

Response to Short Comment

We thank Juliette Koppel for submitting the reviews to our manuscript. We are glad our manuscript was selected as part of an introductory course in the MS in Earth Environment at University of Wageningen. Our responses to Juliette's comments (*in italics*) are listed below.

General comments

The goal of this research is to quantify the vertical distribution of fire smoke across the Amazon and to identify the key factors that control the plume height and rise. In order to achieve this goal, the smoke plume height and its variability will be characterized and the influences of different biome types, fire intensity, local atmospheric conditions and regional drought on smoke height will be studied. The climatology of 2005-2012 is limited for the burning seasons (July – November) and retrieved from space-borne observations from MISR and CALIOP. For all biomes there is a plume height seasonal cycle and also for all biomes most smoke is located below 2 km. No clear relationship is found between drought conditions and fire radiative power. MISR and CALIOP show contradicting results regarding smoke plume heights and DSI, but CALIOP systematically detects higher smoke plumes than MISR. This work highlights the importance of biome type, fire properties and atmospheric conditions for plume dynamics, as well as the effect of drought conditions on smoke loading. The study demonstrates that combined observations of MISR and CALIOP allows for better constraints on the vertical distribution of smoke from biomass burning over the Amazon.

What is new in this paper is that there has not yet been any research on the vertical distribution of smoke plumes in the Amazon and also no research has yet been done on the key factors that influence the vertical distribution of fires. This research is of importance because of the great impact of Amazon fires on global biomass burning emissions. These emissions have a large influence on air quality, atmospheric composition, climate and ecosystem health. Therefore, it is necessary to gain a better insight in the vertical distribution of fires and the key factors influencing this process.

In my opinion, the paper is written very clear and has a good structure. The introduction is very strong, including societal significance, previous research, the reason of the study area, the gap in research and good funnelling. In general, in the results/discussion section the results that are found are almost all compared with previous studies and explained well. The overall text is easy to read and written in a nice way so that the attention keeps to be drawn to reading the paper.

I think this paper fits well to the scope of the journal. The study is about smoke plumes present in the Earth's atmosphere and the underlying physical processes. One of the main research activities of the journal is Remote Sensing, which is in this paper is present in the method because of the use of MISR and CALIOP.

However, there are some sections in the paper that need to be revised in order to have this paper published. These adjustments are needed especially in regard to the methods of both MISR and CALIOP, the added value of using both MISR and CALIOP, the importance of land-management policies and some other minor aspects which I will elaborate on later in the review.

We thank Juliette for these valuable comments. We have addressed the major and minor comments below in a point-by-point basis.

Major arguments

1) MISR and MODIS are both aboard on the NASA Terra satellite, which crosses the equator between 10:00 and 11:00 a.m. local time. This means that observations of smoke plumes will only be available for this time step every day. In this research also the smoke plume heights are related to boundary layer height and atmospheric stability. Specifically, this is done in the results/discussion section, page 9 line 25-34 and page 10 line 1-6. In principle, stable boundary layer conditions occur when $\theta(K)/Z(km) > 0$ and unstable boundary layer conditions occur when $\theta(K)/Z(km) < 0$ (Vilà J., 2017-2018). In the results and discussion section of this paper an atmospheric stability of 4 K/km is designated as strong, see page 9 lines 33-34. But on what are these values based? All the MISR smoke plumes are categorized as having this weak or strong stability and results (further elaborated in the paragraph below) are based on this. The results can be doubted, since no explanation is given for the criteria values of atmospheric stability and the values thus cannot be validated.

We have no direct measurements of near-surface atmospheric stability. Model results vary enormously, and must be considered qualitative. As such, we make a reasonable division between low-stability (i.e., small lapse-rate cases) and higher-stability (i.e., larger lapse-rate cases) for the purpose of assessing qualitative differences between these limiting regimes. Our classification is based on the atmospheric stability estimated at the location of the fire plumes over the Amazon, which ranges from -3 to 23 K/km (page 10 line 17). We define the cut-offs in order to have a good representation of data within the two classifications.

To make this point clearer we modify the text as:

Page 10 lines 15-16

To analyse the influence of atmospheric stability over Amazon fires qualitatively, we divide our plume dataset into two groups that we define as having weak and strong atmospheric stability conditions based on MERRA-2 reanalysis.

On a side note, we kindly ask Juliette and the students in the MS in Earth Environment at University of Wageningen not to reference class notes (e.g. Vilà J., 2017-2018) in future published reviews. Readers outside University of Wageningen do not have access to that material.

