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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 #1 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 #1

- In this paper, the author use MODIS detection of agriculture fires, together with an estimate of its intensity, ECMWF analysis that provide the boundary layer height, and an estimate of the aerosol layer height derived from Calipso measurements. The goal is to analyze the injection height of the plume associated to the fire and how it re-

lates to the fire intensity and/or the boundary layer height. Such papers are certainly needed. Indeed, better parameterization of injection heights are needed for a proper modelling of aerosol transport. In addition, the fate of biomass burning aerosol strongly depends whether they are emitted within or above the mixing layer height. The authors also choose the proper data for such tools and they reach in-teresting conclusions. I have several criticisms however on the methodology and the result interpretation that should be corrected prior publication. As I understand, the authors have selected MODIS/CALIPSO pixels when a fire is de-tected by MODIS on this particular pixel. They then interpret the CALIPSO vertical profile that is just above it (or within 2.5 km along the Calipso track) as being repre-sentative of the plume. My experience, and this is well shown with MISR data, is that the plume extends downwind of the fire. I do not expect the aerosol profile just above a fire to provide an indication of this particular fire injection height. Rather, it is probably representative of another fire upwind, that may have a very different intensity than the one that is looked at.

In our work, we consider as injection height the height of smoke emitted by the agri-cultural fires "near the fires". CALIPSO profiles above MODIS hot spots were care-fully selected with the criterion that the attenuated aerosol backscatter coefficient should be constant with height, indicating that no elevated layers from distant sources had been advected over the pixel under study. This argument is additionally supported by the light winds prevailed over the area during the fire. The reviewer is right on this, indeed the smoke plume usually extends downwind the fire, however the injection height for modeling needs is associated with the initial height that the smoke emission reached within the troposphere above the source. In this study we attempt to provide this parameter and not to characterize the plume geo-graphical extent. Moreover, this task is not possible to be reached using CALIPSO, since lidar data refer to small pixels (MISR data would be indeed required for such a study).

- Another criticism is the procedure that is used to derive the aerosol height. The authors chose not to use the official aerosol layer product for reasons that are not really convincing. As an argument, they show one case where the official CALIPSO product indicates three distinct layers, although the top of the heighest layer is in agreement with their own estimate.

The reviewer refer to the case presented in Figure 3-b. Considering the scope of our paper to derive smoke injection heights, the most reasonable choice for the Level 2 user in the example of Figure 3-b, would be the lower reported layer given that the smoke plume source is at the surface while the elevated second or third layers would be interpreted as advected air masses from remote sources. The user of CALIPSO layer product is not able to observe the complete Level 1 profile of the attenuated backscatter coefficient. For the same example, Level 1 profile indicates a vertical homogeneous distribution (shown in Figure 3-b) up to the upper limit of the most ele-vated layer, thus, the Level 2 user would have eventually used a false injection height from CALIPSO level-2 product, 2 km instead of 4.5 km. In any case, the identifica-tion of 3 distinct aerosol layers in level 2 data is not consistent with the attenuated backscatter profile reported for the same day and time, where only one layer is visible from the surface up to about 4.5 km

- There has been a lot of work into the aerosol layer detection algorithm, as well as evaluation and validation, and I doubt that a very simple method like the one suggested by the author can do any better. There is no clear demonstration that one can trust the aerosol height derived through such a simple method. In the text, the official product is compared to the authors estimates which is used as a "truth" (ie they discuss whether the official product underestimate of overestimate the aerosol height). The authors should be more careful as their own product has not be evaluated against independent data;

In our work we are not trying to suggest a new method for the retrieval of aerosol lay-ers, and we fully agree with the reviewer that CALIPSO's algorithm is much more evaluated (although not adequately validated) than our extremely simple approach. 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 differ-ent 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 automated 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). We do not suggest here a more sophisticated algorithm for layer retrievals, but we point that due to the limited dataset that we had to analyze, we were able to go through a detailed analysis and avoid false detections of aerosol layers due to signal noise, wrong cloud discrimination etc, that are always possible for automated algorithms such as CALIPSO's. However, it is more than clear that such algorithms are essential to analyze large data sets of lidar backscatter profiles as those of CALIPSO for the detection of geometrical/macrophysical aerosol properties with no alternative. To avoid misunderstandings and since it is not our scope to suggest an algorithm in our paper, we have removed 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 differ-ent from CALIPSO's level 2 retrievals. The related comments were also deleted.

