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Atmospheric Measurement Techniques Discussions

Interactive comment on "Characteristics of aerosol vertical profiles in Tsukuba, Japan, and their impacts on the evolution of the atmospheric boundary layer" *by* Rei Kudo et al.

Anonymous Referee #1

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General Comments: This paper deals with rage resolved observations of aerosol properties, over Tsukuba, Japan, and a comprehensive study on how they affect the atmospheric boundary layer evolution. For the latter, the authors used the remote sensing observations as initial inputs in a 1-D atmospheric model. The paper rightly acknowledges previously related studies. The manuscript is well written, but in order to be improved, I would suggest to the authors to take into consideration the following comments. Minor Comments: 1. The Abstract section is well written. However, I would like to draw the attention to the authors to consider stating with numbers the main outcomes of their study. 2. Page 1, line 21: "compared to". 3. Page 1, lines 24-26: This sentence is too long. The authors are kindly requested to rephrase it, and make their statement clearer. 4. Page 3, line 8: Apart from the coordinates, please provide also the elevation of Tsukuba station. It would be useful also for the reader, if you could provide the link of the used station which operates under SKYNET network. With a quick search I was not able to find this station here http://www.skynet-isdc.org/quicklooks.php. 5. Page 3, line 12: The authors are mentioning that among other aerosol properties the lidar data obtained by the AD-Net, contains also the depolarization ratio for particle and molecular scattering. Maybe the authors are referring to the physical quantity of volume depolarization ratio, which includes the contribution of molecular and particle depolarization. Please clarify this. 6. It is not so clear in the text, the contribution of the auxiliary data mentioned by the authors (page 3 lines 13-18). I suppose that this dataset was used in the radiative transfer module of the 1-D atmospheric model, but it would be nice if this is mentioned here. 7. Moreover, I would suggest to the authors to consider producing a flowchart diagram as the very first figure of their manuscript, in order to clearly demonstrate there the inputs and outputs of their approach/methodology. 8. Page 4 line 21: Define here the % of successful retrievals of ABL estimation from your dataset. 9. Page 4 line 23: What is the time window used for smoothing the time series of ABL height? 10. Page 7 line 4: Consider providing the appropriate reference for Ångström exponent namely: "Ångström, A., On the atmospheric transmission of Sun radiation and on dust in the air, Geogr. Ann., 11, 156– 166, 1929". Moreover, it is not clear, if this quantity refers to the entire atmospheric column, or is the range resolved backscatter or extinction related exponent calculated from the 532-1064 nm pair of wavelengths from the lidar measurements. Please try to clarify this in the manuscript. 11. Figure 3: Title "Ångström Exponent". Please go through the entire manuscript (tables, figures and text) to correct "Ångströme" to "Ångström". 12. Page 7 lines 14-15: The authors are kindly requested to provide some indicative studies related to the single scattering albedo of dust and black carbon. 13. Page 7 lines 29-31: The authors are commenting in lidar ratio values. But it is confusing since in 2.1.1 they mention that they operated a two wavelength Mie-scattering lidar from AD-Net. Since this system is not a Raman or

HSRL to obtain independently the lidar ratio, please comment from where these values came from. 14. Figure 6 (c): The authors are demonstrating two-days of air mass back-trajectories, in order to identify the aerosol source. Even though they provide some stars to indicate the starting altitude, the final height of the air mass arriving over the station as well as the height through the entire travel path is not shown. The authors are kindly requested to update this figure or at least mention these heights in the text. 15. I noticed that during the entire paragraph 3.1.2 the authors are describing qualitatively (e.g. “values were small; large; was large; was small” etc.) the aerosol optical and physical properties. They are kindly requested to provide also some numbers in the text. 16. Page 9 lines 5-6: Please specify also the warming range of the FA due to the direct aerosol heating. 17. Page 9 line 21: “…the decrease in the ABL height was smaller…”. Please specify how much. 18. Figures 7 & 8: Please consider to make this graphs clearer. I think that it will help if: (1) units exist next to the parameters EXP0 and EXP1- EXP0 (2) provide altitude (y-axis in each graph) in km for being consistent with the previous figures. Finally I think that in the primary x-axis of (d) and (e) the x-title should be Potential Temperature (K) and Specific Humidity (g kg-1) respectively, and not EXP0. 19. Please update the link related to SKYNET, as provided in Page 11 line 3. Major Comments: 1. A general comment to the authors. Select some indicative numbers from the tables, representing the major findings of this study, and provide them into the manuscript (sections: Abstract, Results, Conclusions). 2. The authors apart from the mean statistical values are also using values observed from 5 case studies. The three of them where characterized as dust, one as smoke and one as mixture of dust and smoke. From their findings, the case studies differ only at the Ångström exponent and the aerosol load. I wonder if during their study, they can also draw any conclusion related to the influence of each individual aerosol type on the ABL evolution. 3. Page 4 lines 26-27: How much uncertainty this assumption may introduce? This maybe implies an overestimation of the aerosol load inside the ABL and an underestimation in the free troposphere. In any case the authors are kindly requested to comment on this. 4. Page 5 lines 21-22: Please provide an

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Discussion paper

estimation of the error that is introduced especially in the all wavelength net radiation that is discussed later, by ignoring the influence of aerosols on the infrared region (λ>1064nm), since dust particles are mostly in the coarse mode with diameters > 2 um. Moreover, the linear interpolation of the aerosol optical properties between 532 and 1064 nm can also influence the final retrievals. Please elaborate more on this. 5. Page 7 lines 2-3: The authors are giving an explanation of their findings regarding the seasonal variation of the mean ABL values, presented in Figure 2. However, I wonder if they compared these mean seasonal ABL values with corresponding measurements from radiosondes. In the same figure, it seems that there are a few cases that aerosol layers are detected up to 9 km height. Are these smoke particles advected from Russia? 6. Page 7 lines 23-25: Since particles located in FA and ABL may have totally different optical and chemical properties how comes and the refractive index is the same for the aforementioned atmospheric regions? Is this a bias coming from the retrieval method of SKYLIDAR? In any case the authors are kindly requested to comment more on this. 7. Page 8 line 15: Even though the aerosol type detected in 08 May2013, was characterized as smoke, no clear differences compared to the dust and dust smoke days can be found, regarding the values of single-scattering albedo and asymmetry factor (Table 2). Is this coming from the retrieval method of the SKYLIDAR?

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-462/amt-2017-462-RC1-supplement.pdf

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