in equations 5 and 6 to better see their difference.

Corrected in text

Line 271: Where did these values come from? I cannot see 4.62%, 4.09% in Table 1.

The table includes the "total" fuel moisture, cure rate + Viney

Lines 306, 308: Similar to the previous comment, 3.47 or 3.3? 2.5 or 2.9? Please check all of the values carefully. If I misunderstood, please clarify this in the text.

Corrected

Line 308: The authors should also be consistent with the number of digits after the decimal point in the text and in the tables.

Done

Line 311: 313.4 kW/m to 109.0 or 366.9 to 124.8 in Table 3? Did I misunderstand something?

Corrected

Line 425, 427: 2 or 3 digits after the decimal point? should be consistent.

Corrected

Tables 1-3: I suggest using "early dry season (EDS)", "mid-dry season (MDS)", and "late dry season (LDS)" to replace "early", "middle", and "late", respectively *Done*

1. S. Hantson *et al.*, The status and challenge of global fire modelling. *Biogeosciences* **13**, 3359-3375 (2016).
2. F. Li *et al.*, Historical (1700–2012) global multi-model estimates of the fire emissions from the Fire Modeling Intercomparison Project (FireMIP). *Atmos. Chem. Phys.* **19**, 12545-12567 (2019).

We a*dded a paragraph on this point to discussion and the first reference*

Citation: https://doi.org/10.5194/bg-2020-476-RC2

Review #3

This study conducts several site experiments using an approach grounded in the burning practices of people who set fires to working landscapes and collect fire-related data in West African Savanna. They find that in the dry season, methane emission factors ranged from 2.86 g/kg to 3.71 g/kg and methane emission densities ranged from 0.981 g/m^2 to 1.102 g/m^2. Overall, the results improve estimates for savanna fire emission and have important implications on earth system model development and policy making. However, I have some concerns about the presentations and hope the authors can further improve this study.

My major concern is that the connections between fire emissions and environmental factors are not explored in depth. For example, the study measured simultaneous meteorological conditions including temperature, air humidity and wind speed. How these parameters affect the fire emissions of CH_4? The authors need to plot some figures to show the relationships between emissions and weather conditions, and to identify the possible driving factors determining the differences of emissions at different stages. To make the results more robust, the author should add more discussion about (1) the possible causes of the differences in fire-related variables at early, mid-, and late dry season and (2) reasons for different change trends among variables in dry season. In addition, the representativeness of the selected two sites and uncertainties of experiment methods need to be discussed explicitly.

We added a section on statistical analysis, this was not the main purpose of our paper as we state above. The purpose was to present data on more realistic fires (based on actual human practices and landscapes). We do present the findings of the statistical analysis in the revision.

This manuscript is full of tables, one or more of them can be converted into figure (figures) to make the information more intuitive.

We removed one table and added several figures

Pay attention to the consistency of tenses in the manuscript. For example, "finds" in Line 22 and "found" in Line 23.

Done

Acronyms should be marked in the main body at the first time, not just in the abstract. For example, the author does not specify what does "MCE" stands for in the main body.

Done, added section on MCE

Line 191-195: The unit of *I* in *equation (1)* is wrong. The unit of product of *H (kJ/kg)*, *w* (kg/m^2), and *r* (m/sec^1) might be $kJ/m/sec^1$.

We doubled checked units, they are correct

Line 272: "12.04% and 3.65% in the LDS" might be "12.04% in the MDS and 3.65% in the LDS". Reviewer: Comments on ms bg-2020-476-manuscript-version1

Corrected, thank you

Methane gas emissions from savanna fires: What analysis of local 2 burning regimes in a working West African landscape tell us

Reviewer #4

General Comments:

The ms tackles an interesting issue and a globally important one at the same time. Improve estimation of methane (one of the most important GHG) in the fire continent (Africa) is a very interesting study. Moreover, this study will help to improve estimation of methane emitted during different fire seasons in West Africa (with high fire activity). This study is also very important for management purposes, since it is helpful to mitigate GHG emission through the best fire season applying.

The manuscript will benefit from a revision of the plan, data analyses, results and discussion sections. Data analysis section have to be rewrite. I suggest that Fire intensity and fuel moisture formula to be include in methods section.

