Response to reviewer #3

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 Comment:
In this manuscript, the authors characterize burning biomass plume height over the Amazon using MISR and CALIOP observations. They investigate the effect of FRP, atmospheric stability and drought while considering seasonal and interannual variabilities. This is the first time that such work was performed over the Amazon. The manuscript is well structured and well written. The discussion on the drought is particularly interesting. I would recommend this manuscript for publication in ACP after considering the comments listed below. The important point that need to be addressed in the correction is the definition and the use of FRP that cannot be directly linked to fire intensity (see third comment below).

Specific comments:

P2 L3: consider including in the text some geographical location of where we should materialize this arc of deforestation.
Defined as suggested.

Page 2 Line 5-6

> Most of these fires burn in the so-called arc of deforestation, along the eastern and southern borders of the Amazon forest, during the dry season.

P2 L11-14: consider referencing the review on plume injection height from Paugam et al. 2016 when discussing the effect of plume injection height.
Cited as suggested.

P2 L21: fire intensity is a specific metrics in fire science expressed in [W/m] and is not the same as FRP[W] or FRP density [W/m2]. However, when dealing with satellite observation, FRP density is usually related to fire intensity. Your definition of fire intensity should be discussed at this point in the introduction. You use in the remaining of the manuscript FRP as a metric for fire intensity. FRP is an estimate of the total radiant energy emitted by the active surface area of the fire, flaming and smoldering area all included. FRP is probably better defined as a measure of fire activity including size and radiant heat flux (FRP density).
We thank the reviewer for this clarification, and we agree. As discussed in previous work, ours and others (e.g., Kahn et al., 2007; 2008), FRP tends to be a gross underestimate of dynamical heat flux, which is the quantity of interest for plume-rise calculation. The MODIS FRP product, in particular, is reported in the standard product as MW/pixel, and as a MODIS pixel is ~1 km^2 except toward the edges of the swath, this amounts to W/m^2. So in response to the reviewer comment, we defined FRP more precisely, as suggested, and clarified its meaning throughout the manuscript. Despite the limitations, FRP is one of very few indications of the energy associated with a fire that can be retrieved with remote sensing. So we use it qualitatively as a proxy for fire intensity, with this understanding.

Page 2 lines 24-26

> Related work also demonstrated the important effect that fire radiative power, i.e., a proxy of fire intensity, and atmospheric conditions have on the initial rise of fire emissions (Freitas et al., 2007; Kahn et al., 2007; Val Martin et al., 2012).

Page 4, lines 7-8

> […] We note that MINX provides FRP values in MW, although they are actually in MW per 1-km pixel, which corresponds to W/m^2 except toward the edges of the swath.

P3 L11: capital letter for Fire Radiative Power (FRP). You could add the MODIS collection version here.

Corrected as noted. We added the MODIS collection version in the Section 2 (Data and Methods)

Page 4, lines 5-7

> The MODIS reports fire radiate power based on a detection algorithm that uses brightness temperature differences in the 4 um and the 11 um channels (Giglio et al., 2003); this FRP parameter is used as an indicator of fire location and intensity. We use MODIS Collection 6 (Table S1 in SI).

P3 Section2.1: Consider grouping the paragraph on MISR and MODIS, ie l21 to 28 could be moved to the start of page 4.

We do not understand well what lines need to be moved and where, as we find difficult to match the reviewer's comment to the submitted version of our manuscript. In any case, we consider that grouping the MISR and MODIS discussion will make a lengthy paragraph and prefer to leave it as it is.

P3 L32: see comment above on FRP and fire intensity.

See our response to the earlier reviewer comment on the FRP definition.

P5 L11: replace "),in" to "), in"

Replaced as suggested.

P5 L20-23: consider moving the discussion on the red and blue band in the Supplementary Information. As you showed the added error is negligible compared to the MINX uncertainty. You could just mention it once and refer to the SI for more details.

