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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.

Anonymous Referee #1

Received and published: 24 May 2020

General comments: The manuscript shows results from an original dataset on biomass burning emission of green house gases at a Brazilian savanna reserve. The authors investigated the intra-seasonal variability of emission factors (EF), as well as the influence of vegetation type and fire history. Results are relevant, well presented and well discussed, and I recommend publication after a minor revision.

Specific comments:

Abstract:

* 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

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factors within the dry season.

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

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

* 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 signifficant difference between EF at LDS and EDS.

Introduction:

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

* Page 3, Lines 25-26: Are there updates on the zero-fire policy in the Brazilian cerrado? Is it still a current policy?

Methods:

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

* Page 5, Line 16: here you refer to minimum daily temperatures?

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

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

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

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

* 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).

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Results:

* Table 3: Include in the table caption the EF units.

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

* 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 $p<0.1$?

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

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

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

* 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 measurement/calculation uncertainty. Please clarify.

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

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

* Page 11, Line 23: Fig 8 shows CO2-eq EF, and not MCE. Please check the figure reference.

* 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.

* Page 12, Line 11: What is RSC? You did not define it in the text.

* Page 12, Line 11: In this paragraph you refer to Fig.10, but I do not see a discussion about the relationship between CH_4 EF and RH, which is the main feature in Fig. 10. It would be better to discuss the spread of CH_4 EF during EDS and LDS based on the boxplots of Figure 5.

* 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?

Technical corrections:

* Check the numbering of the subitems in section 4.

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

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