Figure 4 shows the vertical distribution of MISR plume height retrievals, classified under the weak and strong stability categories that are designated here. In lines 2-6, page 10 it is stated that “Our comparison supports previous observations that plumes under weak atmospheric conditions tend to inject smoke to higher altitudes than those experiencing strong stability, with average maximum plume heights of 1150 m and 654 m, respectively.” It is also stated that same patterns are found for median and average plume heights. Another statement is that weak stability conditions are associated with deeper boundary layers than strong stability conditions, but it is also stated that this is not even shown. So, first of all, when the categories for weak and strong stability are not appropriately defined, this will cause non appropriate values for the percentage of plumes per category (presented on page 9 line 34 and in figure 4) and maximum, median and average plume heights per category as well (presented on page 10, lines 3-4. Second of all, since it is not even shown that deeper boundary layer heights are associated with weak stability conditions, this statement “Weaker atmospheric stability conditions are also associated with deeper PBLs (~1500 m) than strong stability conditions (~1200 m).” can’t be made. On top of that, this very same

statement is also a conclusion that is based on the weak/strong stability categories, so when these categories are not defined right, this statement might not even be true.

The PBL properties cited here are basic meteorology, common knowledge in the field. A detailed discussion of the relationship between PBL stability and MISR-observed plume heights in particular is contained in Val Martin et al., (2010; 2012), which are cited in the current paper.

Furthermore, the MISR observations are only taken in the morning (10:00-11:00 local time) and thus all the conclusions regarding MISR observations that are made only gives us information for this time step. Since the boundary layer processes and height and atmospheric stability changes a lot during the day (Vilà J., 2017-2018), this time step might not be very representative. Information about the changing boundary layer processes during the day is missing in this paper, where I think it is necessary to include this specifically in the discussion section, page 9 lines 25-24. Also for the conclusions I think it should be stated clearly that this only accounts for the specific time step of (10:00-11:00) and cannot be generalized for the day. In order to be able to test what the effect of changing atmospheric conditions during the day on plume height is, it is necessary to model (with for example model Daysmoke, Liu Y., et al 2010) the hourly PBL height and 6-hourly potential temperature profiles (obtained in this study) against the vertical distribution of smoke plumes.

The limitation of MISR diurnal sampling is already mentioned in several places in the paper, including the conclusions. Modelling the diurnal cycle would be worth doing, but it is beyond the scope of the current paper.

2) In the paper it is stated at page 14, lines 5-7, that the initial objective of this research was to compare data from MISR with CALIOP. However, in the paper of Kahn et al., 2008 it is already stated that MISR and CALIOP observations are in fact complementary. Since this is known on beforehand and is mostly due to the properties of both instruments, I don't understand how the authors came to this initial objective. On top of that, in the abstract of the paper, page 1 lines 20-21, it is said that combined observations of MISR and CALIOP allows for better constraints on the vertical distribution of smoke from biomass burning over the Amazon. However, most conclusions in this research are based on the MISR data.

Our initial aim was to compare smoke plumes observed from both instruments on a plume-by-plume basis, to study the diurnal variability of smoke heights over the Amazon. We developed a new approach to estimate smoke heights on a single-plume basis from CALIOP, and considered a long-term record of observations (7 years). However, despite our efforts, differences in swath widths and sampling times complicate the interpretation of this comparison (page 8 lines 16-23).

Kahn et al., (2008) points out that MISR provides near-source constraints on aerosol plume vertical distributions, whereas in general, CALIOP offers more regional constraints. The current study compares CALIOP and MISR plume-height data on a regional basis, which is both appropriate and useful. As also suggested by reviewers 1 and 2, we clarified this point throughout the manuscript.

At page 7, lines 2-4, it is mentioned that for CALIOP, both day and night observations will be analysed, to allow a better comparison with the smoke plumes of MISR. But it is already known that comparison of observations of both instruments is not appropriate, and a cause of that is the difference in sampling time. This difference makes it even harder to compare data,

because not the same smoke plumes are observed. This is also mentioned in the paper at page 14, lines 5-10. In the results section of the CALIOP smoke plume observations, it is found that the years with highest or lowest number of plumes are the same as observed by MISR and also the peak and biome type with highest biomass burning agree with MISR, page 13 lines 21-24. The only difference in smoke plume heights between MISR and CALIOP were that CALIOP observes smoke at systematically higher altitudes than MISR, stated at page 14 line 31, but this is also already found in previous studies. So for these results, CALIOP has no added value. Also, at line 25 page 14, it is stated that Huang et al., 2015 found the same smoke plume height values over the Amazon. Even though the method of Huang et al., 2015 is different, AOD is calculated for the whole Amazon area, while in this paper the AOD is calculated for individual plumes associated with active fires, no new information is found in this research. Maybe even Huang's results could have been used, because it could have been known that the individual plumes of CALIOP cannot be compared with MISR, so there is no added value in deriving them.

