Response to Reviewer #1

We thank the anonymous reviewer for her/his thorough evaluation and constructive recommendations for improving this manuscript. Her/his comments (in italics) and our responses are listed below.

General comments

The paper presents interesting but contradicting findings regarding Amazonian smoke layer heights retrieved from passive and active satellite remote sensing. Most parts of the paper are well written. However, some clarifications are needed. As an example, we need precise wording throughout the article.

We have 'smoke plume height'! What does that indicate: layer base, layer center, layer top? Only after checking the paper back and forth, it became clear to me what is meant...... For meteorologists, cloud height, for example, means cloud base height, in your case it probably means top height.

For MISR, we report the elevation above the geoid of the level of maximum spatial contrast in the multi-angle imagery. This is generally near the plume top, but it actually provides a distribution of heights in most cases, because aerosol plumes are rarely uniform. The centroid of this distribution is typically somewhere within the plume (e.g., Fig. 2 in *Flower and Kahn*, J. Volcanology, 2017). On the other hand, CALIOP tends to report higher plume-height when very thin aerosol, to which the lidar is more sensitive, resides above the main plume deck. We have reworded the manuscript to clarify the definition of smoke plume height. We added this information in Sections 2.1 and 2.2.

Page 4 lines 24-27

> [..] MINX plume heights are reported above the geoid, which correspond to the level of maximum spatial contrast in the multi-angle imagery, typically near the plume top, but actually offering a distribution of heights in most cases, because aerosol plumes are rarely uniform (*Flower and Kahn*, 2017). Additionally, MINX provides local terrain height from a digital elevation map (DEM) product.

Page 5 lines 17-19

> MINX computes several plume heights that describe the altitude that smoke reaches in the atmosphere. In this work, we use the best estimate maximum and median smoke plume heights, which represent the distribution of stereo heights, obtained at the level of maximum spatial contrast over the plume area [Nelson et al 2013].

Regarding averaging...: Could be temporal and/or spatial (horizontal) averaging... so be more specific, say clearly what you did! ...throughout the manuscript.

We edited the manuscript to make clearer what our averages refer to.

The conclusions must be improved! What can we do with these so different findings (active vs passive remote sensing).

The multi-angle and lidar techniques are sensitive to different aspects of plume height, and are essentially complementary (e.g., Kahn et al., 2008). As also suggested by reviewer 2, we have added more information in the Conclusions to make our findings clearer.

Details:

P1 L7: you write ....1100m maximum plume height average... lowest plumes occur over tropical forest fires (800m). What do you mean here? What is the maximum plume height (is that related to layer top)? The lowest plumes occurred at 800m (intuitively that means layer base...) ...? Please improve this unprecise wording! ...throughout the entire Abstract! ...and the entire paper! And regarding averaging: you mean...spatial averaging, temporal averaging, or just avergaing of all cases?

As discussed above we have reworded the manuscript to make clearer what smoke plume heights mean and what our averages refer to.

P3, L8: There is this Baars et al. paper (JGR 2012), now mentioned in the introduction. This is the first systematic investigation of smoke layer geometrical and optical properties over an Amazonian site (a bit north of Manaus). You mention it, but you do not make any attempt to compare their results with yours. They measured smoke AODs with Raman lidar, they have measured lidar ratios, they have multiwavelength information for aerosol typing (fresh vs aged smoke etc), and layer base and top heights and depths for the fire season of 2008. But you use the much more uncertain CALIOP observations. In the case of CALIOP, the lidar ratio is more or less a look up table value, the CALIOP return signals are rather noisy, the CALIOP data analysis team even estimates the aerosol type from some kind of look up tables!

So my simple question is, why not using the Baars et al. (2012) results for comparison in addition?

By the way, this reviewer is not Dr. Baars, but an EARLINET Raman lidar specialist.

We thank the reviewer for pointing to Baars et al., (2012), which had initially been overlooked in our first version of the manuscript. We carefully thought about the reviewer's suggestion about comparing our results with Baars et al (2012). Our manuscript presents a comprehensive climatology (2007-2012) of smoke plume heights retrieved from CALIOP over the entire Amazon domain, whereas Baars et al, (2012) cover year 2008 and at one specific point location (2.5ºS, 60ºW). On the other hand, and as the reviewer mentions, Baars et al (2012) presents a more detail analysis of smoke layer geometrical and optical properties. We feel that mentioning results from Baars et al (2012) with respect to smoke plume height and aerosol loading, which we do, is appropriated for the scope of our paper.

