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Comment on bg-2020-476

Sally Archibald (Referee)

Referee comment on "Methane gas emissions from savanna fires: what analysis of local burning regimes in a working West African landscape tell us" by Paul Laris et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-476-RC1, 2021

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.

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.

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.

Line 107 - "emit less"

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.

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.

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

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?

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

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?

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

200 repetition?

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?

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 discusison

209 repetition again?

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?

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

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

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.

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.

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).

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

280. Where is table 1?

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

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.

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

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?

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.

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 defintions of "early" "mid" and "late" dry seasons. I would advocate for using a more empirically-based approach -see general comments above.

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?

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.

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.

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)

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.

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

Sally Archibald