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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.

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The paper is well written the results are new and unique. Nevertheless there are some points which need clarification and may lead to a change of results.

Main points:

* the authors use the method of Lenschow et al (2000) to derive statistical parameters from the (inherently) noisy data of a Raman Lidar. Within this method the autocorrelation functions of different order have to be extrapolated to lag zero to remove components generated by noise in the data. At least for the variance a power law function

deduced from Kolmogorov scaling is used. Kolmogorov scaling is only valid in the inertial subrange but it is not ensured that this is the case. I therefore recommend a re-calculation which will probably give different results.

* the derived Skewness profile shows within the boundary layer values around zero and accordingly deviates substantially from what is usually found in LES simulations, aircraft or other experimental data (e.g. Mironov et al 1999, fig. 1). As in the middle of a common convective boundary layer rising plumes of warm air are narrow and surrounded by large areas of air close to the average temperature the skewness can be expected to be positive. This deviation should be discussed.

* A comparison with airborne measurements, LES results or parameterizations from the literature is missing.

Details:

3.1 Data set

page 29025:
Some information about general meteorology (wind, minimum and maximum temperature, surface fluxes, etc.) would be nice.

page 29025, line 14:
"βpar was measured with the rotational Raman lidar technique by use of a temperature-independent reference signal."
please explain or give a reference.

p. 29026, l.1.
calibration
There are missing some details of the calibration: is one factor for the whole height range derived, or several for different height intervals. How large is the uncertainty in the calibration factor (called $\partial T/\partial Q$ in equation 5 below).

p. 29026, l. 23.

... the potential temperature (...) [profile] is nearly constant ... fig. 3 ...

Just a remark: around 800m is a region with stable stratification. For the radiosonde one could assume that it entered a plume with warmer air, but i am wondering why it is also visible in the RRL data even in the one hour average (see also the gradient profile in fig 4). Around that height is also a minimum in variance (fig 9) and skewness (fig 10) - any comments on that ?

3.2 Turbulent temperature fluctuations

p. 29027, l.9., eq. 1

There is missing a time dependence in the equation. And it is not clear whether the trend is equal for all levels. To additionally include the trend removal one may write:

$$T(z,t) = \overline{T}(z) + a(z) * (t - t_m) + T'(z,t)$$

where $\bar{\cdot}$ denotes a linear temporal average operator which fulfills the Reynolds averaging rules (e.g. Wyngaard 2010), a is the trend, etc. ...

p. 29027, l.12.

"... a linear fit to the temperature time series was sufficient due to the quasi-stationary state of the CBL ..."

In a quasi stationary turbulent medium parameters like mean and variance of a variable are constant although the variable itself is fluctuating. If it is necessary to remove a temporal trend, no matter whether linear, quadratic, sinusoidal or whatever, it is strictly speaking not stationary any more. Please reformulate.

Were there any objective methods applied to ensure that the dataset is quasi stationary ?

p. 29028, l. 12:

"By calculation of the autocovariance function (ACF) of a variable and extrapolating the

function to zero lag with a power-law fit ..."

This probably refers to equation 32 in Lenschow et al (2000) which can be written as ACF $A(\tau) = \overline{T'^2} - C \cdot \tau^{2/3}$ with lag τ. At least it is mentioned further down (p.29032) that the exponent in the power law is $2/3$. This power law with an exponent of 2/3 for $A(\tau)$ stems from Kolmogorovs structure function and is only valid in the intertial subrange i.e. at scales clearly smaller than those of the external forcings and larger than those of viscosity. This becomes obvious as for large lags $A(\tau)$ becomes increasingly negative although one would expect that the autocovariance (i.e. the autocorrelation multiplied by the variance) asymptotically tends to zero. As an indicator for the scale where the Kolmogorov ACF becomes invalid could serve the zero intercept of the measured ACF.

The ACF is fitted to lags up to 200s (p.29028, l. 19) which can be related to a length scale of 2km by assuming a horizotal wind of 10 m/s. This is clearly larger than the height of the convective BL (1.23km) which can be regarded as the length scale of the external forcing. Thus $A(\tau)$ is applied to regions outside the intertial subrange. I recommend to reduce the range of lags to which the ACF is fitted. Otherwise I would recommend to fit a function which aproximates asymptotically zero for large lags and which is not restricted to the interital subrange. I expect that this will change especially the integral timescale but also the other estimated parameters.

p. 29028, l. 14.:

"Alternatively, one may calculate the power spectrum of the fluctuations and use Kolmogorows $-5/3$ law within the inertial subrange in order to determine the noise level."