P4, L16, L18, L19, L21, ...: Plume heights, yes I know, you mean plume top heights. Please, write that explicitly!

We mean as plume height the level of maximum spatial contrast in the multi-angle views, not the mean plume top, as explained above. We clarified this point in the manuscript as discussed above.

P5, L10: MINX computes several plume heights... you mean....top heights.

Clarified as discussed above.

P5, L11: We use maximum and median smoke plume heights... Top heights? Median heights... regarding.... the entire season of a year, for the entire region you cover with observations??? Just all plumes, you collected???

These statistics represent the distribution of stereo heights, obtained at the level of maximum spatial contrast over the plume area, and stratified by season, year, etc., as appropriate. We have clarified it as discussed above.

P6, section 2.6: You concentrate on the comparison with CALIOP observations! Howis the comparison of CALIOP with the Baars 2012 results for the fire season of 2008 regarding layer base and top heights, aerosol typing, lidar ratios?

Discussed above.

P7: again precise wording is necessary...

Reworded as discussed above.

P8, results and discussion sections 3: I would like to see a 1:1 case study, with a CALIOP fire smoke profile with indicated base height, center height, and top height, and then what you got from your MISR retrieval ... as layer top height (even if the measurements are done at very different times of the day and PBL evolution...). This would provide better grounds to discuss the huge discrepancies between passive and active remote sensing products regarding smoke layer tops.

The differences between CALIOP and MISR can be large in some case, but they are not huge, and they are consistent with the differences in overpass time and sensitivities of each measurement to actual aerosol plumes. These two sensors complement each other as explained above. We have made this point clearer within the manuscript. For example, we moved and expanded the discussion of CALIOP/MISR differences to Section 2.6 (Methodology), so the readers can learn about the differences and complementarities of these two satellite products before facing the results.

Page 8 Lines 16-23

> Our initial objective was to compare the CALIOP with the MISR plumes to assess the diurnal smoke evolution on a plume basis, as CALIOP has a later sampling time than MISR over the Amazon (13:00–15:00 LT versus 10:00–12:00 LT). However, despite our effort to develop a comprehensive CALIOP climatology none of the CALIOP plumes coincide with the MISR plumes. As previous studies discuss (e.g., Kahn et al., 2008; Tosca et al., 2011), CALIOP and MISR, in addition to having different sampling times, also have different swath widths (380 km versus 70 m). These differences make it difficult to observe the same fire on the same day, but they make CALIOP and MISR observations complementary: MISR provides late-morning near-source constrains on aerosol plume vertical distribution, whereas CALIOP in general offers more regional constrains, later in the day (Kahn et al., 2008). Some differences between the products are thus expected.

Reviewer suggests to show a comparison MISR-CALIOP on a plume basis. That was our initial intent but, given the differences in swath widths and temporal coverage, that is not possible. We have moved this discussion to section 2.6 (page 8, lines 16-23) as mentioned above. In addition, our Figure 1 provides the CALIOP fire smoke profile that reviewer would like to see. We have modified the caption to make Figure 1 clearer as discussed below.

P14, L10: 'complementary' What is complementary when the CALIOP and MISR products are so much different?

MISR provides extensive near-source mapping, whereas CALIOP provides downwind sampling. This is the subject of Kahn et al., (2008). We have added a discussion on the manuscript to clarify this point as mentioned above.

P14, L30: Nice to have all these references from very different regions. But the main question remains: What did Baars et al. (2012) report for the Amazonian forest in the Manaus area? And how does that fit into the picture seen by MISR and CALIOP?

We have modified the discussion to put results from Baars et al (2012) into context.

Page 15 Lines 3-8

> Smoke plume height values over the Amazon similar to ours were reported in other studies for CALIOP (Huang et al. 2015) and surface-based lidar measurements (Baars et al 2012). Using the CALIOP vertical feature mask and AOD profiles, Huang et al. (2015) reported an average for the most probable smoke height of 1.6–2.5 km for September fires. Their definition is comparable to our CALIOP median plume height, which produced a value of 2.3±0.7 km for the September months. Over Manaus in 2008, Baars et al., (2012) reported biomass burning layers at 3-5 km elevation, with most of the smoke trapped below 2 km.

