Atmos. Chem. Phys. Discuss., 15, C10243–C10244, 2015
www.atmos-chem-phys-discuss.net/15/C10243/2015/
© Author(s) 2015. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Chemistry and Physics Discussions

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

**Anonymous Referee #2**

Received and published: 7 December 2015

This paper presents some nice data on numerous VOCs in a single, small biomass burning plume in Georgia. While the data appear to be high quality, I have a significant problem with the authors' analysis, especially treatment of dilution. To me, the treatment of dilution invalidates some of their primary conclusions. While I believe the authors could eventually generate an acceptable manuscript, it will require a substantially reworked analysis. For this reason, I suggest that this paper be rejected and the authors be encouraged to resubmit after the analysis is corrected. Details below.

31508, line 17: Unclear why you can not use linear regression. 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. Line 25: Above you say you can not use linear regression, but now you say you can??? 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. Line 20: It would be better to use MW, not Mx. Line 25: 2% seems very odd, since there are many hundreds of VOCs, many unidentified. 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. 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.

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