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Atmospheric Chemistry and Physics Discussions

Interactive comment on “ Smoke injection heights from agricultural burning in Eastern Europe as seen by CALIPSO” *by* V. Amiridis et al.

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We would like to thank anonymous referee #2 for his/her constructive comments and suggestions that have really helped us prepare a new and improved version of our work.

Response to Anonymous Referee #2

General comments

This paper deals with a data-only analysis of biomass burning emission heights. Several sources of data are used 1. CALIOP aerosol heights 2. MODIS fire radiative power 3. ECMWF winds and boundary layer heights The main results are that (i) a

substantial amount of plumes penetrates the boundary layer height diagnosed by the ECMWF (ii) a relation exists between Fire Radiative Power and smoke plume injection height. These results are interesting for a broad public. However, I miss a detailed discussion about the relation between this work and other work (e.g. Val Martin, 2010). Moreover, figure 6 calls for additional analysis. Finally, I think that the reference to the CALIPSO level 2 products should be removed from the paper. It serves no purpose and the authors decided to use their own method based on the level 1 data, in combination with a smoothing algorithm. This smoothing algorithm cannot be applied to the level 2 data (how to smooth aerosol layers?) so the authors should better spend the space to explain their smoothing algorithm, which seems rather arbitrary. These three points should be addressed in the final ACP paper.

In the revised version, figure 6 has been updated to include additional analysis results also addressing issues raised by reviewer 1. More detailed discussion about the rela-tion between this work and other studies has been included. Additionally, the CALIPSO level 2 products have been removed and a detailed description of our CALIPSO's data processing has been included. Detailed answers on the abovementioned issues raised by the reviewer are given in our answers below.

Minor comments

- page 19250 line 22: CALIPSO lidar show a.....studies. Sentence can be removed.

The sentence has been removed

- page 19252 line 15: ..that was launched...

Corrected

- page 19253 line 2: CALIPSO produces Level 1 and Level 2 scientific data products. CALIPSO is the instrument, and scientific products are generated by algorithms or people. Please rephrase.

The sentence has been rephrased as: “Level 1 and Level 2 scientific data products de-rived from raw CALIOP lidar measurements are archived and distributed by the At-mospheric Science Data Center (ASDC) of NASA (http://eosweb.larc.nasa.gov/)"

- page 19253 line 7: The CAPILSO level-2 aerosol. I think that a detailed description can be left out here. Just mention that you use the level 1 data (see main comment above).

Most of the CALIPSO's algorithm description text has been left out. In the new ver-sion of our manuscript, we just mention the versions and levels of products that we use.

- page 19254 line 4: This seems logical. It is better to explain here that the ECMWF mixing layer depth at 12 UTC is closest to the measurements. Also explain that this might result in a bias, since normally the boundary layer develops further in the 10:00 UTC to 12:00 UTC time frame. Also, the ECMWF mixing layer height is just a model value, and validation efforts show that the mixing layer depth is not the best represented quantity in the ECMWF model.

Explanations suggested by the reviewer and related comments have been included in the new version of our manuscript.

- page 19254 line 7: The bulk ...conditions. Sentence unclear, please rephrase.

The sentence has been rewritten.

- page 19255 line 15: The numbers can be given with less accuracy (e.g. 11 to 439) and the unit should be MW per square kilometre I think. Correct this throughout the doc-ument and also add the unit to figure 6.

The unit is indeed MW per square kilometer, since the pixel resolution that the num-bers refer to is 1X1km. The units have been corrected and the numbers are given with less accuracy in the new version of our manuscript.

- page 19255 line 20-26: Following .....to 5. I do not see the relevance for this paper and suggest to remove these sentences.

The sentence and the corresponding reference have been removed.

- page 19256 line 7 and further: ECMWF has a surface wind product, which is much more relevant than the 850 hPa winds. Also, to show that fires are ignited only with low wind speeds, you should compare winds with fires to winds without fires. Now we have to rely on your classification: "These data indicate weak to moderate horizontal winds...".

