Atmos. Chem. Phys. Discuss., 15, C10544–C10546, 2015
www.atmos-chem-phys-discuss.net/15/C10544/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the vertical distribution of smoke in the Amazonian atmosphere during the dry season" *by* F. Marenco et al.

Anonymous Referee #2

Received and published: 17 December 2015

Review of Smoke in the Amazonian atmosphere by F. Marenco et al.

The paper is a solid research paper presenting experimental results from 2 weeks of airborne lidar measurements in southern Amazonia. The results are additionally compared to the output of two atmospheric models. I think the results presented are interesting for the atmospheric research community, both in terms of observations and modelling. The paper is very well written, all methods applied are precisely described, and reasonable conclusions are drawn. Therefore I can recommend the publication for publication after dealing with the few comments listed below.

General comments:

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Interactive Discussion
Discussion Paper

Introduction:

The Introduction is very well written and many details concerning Amazonia are given. However, some of the information, e.g. time of dry season etc., are valid for southern Amazonia only. Therefore, the authors should not generally speak of Amazonia (which reaches much far more North) in the introduction but specify it to southern Amazonia or similar expression.

Methodology:

Even if I have some doubts concerning the retrieval of extinction coefficient and lidar ratio from a backscatter lidar, I understand that the author has made many efforts to get as much out as possible from the nadir looking lidar measurements. The applied methodology is clearly presented and also error estimation is made, even if I think that the error could be even larger when lidar ratio is varying much stronger than +-6sr. However, I would like to know which reference value has been chosen in the far end of the lidar profile to obtain the backscatter/extinction coefficient and why. This is certainly the most crucial input parameter with this method. In the same manner, it would be good to discuss if the alternation by +-50% is realistic enough.

Have there been a priori information for the use of the reference value - for example from some surface observations? Or is it sufficient to change everything until in clear air regions the Rayleigh value is obtained? Can you comment on this?

Have there been some comparisons in one of the other research flights with the UV Raman lidar operated by the University of Sao Paulo near Manaus to validate the new retrieval? Or are there other sources for validation (e.g. Calipso overpass converted to 355 nm?). This would clearly enhance the value of the paper concerning the newly applied method.

Specific comments:

31742, line 5-18: In a recent paper(Seifert, 2015) the influence of smoke aerosol on

ACPD

15, C10544–C10546, 2015

ice formation in Amazonia is discussed and might be cited here as well to prove again the importance of investigating the vertical distribution of smoke aerosol.

31745, line 33: Are the signals acquired in photon counting or analog mode, or both? If both, is there any merging of the signals done?

31749, line 10: Something went wrong in the type setting of the formula concerning the definition of the layer depth. At least, I do not know yet how it is defined.

31754: line11: This is not clear to me, why should the coarser resolution be less sensible to the displacement of the plume. Can you explain more clearly? I also would recommend to introduce Figure 12 not just in the conclusions but already before.

Technical:

“ff” and ” ffi” are often not correctly type set.

In the final version, Figures 5,8,10,11 should be clearly one full page each!

References:

Seifert, P. et al. (2015), Seasonal variability of heterogeneous ice formation in stratiform clouds over the Amazon Basin, Geophys. Res. Lett., 42, 5587–5593, doi:10.1002/2015GL064068.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31739, 2015.