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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 #3

Received and published: 24 December 2014

The paper demonstrates the capability of the UHOH RR lidar to study temperature fluctuations in the CBL. Statistical moments of higher order have been derived including its uncertainties and partially discussed. The paper is well written, results are novel and well presented. I recommend publication in ACP after minor revisions as specified below.

General comments

A big part of the paper is written in the first person plural. This is not good scientific

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language and I would like to motivate the authors to use other formulations.

The approach to characterize the noise of the analog signal with Poisson statistics is not justified. The authors need to provide a justification and a validation of this approach.

The language lacks sometimes precision. Often no distinction is made between an error source (instrument noise or shot noise) and the resulting uncertainty in temperature. Further, the term “Poisson statistics” is heavily overstressed in this work. It is often used as synonym to “instrument noise” even though it is not even clear, whether the instrument noise follows Poisson statistics. This has to be corrected.

Specific comments

P29020, l2: This implies that nighttime measurements are not possible or usually not done. Please comment.

P29020, l14: What is “noise variance”. “Noise” on l4 refers to signal noise. Here it seems to be the fraction of the temperature variance which is due to signal noise. Later in this phrase the authors say “statistical temperature measurement uncertainty based on Poisson statistics”. Only for the lidar expert it is obvious, that “Poisson statistics” must refer to the noise of the measurement and for non lidar experts this phrase is quasi incomprehensible. Please be very precise here.

P29020, l16: How can an agreement confirm a difference? use “comparison” instead of “agreement”.

P29020, l17: Here and on P29029, “extrapolate” is not the good word for the process. Wouldn’t “scale” describe better the data manipulation? Second, here the authors use “extrapolated analog signal”, while in the text (P29029) they use “extrapolated count-rates”. Reformulate to achieve consistency and the necessary precision in the language.

P29021, l24: I doubt that “temperature turbulence” is a correct term?

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P29022, l7: While the range resolution is certainly a limitation for this purpose, I disagree that time resolution can be included here. Kadygrov et al. 2003 (not 2011) used a scanning radiometer, which reduces the time resolution. Multichannel MWR's can do profiling at a much higher time resolution. Further, what do the authors mean with "time dependent errors"? All remote sensing instruments have uncertainties that depend on the atmospheric conditions and hence on time.

P29022, l13: The achievements in a better understanding of boundary layer processes based on the radar technology (active remote sensing) needs to be briefly reviewed here (e.g. Jacoby-Koaly et al. 2002, Boundary Layer Meteorology, 103, 361-389, and references therein).

P29023, l19: ++Raman++ backscatter spectrum.

P29025, l4: mention here, that this setting optimizes SNR for high background conditions.

P29026, l4: Why do the authors chose a 20 min interval for calibration when the sonde takes only a few minutes to cover the altitude range under consideration? Why does this reduce the sampling error?

P29026, l20: "the lidar temperature data" is too general here, emphasis has to be put on the 20 min average.

P29026, l29: . . . height of maximum ++gradient++ agrees . . .

P29027, l14: Above the CBL. . . reformulate this phrase and say that the observed temperature fluctuations above the CBL are governed by measurement noise.

P29028, l5: The separation. . . this phrase is not well written.

P29028, l10: There is not extrapolation "needed" since we know the ACF at lag zero. However, we want to estimate the atmospheric variability from the ACF by extrapolating the portion of non-zero lag to values of zero lag. The authors have to be more precise

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here, if not, the phrase starting on l12 is not comprehensible.

P29028, l12: What does “at the extrapolated value” refer to here? I think one can just leave it out.

P29028, l20: This looks actually more like an interpolation. Do the authors confirm that mathematically an extrapolation has been performed? and if so, From which side?Replace “the increase of the zero lag” by “the increase of the value at zero lag”.

P29029, l2: Why is this now called “total noise error”? Specify that the “total noise error” refers to the noise variance derived with the ACF fit.

P29029, l4: I guess the authors are referring to the error in temperature DUE TO photon shot noise. The error of photon shot noise does not make sense.

P29029, l6: Bad use of the term “Poisson statistics”.

P29029, l9: Give examples of other statistical error sources.

P29029, l18ff: To what extent is this fitting process similar to the “glueing”, i.e. the determination of an offset and scaling factor a and b? The text gives the impression that the photon count signal is fitted to the analog signal, while in the standard “glueing” it is the other way around. It is not clear why the word “extrapolation” has been used. Finally, the authors must justify their approach here. Why can the noise of the analog signal be characterized by Poisson statistics, what is the physical explanation? To convince the reader further of this approach, the authors need to show (or at least verify and mention it), that this approach is in reasonable agreement with the straight forward determination of the noise, namely the standard deviation of a short de-trended portion of the analog signal. Couldn’t the difference between the statistical uncertainty in temperature derived from the ACF fit and the statistical temperature error due to shot noise be related to the fact, that the analog signal cannot be described with Poisson statistics?

P29034, l22: It is not clear from the text here which definition is used. Instead of

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making the reader looking it up in Lenschov et al. 2000, it should be specified here.

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

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