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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Elevated ozone in boreal fire plumes – the 2013 smoke season" *by* T. Trickl et al.

Anonymous Referee #1

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General comments: This paper by Trickl et al, presents and discusses four case studies concerning the appearance of atmospheric layers with elevated aerosol contents as detected by the aerosol Lidar working at the Garmisch-Partenkirchen (Germany).

Since several years, the lidar systems working at IFU provide a set of high-quality observations concerning aerosol, ozone and water vapor, very useful to investigate the transport of these atmospheric tracers in the free troposphere and the atmospheric processes affecting their variability.

Concerning the paper significance, the presented research is well within the scope of Atmospheric Chemistry and Physics. The most original findings is related to the possibility that the ozone increases observed during some of the aerosol plumes are

related to Stratosphere-to-troposphere exchange and not to photochemical production by ozone precursor emitted by boreal fires. However, the authors relies too much to the "large-scale" HYSPLIT back-trajectories for interpreting and attributing the occurrence of ozone and aerosol layers. I think this represents the major weakness of this work that should be addressed or better discussed before publication to ACP.

Moreover, more discussion should be deserved in commenting the implication of the achieved results to scientific progress. E.g., as stated from the authors at pag 13266, events like those presented in this work are not frequent (at least as deduced by Garmisch Lidar measurements): thus, why we should be concerned about that?

The presentation quality is fair. However, too many figures are presented and they should be significantly re-arranged before publication. I recommend to make use of the supplementary material to show "ancillary" information and to improve the manuscript readability.

Finally, if I can make a personal comment, it is really a shame that the very valuable continuous in-situ measurements at Zugspitze peak have been interrupted.

Major comments: To attributing the origin of the aerosol/ozone/water vapor layers, the authors made a massive use of the HYSPLIT back-trajectories. However, the authors did not provide many details about simulations set-up (i.e. which meteorological data-set is used for input, at which spatial resolution?) or comments/quantification of trajectory uncertainities. The figures showing HYSPLIT back-trajectory plots reported CDC1 meteorological file (e.g. Fig. 18-19). If the NCEP re-analysis data provided by the READY laboratory is used, they are characterized by 2.5 degree latitude-longitude on a global grid. I'm rather skeptical about the possibility that single back-trajectories initialized by this coarse meteorological data set can be able to diagnose the origin of the thin atmospheric structures like those presented by the lidar vertical profiles. I recommend that the authors re-calculated (at least) ensembles of back-trajectories for evaluating the robustness of the identified transport patterns and discuss the uncer-

tainty related with layer attribution (as partially done for instance for the event on 13 and 16 July). In the case, the authors already done this "sensitivity" study, the results should be more clearly discussed in the paper.

By Table 1, you listed 14 events. But, you described just 4 of them. The information from this table is used only sporadically in the paper. I would suggest to move it to Supplementary Material or to better discuss the table information (as an instance in the Section 5).

Specific comments Please use the nomenclature" equivalent black carbon" (eqBC) along the paper, because you used an optical method (MAAP 5012) for determining its concentrations.

Pag 13273: I'm not sure if "personal communication" can be used as a reference. A published result can be preferable.

Pag 13274 (figure 4): it is difficult to clearly see the "specific broad hump" in the aerosol "structure". Several features are visible in the CO and aerosol variations. Probably, you should superimpose colored areas to clearly identify the periods possibly affected by the fire events and the dust outbreaks. Please consider the possibility to move this plot to the Supplementary Material.

Pag 13275: please join in a single figure (e.g. plate A and B), Figs 5 and 6. This will allow the reader to directly compare the two profile. Line 10: "moderate ozone": looking at the whole profile, it looks that a minimum in ozone appeared between 5.5 and 8 km, thus not so "moderate"... Line 15: please describe how behaves the radiosonde RH profile (maybe adding it to the ozone profile reported in Fig. 6). Moreover, a map with location of experimental sites would help the reader to better evaluate the spatial representativeness of radio-sounding stations. Line 18: I do not see the need to cite STE forecast (not used) in this context. Line 20: you should show these back-trajectories outputs (even in the Supplementary Material).

Pag: 13276, line 1-10: here the authors admit that a slight change of back-trajectories initialization (time or altitude), change the analysis result (no fire influence). This is a hint for uncertainty in the attribution of air-masses path and origin. A more sophisticated analysis (e.g. calculation of back-trajectories ensemble) is required to better assess this point). Section 4.2.2.: Again, merge Fig 8 and 9 and show the RH profile. Also show back-trajectory analysis. This can be valid for all the case studies.

Section 4.2.3: what do you mean by "HYSPLIT detects the layer exactly at 3.2 km, but locates the import from the zone of fires slightly above this"? HYSPLIT do not provide information about aerosol contents. lease explain better in the text. Line 21-26: are HYSPLIT transport simulation enough reliable? Also these sentences, point out the necessity of better discussing and quantify the uncertainties related with these back-trajectory results... Line 30-31: please quantify the RH values seen by radio-soundings.

Pag 13278: the pictures by Figure 12 and 13 are really impressive but I would move them to Supplementary Material. Line 21-23: please provide a reference where it is possible to see a typical stratospheric intrusion layer development. Figure 14. I'm not a Lidar expert but there are no possibility to separate fire from PBL fine particles? This would help in assessing the possible intrusion of the BB plume to the PBL...

Pag 13279, line 4-5: please provide a reference. Line 14-24. It is rather complicated for the reader to match the different layers in aerosol, ozone and water vapor profiles. E.g. you mentioned low humidity in"two partial plumes". What do you mean for "partial"? To provide the exact altitude of the layers can help the reader in identifying what feature are you discussing...

Pag 13280. Please, merge Fig 18-19 in one figure (plate A, B). Did you try to calculate forward trajectories from the end-point of the backward trajectory calculated on 9 July 2013 to evaluate if you return to the starting location of the back-trajectory?

Pag 13281, line 13, I would change "correlation" with "link or relationship". Line 14.

This sentence is rather “strong”. I would specify that your results can be valid for the presented case studies.

Discussion and conclusions: I think that to refer to intrusion “Type X”, do not really add important information to the paper. Line 7 -12: this sentence seems a little bit out of place: you did not investigate STE seasonality in this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13263, 2015.