We disagree with the reviewer here. To our knowledge our study is the first to compare MISR and CALIOP on a plume-by-plume basis over the Amazon. As discussed above, despite having a large sample of plumes in both cases, there were serious limitation to this comparison that we highlighted in the manuscript. Then, to provide context for the MISR observations, we compare them with regional results from CALIOP. One would not expect the two to be identical; the similarities and differences contain important information about both the respective measurement techniques and the regional behaviour of smoke plumes in the Amazon. The MISR data adds considerably to the work of Huang et al., (2015), which used only CALIOP data, and the fact that they reach similar conclusions in many respects adds rather than detracts from the value of analysing this independent dataset.

So it should be stated more clearly in the methods section of paper, why both instruments are being used in this research and in the results/discussion or conclusions section of the paper, what the additional value is of using both MISR and CALIOP instruments and not just MISR. We have revised carefully the MISR-CALIOP comparison throughout the manuscript, as suggested also by reviewers 1 and 2.

3) In the introduction at page 2 line 4, it is stated that land-management policies cause significant variability in (not mentioned clearly) the spatial variation of fires. After this, in the methods section at page 5 lines 15-16, it is also indicated that one of the years from the climatology (2006) is a year when land-management policies measured limited deforestation. Finally, in the conclusions section at page 16 lines 17-19, the paper states that strong land-management policies can become crucial for the Amazon in controlling fires with changing future climate conditions. Apparently, land-management policies are of importance regarding this research. However, even though one year of adjusted land-management policy is included in the climatology, nothing is mentioned about this in the results/discussion or in the conclusions section. This feels like a missed opportunity, because even though it is only one year in the climatology and maybe nothing significant is found, in the introduction, methods and at the end of the conclusion this research implies that land-management policies could influence biomass burning. Because of this I think this research should include some results or discussion points about this year in the research.

We mention the land-management policies to inform the reader about specific factors that may affect the number of fires and/or their distribution across the Amazon. However, we do not

analyse the influence of land-management policies on biomass burning as it is out of the scope of our manuscript, and this topic has been covered extensively in the referenced literature (e.g., Nepstad et al., 2005, Aragao et al., 2010 and 2014, Reddington et al., 2015).

Minor arguments

Page 1, abstract/methods: It is nowhere explained why the dataset of MINX is 2005-2012 but the dataset of CALIOP is 2006-2012. CALIOP was launched in 2006, so data of 2005 are impossible to obtain, but why does MINX also includes 2005 in the dataset? Please explain this in the methods.

The digitalization of MISR smoke plumes is time consuming and requires a huge effort. For this work, we made use of all the smoke plume datasets that had been digitised over the Amazon prior to the focused effort for the current paper (2006, 2007 and 2008). To extend the record to a climatology we added 5 more years. We included 2005 as it was a year with severe drought as 2007 and 2010, and having three years to study the influence of dry conditions on smoke plume heights strengthens the conclusions of this work.

Page 2, line 4: It is stated that significant variability exists. But it doesn't say between what aspects significant variability exists, so please indicate this more clearly in the text.

We clarify in the text that significant variability refers to fires and note to the reviewer that all references on line 8 address this point in detail.

Page 3, lines 15-18: At the end of the introduction the objectives are mentioned. However what is missing here is the influence of land cover/biome type, because that is also studied in the paper. Please include this in the objectives.

Added as suggested.

Page 4, lines 13-14: The paper states that a user has to digitise the boundaries of the plume and indicate the direction of the smoke transport. How this should be done however, is not given in the paper. In order to be able to repeat the method I think it is necessary to indicate more clearly how the user should do this, or refer to a paper where this is done.

The procedure is described in great detail in Nelson et al. (2013), which is cited on page 4 line 24.

Page 5, lines 10-11: The best estimate maximum and median smoke plume heights are used, but it is not stated how these values are derived. In the paper of Martin M. V., et al 2010, the generation of these values is explained, but is it the same as for this study? And why are these two specific height definitions used and not the other ones that are given by MINX? Please explain this choice.

These are the same metrics as used in previous studies. They are the main ones produced by MINX, and are derived as described in Nelson et al. (2013), which is cited on page 5 line 19.

Page 5, lines 11-12: Smoke plumes are categorised with quality retrieval flags, but it is not explained how these categories are derived. The quality retrieval flags determine which plumes are taken into account for the climatology and which are not, so this could affect the total number of observations and it is important to have the right criteria for when a smoke

plume should be qualified as good or bad. Thus it is important to be transparent about these quality retrieval flags, so please explain how these are derived.