Description is a somewhat vague. The method is e.g. described and investigated in O'Connor et al 2010.

Allthough the authors do not use the spectral method to correct for the noise in the data it would be interesting to see powerspectra of the data as they show an independent measure of noise.

3.3 Noise errors

p. 29029, l. 9:
"The comparison confirms that the photon shot noise gives the main contribution and that other statistical error sources are comparatively small."
If i estimate correct from fig. 7 the Poisson statistics account for about 75% of the error. Where do the remaining 25% come from ?

p. 29029, l. 17:
"... analog signals and not photon-counting signals have been used ..."
I understand what analog signals are but i do not understand what the additional not photon-counting signals are, please specify.

p. 29030, l. 11, eq. 6:
I would find it more didactically to start with the basic retrieval equation to come to the error estimate.

p. 29030, l. 5, eq. 5:
This means that the calibration constant $\partial T/\partial Q$ is assumed to have a negligible uncertainty. Is this reasonable ?

p. 29031, l.11:
If i understand right the uncertainty estimate given in eq. 10 does not represent the dominating part of the error propagation. Would it not be more meaningful to write the equation in a way including this important part ? This would also clarify why error profiles for different temporal and spatial resolutions can be derived by a simple scaling from the 10s/109m profile.

3.4 Integral scale

p. 29032, l.7:
The variance at z/z_i is close to zero because the noise estimate by the Lenschow et al. (2000) method attributes all the observed variance as noise. I would expect that the temperature variance inside a convective BL would never go to zero. Thus probably the noise removal method does not work properly. Please reformulate the sentence.

p. 29032, l. 10.:
"The integral scale shows values between (40±22) s and (122±12) s in the CBL."
These values are smaller than the 200s maximum lag used to fit the theoretical ACF. The integral scale is sometimes interpreted as the size of the energy containig eddies, accordingly the ACF has been fitted to regions beyond the inertial subrange.

Interestingly is that below 800m, the height of the small stable layer, in average larger time scales appear - any idea ?

p. 29032, l. 10.:
"The integral time scale (which can be related to a length scale ..."
It would be great if this could be done. These derived Length scales could be compared to length scales derived e.g. from LES (e.g. ed Roode et al. 2004, fig. 5 and 6, or Moeng and Wyngaard 1989)

3.5 Temperature variance

p. 29032, l.23:
sampling and noise error are probably determined by the method of Lenschow et al. (2000). This should be mentioned here or in the description of the method above. The different meaning of these two error estimates should be explained.

p. 29033, l.5:

The variance profile could be compared with profiles found in the cited literature. Especially with parameterizations as e.g. mentioned in Moeng and Wyngaard (1989). The latter also assumes a relation to the surface to entrainment flux ratio.

3.6 Third-order moment and skewness

p. 29033, l. 11, eq. 11:
Are skewness and TOM really determined directly from the measured temperature time series? Or is here also the method from Lenschow et al. 2000 applied? The latter corrects for the skewness of the (Poisson) noise which i would expect to be nonzero. Please clarify.

p. 29033, l. 17:
"Up to about 0.9 z_i , TOM was not different to zero ..."
This is somewhat astonishing if one takes into account the structure of the turbulence in the CBL: rising warm plumes are preferably narrow or organized along the borders of hexagonal cells (see e.g. Taeumner et al 2014). They are surrounded by large areas of air close to the average temperature. This results in positive Skewness as also can be seen in e.g. Mironov et al. (1999), fig. 1 which shows data from different sources. Why is this different here ?

3.7 Forth-order moment and kurtosis

p. 29034, l. 17:
"Forth-order moment (FOM) is a measure of the steepness of the distribution."
Although this seems to be a common interpretation it is somewhat misleading: the larger the Kurtosis the steeper is the slope of the pdf at the sides of its peak. But also the more flat or less steep are its tails.

p. 29034, l. 20, eq. 12:
As for the TOM: is the FOM and K calculated with this equation or with the method of Lenschow et al. (2000) which accounts for the FOM of the noise ?

p. 29034, l. 22:
K-3 is sometimes called 'excess Kurtosis'.

4 conclusion

p. 29037, l. 7:
Gryanik et al. (2005) postulate in equation 3 a relation between the fourth order moment, skewness and variance of temperature. Alhough the errors in FOM and K are large it might be interesting to investigate this here. With the same caution one may add some points to their figure 3.

