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Interactive comment on "Seasonality of greenhouse gas emission factors from biomass burning in the Brazilian Cerrado" *by* Roland Vernooij et al.

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We thank Reviewer #1 for the time and effort in assessing our manuscript. Please find below our point-to-point response to the review of referee #1. The revised text and updated figures are included in the updated manuscript.

1) Line 17: You used the word "seasonality" along the manuscript, but I suggest the use of "intraseason variability" instead, since you are looking at the variability of emission factors within the dry season.

While the term "seasonality" is often used in literature regarding measurements within

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the dry season (Archibald et al., 2010; Hoffa et al., 1999; Meyer et al., 2012), we agree the 'intraseason variability' better captures the fact that all our measurements occur in the dry season and we have modified this throughout the manuscript, including the title.

2) Line 21: I suggest that you include the years in which the measurements took place.

We have added the years to the abstract

3) I suggest including ranges of observed EF somewhere in the abstract.

Given that we collected a very large number of samples of which a few were in non-representative humid grassland and that especially the N2O measurement has a substantial amount of noise presenting the range here would be misleading: Observed EFs ranged from 1039 to 1930 g kg-1 for CO2, 11 to 525 g kg-1 for CO, 0.1 to 7.6 g kg-1 for CH4 and -0.9 to 2.1 for N2O. Instead, we present the average and standard deviation now in the abstract to: Observed EF averages and standard deviations where 1651 (+/- 50) g kg-1 for CO2, 57.9 (+/- 28.2) g kg-1 for CO, 0.97 (+/- 0.82) g kg-1 for CH4 and 0.096 (+/- 0.174) g kg-1 for N2O.

4) Lines 22-23: are these differences statistically significant? According to Table 3, they are not, considering a 95% significance level. Therefore, your conclusions should be that, overall, observations did not show a significant difference between EF at LDS and EDS.

We agree that the statistical significance should be mentioned in the abstract and have included it in the revised abstract.

5) Page 2, Line 8: do you mean 10% of global savanna fire emissions? It is not clear in the text.

Indeed, we have revised the text to "10% of global savanna fire emissions."

6) Page 3, Lines 25-26: Are there updates on the zero-fire policy in the Brazilian cer-

rado? Is it still a current policy?

We changed the sentence to "until the first integrated fire management approach for some protected areas was launched in 2014, a 'zero-fire' policy had been maintained in the Brazilian Cerrado for decades"

7) Are you aware of similar UAV-based fire emission measurements, elsewhere? If so, you may cite it, and compare the sampling strategies.

To our knowledge, this is the first published study using UAV's to estimate fire emission factors. Page 5,

8) Line 16: here you refer to minimum daily temperatures?

When revising the text to address the reviewer's comment (including this one) we realised that the role of temperature is minimal and may only lead to confusion. Hence, we have excluded that sentence now

9) Page 5, Line 21: include a reference to Fig 1b.

Added a reference

10) Page 5, Line 22: How was the burned area monitored? Is there a reference for the data in figure 2a?

The burned area is calculated from MCD64A1-C6 (Giglio et al., 2018). It represents the average BA over the 2013-2018 period area within EESGT. We have added the reference to the caption.

11) Page 5, Line 32: Was the RH measured at the surface? Or on board at the UAV?

This RH is the value measured by the UAV (15m) during the background sampling. We have clarified this in the revised text.

12) Page 6, Line 26: What was the sampling flow of the gas analyzers?

For the CO_2 and CH_4 this is 1.3 L min-1, for the CO and N_2O this is 0.25 L min-1. We have added this information in the revised text in section 2.4.

13) Page 8, Line 20: Consider moving part of this paragraph to section 2.1. You might refer to Table 1 and Figure 1b (which was not referred to in the whole manuscript).

We have added references to Table 1 as well as Fig 1b. Though we agree that this also fits well with the study area description, it is also important to mention it here. We have also added a reference to Fig 1b in section 2.1.

14) Table 3: Include in the table caption the EF units.

added

15) Page 9, Line 16: Where are the MCE results? I suggest that you include statistics for MCE in Table 3 or as a new boxplot in Figures 5-7.

Since MCE is very closely related to the CO EF, we chose to only present 1 boxplot figure to avoid 2 graphs with the same information. The CO EF was chosen in our case because it is a more natural introduction to Fig. 8. The graph below compares the spread in CO EF and MCE. However, we do agree that adding the MCE is important and have added a column with MCE to Table 3 as suggested by the reviewer. Since the spread in MCE will be the same as the spread in CO EF, we don't feel that adding an additional boxplot would add much more information.

16) Page 9, Lines 21-22: What if you choose a lower significance level, for example, 90%? Would some of the differences between LDS and EDS be significant, with $p<0.1$?

This would not change the significance of the results. We have changed the significance level to 90% as this is more informative and changed the sentence to: "only the slight differences in open grasslands and the 14% and 34% increases in N2O EF for open cerrado and typical cerrado, respectively, were statistically significant using a two-tailed t-tests with unequal variance at a 90% significance level."