The ECMWF wind data used in our study have been re-examined following the re-viewer's suggestion. At 850 mb the mean wind speed (at the location and the time of the fires) is 6.18 m/s (stdev=3.46 m/s), while the mean wind speed at the same loca-tions during the July-August 2006-08 period, excluding the time of the fires, is 6.85 m/s (stdev=0.626). One must bear in mind that the latter mean value (of the cases without fires) is the average of a much larger dataset, than the one of the cases with fires. Therefore it appears that at 850 mb the wind speed of the "fire" cases is lower than that of the "non-fire" cases. SEE Table on Figure 1 of our answer Similarly, at 10m above ground, the mean wind speed (at the location and the time of the fires) is 3.95 m/s (stdev=1.95 m/s), while the mean wind speed at the same loca-tions during the July-August 2006-08 period, excluding the time of the fires, is 4.34 m/s (stdev=0.4). Therefore it also appears that at 10m the wind speed of the "fire" cases is lower than that of the "non-fire" cases. Moreover, it is obvious that the mean wind speed during the "fire" cases is weak (3.95+-1.95 m/s) near the ground. Regarding the frequency of the cases that the wind speed at a specific fire location is stronger or weaker than the mean wind speed averaged during July-August 2006-08 (without the fire case), it appears (from the table and the two histograms following in Figure 1 of our answer) that in the majority of cases at 850mb and 10m (61.78%, 60.53% respectively) the "fire" cases have weaker wind speeds. (SEE Table on Figure 1 of our answer) In the new version of our paper, the wind speed at 10m above the surface is reported (instead of 850 hPa), and the mean value of wind speed for the area (cases without fires) is additionally mentioned for comparison.

- page 19256 line 14: Thus, ...to the atmosphere. This analysis is too easy. First, it is based on the 850 hPa winds and not on surface winds. Furthermore, wind and wind shear may play an important role in heat dispersion of the plumes. So, the poor relation between intense fires and injection heights in figure 6 may have to do with wind (although you briefly mention later that there is no relation). Since you have access to ECMWF data, a straightforward analysis would be to analyse wind and stability profiles for all the fires. This connects also to the Val Martin paper (2010), who presented a more mechanistic analysis of plume rise.

Following the reviewer's suggestion, new ECMWF data concerning stability and wind fields have been used in our study. In Figure 2 (left panel), the winds at 10m from the surface during the fires are presented. Figure 2 (right panel) illustrates the static stability of the layer 850-925 hPa, through Âűθ/Âűp (where θ is the potential tem-perature and p the pressure), against the one of the lowest layer 925-surface for the "fire" cases. Negative values of the vertical gradient (Âűθ/Âűp) indicate stability. The vast majority of the cases is associated with an unstable lower layer (surface-925 hPa), probably due to the fact that 12 UTC analyses were utilized, and a nearly neutral upper layer (850-925 hPa). The rest of the cases are associated with stability between 925 and 850 hPa. Wind shear for the layers 850-925 hPa and 925-surface were additionally calculated (see the figure that follows in our answer), but not presented in the paper.

In any case, the wind and stability profiles did not help to explain the scatter in Figure 6, or group the data in a form of Val Martin's classification. We believe that these meteorological parameters are not trustworthy for the fire cases examined in our paper, since ECMWF do not consider the effect of fires in the temperature profile in the simulations. Moreover, we believe that the scatter of our results in Figure 6 could be most likely attributed to the fact that the smoke plume usually extends downwind the fire (as the reviewer pointed in his previous comment), and this effect cannot be excluded from our plot, since this effect has to do with data limitations (CALIPSO). All the above changes and respective discussion is included in the revised text.

- page 19256 line 17: I suggest to rewrite this paragraph. Now there seems to be a conflict between "not easy to implement" and "The method is very simple". The need for smoothing in the horizontal and vertical is not well explained. Moreover the discussion of the CALIPSO level 2 products is not necessary. Since you apply horizontal and vertical smoothing, I do not see how you can compare to the level 2 product in figure 3. Still you say at line 12 in page 19258: "...is not consistent with the attenuated backscatter profile..."; and "After the demonstration of the cases that CALIPSO level 2 product fails to estimate top layer height...". This comparison does not add much to the paper and the arguments are not very strong either. Better explain your smoothing is relation to the horizontal dispersion of the plume. This sounds as a difficult problem to me, since the CALIPSO instrument crosses at high speed with a tiny footprint. Figure 4 and discussion can also be removed for the same reasons.

