

Interactive comment on “In situ measurements and modeling of reactive trace gases in a small biomass burning plume” by M. Müller et al.

M. Müller et al.

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We thank reviewer #2 for her/his feedback but must clearly state that most of her/his criticism appears to be unjustified. Given that reviewer #1 reassured us in our conviction that we explained things properly (“The results are extremely well presented in a very concise manner”), we invite reviewer #2 to carefully re-read the manuscript and find most of her/his points already being addressed in the text.

COMMENT: 31508, line 17: Unclear why you can not use linear regression. This is well explained in the text: “Given that the P-3B spent about two seconds in the plume during fire overflights and that CO was only measured at 1 Hz, it was not possible to perform linear regression analyses, X vs. CO, on data from individual plume intercepts.”

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REPLY: We do not feel the need to explicitly state that two seconds of measurements at 1 Hz result in two data points and that a two point linear regression is inadequate.

COMMENT: Line 23: I don't see 4 points. You have averaged the two values inside the plume and a value outside of the plume. This is two points. REPLY: Again, this is already well explained in the text: "For each plume intercept, we calculated...". It is detailed in paragraph 4.1 that there were 4 plume intercepts, followed by "This analysis resulted in four data points".

COMMENT: Line 25: Above you say you can not use linear regression, but now you say you can??? REPLY: Again, it is clearly and explicitly stated that the linear regression analysis is not performed on data from individual fire overflights but on data from the four fire overflights.

COMMENT: 31509, line 10, equation: This equation is incorrect in that it treats all tracers the same regardless of background mixing ratios. This is a significant error. The apparent dilution ratio will be very different for compounds with sig background concentrations (like CO and O3) then for reactive VOCs, which for have nearly zero backgrounds . . .In addition I am really guessing on the interpretation of this equation, since none of the terms are defined. This is a significant error, but it is not possible to gauge the magnitude of the error since the authors have omitted primary data (eg the CO dilution) that would allow us to estimate the size of dilution terms. Its important to note that this error makes it impossible to interpret the results on production and loss of many species. REPLY: The reviewer is invited to carefully read the entire paragraph 2.3 which introduces the concept of dilution-corrected molar excess mixing ratios. We refrain from giving further explanations, also because we could not explain it in a clearer and simpler way. As a matter of fact, we did define all terms of the two equations given. Once it has become clear that our analysis is based on excess mixing ratios (i.e. on background-subtracted values) the reviewer's arguments are no longer valid. For further clarification, we have added the information that the background mixing ratios of all species discussed in our work were stable in the investigated domain.

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COMMENT: Line 20: It would be better to use MW, not Mx. REPLY: This abbreviation is taken from the original publication we are citing.

COMMENT: Line 25: 2% seems very odd, since there are many hundreds of VOCs, many unidentified. REPLY: Carbon emissions are dominated by CO₂, with additional major contributions from CO and CH₄. The contribution from VOCs, even if hundreds, is very small (see Tables 2 and 3). 2% is a conservative estimate.

COMMENT: 31510, line 10: The box model is not well described with respect to dilution. I am unclear what is meant by “CO was used as a dilution tracer”. Did you include background concentrations and use a dilution factor which allowed you to match the CO obs? This would be a reasonable approach. If so, you need to list the background concentrations used for each species. REPLY: We now state that “the dilution rate was obtained from the measured molar excess mixing ratios of CO”. Background concentrations are irrelevant as long as they are subtracted (in addition to being stable and significantly below plume levels).

COMMENT: 31514, line 1 and Figure 6. I really cant interpret “dilution corrected” due to the error mentioned above. It is impossible to interpret Figures 6-9 without knowing more about the impacts of dilution on these mixing ratios. So as indicated above, I suggest this ms be rejected and the authors to resubmit after fixing this significant error. REPLY: We are firmly convinced that there is no error in our analysis (see comments above).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31501, 2015.

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