17) Page 9, Line 29 and Figure 5: Your EF values for CO and CH4 were in the lower

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range of previous observations at savannas (Andreae, 2019), as shown in Fig 5. Do you think that the lower EFs are characteristic of Brazilian cerrado? Or characteristic of EESGT? Please comment on that.

Our EF's were low also compared to earlier measurements from Cerrado vegetation, particularly the CH4 EFs were low. Ferek et al. (1998) found an averaged CH4 EF of 3.7 g kg-1 and CO EF of 57 g kg-1 and Ward et al. (1992) found CH4 EFs ranging from 1-1.6 g kg-1 and CO EFs ranging from 46-70 g kg-1. This indicates that the findings may not be representable for the larger Cerrado. We have added text addressing this in section 4.2: 'Also compared to earlier measurements from Cerrado vegetation the CH4 EFs were low; Ferek et al. (1998) found an average CH4 EF of 3.7 g kg-1 and Ward et al. (1992) found CH4 EFs ranging from 1-1.6 g kg-1. This indicates that more research is needed over ideally a larger range of Cerrados and regions to understand what drives this variability.

18) Page 10, section 3.2: How about MCE? Did you observe differences related to vegetation type and fire history?

Differences in MCE would be more or less similar (though opposite) to the CO EF. In the revised manuscript we emphasized this in the text: "Fire history had some effect on the burning efficiency. We found a decrease in the CO EF and CH4 EF (and thus increase in MCE) with increasing time between fires ranging from 2 to 4 years in samples from the open grasslands (Fig. 7)." As we mentioned earlier in our response, we have also added an additional column to Table 3 with the MCE results.

19) Page 10, Line 16: Do you mean propagation of error, instead of prorogation?

Yes, corrected.

20) Page 10, Line 16: It would be reasonable to show the overall uncertainty on CO2-eq EF, instead of showing only N2O uncertainty, as you did in Fig 8. Also, it is not clear whether you are talking about data variability (standard deviations) or about measure-

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ment/calculation uncertainty. Please clarify.

We have changed Figure 8 and the error bar now represents the combined standard error of the mean (propagated into CO2-eq emissions) of all species.

We also made changes to Section 3.3: “The black error bar represents the propagation of the combined standard error of the mean for each specie to the net CO2-eq emissions. 30% to 60% of this error comes from the propagation of the uncertainty in N2O EFs.”

21) Page 11, Line 2: You might state that the difference is small and not statistically significant (considering a level of significance of 95%).

We have added this to the discussion

22) Page 11, Line 8: I miss the presentation of MCE values in your figures and tables.

We have included an additional column to Table 3. As mentioned before, since the MCE would more or less be the inverted graph of the CO EF, adding 3 extra box plot graphs would not add much additional information.

23) Page 11, Line 23: Fig 8 shows CO2-eq EF, and not MCE. Please check the figure reference. You are correct, we have corrected this.

24) Page 11, Line 31: The lower CO and CH4 EF, as compared to the literature, is more clearly depicted in Figure 5. I suggest that you refer to Fig 5 instead of Fig 9.

In the revised manuscript we now refer to Fig. 5 to illustrate the lower CO and CH4 EF compared to the literature. We changed the text in the revised manuscript: ‘Overall, the weighted average CO and CH4 EFs for these combined savanna fuel types were lower than most of the existing literature on savanna fires (Akagi et al., 2011; Andreae, 2019) (Fig. 5). The discrepancy with literature is particularly strong for CH4 as shown in Fig. 9 where the individual CH4 EF measurements are plotted as a function of MCE measured for the Cerrado vegetation types.’

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25) Page 12, Line 11: What is RSC? You did not define it in the text. It refers to Residual Smouldering Combustion and is now spelled out in the text

26) Page 12, Line 11: In this paragraph you refer to Fig.10, but I do not see a discussion about the relationship between CH4 EF and RH, which is the main feature in Fig. 10. It would be better to discuss the spread of CH4 EF during EDS and LDS based on the boxplots of Figure 5.

We have adjusted the reference to Fig. 5 and included a reference to Fig. 10 later in the section, where we discuss the difference in CH4 EF spread compared to EMR and RH.

27) Page 16, Line 8: Improvements in which software? Could this adaptation affect significantly the results and the comparison of measurements taken in 2017 and 2018?

This improvement relates to the use of an algorithm to account for some of the background noise in the measurement. This only works when the samples are analyzed with background measurements in between long enough to identify the noise. This was not the case for the 2017 measurements. The measurement drift appears to be a random oscillation around zero, possibly related to internal heating and pressure cycles in the analyzer. When the absolute measurement is low this measurement noise may become significant (the effect will be larger in 2017 than in 2018). However, for the weighted averages this should not significantly affect the results.

28) Check the numbering of the subitems in section 4.

We resolved the section numbering problem.

29) Page 15, Line 31: should refer to Fig 11 instead of Fig 10.

Assuming you mean the Fig. 10 reference on Line 28, we changed it to Fig 11.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-86/bg-2020-86-AC1-supplement.pdf

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