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.

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 same grasses or vegetation types for each season. 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