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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.

Anonymous Referee #1

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Review of "Smoke injection "eights from agricultural burning in Eastern Europe as seen by CALIPSO", by V. Amiridis et al., submitted for publication in ACP

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 relates 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 interesting 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 detected 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 representative 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.

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 realy 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. 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 the text, it seems that the authors want to show that there is a clear relationship between 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.

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 ?

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

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