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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Automated time-height-resolved airmass source attribution for profiling remote sensing applications" *by* Martin Radenz et al.

**Anonymous Referee #2**

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Characterization of atmospheric aerosols using ground based lidar measurements depend upon accurate attribution of the sources of the aerosol. Often this is achieved by selecting specific altitudes and times representing interesting features in the lidar profiles and then running back trajectories from those locations. This paper describes a method to do this in a continuous way by using ensemble trajectories from the HYSPLIT or particle dispersion modelling from FLEXPART. The authors had presented elements of this methodology in earlier publications and have now presented them in a consolidated way. They give examples showing the application of the methodology as well as give an assessment of the representativeness of time limited ground based lidar observations. This is an interesting paper and may have ramifications for other applications.



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Discussion paper

The basic contents of the manuscript are clear. However the presentation suffers from many typos, grammar and English usage issues. The paper is well within the scope of ACP and will be useful to the remote sensing community. I recommend publication after some revisions. Here are my comments/suggestions in no particular order:

1. While the paper is geared towards the ground based lidar observations, I am wondering if the methodology can be adapted for global spaceborne observations by say CALIPSO and other forthcoming lidars. If the sources of, say the dust layers observed at a remote location by CALIPSO could be reliably and continuously attributed in an automated way, then as a first approximation, one may be able to assign the lidar ratios corresponding to those sources which are known to vary significantly. Similarly, it is conceivable that variable lidar ratios may be assigned to the ageing smoke layers using this method. Using variable lidar ratios in this way should improve the extinction products from elastic lidars like CALIPSO. It will be good if the authors could discuss the feasibility of this scenario, which would add to the value of the paper.

2. I think it will be nice to have validation of some of the results presented. For instance, in Figures 10-12 the authors analyze an aerosol blob between 2-6 km which is estimated to be originating from Australia. From the retrieved lidar ratios and depolarization ratios, it appears that the layer is likely to be lofted smoke. However, it would be good to provide evidence of fire events in Australia around 15-20 May 2019. Do CALIPSO transects close to Punta Arenas on May 20, 2019 show any lofted smoke layer? In Figure 11, The lidar ratio at 532 nm between 2-3 km is about 50 sr and below 1 km is even higher. Would the authors comment on these. There are also some differences between the HYSPLIT and FLEXPART simulations for this case (Figure 12c and 12d). Perhaps a better example would be transported plumes from Australian fire events in January 2020—as described in Ohneiser et al. (2020, ACP, 20, 8003).

3. Why not give the altitude scale in Figures 4, 6 and 10 in km as in the other plots for uniformity? Also the plots for the lidar data and residence time profiles may be shown up to the same altitude for easier comparison.

4. I think some sort of sensitivity study will be useful, e.g running the trajectories for different number of days and checking if this leads to any difference in the results. Similarly, does varying the number of trajectories improve the difference between the HYSPLIT and FLEXPART results discussed (line 135-136) in relation to Figure 5 or Figure 12?

5. Adding location maps showing the points of observation will add context to the Figures.

6. Adding the corresponding depolarization plot for Figure 4 will be helpful.

7. I am a little confused about the features at the lowest altitudes in the lidar observations. For instance in Figure 4, the highest backscatter values occur at the lowest altitudes below 1 km. Firstly, for the sake of completeness, I think it would be better to reproduce the Yin et al. (2019) Figure 14, instead of the reader having to go back to that paper or better still, present the manual analysis for another segment (and include the profile of lidar ratios in that plot). As mentioned in Yin et al. (2019), the extinction coefficients within the MBL are too large to be entirely from the marine aerosols. Could these really be explained by the pollution coming from Europe with their relatively small contribution to the residence times? Similarly, in Figure 10, very high backscatter can be seen between 14-18 UTC around 1.5 km, but the authors do not mention this in the discussion. Is this a measurement artifact? Similar high backscatter blobs can also be seen between 4-8 km at different times in this Figure.

8. Add unit of accumulated residence time in Figure 1b. The accumulated residence times from HYSPLIT and FLEXPART are very different in Figures 5, 9 and 12 and creates confusion for comparison. The reason for this should be clarified in the text.

9. Define NR in legends to the lidar Figures.

10. For completeness it would be good to include an example of the FLEXPART simulations.

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