We thank the reviewer for this suggestion. We agree that the red/blue band error is small compared to the other uncertainties, but decided to keep the discussion in the methodology (section 2.2) as this discussion only adds a small paragraph (6 lines) to the section.

P6 L2: consider also mentioning here when you consider an atmosphere stable or not.

The definition for weak and strong atmospheric stability conditions at the plumes is qualitative, based on the atmosphere stability distribution at the smoke plume locations. This definition is addressed once the plume database has been introduced, after section 3.1.

P7 L2: "wide range of condition as in MISR". I am not sure I understand why your methodology is ensuring a wide range of condition.

Clarified as noted.

P5 paragraph 3: I would move this section after you mention the choice of your horizontal resolution (line 5).

We do not understand what the reviewer refers here. In all versions of the manuscript, page 5 does not mention any resolution. We assume the reviewer refers to the CALIOP horizontal resolution discussion in page 7. We have clarified the choice of horizontal resolution, as also suggested by reviewer 2

P5 paragraphs order: Consider rearranging the paragraph order in this page to make it easier for the readers. For example, you mention twice how you define CALIOP plume height. This is only a suggestion: the last paragraph on the definition of CALIOP plume height should come after you first mention how you define plume height (line 8). Then would come the discussion on how you link the plume to fire activity.

We are not sure what order the reviewer means. In any case, to clarify the choice of the CALIOP horizontal resolution we have reordered some paragraphs within this section and we hope the reviewer finds the discussion easier to follow now.

P5 L18: why do you expect a bias?

Because of the coarser grid used to estimate the CALIOP smoke plumes. We have clarified this section as suggested by reviewer 2 to make this point clearer.

P5 L29: Most Probable Height.

Corrected as suggested.

P5 L10: consider mentioning that "those grid cells" are the grid cells of your gridded CALIOP injection height product.

Mentioned as suggested.

P5 L10: How do you cluster MODIS Fire pixels? Are you taking the larger cluster or do you sum all fire pixel in the grid?

We assume the reviewer refers on how we use the MODIS fire pixels to consider active fires within the CALIOP smoke plumes. We sum all fire pixels within the grid and only select those grids with at least 2 fire pixels, as explained in page 7, lines 26-28.

P5 L12: Are you using the same elevation model than in MISR?

We use a different elevation model than MISR. For CALIOP, as we use a ~50x50 km grid, we estimate the average terrain elevation within the grid based on the CALIOP digital elevation map (GTOPO30). We added this information in the manuscript.

P8 L15-16: as mention above, I think this is not brining any added value to the discussion here. Move the discussion on MINX band retrieval in the SI.

We thank the reviewer for the suggestion. As the distinction between red/blue band retrievals is important here, we keep the discussion in section 2.2, but feel this additional reference is helpful here. It only adds one sentence to the paragraph.

P9 L5-6: Could you mention how does this stability metric relate to the definition of the stability flag (stable/unstable) defined in Val Martin et al 2010? I might miss the point, but why do you define a new metric as you seem to only define atmosphere as weakly and strongly stratified as in Val Martin et al 2010.

We qualitatively classify atmospheric stability conditions as strong and weak, based on the atmosphere stability distribution calculated at the plume location and time. The atmospheric conditions at the Amazon and North America plumes are different and we cannot use the same classification used in Val Martin et al., (2010). We have clarified this definition to make clear our classification is qualitatively.

P9 L8: "a summary of these"

Corrected.

P9 L14: your value of FRP contrast with the FRP density values reported by Freitas et al 2007 for the same biomes. Grassland is reported to have an FRP density (3.3 kW/m2) an order of magnitude lower than tropical forest (30-80 kW/m2).

It is difficult to compare our MODIS averaged FRP values and Freitas et al. (2007) heat fluxes.

