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

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

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"Note to the editor and authors: As part of an introductory course to the Master programme Earth Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for Copernicus Journals, and the top students in the class have been asked to submit their reports to the discussion in order to help the review process. While these reports are written in the form of official (invited) re-

views, they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published. This report/review was supervised by Prof. Wouter Peters."

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.

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 θ(K)/Z(km) > 0 and unstable boundary layer conditions occur when θ(K)/Z(km) < 0 (Vilà J., 2017-2018) . In the results and discussion section of this paper an atmospheric stability of <2 K/km is designated as weak and 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 ex-

planation is given for the criteria values of atmospheric stability and the values thus cannot be validated.

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 (âĹij1500 m) than strong stability conditions (âĹij1200 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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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

Page 7, lines 22-24: To ensure there is no bias in the 0.5°x0.5° horizontal resolution, a 0.1°x0.1° 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.

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 is the conclusion section.

Minor issues

Page 2, line 14: There seems to be a missing reference after the sentence: "The altitude. . .environmental impact", please include the source. 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. Page 4, line 24-33: This paragraph is about the limitation of the instruments and might be better for the discussion. Page 4, line 33: There seems to be a missing reference after the sentence: "In contrast. . .smoke layers", please include the source. Page 9, line 10: The word "of" is missing before the word "these". Page 13, line 4: The word "swallower" should be the word "shallower". 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. Page 23, Figure 2: The time series that the figure is given for is not mentioned in the caption, please include this. 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. 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.

References

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Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-931/acp-2018-931-SC1-supplement.pdf

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