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Interactive comment

Interactive comment on "Measurements of wind turbulence parameters by a conically scanning coherent Doppler lidar in the atmospheric boundary layer" *by* Igor N. Smalikho and Viktor A. Banakh

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Referee #1 In this manuscript, the authors describe how various turbulent parameters can be measured with a continuously conically scanning Doppler lidar. The techniques for measurement of the parameters are described in detail, and sample results of the measurements are shown. Doppler lidar measurements of the dissipation rate are compared with a sonic anemometer at 43 m, and are shown to generally agree well, except with some low biases under stable conditions when the lidar is unable to resolve the any portion of the inertial subrange. The turbulence kinetic energy from the Doppler

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Discussion paper

lidar is shown to generally agree with measurements from a sonic anemometer at a lower height. In all, the scientific quality of the manuscript appears to be solidly based in theory and good. The work builds on previous work, with new refinements made to the strategy. However, there are a few areas of the manuscript that could be clarified, as sections of the text are difficult to follow. As such, I recommend this manuscript be suitable for publication in AMT after minor revisions, in which the following comments, which are mostly of clarification, are addressed. Specific Comments: a) P. 1, line 19; p. 2, line 5 (and elsewhere): Change ‘raw lidar data’ to ‘radial velocities’. By ‘raw data’, I interpret that to be the measured Doppler spectrum, which are not used directly in the referenced techniques to measure turbulence. => The phrase “raw data measured” has been replaced by “measurements”.

b) p. 2, line 9: By ‘averaging over the sensing volume’, clarify that you mean the spatial-temporal averaging of the pulse length over one beam accumulation and not the averaging over the entire conical area. => Page 2, line 9 : “(see Eq.(6) in paper of Smalikho and Banakh, 2013)” has been added.

c) p. 2 line 12: What are dr and σr? => Page 2, line 13: “and .. is the variance” and “radial” have been added. “dr” is an infinitesimal increment of the integration variable “r” (separation between two points).

d) p. 2 line 20: Quantify ‘high spatial resolution’. => Page 2, lines 21-22: “(longitudinal size of the sensing volume can be around 30 m)” has been added.

e) p. 2 line 23: What disadvantages of the earlier methods, precisely? The averaging over the sensing volume? => We do not know publications in which authors would take into account the effect of averaging of the radial velocity over the sensing volume when estimating the kinetic energy of turbulence.

f) p. 2 line 24: Change ‘spatiotemporal’ to ‘time and height’. The term ‘spatiotemporal’ is too general, and generally means that information on the horizontal variability is measured/known. => Fixed.

g) p. 6 lines 22-24: This section is difficult to follow. Providing more text to describe the different terms and how they are related would be helpful. => Text on page 6 (lines 18-24) of initial version of the manuscript has been replaced by the text on page 6 (lines 19-26) and page 7 (lines 1-3) of the revised manuscript. Page 7, line 13: "(Banakh and Smalikho, 2013)" has been added.

h) p. 7-8: For this section in particular, it would be helpful to add a figure providing a few examples of the 2-dimensional spectrum and showing how the different parameters are calculated from it (particularly interested in σe, σt), including adding a paragraph discussing the figures. This would be similar to showing how different parameters are calculated in Fig. 5. => Page 9, lines 3-13: The paragraph "With increasing range . . . without taking into account the averaging of the radial velocity over the sensing volume." has been added. Page 9, lines 14-26: The paragraph "Figure 2 shows vertical profiles . . . the underestimation does not exceed 5%." has been added. Page 24: Figure 2 has been added. The sentence "The analysis of results for the kinetic energy of turbulence . . . is understated by 10 - 20%, especially, in the layer up to 200 m." (page 13, lines 9-12 in the initial variant of the manuscript) has been removed. Page 14, lines 22-30 and page 15, lines 1-4: The paragraph "Under the condition . . . then underestimation of the integral scale will be from 15% to 40%." has been added. Page 32: Figure 10 has been added.

i) p. 10 line 10: How much of the data was unusable exactly? The percentage of unusable data would be helpful. => Page 11, line 16: "(around 15%)" has been added.

