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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Profiles of second- to third-order moments of turbulent temperature fluctuations in the convective boundary layer: first measurements with Rotational Raman Lidar" *by* A. Behrendt et al.

Anonymous Referee #1

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This manuscript shows a case-study wherein Raman lidar data is used to derive second through fourth order moment statistics of temperature throughout the convective boundary layer and in the interfacial layer. Overall, the authors describe well how the statistics are computed, including an analysis of the significant errors from the noisy lidar data. The results are discussed well and the authors consider the large errors in their interpretation of the data. The method outlined in the paper, using Raman lidar measurements, will allow investigation of various boundary layer questions, which the authors lay out within the conclusions. The manuscript is well-written overall and data

analysis is generally good. However, there are some questions over the application of the power law, which is the crux for many of the derived higher order statistics. In order to address this question, and several other minor points listed below, additional analysis may need to be performed leading to major revisions.

Specific Comments

Title: The title should be "Profiles of second- to fourth-order moments of . . ."

p. 29022 Line 1: Martin et al. (2014) used UAS to identify and examine process in the entrainment zone, which should be referenced here and invalidates part of this sentence. However, UAS of course cannot continuously examine it due to a short endurance (among other issues).

p. 29022 Line 10: There should be more information about the Kadygrov et al. study here, as this sentence seems incomplete. How did the thermal turbulence characteristics compare with the expected power-law within the lowest 200 m?

p. 29026 Line 28: What is meant by "is not necessarily the case for the other cases"? What are the other cases?

p. 29026 Line 29: Seems like several words (or variable) were left out. Should it be '. . . the height of maximum dT/dz agrees. . . while the height of the maximum gradients from the radiosonde is about 60 m lower'?

p.29027 Line 13: Was the detrending performed for time series of temperature at each separate height? Should clarify.

p. 29028, line 12: What is the power-law fit? It should be provided within the present manuscript for clarity. Does this refer to Eq. 32 in Lenschow et al. (2000)?

p. 29028, line 20: Why was the interval of -200 – 200 s chosen for extrapolation of the power law fit? If Eq. 32 from Lenschow et al. (2000) was used, the power law fit was derived using the inertial subrange hypothesis. With independent temperature samples of 10-s, it is unlikely that the resolved temperature fluctuations are due to turbulence within the inertial subrange. For much of the time period of -200 – 200 s that is used for the fitting, the ACF shown in Fig. 6 is negative and near zero, which would indicate that the fluctuations of temperature are from turbulent motions outside of the inertial subrange. By fitting the curve to data outside of the inertial subrange, the estimate of the noise would tend to be too high, making the temperature variance underestimated.

p. 29032, Section 3.5: It would be beneficial to compare how the magnitude of the temperature variance within the CBL compares to those measured in other studies in similar environments. While it would be difficult to compare actual values, it would be beneficial to show if those measured within this study were of the same magnitude as those measured using in-situ measurements.

Conclusions: Overall, the conclusions were well-supported by the data presented and were well written. It may be better to highlight some of the key findings with it by using a bulleted list, though.

p. 29037, line 12: By collecting data closer to the ground, it may also be possible to compare measurements to high-quality in-situ sensors, at least at one location. Although that is outside of the scope of this paper, I would recommended doing comparisons with a fast-response thermistor mounted on a tower, if possible. Statistics from the in-situ sensor could be compared with those from the RRL using this technique. This would better allow for quantification of errors, and for assessing how this method could possibly be improved for accurately measuring temperature variance and other higher order statistics.

Figure 3: The profile of temperature should be removed, as it provides no additional information. Within the text, any discussion that uses the temperature profile could be made with the potential temperature profile instead.

Figure 4: There should be errorbars for the statistics from the 1100-1200 time period as well.

Figure 5: Again, showing only either the potential temperature or temperature cross-sections is sufficient, do not need to show both. I would learn towards showing potential temperature, as the top of the CBL is shown better. In the temperature fluctuation plot, the colorbar would be better if T'=0 were grey, not green, as the more subtle fluctuations would be more visible.

Figure 6: Including the fit of the power-law to the data on the graph (at least for one height) would be useful, so that the goodness of the fit is shown.

Technical Corrections

P. 29021 Line 27: In citation, year should be 2011. Should check other citations to make sure they are all correct, as well. One other incorrect citation was noticed that is listed below.

P. 29022 Line 1: Should be in-situ sensors

P. 29022 Line 16: Should be Lenschow et al. 2000, not 2010.

P. 29023 Line 20: Should be ‘out of’, not ‘our of’

Additional reference:

Martin S., F. Beyrich, and J. Bange. Observing Entrainment Processes Using a Small Unmanned Aerial Vehicle: A Feasibility Study, Bound.-Lay. Meteorol., 150, 449-467, 2014.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29019, 2014.