This paper discusses methods for retrieval of turbulence kinetic energy and dissipation rate from VAD (i.e. conical) Doppler lidar scans. Starting from established methods (structure function methods), they introduce changes that permit retrievals using a smaller number of scans, and a correction for the effects of advection. The methods are evaluated by comparison sonic measurements in one field campaign.

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 organization of the paper is fine, and it is written well enough to be understood. However, I do feel that the authors could have done a better job at explaining a number of things, which I highlight below. As it stands the paper requires fair significant revision before acceptance.

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.

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), …"

page 4 line 15: change "…and were finally fixed…" to "…and were finally choosen…"

Table 1. The Stream Line wavelength is 1.548 μm.

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.

Equation 3: The condition that $\varphi = 35.3^{o}$ should be made more explicit to prevent possible misused. I suggest something like

$$E_{TKE} = \frac{3}{2}\bar{\sigma}_r^2\Big|_{\varphi=35.3^o}$$

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.

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.

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).

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.

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.

$$\sigma_L^2 = \sigma_a^2 + \sigma_e^2,$$

$$\sigma_a^2 = \sigma_r^2 - \sigma_t^2,$$

and

$$\sigma_r^2 = \sigma_L^2 + \sigma_t^2 - \sigma_e^2$$

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.”

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:”

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.

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°.

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 $l\Delta\theta = 9^o$ 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.

Page 8, line 24: change "…radial wind speeds…" to "…radial velocities…"

Page 8, line 28: change "…radial wind speeds…" to "…radial velocities…"

Page 10, line 1: change "…radial wind speeds…" to "…radial velocities…"

Page 10, line 3: change "…radial wind speed…" to "…radial velocity…"

Page 10, line 6: Recommend changing "Since the mean of the radial wind speed fluctuations $v_r = 0$ by definition, it is:" to something like "Since the mean of the radial velocity fluctuations is zero by definition, equation (20) becomes "

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.

Equation 20: The summation is over j, but there is no dependence on j in the quantity being summed. Please explain.

Page 10, lines 11-12: change "…radial wind speeds…" to "…radial velocities…"

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.

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?

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.

Recommend something like …

"When advection is not considered, the spacing between samples is given by

equation (25).

An estimate of the mean spacing can be obtained from

equation (26),

where

equation (27).

We propose a simplified correction in which

equation (30),

where

equation (28),

and

equation (29).

Appendix C, lines 17: The author introduces the quantity X_j (i.e. a 1D vector), and then in equation C1 it is indicated to be a 2D matrix, i.e. X_{ij}. Please explain.

Appendix D: I find no mention of the “FSWF-retrieval” in the paper (did I miss it?). Please elaborate and provide relevant citations.