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Atmospheric Measurement Techniques Discussions

Interactive comment on "Towards improved turbulence estimation with Doppler wind lidar VAD scans" *by* Norman Wildmann et al.

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1 Author response

We want to thank the two anonymous reviewers for their valuable feedback and valid points of criticism to our manuscript.

1.1 RC1, General Comments

1. *Through the comparison with sonics they are able to show a slight improvement when pulse averaging effects are considered (Fig 6). The main novelty here*

seems to be the advection correction, which they claim improves the retrievals, but the results are a bit underwhelming (see Fig. 6 and 7). I'm not at all convinced that the tiny improvements in the metrics (bias and correlation) are significant. I would be more inclined to conclude that the advection correction doesn't have a significant effect.
The novelty of the paper is not only the advection correction. We re-evaluate a method introduced by Smalikho et al. and validate it in two different campaigns. The analysis method has been modified in ways that are relevant for practical implementation (smaller number of scans, VAD at higher elevation angle) and it was shown that these modifications are legit. The paper describes the first comparison of in-situ aircraft data with lidar turbulence retrievals above 800 m. We show that more validation of this kind will be necessary in future because lidar measurements in low turbulence regimes can significantly underestimate TKE and especially its dissipation rate.
The advection correction has only a small effect for low elevation VAD scans, which is a positive result as it shows that the Smalikho-method can be used with less restrictive advection filters, if the respective uncertainties are acceptable. Nevertheless, we do show an improvement of the TKE error which increases with higher wind speeds (Figure 8) especially at the 50-m level if the correction is applied. Even more importantly, the advection correction is highly effective if higher elevation VAD scans (here: 75°) are performed to retrieve TKE dissipation rate as is shown in Figs. 9 and 10.

1.2 RC1, Specific Comments

1. *Abstract: The first 3 or 4 sentences could be probably be reduced to a single sentence in favor of allowing for a more quantitative summary later in the abstract. As it stands, the abstract lacks sufficient substance. The author should incorporate more hard results from the comparisons with sonic anemometers.*

We believe that we need to explain the basic idea of the measurements with the introductory sentences even in the abstract but can still add more substance to it in the end. A modified version is given in the revised manuscript.

2. *page 4 line 3: Not everyone will know where Upper Silesia is (including myself until I looked it up), I suggest "...were installed in Upper Silesia, in southern Poland (or where ever), ..."*
   We will add the country Poland in parantheses, the exact location is indicated on a map of Europe in Figure 2.

3. *page 4 line 15: change "...and were finally fixed..." to "...and were finally choosen..."*
   Ok.

4. *Table 1. The Stream Line wavelength is 1.548 μm.*
   From the lidar manufacturer Halo Photonics, we were only provided with the official value of 1.5 μm. If the reviewer can give us a reference for the value he suggests, we will very much like to correct it in the table. On the other hand, this value is not very important for the study and if serious doubts about this value persist, we would rather not mention it at all.

5. *Section 3.2: The author should include the equation for the measured azimuth structure function – since this is key for the dissipation rate retrieval methods.*
   We will include the equation just before Eq. 8 in the revised manuscript:
   $D_L(\psi_l) = \langle [v_r(\theta) - v_r(\theta + \psi_l)]^2 \rangle$
   $D_a(\psi_l) = D_L(\psi_l) - 2\sigma_e^2$

6. *Equation 3: The condition that $\varphi = 35.3°$ should be made more explicit to prevent possible misused.*
   The sentence introducing Equation 3 explicitly states that it is for the special case

of $\varphi = 35.3°$.
   We add the suggested addition to the equation.

7. *Page 6, lines 4-6: The author states that the "TKE dissipation rate is estimated through a fit of the measured second-order structure function of horizontal velocity to the theoretical ..." This statement implies that the observations are adjusted to fit the model, when in fact it's the other way around, i.e. the model parameters are adjusted in order to fit the observations. Please rephrase.*
   We apologize for the mistake in language and correct it in the revised manuscript.

8. *Page 6, lines 23-24: Similar to last comment. The author states that "A fit of the azimuth structure to the equation ..." again implies that the observations are being adjusted to fit the model, when in fact it's the other way around. Please rephrase.*
   We apologize for the mistake in language and correct it in the revised manuscript.

