

## ***Interactive comment on “Profiling of formaldehyde, glyoxal, methylglyoxal, and CO over the Amazon: Normalised excess mixing ratios and related emission factors in biomass burning plumes” by Flora Kluge et al.***

**Anonymous Referee #1**

Received and published: 9 June 2020

This manuscript presents DOAS measurements with an airborne instrument of formaldehyde, glyoxal and effective methylglyoxal atmospheric concentrations as well as in situ CO observations over Amazonia during a series of flights carried within the ACRIDICON-CHUVA campaign. Those measurements have been done within different type of air masses and at different altitudes: pristine tropospheric air, air masses dominated by VOC emitted of biogenic origin or affected by biomass burning events and/or pollution coming from the city of Manaus. Those measurements allow deriving for those species vertical columns but more interestingly concentration profiles. The

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results are extensively discussed and compared with existing data in the current literature. This work fits well in ACP and definitely deserves publication after the following minor comments are taken into account.

### **Comments:**

- Having this gap within the glyoxal fit window to avoid interferences with H<sub>2</sub>O is interesting. Would it be possible to better illustrate the impact of doing this (e.g compare SCD with/without this gap)? Why is not needed for the methylglyoxal fits?
- In Fig. 8, there are some peaks in CO measurements to which no specific event is allocated and the other way round, there are biomass burning events without any increase in CO concentrations. Do we understand that? A similar comment can be done for other species. For example in the fire event 2, a spike is visible in HCHO measurements but not in C<sub>2</sub>H<sub>2</sub>O<sub>2</sub> nor in C<sub>3</sub>H<sub>4</sub>O<sub>2</sub>, while in the event 6, this is the reverse. Are those fires from different origins?
- In figure 9, in case the measurement sampling allows to do so, it would be very interesting to see mean concentration profiles classified as a function of the type of air mass (pristine, biogenic, fires, pollution).
- During the discussion on the R<sub>gf</sub> and R<sub>mf</sub> profiles, I would be much more cautious as the level of noise is very high. I think that the only thing which can be said is that the ratio is lower in the boundary layer compared to higher altitudes. Other conclusions on the profile shape are quite hazardous.
- When computing R<sub>xf</sub>, how do you define the background? Out of the different marked events (red, green, blue events), there is still a very large variability in the measured concentrations with values sometimes as large as during the selected events. So the notion of background is unclear here. Please clarify this aspect.

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### Minor/Technical comments:

- Line 13: Correct “applaying” by “applying”
- Line 31: remove “is”
- Table 1 caption: write “Temperature” instead of “Temperatur”
- Line 162: add “transfer” after “radiative”
- Line 177: replace “reminder” by “remainder”
- Line 185: the statement “dominates with > 50%” is not clear. Please rephrase.
- Figures 2 and 3: All the different blueish curves tend to mix together. Please use more contrasted colors to improve visibility.
- Lines 196-198: remove “on spatial scales...analysis of our data”, which is unclear.
- Line 203: Uncapitalize “Fit”
- Line 247: add “s” to “flight” (“flights”)
- Line 268 : remove “in”
- Line 269: replace “more closely” by “further”
- Figure 8: add a colorbar for Rgf/Rmf. Please mention in the caption what is the shaded blue area in the three upper panels.
- Line 313: remove either “are” or “appear”

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- Line 335: this factor 2 is likely a very rough way to get C<sub>3</sub>H<sub>4</sub>O<sub>2</sub> concentrations since I suppose that the relative amounts of the C<sub>3</sub>H<sub>4</sub>O<sub>2</sub>\* family members strongly depend on the precursor concentrations, and thus on the geolocation, altitude, geophysical regime. Please add a small statement to draw attention on this.
- Line 381 and figure 9 has as consequence that the basic assumption for error estimates is not verified (line 194). I agree however that some approximations are needed for such estimates. Please state when you make this hypothesis that it is not fully correct.
- Section 5.3.1: Please mention the type of measurements that Kaiser et al. and MacDonald et al. have performed (altitude range, season, measurement type).
- Line 444: replace “Contrary” before “On contrary”
- Line 481: The sentence “As a result,... burning plumes” is a repetition of the previous one to me. I suggest you delete it.
- Line 570: replace “applaying” by “applying”
- Line 583: to my knowledge, there is very little direct emissions of HCHO from biogenic sources. It comes mostly from indirect production from other biogenically emitted VOCs such as isoprene.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-129>, 2020.

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