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Discussions
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EGU

# *Interactive comment on* "Biomass burning smoke heights over the Amazon observed from space" *by* Laura Gonzalez-Alonso et al.

**Anonymous Referee #2**

Received and published: 22 November 2018

The study by Gonzalez-Alonso et al. explores the injection height of biomass burning emissions across the Amazon during the dry season and produces a climatology of smoke plumes heights derived satellite observations. Overall the paper is well written and concepts are described clearly (although some sections in the methods may benefit from a summary figure or table – see comments below). Figures are really nicely displayed and, mostly, very easy to understand. The topic of the study is well within the scope of ACP and I can see that this dataset will be very useful, particularly to modellers simulating processes & impacts of biomass burning in the Amazon region. I recommend publication once the comments below have been addressed.

General

1. The methodology explanation is very thorough and well written. However, the meth-



ods section is very long and sometimes a bit hard to follow, particularly for readers that are unfamiliar with these satellite products. I suggest adding a table or two (or extending Table 1) summarising the main datasets and tools used including information on the satellite products and the version used, resolution, overpass time etc. The methods section would also benefit from a schematic diagram perhaps just of the MINX software, to make the analysis process clearer.

2. In the paper, the authors make a good attempt to compare parts of the methods and results to previous studies. However, these sections are buried in the text. I wonder if it would make the manuscript clearer if you had a separate section where you compare the methods and results to previous studies? You could add a table including previous studies on plume heights in the Amazon either using similar or different methods (e.g. Baars et al. 2012; Marenco et al., 2016), summarising/comparing the findings of these studies and yours.

3. I agree with Referee #1, the results from CALIOP and MISR are so different that I believe the reader will be left feeling a bit unsure of what information to take from the paper (particularly atmospheric chemistry/aerosol modellers who may not be familiar with the details of these satellite products/tools). Can you make some recommendations in the conclusions?

4. I strongly suggest comparing your results to what is currently used/assumed in atmospheric aerosol/chemistry models for fire emissions injection heights. Do your results contradict or confirm what is currently used?

5. Following on from the previous general comments, for modellers it would be extremely useful to have idea of whole vertical distribution of the plumes rather than just median/max plume height. For example, the average percentage of the plume in specified altitude bands. Could this information be estimated from the CALIOP data? Or perhaps this would be unreliable given the large difference between CALIOP and MISR results?

Specific

1. Abstract, L2: Specify the dry season months.

2. Abstract, L4-6: Sentence not written very clearly "About 60% of smoke plumes are observed during drought years, at the peak month of the burning season (September; 40–50%) and over tropical forest and savanna regions (94%)." Does this mean: 60% of smoke plumes were observed in drought years (relative to non-drought years); 40-50% observed in the peak month of burning season (relative to the other months); and 94% observed over tropical and savannah regions (with the remainder over grassland)?

3. Introduction: Order citations correctly (by year).

4. Introduction, P2, L12-14: Can you include any refences for why altitude to which smoke is injected is critical? Perhaps give examples of modelling studies where this has been tested e.g. some of the SAMBBA modelling papers, or observational studies.

5. Introduction, P3, L1-3: The Kolusu et al. (2015) and Thornhill et al. (2017) papers are modelling studies not observational (correct this sentence).

6. Introduction, P3, L9-10: "…no analyses yet that seek to quantify the vertical distribution of smoke from fires across the Amazon…" Suggest changing to: "…quantify the long-term average vertical distribution".

7. Sect. 2.1: How are the MISR and MINX vertical resolutions accounted for? Apologies if this explained later in the manuscript.

8. Sect. 2.6: Why was 0.5x0.5 degrees resolution chosen for CALIOP?

9. Sect. 2.6 (P7, L26-35 – P8, L1-5): Nice explanation of the other CALIOP products that have been used by previous studies. This may be helped by a table summarising: the studies (including yours), different products used, region studied etc. Also, is it possible to briefly say what the implications are for using these different products and explain why the specific product and plume height definition were chosen for your study



over the others?

10. This sentence from Sect. 3.1: "The majority of the plumes in this record are digitised with blue band retrievals (Table 1)…", seems to contradict the following two from Sect. 2.2 (or at least have confused me): "This screening leaves a total of 5393 plumes, about 56% of the original database, with 77% and 23% plumes digitised in the red and blue bands, respectively.", and "In our dataset overall, most of plumes are digitised with red band images, as it was the default option for MINX v2–3."

11. Be consistent with use of "PBL"/ "BL".

12. P12, L7 & L17: What are the p-values and at what confidence level is the relationship statistically significant?

13. P13, L15-16: "Our results suggest that fires during drought periods might significantly degrade regional air quality, as they are associated with low smoke altitude and large aerosol loading.". The finding that drought periods are associated with large aerosol loadings, which substantially degrade regional air quality is consistent with Reddington et al. (2015) (and other studies?). The higher aerosol loadings are likely due to the increases in the number/size of fires (e.g. Aragão et al., 2007; 2014) and subsequent increases in aerosol emissions. However, the potential for lower smoke altitudes in drought years, I'm assuming, is a new finding and should be highlighted/made clearer.

14. P14, L18-19: Would it not be possible to check the PBL height around CALIOP overpass time with MERRA2 data?

15. I'm not sure I understand how Figure 8 demonstrates the following statements: - P12, L7-9: "We find a significant positive relationship between MISR maximum plume heights and MODIS DSI (r=0.7) in tropical forest and savanna fires, with higher maximum plume heights in wet (1000–1100 m) than severe drought conditions (800–900 m) (Figure 8)." - P13, L3-4: "…tropical forest fires inject a larger percentage of smoke

plumes into the FT in wet than extreme-dry conditions (12 versus 20%, Figure 8)". Since I can only see data points for tropical forest in "Extreme- Severe" and "Mild-Moderate" conditions, with one point in "Normal" and none in "Wetter than Normal".

Tables & Figures

1. Table 1: suggest adding a "total" row in the table.

2. I suggest adding a table to summarise smoke plume heights for the main biomes (could also add drought/non-drought year averages) to compliment Figure 10. So that readers can get this info quicker than reading it off a figure.

3. Figure 2: I suggest adding one or two figures (perhaps to supplementary) to show the month and/or year the plume occurred e.g. with different colours.

4. Figure 10: If the differences between Day and Night CALIOP Median Plumes are not significant then is it worth just combining these here (keeping the separation in the previous figure)? It is really the difference between the CALIOP and MISR estimations of plume heights that is the significant result.

References (not already included in the paper)

Aragão, L. E. O. C. et al. Environmental change and the carbon balance of Amazonian forests. Biol. Rev. 89, 913–931 (2014).

Aragão, L. E. O. C. et al. Spatial patterns and fire response of recent Amazonian droughts. Geophys. Res. Lett. 34, L07701 (2007).

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