In our analysis, we obtain the averaged FRP at the location of many fires in early morning. These fires are subjected to many different burning conditions within a particular biome. Freitas et al., (2007) however report a minimum and maximum heat flux per biome. It is not clear to us how those values are estimated as the Authors do not specify it. In Freitas et al., (2006), the Authors only reference a total energy emission measurement over a forest fire in North America as in agreement with their tropical forest fire heat fluxes (30-80 kW/m2). For grasslands, the Authors report one value (3.3 kW/m2) and mention a lack of observations on that type of biome. Our MODIS FRP over grassland is on an average larger than FRP over tropical forests. Our observation is also consistent to that reported in Val Martin et al., (2010) for grasslands versus dry tropical forest over North America.

P9 L21: "obscuring the fire emitted 4-micron radiance [...] as well as low radiant emissivity". Consider reformulating this sentence. Why the flame emissivity should alter the FRP retrieval in tropical forest? The FRP formulation relies on the gray body assumption. Flaming combustion (because of soot presence in the flame) is more prone to violate the gray body assumption than smoldering. In case of smoldering fire, vegetation absorption is more likely to alter FRP estimate.

As FRP is measured remotely, we have no way to identify the occurrence, let alone the cause (e.g., due to soot or vegetation absorption or any other factor) of non-unit emissivity at the wavelengths used to measure MODIS FRP. As such, we list smouldering as an example of where non-unit emissivity tends to occur over a broad part of the observed spectrum.

P9 L25: you could mention that some simulation studies also work on the impact of atmospheric stability and that this is still an open problem in plume rise parameterization. The plume rise model proposed in Paugam et al 2015 (based on the original work of Freitas et al 2007) was shown to be sensitive to atmospheric stability unlike others existing parameterizations. However, this work was refused for publication in ACP, and despite this publication refusal, results of the same model implemented in GFAS were published in ACP in Remy et al 2017.

We thank the reviewer for this note. We are sorry to hear about the history of Paugan et al., (2015) ACPD work, and agree about the role of the atmospheric stability, which was also included in our own much simpler diagnostic model (Kahn et al., 2007). We extended the discussion and emphasized that there are still some uncertainties in the role of atmospheric stability in plume rise parameterizations.

P9 L27: consider reformulating: "and weaker atmospheric stability conditions when low altitudes plumes then to be trapped with the boundary layer".

We do not understand the reviewer's comment, as the suggested sentence does not make sense grammatically. In any case we have reworded the sentence to make it clearer.

Page 10 lines 11-13

> […]. For instance, Val Martin et al. (2012) showed that, in North America, fires that inject smoke to high altitudes tend to be associated with higher FRP and weaker atmospheric stability conditions than those that inject smoke at low altitudes, in which smoke tends to be trapped within the boundary layer.

P9 L31: as mentioned above, why not using the same flag as in Val Martin et al 2010 to define the state of the atmosphere.

Addressed above.

P9 L32: Figure 4

Unclear note.

P10 L12-13: I am not sure this sentence brings much to the discussion. Consider removing it.

Removed as suggested.

P10 L20-21: combustion efficiency is probably more related to FRP density than FRP. Active fire area is important in your discussion here and should be mentioned.

We only use the FRP in our assessment and not the active fire area. As such, we consider that mentioning active fire area is out of scope for our study.

P10 last paragraph: AOD correlate also to the FRP time integration (= Fire Radiative Energy, FRE), see Pereira et al 2009.

FRE requires integrating a measure of fire energy flux over time. We have only snapshots with MISR and MODIS, so we would need to introduce modelling of some sort to include FRE in the analysis, as Ichoku and Ellison, (2014) do. This is beyond the scope of the current study. We use FRP only as a qualitative indicator, which seems sufficient here.

P11 L17: why smaller fire in size require less conservative definition in FT injection?

Smaller fires tend to be less energetic and have lower injection heights. We think that the definition of 'smoke in the FT' proposed for other studies, in which fires were larger and more energetic, is too conservative for the Amazon.

