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 Aerosols Effects 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.

**Response to comments from Anonymous Referee #3**

We thank the referee for the constructive comments to help us to improve the manuscript. Below please find our answers to the comments.

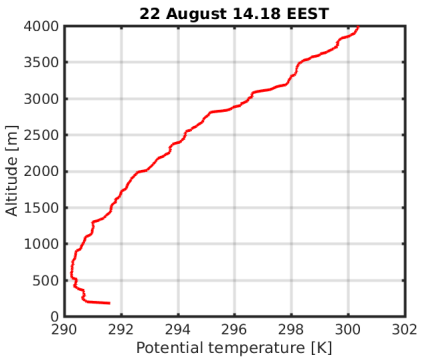
1. The work makes a point of using relative humidity (RH) to help define layers and to better understand some of the differences in the observed size distributions. The authors point out that the ambient RH was measured near the aerosol inlet, but there is no discussion of how the RH might change as 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).

Added to the text: a relative humidity sensor (Rotronic HygroClip-S, accuracy 0.8 % at 23 °C)

For the SMPS measurements the change in the RH in the system was not an issue, as the air was dried before aerosol size distribution was measured (already mentioned in the text on p. 5). As for OPS measurements, RH higher than 40% could accelerate hygroscopic growth. Particles spend some tens of seconds in the sampling line. Humidity inside the cabin was lower than outside (higher temperature inside) so hygroscopic growth should have not taken place. Most of our case studies have lower than 50 % ambient humidity.

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.

We have now examined the potential temperature, and we still think that the thermodynamic structure during this day was not easy to interpret. As seen from the profile, there are no clear sublayer boundaries:



Minor comments:
1. Page 1, line 14: The acronym DOE (rather than DoE) is generally used for the Department of Energy
-Changed as suggested to DOE

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.
-Changed as suggested from ‘low/high’ to ‘small/large’ for the term variability (page 1, 8, 12). We think ‘low/high’ is appropriate to use for aerosol concentration.

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.
-Added suggested reference

4. Page 3, lines 2-6. Muller etal. (2014, AMT) also showed comparisons of aerosol microphysical properties derived from HSRL with in situ data.
-Added suggested reference

5. Page 2, line 20. Suggest using “aerosol” or “particles” rather than “aerosol particles”.
-We think “aerosol particles” is the best word to use when discussing aerosol particles in this case.

6. Page 5, line 16. Does the reference to BLs really mean convective BLs or all Bls?
- The reference is to all BLs, and on a clear day the BL is usually convective.

7. Page 5, lin3 24. The manuscript cites the work of Laakso et al. (2004). Are their results for the same geographic area?
-Yes, they are. Added to the text: a growth factor (GF) calculated for a boreal forest environment using measurements from SMEAR II station.

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?
-Figure 1 was intended to show the general overview of the situation during three days, while humidity profiles for case studies could be seen on Figure 2 with scales.

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.
-Added to the text: The interface zone was a shallow zone situated at the boundary between two more substantial layers and was characterized by large backscatter values and depolarization values different from the surroundings. No corresponding thin layer was detected in the humidity profiles, whether from the radiosonde or aircraft.

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).
-Specific layer boundaries were added to the RS profiles panels of Figures 2 and 7.

11. Page 8, lines 4-5. Should a reference or references for NPF be added?
-Added references:

Dal Maso, M., Kulmala, M., Riipinen, I., Wagner, R., Hussein, T., Aalto, P. P., and Lehtinen, K. E.: Formation and growth of fresh atmospheric aerosols: eight years of aerosol size distribution data from SMEAR II, Hyytiala, Finland. Boreal Environment Research, 10(5), 323, 2005.

Kulmala, M., Kontkanen, J., Junninen, H., Lehtipalo, K., Manninen, H.E., Nieminen, T., Petäjä, T., Sipilä, M., Schobesberger, S., Rantala, P. and Franchin, A. et al.: Direct observations of atmospheric aerosol nucleation. Science, 339(6122), pp.943-946, 2013.

Tunved, P., Hansson, H. C., Kerminen, V. M., Ström, J., Dal Maso, M., Lihavainen, H., Y. Viisanen, Y., Aalto, P.P., Komppula, M. and Kulmala, M.: High natural aerosol loading over boreal forests. Science, 312(5771), 261-263, 2006.

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.
-This sentence is removed, because only aerosol concentrations higher than 0.1 $cm^{-3}$ are shown now on the figure. (This sentence is not needed anymore as it explained large variability in the concentrations below that).

13. Page 10, lines 9-10. How would small scale mixing lead to the behavior that is shown in the figure?
-We suspect that this is a response of aerosol growing rapidly as it moves from very dry air to much moister conditions, supported by the lowering of the depolarization ratio in the same region. This requires some mixing over small vertical length scales between two otherwise stable layers, otherwise such a signal would be rapidly ‘smeared out’. Additional evidence is required to confirm this hypothesis.
The sentence is rephrased: This thin layer could be either a result of limited small-scale mixing between two layers, that were probably stable, or the result of large-scale transport of smoke or dust; however, we suspect that this is a response of aerosol growing rapidly as it moves from very dry air to much moister conditions, especially since the low HSRL circular depolarisation values suggest more spherical particles in this thin layer.

14. Page 11, line 10. Suggest using the same units as shown on the figure.
-We have changed the units as suggested.

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?
-The HSRL does not see the cloud top heights (there is complete attenuation of the signal where there are clouds), but the cloud-top height was seen in the cloud radar that operated during the BAECC campaign.
We do not intend to add a figure showing the radar reflectivity so we have removed ‘deep’.