Figures:

fig. 1.:
Caption mentions some elements which are not in the schematic: e.g. OF for optical fiber, L5 and L6...

fig. 2.:
Backscatter shows some structures in the BL: (a) higher backscatter before 11:30 above 800m and (b) increasingly lower backscatter after 11:30 in the lower half). These structures seem to have no relation to the temperature field or show at least much larger scales than the features which are visible in the T' plot (fig. 5). Is there any interpretation ?

fig. 3.:
Temperature profiles: ranges of the abscissa could be smaller to make differences between the the profiles better visible.

fig. 4.:
One may mark the adiabatic temperature gradient to identify stable and instable regions.

fig.5:

I think the temperature plot does not give that much information as it is dominated by the temperature decrease with height. One less subplot would give more room for the others making them easier readable.

The ranges of all the color scales seem to be larger than the range of displayed temperatures: e.g. nearly all potential temperature values lie in the range 290-305K whereas the color scale goes from 280-310K. Adpating the color scale would make turbulent structures better visible.

Both ends of the Temperature fluctuations T' color scale are black making it difficult to decide whether a black pixel denotes for $+2$K or -2K. please change.

Fluctuations T' in the free troposphere are larger than in the BL, they are probably noise. It might be an idea to remove data (i.e. heights) where noise in the data comprises for more than say 50% of the measured variance.

I do not fully understand why there are gaps in fig 5c (i.e. time height section of T'). These gaps seem to be about 10-20sec long and appear about every 3 1/2 minutes or every 21 mesurement. If i interpret this in the way that after 21 timesteps more than 21*10sec have elapsed this would mean that a time step is by 1/21 = 5% longer than 10sec i.e. 10.5sec. This would affect all derived parameters. Please clarify.

Last sentence of caption is unclear.

fig 6.:
It is not necessary to show the autocorrelation function for negative lags as it must be from its defintion symmetrically (even if you calculate it for negative lags you are

multiplying and adding the same numbers). Reducing the abscissa to only positive lags might increase clarity of the plots. It would be great to see at least one fitted ACF-function and an ACF=0 line would be helpful.

The slow fluctuations of ACF around zero for larger lags might be due to some large scale variability in the data. Although such patterns may also occur in ACF from pseudo random generated time series with no longterm variations, it might be worth to think about high-pass filtering of the data.

Caption does not explain the second plot.

fig. 8:
"Error bars show the remaining root-mean-square variation of the noise-corrected data" I do not understand - the integral scale of a time series is one value - it does not vary. Please clarify. The same formulation was used in the following figure captions, please adapt accordingly.

fig. 9:
It is a bit surprising to see nearly zero variance in the CBL and even negative (or exact zero) values at 1.4km, especially as the time height section of T' in fig.5 shows variability at these heights. It would be helpful to see the uncorrected variances to get an idea how large the noise in the data is (see e.g. Wulfmeyer et al. 2010).

fig. 10 and 11:
If TOM, S, FOM and K have been calculated including a noise correction it would be intersting to see the uncorrected profiles (see e.g. to Wulfmeyer et al 2010)

The dashed line marking zi is missing in the Kurtosis plot.

Technical:

p. 29022 l.8: year in citation Kadygrov must be 2004 - there seem to be several years wrong (Lenschow 2010 inestead of 2000 ...), please check.

p. 29024 l. 12: scanner mirror is anotated BSU (beam steering unit) in fig 1.

p. 29024, l. 17: iris = field stop in caption for fig 1 ?

p. 29024, l.30: i guess specification of filters etc. is given in Behrendt et al ...

p. 29028, l. 4: remove 'or'

p. 29028, l. 5: 'mean temporal average' is a double average ?

References:

Wyngaard 2010, Turbulence in the Atmosphere, Cambridge University Press

O'Connor, E. J., Illingworth, Anthony, J., Brooks, Ian, M., Westbrook, Christopher, D., Hogan, Robin, J., Davies, F., and Brooks, Barbara, J., 2010: A Method for Estimating the Turbulent Kinetic Energy Dissipation Rate from a Vertically Pointing Doppler Lidar, and Independent Evaluation from Balloon-Borne In Situ Measurements, J. Atmos. Ocean. Tech., 27, 1652–1664, doi:10.1175/2010JTECHA1455.1 .

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