Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-140-AC2, 2017

Atmospheric Measurement Techniques Discussions

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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Received and published: 11 September 2017

Referee #2 General comments: This manuscript presents a methodology for deriving turbulent parameters from scanning Doppler lidar observations in the lower atmosphere. The methodology is sound and the results show that the parameters derived from Doppler lidar measurements usually agree well with reference parameters obtained from a sonic anemometer. The methodology uses a particular turbulence model which dictates how certain properties of the observed turbulence are expected to behave and so enable them to be derived. A clear statement describing atmospheric

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situations when this model is applicable, and situations when it is not likely to be applicable, should be included in the conclusion. Are there methods for checking whether the turbulence model is applicable in a particular situation? For example, can you use the Doppler lidar observations to check for stationarity? In addition, what are the likely biases if the model is not strictly applicable, but provides reasonable results? An example here is the slight underestimates in turbulent energy dissipation rate provided by the Doppler lidar at low values. Is this expected because of unrealistic integral scales used, or is it an issue in accounting for radial velocity measurement uncertainty correctly? => To answer these questions, more research is needed. In this manuscript, we propose a method that is applicable for determining the parameters of wind turbulence from lidar measurements in the atmospheric layer of intensive mixing. The turbulence model, on the basis of which this method was developed, is quite applicable for such a layer. To obtain information about wind turbulence from measurements by a lidar in a stably stratified boundary layer (especially inside a low-level jet stream), it is necessary to apply another data processing procedure that is not known to us. Also it is necessary to take into account that at very strong stable temperature stratification the turbulence becomes intermittent and the inertial subrange can disappear. Page 17, lines 23-25: The sentence “However, as shown by the lidar experiment conducted under stable temperature stratification outside the layer of intensive turbulent mixing (Smalikho and Banakh, 2017), this method is not applicable and, consequently, further investigations and development of new approaches are needed.” has been added.

The manuscript contains all of the information necessary for publication, but in its current state is difficult to read. There are a huge number of variables and subscripts introduced, which although necessary for completeness, make it difficult to follow. It would be easier to comprehend if large parts of the derivation were placed in an appendix, with terms directly related to the parameters that will be derived from observations included in the text. In addition, the instrument should be introduced first in Section 3, so that it is easy to refer to the instrument specifications when introducing the measurement strategy. Add a table presenting the relevant instrument specifica-

tions, e.g. pulse-repetition-frequency, receiver bandwidth/Nyquist velocity, range gate length, azimuthal scanning speed, lidar wavelength, telescope type, rather than referring the reader to another paper. As an aid to the reader, this table could also include the associated variable in the equations. After some minor modifications, I feel this manuscript will be suitable for publication. => Pages 18-20: Appendix with a list of symbols has been added. Main parameters of the Stream Line lidar are given in Table 1 of our paper published last year in AMT (see page 10, lines 9-10). In our opinion, the inclusion of this table in the manuscript submitted to the same journal would be superfluous. The parameters of the lidar experiments conducted in 2014 and 2016 differ and are given in Sections 4 and 5, respectively.

Specific comments: Page 1, line 19: The data provided by these instruments is not really 'raw' data, but radial velocities. => The phrase "raw data measured" has been replaced by "measurements".

Page 2, line 13: Suggest replacing 'were proposed' by 'have been proposed'. => Fixed.

Page 2, line 24: Need to state that this is '100 to 500 m in altitude', as it could be assumed that the distances refer to range. => Fixed.

Page 2, line 27: Suggest starting the paragraph with 'First, we describe the equations that will be used to develop the measurement strategy and method for deriving the wind turbulence parameters:' => Page 2, lines 29, 30: "First of all, derive the equations to be used as a basis for development of the measurement strategy and the procedure of estimation of wind turbulence parameters:" has been replaced by "First, we describe the equations that will be used to develop the measurement strategy and the procedure of estimation of wind turbulence parameters:".

Page 2, line 28: The measured 'raw' radial velocities are not strictly instantaneous, as they are obtained by averaging a large number of samples internally. => Here we do not consider the radial velocity measured by a lidar.

Page 4, line 4: Suggest replacing 'some or other' with 'an appropriate'. => Fixed.

Page 4, lines 8-9: It would be clearer for the reader if these expressions were placed on separate lines. => Fixed.

Page 3, line 15; page4, lines 20-24; and Figure 1: It should be made clear, especially in the Figure caption, that the azimuth angle refers to the azimuthal resolution (if continuous scan) or separation between 2 adjacent rays in a scan (step-stare scan). => In Section 2 we find the condition under which the azimuth structure function of the radial velocity is equivalent to the spatial transverse structure function of the wind speed. Here we do not take into account the spatial averaging of the radial velocity over the sensing volume, which takes place in lidar measurements. For a transverse structure function, it is easy to take into account the spatial averaging over the sensing volume. In our experiments we used continuous scan and, therefore, the azimuth angle resolution is equal to the angle between two adjacent rays.

Page 5, line 1: Suggest replacing 'the both' with 'both'. => Fixed.