Page 15 Lines 10-13

> In our study, CALIOP observes smoke at systematically higher altitudes than MISR, with median plume heights up to 1.4 km higher (2.2 km for the maximum plume heights). However, CALIOP still shows that the majority of the smoke is located at altitudes below 2.5 km above ground, consistent with previous observations from lidar measurements (Baars et al., 2012).

P15, P16: At the end what is now the conclusion, having these huge discrepancies between spaceborne lidar and passive remote sensing lidar in mind? I am l ost after the discussion, and even after reading the conclusions. How to proceed with this? How can modellers make use of such contradicting MISR/CALIOP results?

We disagree with the reviewer. We do not find "huge discrepancies" between CALIOP and MISR. Differences in sampling, and in what each technique is actually sensitive to, explain the differences. Such differences are not discrepancies. We have reworded the manuscript, including Conclusions, to make the MISR-CALIOP comparison clearer, as mentioned above.

Figure 1: Yes, I am a lidar scientist, but nevertheless, I had trouble to understand the text in the figure captions: smoke plume median heights? What does that mean here? There are then two color scales, what belongs to what? Yes at the end, I got it after minutes of 'research'. Colored circles for different aerosol types: green for dust, up to 12 km, really? Any idea bout the dust source? Next: Dashed black line represents the averaged extinction profile (??) What did you average, and why is that a function of height? So, smoke is indicated by pink dots! Fine! But there seem to be a lot of clean/continental air particles on 25 Sep, scattered all over the insert display, even at dust level heights of 10-12 km? Confusing! ... but understandable. The aerosol typing is based on questionable CALIOP look up tables!

We reworded the caption to make the figure easier to interpret.

Page 25, Figure 1

Example of the approach followed for the CALIOP smoke plume characterisation. The map shows estimated smoke plume median heights (gridded at 0.5x0.5 horizontal resolution) for September 25th, 2010 at 06:25 UTC. MODIS active fire pixels associated with the CALIOP smoke plumes are represented with open circles. The insert displays the vertical distribution of aerosol extinction for a specific smoke plume in the map, with extinction values coloured by classified aerosol types. Dashed black line represents the averaged extinction profile for the aerosols classified as smoke (pink dots). In this profile, the CALIOP smoke plume has a median height of 1.7 km (green color in the smoke plume median height scale) and a maximum height of 4.5 km above the terrain

The dust at 12 km is most likely transported from North Africa. There is a vast literature about Saharan dust transport to the Amazon, e.g., Yu et al., (2015), Ben Ami et al., (2010).

Figure 9: What did Baars et al. (2012) observe in 2008?

Added a discussion on results from Baars et al (2012) as mentioned above.

Figure 10 shows the final result!... and my personal spontaneous conclusion and main question after reading the entire manuscript is: Having these huge differences in the findings in mind, what is then complementary (after analysing CALIOP and MISR smoke observations)? How should modellers (most are not experts of passive and active remote sensing) use the 'combined' information? Can we, e.g., quantify ... from the combined observations... how much smoke AOD is in the layer below the MISR-derived top height, what is the residual AOD for the layer between MISR and CALIOP-derived top heights?

Please, explain that in the conclusion section what is now the concrete result of this work. How can we use these data sets...? What is the true information content. Many readers will not be familiar with passive or active remote sensing, but are interested in Amazonian fire smoke and the horizontal and vertical distribution, and potential consequences for long range transport and deposition.... Please help them to understand the findings.

I like the results! Many authors would hesitate to show us the 'real world' of observations, retrievals, and apparently contraditing products. I think it will not be so much work to revise the mansucript a bit to meet (some of) my points.

We think that these results have been shown throughout the manuscript. We have emphasized the key points as described above. Although both the lidar and multi-angle imagery measure some aspect of aerosol plume elevation, they do not measure the same thing. We have clarified this in the manuscript as discussed in detail above. The height differences shown in Figure 10 are not that large, given the differences in the sensitivities and sampling of these techniques (1 vs. 2 or 3 km). Most of the plumes are likely within the PBL. We have reworded the manuscript, including the Conclusions, to make clearer our MISR-CALIOP results. In addition, as indicated by reviewer 2, we included an analysis of the PBL heights at the time of the CALIOP overpass, which help explain some of the MISR/CALIOP differences.