In our work we are not trying to suggest a new method for the retrieval of aerosol layers, and we fully agree with the reviewer that the comparison between our method and CALIPSO's level 2 products is not necessary. It would be much easier for us to simply use CALIPSO's level 2 aerosol layer retrievals but there are lots of reasons that we have decided to proceed with a deeper analysis, by examining the complete level 1 profiles along with the aerosol layer product. First, by examining the level 1 profiles we were able to separate the cases of strong aerosol convection, by selecting only the cases of constant attenuated backscatter coefficient with height. This information is not available on Level 2 products, since only the aerosol layer heights are provided in these files. Second, we were able to apply different spatial averaging to CALIPSO level 1 profiles to better collocate lidar and MODIS data and decide the spatial resolution that permits a sufficient noise reduction for layer retrievals for the fire cases under study. The comparison of our results with level 2 CALIPSO products was then revealed for the same spatial resolutions that CALIPSO uses for layer 2 products, showing a discrepancy in very few cases. In our opinion this is expected for an auto-mated lidar algorithm and is pointed also to previous work of other researchers who suggest the need to better analyze CALIPSO retrievals on level 1 than rely on level 2 automated products (e.g. Lenobe et al., 2001). To avoid misunderstandings and since it is not our scope to suggest an algorithm in our paper, we have removed Figure 3b and Figure 4 from our text. In any case, the injection heights that we have used in our paper following our method are in few cases slightly different from CALIPSO's level 2 retrievals. The methods that we followed are better described in the new version of our paper.

- page 19260, line 1: Say something about the correction you applied for orography (and the general lack of orography in the region in general).

In this paragraph we report the CALIPSO retrieved injection heights with respect to mean sea level (which is stated in the text), so the results would be comparable with other studies as well (e.g. Labonne et al., 2007). The correction for orography and the general lack of orography in the region is written in the first paragraph of section 3.3: "The injection heights from CALIPSO used for this comparison are corrected to refer to the surface elevation instead of sea level, for consistency with FRPs which refer to surface fires. This correction could be significant in case of profound orography. However, the exact surface elevation levels of the points included in Figure 6, taken by the Digital Elevation Model used by CALIPSO at the lidar footprint (GTOPO30 digital elevation map), vary between 0 and 196 m (mean = 125 m, standard deviation = 65 m), and this is rational since we are dealing with agricultural fires initiated in crop fields which are mostly located at non-elevated terrains." The boundary layer height (BLH) field in the ECMWF data corresponds to the BLH height above topography. The figure that follows in our answer shows the topography (m) of the area of interest. It appears that most of the area has a height less than 200m. From the histogram that follows, it is additionally shown that the 90% of the "fire" cases occurred at locations with an elevation smaller than 250 m, while approximately 60% of the "fire" cases occurred at locations with an elevation up to 150 m.

- page 19260, line 23: Please report units for FRP.

Units for FRP have been reported.

- page 19261, line 8 and further: This is pure speculation. A more detailed analysis is required here. You have access to ECMWF data and you can verify under which cir-cumstances meteorological factors might dominate. The most simple approach is to filter the data for specific atmospheric conditions (e.g. very (un)stable temperature profiles, very windy days) and to redo the analysis on the filtered data. You mention somewhere that "no significant anticorrelation between horizontal wind speed (at 850 hPa!) and injection heights is revealed. Extend this analysis to thermal stratification. Also the fact that the FRP from MODIS cannot be trusted due to dense smoke clouds sounds like a weak argument, since obscured fires would have an underestimated FRP, which makes the situation analysed in figure 6 only worse.

