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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Combining airborne in situ and ground-based lidar measurements for attribution of aerosol layers" *by* Anna Nikandrova et al.

Anonymous Referee #3

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I have reviewed "Combining airborne in situ ground based lidar measurements for attribution of aerosol layers" by Nikandrova et al. The work presents a combination of results derived from a range of instrument systems deployed during the BAECC (Biogenic AerosolsâĂŤEffects on Cloud and Climate) campaign conducted at a field site in southern Finland. The work presents evidence of lofted layers of aerosol and examines differences in the aerosol size distributions, lidar backscatter, and lidar depolarization ratio for two case studies. Overall, the results presented in the manuscript should be interest to the larger research community. I believe that he manuscript will be acceptable for publication after addressing a few significant concerns: 1. The work makes a point of using relative humidity (RH) to help define layers and to better understand some of

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the differences in the observed size distributions. The authors point out that the ambient RH was measured near the aerosol inlet, but there is no discussion of how the RH might change as the particles move through the system. Perhaps the impact is small, but it should be addressed in the manuscript in some fashion. In addition, there is little detail given about the RH measurements themselves (for example, what instrument is used to make the measurements). 2. The authors describe that the atmospheric thermodynamic structure is hard to interpret for Case II. Based on the figure derived from the radiosonde, it looks like it could be a more typical profile with clear subcloud layer to an altitude of 750 m, and then a cloud layer from 750 m to approximately 1700 m. It could be helpful to examine the potential temperature profile in addition to the humidity in this case.

Minor comments: 1. Page 1, line 14: The acronym DOE (rather than DoE) is generally used for the Department of Energy 2. Page 1, line 17 (and other locations): The terms “low” and “high” are used throughout the manuscript when referring to variability, aerosol concentration, and other meteorological variables. It is better usage to use “small” and “large” unless one is referring to differences related to altitude. 3. Page 2, line 15. The authors might want to consider adding a reference to Wang et al. (2016), a Nature paper also looking at vertical transport of aerosol. 4. Page 3, lines 2-6. Muller et al. (2014, AMT) also showed comparisons of aerosol microphysical properties derived from HSRL with in situ data. 5. Page 2, line 20. Suggest using “aerosol” or “particles” rather than “aerosol particles”. 6. Page 5, line 16. Does the reference to BLs really mean convective BLs or all BLs? 7. Page 5, lin3 24. The manuscript cites the work of Laakso et al. (2004). Are their results for the same geographic area? 8. Page 6 (figure 1). The humidity profiles in the figure are hard to interpret due to the lack of a scale. Is there a way to add a scale or axes that would not make the figure too hard to read? 9. Page 6. line 21. It is not clear to me how you determined the interface zone, and what you really mean by the term. It is addressed a bit later in the manuscript, but some additional explanation here would be helpful when the term is introduced. 10. Page 7 (and Figure 2 and 7). Is there a way to mark the specific layers on the plots of aerosol

backscatter? I know that there is a table, but it could be helpful to add the information to the figure (assuming that the figure remains legible). 11. Page 8, lines 4-5. Should a reference or references for NPF be added? 12. Page 8, lines 20-21. I can see how this sentence is needed, but it seems to be just tacked onto the end of the section. 13. Page 10, lines 9-10. How would small scale mixing lead to the behavior that is shown in the figure? 14. Page 11, line 10. Suggest using the same units as shown on the figure. 15. Page 12, line 2. The text mentions deep convection, but can that be safely said from the data that has been presented so far? Wouldn't the HSRL have issues seeing the cloud-top height?

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