- In the text, it seems that the authors want to show that there is a clear relationship be-tween fire power and injection height, even when the data do not really support this conclusion. The discussion is based on Figure 6. The authors argument is based on the fact that the median values of the injection heights are nicely aligned for four bins of fire power in the range 10-40 MW. Yet, i) there is a huge dispersion around the median values and ii) the values for lower and higher fire powers are in full disagreement with the trend derived from these four points. I do not agree with the argument given to eliminate these bins. For instance, it is said that, "for the largest power bin, the FRP cannot be trusted due to the presence of dense smoke". What is the hypothesis? Does one say there is thick smoke because it is a large fire ? But then the case is in the proper bin. Or is the smoke is so thick that à "medium" fire was incorrectly placed into the large fire bin? But then, why do we trust other cases with a medium fire? There is no indication that the trend is statistically representative, especially since the data that do not match the trend have been eliminated. I therefore strongly suggest that the author be somewhat more careful in their data interpretation. It has been shown that atmospheric stability is a key parameter for injection height, and the variability in the atmospheric stability may explain the dispersion in the results.

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 chosen 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.

- Similarly, I am not convinced by the interpretation that fires with a low confidence show a high correlation between BL height and aerosol top height, while cases with high confidence do not (Fig 7). The explanation given is on the intensity of the fires. But then, why not show the graph for low and high fire intensity rather than low and high confidence?

The "fire confidence" MODIS product is a measure representing the level of confidence that the observation is indeed a "true" fire (ranging from 0 – 100%). Following the user guide for MODIS fire products, the users requiring fewer false alarms, may wish to consider only high-confidence (greater that 80%) and treat low-confidence fire pixels (less than 80%) as clear, non-fire, land pixels. Considering this statement, the data on the left panel of Figure 7 refer to non-fire pixels, showing that in these cases ECMWF BL heights are in good agreement with CALIPSO derived heights of the top of the aerosol layer. This plot could be considered as an indication for the as-surance of ECMWF BL simulation quality in cases with no fire. In the right panel of Figure 7 we show then that ECMWF model is not able to simulate the BL in case of fires with high-confidence, and we speculate on the reasons, which are most attributed to the fire intensity. The relation of fire intensity with the injection height is presented in Figure 6. Low-confidence fires have been excluded from this plot, since FRP values are not trustworthy for these cases. However, wrong impressions could be conveyed from the text, which has been re-written clearly.

- Another point, that is less important, is the discussion on the mean wind speed (page 10). The authors say that the mean wind speed (7 m/s) is weak to moderate because the farmers choose such calm conditions for lighting the fire. I do not think that 7 m/s is really small and wonder what is the seasonal average (ie without a selection of the "fire" days).

The ECMWF wind data used in our study have been re-examined. 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 locations 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. Similarly, at 10m above ground, the mean wind speed (at the location and the time of the fires) is 3.47 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 was found that in the majority of cases at 850mb and 10m (61.78%, 60.53% respectively) the "fire" cases have weaker wind speeds. 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. The above-mentioned results are presented in the tables and figures attached.

- The English writing needs some improvements (just as this review probably does), but this is less important than the content.

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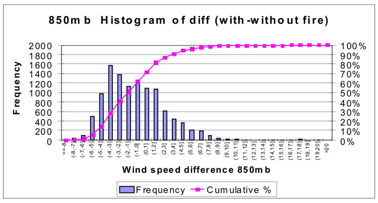
	850mb		10m	
	With fire	Without fire	With fire	Without fire
Mean (m/s)	6.18	6.85	3.47	4.34
Stdev (m/s)	3.46	0.62	1.95	0.40

	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%



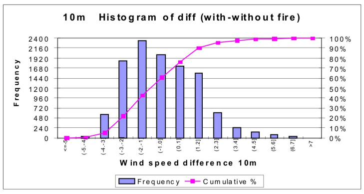


Fig. 1.