9. *Page7, line 1: The author states that "Scanning with Doppler lidar in a VAD implies a volume averaging of radial velocities in longitudinal and transverse directions." Aside from the grammatical errors, this statement is not generally true because transverse averaging is not an issue for step-stair scans, only for continuous motion scans. The author should briefly mention the two different types of scans in their introduction. Also, the author should define what they mean by longitudinal and transverse (i.e. along the beam, and orthogonal to the beam).*
   Velocity azimuth display (VAD) is a term from radar technology where continuous motions of the azimuth motor are the standard. Step-and-stare scans like Doppler-Beam-Swinging techniques are not considered in this manuscript. In any case, even step-and-stare scans with pulsed DWL have a transverse averaging effect due to the pulse-averaging over a certain accumulation time. We add to the manuscript that transverse averaging is regarded for scans with a continuous motion. We also define longitudinal and transverse in the revised manuscript.

10. *Page 7 line 5: change "... radial wind speed..." to "...radial velocity...". Wind speed is a (positive) scalar, velocity is a vector. In this sentence your talking about the radial component of the velocity vector. "Radial wind speed" makes no sense.*
We change the wording in the revised manuscript.

11. *Page 7 starting at line 7: The discussion here is a bit disjointed and difficult to follow. Equations 5-7 should be listed after the sentence on line 5 (starting with "It is based on the decomposition..."). As it is, these equations are introduced without any corresponding text. One suggestion might be:*
"In Smalikho and Banakh (2017), this method has been combined with the E89-method to yield TKE, and the momentum fluxes. It is based on the decomposition of radial velocity variance into its subcomponents, i.e. $\sigma_L^2 = \sigma_a^2 + \sigma_e^2$
$\sigma_a^2 = \sigma_r^2 - \sigma_t^2$
$\sigma_r^2 = \sigma_L^2 + \sigma_t^2 - \sigma_e^2$ *where* σ_L^2 *is the lidar measured variance,* σ_a^2 *is the lidar measured variance without instrumental error,* σ_e^2 *is the instrumental error variance, and* σ_t^2 *is turbulent broadening of the lidar measurement. In Smalikho and Banakh (2017), all of these variances and corresponding structure functions are calculated for single azimuth angles and then averaged."*
We agree that the modifications make the text easier to follow and incorporate the changes in the revised manuscript.

12. *Page 7, line 17: Recommend changing "Substituting* σ_e^2 *in Eq. 7 with Eq. 8 yields:" to "Combining Eq. 7 with Eq. 8 yields:"*
We change the sentence in the revised manuscript according to the suggestion of the referee.

13. *Page 7, lines 18-23, including equations 9, 10 and 11: There is a dependence on the separation distance on the right side of equation 9 that presumably cancels such that the right side is effectively constant, i.e. independent of separation*

distance. This is a subtle point that is not made by the author. Also, in equation 10, the author has substituted Ψ_l *with* Ψ_1 *without any explanation or justification. Please explain.*
We agree that an explanation is lacking here. Since the instrumental error σ_e^2 is assumed to be a constant offset of azimuth structure function $D_a(\psi_l)$ and the lidar measurement $D_L(\psi_l)$, l can actually be chosen arbitrarily in the TKE equation. It is set to $l = 1$ because potential random errors like unstationary flow will be least effective for small separation angles.

14. *Page 8, line 10-11: The author states "...from VAD scans with other elevation angles as well." You should be a bit more specific here, since readers may not know what you mean by "other elevation angles." I assume you're referring to elevation angles different from 35.3°.*
We change the sentence to explicitly say "elevation angles different from 35.3°.

15. *Page 8, line 13-15: The author states "The value of l = 9 is chosen following the example of Smalikho and Banakh (2017) and corresponds to* $l\Delta\theta = 9°$ *as it was found to be suitable in all conditions in that study." The discussion up to this point had been fairly general. Now, suddenly the author is referring to a very specific VAD scan. The author should be a bit more specific as to which scan (and which experiment) they are referring to.*
Here we define the upper separation angle that will be used in the further manuscript and in all experiments. The separation angle is not referring to a specific VAD scan and in our opinion can be introduced here. We rephrase in the revised manuscript to state that this separation angle was found by Smalikho and Banakh (2017) to be a reasonable value in the ABL. It is illustrated by an example structure function in Figure 3.

