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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The vertical distribution of biomass burning pollution over tropical South America from aircraft in situ measurements during SAMBBA" *by* Eoghan Darbyshire et al.

Anonymous Referee #1

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Reviewer's Comments on 'The vertical distribution of biomass burning pollution over tropical South America from aircraft in situ measurements during SAMBBA' by Darbyshire, et al.

General Comments

This manuscript reports findings from the 2012 SAMBBA field campaign with respect to atmospheric distributions of several key pyrogenic pollutants (smoke aerosol, black carbon, and CO). Going further, the manuscript interprets the effects of meteorology on the observed distributions. Much of the detailed information with respect to the data and methods is provided in a Supplementary file. Overall, the paper is scientifically

sound and well written, and should be interesting to many readers of ACP. Most of the suggested revisions listed below are relatively minor.

The only significant objection I have to the content is the lack of any consideration of the effects of deep convection on the transport of pollutants in the Amazon Basin. While this mechanism may become more significant towards the dry/wet transition season, a number of studies have reported finding elevated CO concentrations in the Amazonian upper troposphere, likely caused by deep convection. Among these are Andreae et al., 2001 (Geophys. Res. Lett., 28(6), 951–954, https://doi.org/10.1029/2000GL012391), Livesey et al., 2013 (Atmos. Chem. Phys., 13, 579–598, https://doi.org/10.5194/acp-13-579-2013), and Deeter et al., 2018 (J. Geophys. Res., 123, https://doi.org/10.1029/2018JD028425).

In fact, a significant number of the CO profiles presented in the author-provided Supplementary file exhibit features consistent with vertical transport via deep convection. For example, roughly between a third and a half of the 'complete' SAMBBA CO profiles (including CO measurements in the lower and upper troposphere) indicate CO enhancements in the upper troposphere (e.g., above 500 hPa) of 50 ppbv or more (relative to the minimum in the vertical profile). Interestingly, these enhancements often (but not always) appear without corresponding features in the aerosol extinction profile, possibly indicating 'rainout' of the aerosol. This feature of the CO profiles should be investigated in the revised manuscript and the potential role of deep convection should be addressed generally.

Minor Revisions and Technical Corrections

page 5, line 17. Please provide reference for climatological winds over South America (e.g., Campetella, C. M., and Vera, C. S., Geophys. Res. Lett., 29, 1826. https://doi.org/10.1029/2002GL015451, 2002).

p. 6, l. 26. Unclear if 80% refers to number of profiles where any or all of the considered pollutants indicated a pollutant residual layer.

p. 7, l. 24. What about the east-west distribution of fires? The figure indicates many more fires in the eastern region. Is this typical?

p. 8, l. 1. Might stronger easterlies actually promote (rather than inhibit) the spread of fires?

p. 8, l. 17. Add 'significantly' before 'affected'. There must be some small effect, correct?

p. 8, l. 27. It seems surprising that CO at the surface decreases from W1 to W2 (from 340 to 220 ppbv) whereas CO2 increases slightly (from 394 to 397 ppm). Does this suggest biogenic influence?

p. 9, l. 18. If this statement ('Significantly, the shift ...') is based on Fig. 9, should the end of the sentence actually read ' ... the relative abundances of rBC and sigma_sp_dry *to* CO'?

p. 9, l. 29. This particular paragraph ('The shapes of pollutant vertical distributions ...') seems largely qualitative and speculative. For example, sentences 5, 6, 7 and 9 in this paragraph draw conclusions without providing any quantitative evidence.

p. 10, l. 28. Missing 'and' between 'phase' and 'plume'?

p. 12, l. 14. SAMBBA was conducted in a year which was not considered a 'drought year' for the Amazon Basin. Widespread drought, such as occurred in 2010 and 2015, and may be increasing in frequency, results in different patterns of emissions (and meteorology) compared to non-drought years. Would the main findings of this paper be sensitive to the effects of drought? This would be an appropriate discussion for the Conclusion.

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