In the text we have clearly stated that the initial data showed enhanced dispersion of the values, especially for edge values of FRP (very low or very high) and we have cho-sen the binned data analysis in order to introduce a tendency of increasing injec-tion height with FRP rather than a strong existing correlation. We agree with the re-viewer that the choice of the binned data analysis might allow doubts on the interpre-tation and conclusions; however this is not the case as shown below. We have re-plotted Fig. 6 with the initial data rather than the binned ones and the high dispersion of the data is now additionally evidenced. To be comparable with other studies (e.g. Val Martin et al., 2010), the Figure 6 is presented now in a log-log plot. The scatter was found to be comparable with Val Martin's, the slope is consistent giving a relative dependence of injection height on FRP and the intercept differences revealed are attributed to the latitudinal differences for our study comparing with Val Martin's (Eastern Europe vs. Northern America). Given the particularities of the area, the fire characteristics and our intention to discuss on the statistical limitations of this result, we strongly believe that this trend is very interesting and useful and merits to be shown. Moreover, the elimination of data from an analysis, when they so obviously do not match the trend of the rest of the data, is not forbidden when solid argument is given on why to consider these data as outliers. Regarding the second part of the comment concentrating on the possible presence of dense smoke influencing the larg-est power bin, our explanation was based purely on the fact that FRP depends on the emissivity which in turn depends on the fire dynamics (smoldering versus flaming), the last being the driver for the presence of dense smoke or not (Kahn et al., 2007). However, and we than the reviewer for stressing this point, now we understand that this explanation is valid for the cases of high injection height for low FRPs, since dense smoke underestimates FRP values (Kahn et al., 2007), thus the suggestion of the reviewer that atmospheric stability is possibly the reason for B case mismatch is more solid. However, we tried to classify our results with stability and wind shear, taken from ECMWF fields (stability simulations are now shown in a new Figure). Neither para-meter explained the scatter in our results. We believe that these meteorological para-meters are not trustworthy for the fire cases examined in our paper, since ECMWF do not consider the effect of fires in the temperature profile in the simulations. Moreover, we believe that the scatter of our results in Figure 6 could be most likely attributed to the fact that the smoke plume usually extends downwind the fire (as the reviewer pointed in his previous comment), and this effect cannot be excluded from our plot, since this effect has to do with data limitations (CALIPSO). All the above changes and respective discussion is included in the revised text.

- page 19261, line 20: In both figure 7 left and 7 right: replace by something like: In both panels of figure 7..

The text has been replaced according to reviewer's suggestion.

- page 19261, line 29: Mention that the satellite overpass is always earlier, and that the boundary layer height is usually still growing from 10:30 to 12:00 UTC (possible bias).

The following comment has been added: "However, a possible bias in our comparison could be introduced by the fact that CALIPSO/MODIS overpass is always earlier than 12:00 UTC, and that the boundary layer height is usually still growing from 10:00 to 12:00 UTC."

- page 19262, line 27: 3.0 km for the 17.3 .....remove "the".

Corrected

- page 19264, line 17: CALIPSO lidar show....studies. Remove sentence.

The sentence has been removed.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19247, 2010.

	850mb		10m	
	With fire	Without fire	With fire	Without fire
Mean (m/s)	6.18	6.85	3.95	4.34
Stdev (m/s)	3.46	0.62	1.95	0.40

	850mb		10m	
Wind speed Difference* ("fire"-"non fire")	Frequency	Percentage	Frequency	Percentage
Difference <0	6911	61.78%	6771	60.53%
Difference >0	4263	38.11%	4399	39.33%
Difference=0	12	0.11%	16	0.14%

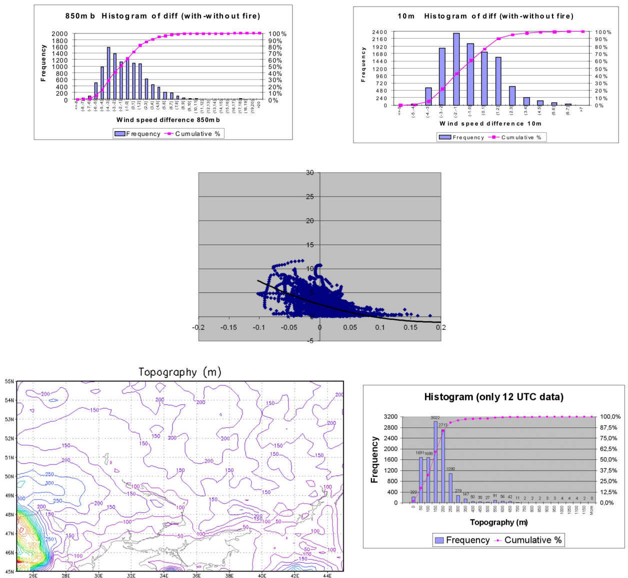

Fig. 1.