This is explained in Nelson et al., (2013) which is cited along the paper, specifically in Section 2.2, and we consider that it is not necessary to be repeated here.

Page 5, lines 23-25: In the paper it is said that the 60m difference in smoke plume heights between red and blue band retrievals can be neglected, because it is lower than the MINX uncertainty of 250 m. However, when this difference is not negligible this might influence the results because not all observations are retrieved with red and blue band, some only with blue or red band. So also for this, it is important to explain clearly why this difference can be neglected and to add a reference for the MINX uncertainty.

Nelson et al., (2013) describes the underlying technique, addressing all the related questions in great detail. As such, it is appropriate to reference that paper rather than duplicate it.

Page 7, lines 14-15: Only the grid cells that contain at least two MODIS fire pixels are associated with active fires, at 80

We do not understand what the reviewer means.

Page 7, lines 22-24: To ensure there is no bias in the 0.5x0.5 horizontal resolution, a 0.1x0.1 horizontal resolution for 2017 is obtained and it is stated that there are no significant differences. But it is not stated clearly between what the differences are, please indicate this clearly.

We have clarified the selection of CALIOP horizontal resolution, as suggested also by reviewers 2 and 3. In any case, we discuss in the manuscript that there is no important bias with respect of the number of plumes and estimated altitude. This is clearly explained in page 7 lines 22-25.

Page 15/16, conclusion and summary: In my opinion there is not enough of a retrospect towards the reason of why this research has been of importance for the Amazon area. This is very well explained in the introduction and I think it would strengthen the conclusion section and the recommendation for further research, so please elaborate on this in the conclusion section.

As suggested by reviewer 2, we have made the importance of our findings clearer in the conclusion.

Minor issues

Page 2, line 14: There seems to be a missing reference after the sentence: “The altitude...environmental impact”, please include the source.

Included as suggested.

Page 4, line 5: In this sentence there is referred to Kahn and Gaitley, 2015. However this reference is not given in the references section, please include this source.

We have added the reference Kahn and Gaitley (2015) in the references section.

Page 4, line 24-33: This paragraph is about the limitation of the instruments and might be better for the discussion.

We thank the suggestion. However, we consider that the discussion of instrument limitations fits well within the methodology.

Page 4, line 33: There seems to be a missing reference after the sentence: “In contrast...smoke layers”, please include the source.

Referenced as suggested.

Page 9, line 10: The word “of” is missing before the word “these”.

Corrected, as suggested.

Page 13, line 4: The word “swallower” should be the word “shallower”.

Corrected, as suggested.

Page 21, Table 2: Underneath the table there is some additional information where is referred to in the table with an “a” and a “b”. However underneath the table there are two “a” and no “b”, please change this.

Corrected, as suggested.

Page 23, Figure 2: The time series that the figure is given for is not mentioned in the caption, please include this.

We do not understand what the reviewer means. Figure 2 shows the MISR plume locations over the Amazon domain, without a time series.

Page 26, Figure 7: For MODIS FRP for the years 2007 and 2009 very high values are found, but nothing is said about this in the results. Also in this figure I don't really understand the necessary of putting the median value also in a number at the top of each boxplot, because it is already indicated inside the boxplot self. If there is no other reason behind putting this numbers here, then please remove them.

We assume the reviewer refers as ‘very high values’ to the averages and 67 and 90 percentiles in 2007 and 2009. The text discusses the annual media averages and percentiles are influenced by outliers, as she should know.

We decided to keep the median and number of observations on the top of the boxplots, as it helps the reader easily extract this information from the figure.

Page 27, Figure 8:

The symbols that are used for the years are hard to distinguish and difficult to interpret. Please use other symbols, or make them bigger, or find another way to indicate years.

We thank the suggestion. We tried to format the symbols in many other ways and that is the setting that we consider clearest. As the reviewer may see, the symbols also include the uncertainty within the annual media, and making the symbols bigger will cover the uncertainty bars in some cases.

To make the figure clearer, we added information to the caption.

“Relationship between MODIS DSI at the location of the plumes and MISR maximum plume height, MODIS FRP and MISR AOD annually averaged, for tropical forest (green), savanna (blue) and grassland (red). Symbols represent the annual average and bars the standard error of the mean. Regression lines are weighted by the number of plumes in each year; relationships with absolute $r < 0.4$ are plotted in dashed lines. Also included percentage of smoke plumes in the FT in each biome and by drought condition. Bar plots indicate the average of [Median Plume--PBL Height] > 0.5 km (light colour) and [Maximum Plume--PBL Height] > 0.25 km (dark colour), based on MERRA-2 PBL heights (see see text for explanation).”