P11 L24-27: I found the discussion slightly confusing. Does the height the PBL relates to the strength of the stable layer located just above? I might be wrong this is just a thought. In the presence of a deep PBL, there might have quite a lot of water vapor that could be used by the convective plume to get stronger, get across the stable layer and reach the FT.

Based on our analysis, we cannot determine if the PBL height is related to the strength of the stable layer above, and we cannot determine whether deeper PBLs are associated with more water vapour that can help plume buoyancy.

P12 L1: "as discussed above". Mention the section.

Mentioned as suggested.

P12 L2-3: "Note that DSI is higher in wetter years." Is this not just the definition?

This comment is to remind the reader how the MODIS DSI is defined, as this is not necessarily intuitive (i.e., the "drought" index is *higher* in wet years…).

P12 L5: than in severe drought condition.

Corrected as suggested.

P12 L23-30: I found the discussion difficult to read. If I well understood your point is that drought effects are correlated with the biome of their geographical location. Drought between 2005 and 2007 move from one biome to another. Could you just discuss FRP and injection height changes for the two biomes between the two years? Why are you using in this discussion the repartition of all observed plume per biome (Fig S1)?

We state in the manuscript that the regional location of drought makes one biome burn more promptly than the other, as the spatial distribution of biomes over the Amazon is very well defined. For example, northeastern Amazon is dominated by tropical forest whereas southeastern Amazon is dominated by savannah and grassland. Biome determines the type of fire (e.g., smouldering vs. flaming), and hence, FRP and smoke plume heights. We do not say that drought effects are correlated with the biome of their geographical location.

We thank the reviewer for suggesting that we discuss FRP and injection heights in the two biomes and two years. We already discussed this topic on page 13 lines 7-15. In our discussion, we refer to Figure S1, as we think it is important to show the percentages of fires per biome in each year to support our observation that more fires in tropical forest in 2005 and more savannah/grassland fires occurred in 2007.

P21 L32: what are the mechanisms that make a PBL deeper in dry year?

PBL is higher in dry years as the surface is warmer, which increases convective mixing. The PBL properties mentioned here are basic meteorology.

P13 L13: However, you mentioned that grassland fire might reach higher injection height in dry condition?

We do not understand the reviewer's comment. In our version of the manuscript (page 13 line 13) discusses MODIS DSI and AOD. In any case, in our manuscript we mention that grassland fires inject more smoke plumes into the FT during extreme dry than wet conditions because these fires are associated with high FRP, which may be sufficient to produce the buoyancy needed to lift smoke directly into the FT.

P14 L17: According to what your argumentation in section 3.4, regardless of PBL, tropical forest fires plumes are lower in dryer condition. So your point here only applies to grassland fire?

We do not understand the reviewer's comment. There isn't any discussion about tropical forest and grassland fires, and PBL on page 14. Apologies again but we have a hard time following the reviewer's notes with the versions of the manuscript we have available, including the version submitted for the current review.

P15 L9: "more opportunity to mix upward". MISR data shows generally a peak injection height near the fire where the convective plume is active (with potentially pyroconvection taking place) and then a downdraft caused by the aerosol loading and the atmospheric stratification. A later updraft is possible on longer time scale for older plume through solar radiation heating (De Laat 2012). I think that the main processes responsible of the differences between plume smoke observed by MISR and CALIOP are changes of atmospheric stability and fire activity which can make the updraft core of the plume stronger, making aerosol spreading at higher altitude. Aerosol that were emitted earlier in the day would not have time to reach higher altitude just by solar heating.

We thank the reviewer for this comment. We have modified the CALIOP and MISR discussion throughout the manuscript to make the results clearer as also suggested by reviewers 1 and 2. Note that differences in the sensitivity of the two techniques would also contribute to CALIOP detecting thin, elevated aerosol above the contrast features detected by MISR in many cases.

P26 L9-14: as already mentioned, the discussion on fire intensity would be better related to FRP density rather than FRP.

We think the reviewer means page 16 (conclusions). We have addressed this above.