We included these in the methods under specific sub-heading

Surprisingly, all the result tables do not include statistical analysis. Statistical analysis must be done and explain in data analysis section. Arguments in this manuscript are based on the comparisons in the table 1, 2 and 3, whereas no statistical analysis have be done. In discussion section, I suggest author to integrate the many important other works undertaken in West Africa savannas on fire ecology in Burkina Faso, Côte d'Ivoire, Togo, ….

We added an entirely new section on statistical analysis

We have reviewed the data from West Africa elsewhere unless there are specific relevant studies to suggest (Laris et al 2017, 2020).

The conclusion is too long, you have to keep the main results only.

We shortened

Specific Comments:

- Line 19: Among the 97 experimental fires, how many for EDS, MDS and LDS? I suggest the precision of the number of experimental fires considered for methane emissions.

We revised and added these values to the text. Note in the revision we only use data from the 36 fires and refer to the 97 fire study in the discussion section to add clarity.

- Line 28: I suggest author to propose this value for the study area as they suggest themselves and it is true that emissions are strongly influenced by the vegetation/fuel type, fire season, weather conditions,…that defer strongly among West Africa savannas.

Unclear comment

- Line 119 to 133: this part have to be move in methods section. A resume part could be kept there.

Disagree, "working lands" is a fundamental concept and not a description or method. It is critical to distinguishing our study from other works.

Line 137 and 119: standardize according to the precipitation in your study area; above 750 or 900 mm?

Clarified as 900 mm see above

Line 150 to 153: The clear description of vegetation (main tree and grass species, density, savanna types …) is necessary as emissions depends on it, and as precise by the authors themselves at line 61-62. Moreover, in abstract (line 20) authors suggest that they considered these aspects in their study for better estimation of emissions.

Unclear comment. We added a section on the importance of vegetation variation (see above)

- Move from line 178 to 187 (Plot design) at line 155 (before Data collection)

OK, Done

- Plot design section (line 178-187) have to be clearly describe. How many plots? How many for EDS, MDS and LDS. How many repetitions for statistical purpose? The dimension of each plot for back and head fire? The distance between plots and sites? Is the seasonality define for each site based on long term data, as one site could burn during the EDS one year and during the MDS the next.

These numbers are in the tables

- Line 198: Why do the amount of ash is take into account for the calculation of amount of fuel consumed since usually the pre-fire and unburnt fuel are consider. Ash being a part of fuel consumed.

We followed the methods of Russell-Smith et al (2009) and others here. Ash represents material that is not completely combusted

- Line 214: for the 36 experimental fires used for emissions estimation, how many were in EDS, MDS and LDS, back and head fires?

We added these to the text, these numbers are in the tables

- In all the result section, authors have to based commentary on clear statistical analysis. Statistical analysis conclusion (for example Tuckey HDS test) could be include in table 1, 2 and 3 (comparison of different parameters between fire seasons). Sections 3.2, and 3.3 are concerned. For example (line 286): the characteristics of the fires vary by season…; the BE increased as the dry season progressed, and elsewhere…this sentences have to be based on statistical analysis showing statistical difference between seasons for BE.

We added a new section on statistical analysis

- Line 286: decline BE as you use it for the first time in result section. I suggest to do it in all the manuscript…..(line 305: for MCE…).

Clarified in text, see above

- Line 286 and table 1 and 2: The lower fire intensity in MDS plots could be explain by higher total fuel moisture (table 1). But I don't understand that while total fuel moisture content is higher in MDS, fire rate of spread slower in comparison to EDS and LDS, the burn efficiency could be higher in MDS than EDS. Could you explain that result please? May be that is not statistically different? I read your explanation from line 405 to 411 and I'm more confused. At line 409 you argue that during EDS grasses are often too moist to carry a fire, whereas your data showed highest moisture content during MDS.

Yes, this is correct. Perennial grasses cannot burn in the EDS and they are set on fire in the MDS and LDS according to local practice. Only annual grasses are burned in EDS by custom and practice. See above, note that we are not burning the <u>*same grasses or vegetation types for each season.*</u> *The study is based on local burning practices.*

- Line 334: may be delete the parenthesis.

Done

- Please add dry (1) between early and season at line 374 and 392, (2) between the and season (line 379)

Not clear

- Line 410: you could add the other important and recent studies on fire characteristics in West African savannas.

Such as? I am not aware of other published studies for W Africa on emissions, we reviewed studies on fire characteristics in Laris et al (2020).

Your conclusion have to be reduce please.

We shortened conclusion