Page 5, line 5: What is the rationale behind choosing delta theta = 3 degrees? And what does L correspond to? => In principle, for calculation of the structure functions shown in Figure 1, we could choose any 'delta theta' which is less than 9 degrees (corresponding solid and dashed curves in Figure 1 almost coincide for azimuth angles less than 9 deg). In the case of 'delta theta' = 3 degrees and 'L' = 30 the maximum angle 'delta theta'*'L' = 90 degrees. The same 'delta theta' and 'L' were used to obtain structure functions shown in Figure 12 (in revised manuscript).

Page 5, Measurement strategy: Do you mean that you perform one conical scan with +ve azimuth rotation, then one scan with -ave azimuth rotation? => Yes.

Page 5, line 24: As defined previously, R_0 should be (delta R / 2) if the first range gate is k=0, unless you define k=0 as the first usable range gate. Then 'minimal distance' should be defined precisely, e.g. define 'R_0 is the distance to the first usable range

gate' before the equation on line 23, and explain why the first gate should satisfy the condition stated on line 25. => Page 5, lines 24 – 25: "... is the distance to the first usable range gate" has been added. "The minimal distance ... depends on the probing pulse duration. At the same time, it should satisfy the above condition. . . ." has been removed. This condition must be satisfied for any ranges ..., as afore noted in Section 2 (see page 3, lines 16 – 17).

Page 5, line 26: The maximum range is effectively determined by the instrument pulse repetition frequency; the maximum usable range depends on the signal-to-noise ratio (SNR) and hence the atmosphere. Suggest rewriting this sentence, stating instead that the 'uncertainty in the radial velocity measurement depends on the SNR'. => Page 5, lines 26 – 27, page 6, lines 1 - 2: "The maximal distance ... the true value of the velocity." has been replaced by "Uncertainty in the radial velocity measurement depends on the signal-to-noise ratio (SNR). At low SNR the probability of "bad" estimate .... To avoid the application of the data filtering procedure, ... not contain "bad" estimates."

Page 6, line 9: Use correct reference (Pearson). => Fixed.

Page 6, line 11: Do you mean azimuthal dimension rather than longitudinal dimension here? => Page 6, line 12: "longitudinal" has been replaced by "transverse".

Page 6, line 14: How do you know if Lv only occasionally exceeds the sensing volume? => Page 6, line 15: "only few times exceeds the size of the sensing volume" has been replaced by "exceeds the size of the sensing volume insignificantly".

Page 6, lines 15-17: Other authors have shown that it is usually safer to always take account of the uncertainty in the radial velocity estimates. => Page 6, line 18: "(Frehlich et al., 2006)" has been added.

Page 6, lines 18-24: This sequence of equations requires much more explanation than is given here. ?? => 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 12: “(Banakh and Smalikho, 2013)” has been added.

Page 8, lines 12-15: Not sure that this can be justified without evidence. => Page 8, lines 18-23 and page 9, lines 1-2 (revised manuscript): The sentence “Since the instrumental error of estimation of the radial velocity . . . it is not necessary here to take into account the instrumental error and the effect from averaging of the radial velocity over the sensing volume.” has been replaced by “Since the instrumental error of estimation of the radial velocity . . . to take into account the instrumental error and the effect from averaging of the radial velocity over the sensing volume. Indeed, as shown by Eberhard et al. (1989), in the case of a horizontally homogeneous turbulence statistics and . . .. Taking into account that . . ., Eq. (25) can also be regarded as exact.”.

Page 9, line 16, and page 11, line 15: The focus of the lidar beam was set to XX m. => Fixed.

Page 11, line 9: Suggest ’To test the method for determining the kinetic energy,..’ => Fixed.

Page 11, line 12-15: Suggest ’The presence of forest fires in the Tomsk region provided lidar measurements with high signal-to-noise ratios ...’ => Fixed.

Page 11, line 15: Suggest replacing ’permanently’ with ’continuously’. => Fixed.

Page 11, line 20: The ’minimum useful range’. => Fixed.

Page 12, line 3: I assume you mean ’horizontal wind speed’. => Page 13, line 11 (revised manuscript): “wind velocity” has been replaced by “horizontal wind speed”.

Page 14, line 21: This assumes that the turbulent parameters don’t change over the time required to obtain 30 scans. => Page 16, line 8 (revised manuscript): “In the case of stationary conditions” has been added.

Figure 3: Suggest replacing ’Time profiles of the turbulence’ with ’Time series of the turbulent’. => Fixed.

Figure 4: Suggest replacing 'Time profiles' with 'Time series'. => Fixed.

Figure 6: Panel (a) should state 'Wind speed' rather than 'Wind velocity' for the colorbar title. => Usually in our publications in English we used "Wind velocity".

Figure 7: Suggest replacing 'Temporal profiles' with 'Time series'. => Fixed.

Figure 7,8: Suggest replacing 'instrumental error of estimation of the radial velocity' with 'uncertainty in radial velocity estimate'. Figure 9: Suggest replacing 'Spatiotemporal distributions' with 'Time-height plots', and 'relative error of estimation of the dissipation rate' with 'relative error in dissipation rate'. => Fixed.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-140/amt-2017-140-AC2-supplement.pdf

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