16. *Page 8, line 24: change "...radial wind speeds..." to "...radial velocities..."*
All occurences of the term "radial wind speed" are replaced with "radial velocity".

17. *Page 8, line 28: change "...radial wind speeds..." to "...radial velocities..."*
All occurences of the term "radial wind speed" are replaced with "radial velocity".

18. *Page 10, line 1: change "...radial wind speeds..." to "...radial velocities..."*
All occurences of the term "radial wind speed" are replaced with "radial velocity".

19. *Page 10, line 3: change "...radial wind speed..." to "...radial velocitiy..."*
All occurences of the term "radial wind speed" are replaced with "radial velocity".

20. *Page 10, line 6: Recommend changing "Since the mean of the radial wind speed fluctuations vr = 0 by definition, it is:" to something like "Since the mean of the radial velocity fluctuations is zero by definition, equation (20) becomes "*
We change the sentence accordingly.

21. *Page 10 line 5: The author states "(here: g=360 for all azimuth angles)". The reference to a specific value of g here is a bit perplexing. Please explain.*
In this study we work with VAD scans with 1° azimuthal resolution, which yields 360 values per scan. Since we introduce a general method here, we will remove this information at this point of the text.

22. *Equation 20: The summation is over j, but there is no dependence on j in the quantity being summed. Please explain.*
This is a mistake. The summation is over the variable m (index of the azimuth angle within one VAD scan).

23. *Page 10, lines 11-12: change "...radial wind speeds..." to "...radial velocities..."*
All occurences of the term "radial wind speed" are replaced with "radial velocity".

24. *Page 10, line 16-17: The author states "Measured PDFs of the variables ... are fit to the model PDFs to obtain an estimation of the corresponding standard deviations σ_1, σ_2 and σ_3 and probability of bad estimates P_1, P_2 and P_3." This statement implies that the observations are fit to the model. In other words, the*

observations are tweaked to get agreement with the model. That's certainly not what is happening. Please rephrase.
We apologize for the confusion in language and rephrase in the revised manuscript.

25. *Section 3.2.2: It seems to me there is some slightly circular logic going on here. From what I gather, your fitting equation 22 to the measured PDFs to obtain estimates of the variance and the false-alarm probability. But since the real distributions aren't Gaussian, you end up computing the variance directly from the data. This begs the question as to why the variance was treated as an adjustable parameter in the first place. Why not just compute the variance from the data to begin with and then use equation 22 to estimate only the false alarm probability. What do the distributions look like? How good (or bad) are the fits?*
A first guess of the standard deviations is needed to find the $\pm 3.5\sigma$ region for the integral over the PDF. All the details of this method are given in Stephan et al. (2018). Since this method is not essential for this study, we do not want to expand too much on it in this manuscript. It is mostly relevant if better measurements in low-signal conditions are targeted.

26. *Section 3.2.3: In this section the author throws down a series of equations without adequate discussion. The authors need to do a better job explaining their line of reasoning.*
We thank the referee for their recommendation on improving the section and incorporate it in the revised manuscript.

27. *Appendix C, lines 17: The author introduces the quantity Xj (i.e. a 1D vector), and then in equation C1 it is indicated to be a 2D matrix, i.e. Xij. Please explain.*
j is the subsample index and i is the index for each element of the subsample. We clarify this in the revised manuscript.

28. *Appendix D: I find no mention of the “FSWF-retrieval” in the paper (did I miss it?). Please elaborate and provide relevant citations.*
Filtered sine-wave fitting is introduced with the corresponding reference in Section 3.2.1, but without giving the abbreviation FSWF. We add this in the revised manuscript.

References

Stephan, A., Wildmann, N., and Smalikho, I. N.: Spatiotemporal visualization of wind turbulence from measurements by a Windcube 200s lidar in the atmospheric boundary layer, Proc.SPIE, 10833, 10 833 – 10 833 – 10, 10.1117/12.2504468, 2018.

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