j) p. 10 line 13: What was the averaging time that the results shown in Fig. 3 were computed over? Based on p. 9 lines 19/24, it seems that 4 PPIs were used (over 5 minutes) while the sonic anemometer used 20 min of data. How were these differences in averaging times rectified? => If the same measurement time is used for the lidar and the sonic anemometer, the distance traveled by the sensing volume and the distance to which the air masses are carried by the mean wind during this time will vary greatly, since the velocity of the mean wind is substantially less than the linear velocity of movement of the sensing volume at the base of the scanning cone. We believe that in order to compare the results of estimating the dissipation rate, it is more appropriate to use the lidar data and the acoustic anemometer data, which correspond to the same distances.

k) p. 12 line 5: Is it possible to discern that the increase in kinetic energy computed over more scans (over longer time periods) is truly a better measure, and not simply due to non-stationarity of the mean wind (as discussed for the stable case at line 15) increasing the variances across the entire conical scan? Based on Fig. 6, the mean wind changes (wind speed slowly decreases, direction shifts) over the 6 hour time window mentioned, thus this may be causing the increase in measured TKE. => The variance of the average (30-minute averaging) of the wind velocity, calculated from the data in Figure 6 (a) for a height of 200 m and a time interval from 12:00 to 18:00, is approximately 10 times less than the TKE given in Table 1 (for 30 scans). Therefore, we can assume that the contribution of the nonstationarity of the mean wind to the kinetic energy estimate is negligible, in comparison with the turbulent fluctuations of the wind field. However, for another case considered in the manuscript (measurement at an altitude of 200 m from 01:00 to 07:00), the variance of the average (30-minute averaging) of the wind velocity is approximately twice the estimate of the kinetic energy obtained by using lidar data for 30 scans. This is the reason that, with an increase in the averaging interval from 10 min to 60 min, the magnitude of the kinetic energy estimate is monotonically increasing (it has no saturation, as in the first case under consideration). Apparently, for conditions of very weak turbulence on the background of nonstationarity of the mean wind, a special procedure for data filtering is required, which is not the subject of this paper.

l) p. 12 line 15: Other possible reasons include the inability of the lidar to resolve any portion of the inertial subrange (thus all derived parameters are not valid) and the low bias of dissipation (denominator for calculation of integral scale) when it is small. => We agree with this comment. Under conditions of stable thermal stratification of the atmosphere, the inertial subrange of turbulence can be much smaller than the size of the sensing volume, or even the inertial interval may be absent. It is obvious that the method of estimating the dissipation rate and the integral scale described in the manuscript is not applicable for this case. Therefore, in this manuscript there are no results of data processing, measured by the lidar in 2016 at night.

m) p. 12 line 20-22: The meaning and significance of 'The value of . . . over azimuth angles' is unclear; it should be rewritten. => Fixed.

n) p. 13 line 2: What is meant by 'close to each other'? A quantitative measure (standard deviation or range of values) is needed. => Page 14, lines 9-10: "(maximum deviation is around 20%)" has been added.

o) p. 15 line 125: Add the qualifier here that these high estimates were under stably stratified conditions. => Probably, the reviewer has in mind line 25. Page 17, line 14: "(measurements in the daytime)" has been added. Page 17, line 16: the sentence "Sometimes such estimates exceed 1 km in contrast to results shown in Figures 6(f), 7(d) and 8(d)." has been added.

p) End of manuscript: With the large number of variables and subscripts in this manuscript, adding a list of the symbols would be extremely helpful in reading this manuscript. I had to keep searching through the paper to find variables that were first introduced many pages earlier in the paper. => Pages 17-20: Appendix with a list of symbols has been added.

Technical corrections: a) p. 6 line 10 (and reference list): 'Pearson' not 'Pierson' => Fixed.

b) p. 6 line 20: Should σe2 have an overbar as well? => Page 6, lines 25-26: The sentence "In Eqs. (13) and (14) it is assumed that... is independent of the azimuth angle ..." has been added.

c) P. 11 line 15: 'continuously' is a better word than 'permanently'. => Fixed.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-140/amt-2017-140-AC1-supplement.pdf

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