Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-8-AC2, 2020

Atmospheric Measurement Techniques Discussions
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EGU

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

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Received and published: 5 May 2020

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. *The method to retrieve ε from sonic anemometer (and research aircraft) measurements is based on averaging 2 min estimates of ε over 30 min sampling*

time. Taking the average means that the distribution of ε_{2min} is assumed to be Gaussian, which is not necessarily true as the magnitude of ε_{2min} may vary over several scales of magnitude. When the distribution is not Gaussian, the average will introduce a bias to the ε_{30min} estimate. Therefore, authors should check the shape of the distribution of ε_{2min} values during each 30 min period and choose an estimate for ε_{30min} that is more representative for the distribution of ε_{2min} values, such as the median of these values.

We thank the referee for this comment which is very valid and should be considered in the evaluation of sonic anemometer data. An arithmetic mean of dissipation rates ε is not the best solution given the exponential character of the variable. Instead of using the median as suggested, we however believe now that for this study it is more reasonable to calculate the structure function over the full half-hour period and estimate ε from it. Muñoz-Esparza et al. (2018) calculated 2-minute periods because they wanted to see bursts of turbulence on shorter timescales. In our case we are however comparing to lidar measurements that are averaged over half-hour periods, so that a comparison can be best made with the same period for the calculation of the structure function of sonic anemometer data.

We did investigate the difference between median of 2-minute estimates, mean of 2-minute estimates and 30-minute estimates and can confirm that the referee is right that the 2-minute mean is skewed towards larger values compared to the the median approach. A systematic error can however also occur when the median underestimates the dissipation rate within the half-hour (see Fig. 1). We will not present these results in the revised manuscript because we believe that 30-minute stucture function estimates are the right choice for this analysis.

In any case, for all possible estimates of ε from sonic anemometers, the differences are small enough to not change the conclusions that are made for the comparison to the lidar retrievals. In Fig. 2 below, we show the differences: a)-c) show the 30-minute structure function estimate, d)-f) show the mean of 2-minute structure function estimate (as in the discussion manuscript) and g)-j) show the median of 2-minute structure function estimate. The best correlations can be found with the 30-minute structure function estimate. The median estimate shows the highest bias for the S17 method, but the general conclusions remain the same.

1.2 RC1, Specific Comments

1. *It is not obvious to all readers that "DLR Cessna Grand Caravan 208B" is a research aircraft, please, add this to the abstract. Further, it is not clear why do you introduce the research aircraft data in this study as the title is about improvement of turbulence estimation using Doppler wind lidars. For validation purposes research aircraft data cannot be considered as robust as sonic anemometer data. In fact, the explanation for using aircraft measurements is provided only in Section 5 on lines 8-9 of page 21. This explanation should be given already in the abstract but also in the Introduction (Section 1) and maybe also in Section 3.3.*
We add the term "research aircraft" in the abstract explicitly and also explain that the research aircraft is a unique possibility to collect in-situ turbulence measurements above the heights that are in reach with sonic anemometers for example.

2. *Section 2, page 2, lines 33-34: "data from two different sites and sets of instruments": This is not clear: you use data from four DWLs (three in Upper Silesia, one at Falkenberg), from two sonic anemometers and from one research aircraft. Although you introduce the measurements in two Figures (Figures 1 and 2), it does not change the fact that you have several type of instruments and sites, not just two of each.*
We change the text to only state that measurements from two different experiments are analyzed. The details of the sites and the instrumentation is given in detail throughout the section: "In this study, data from two different experiments are analyzed. Both of the experiments and the instrumentation is introduced in

this section."

3. *Section 2.1., page 3, line 19: remove “and infrared gas analysers LI7500 (LiCor Inc.)” because you do not use this data.*
   We remove the information about the gas analyzer in the text: "Continuous turbulence measurements (20 Hz sampling frequency) using sonic anemometer of type USA-1 (METEK GmbH) are performed at the 50m and 90m levels of the tower and have been used for validation purposes. The instruments are mounted at the tip of the booms pointing towards South."

4. *Section 2.2, the first two paragraphs on page 4: the description of the CoMet mission is too detailed and not relevant to the topic of this paper, as here the aim is not to investigate CO2 or methane but to develop DWL data processing methods. Please, provide here only such information that is relevant for the present study.*
   We believe that the information about the CoMet campaign is relevant, because it puts the DWL measurement into context for the special issue to which this manuscript has been submitted. This manuscript is important for the CoMet research community, so that we want to make a connection to the overall project.

5. *Page 4, line 19: angle should read 35.5° not 35°.*
   Ok.

6. *Page 4, lines 20-21, 2 comments:*

   (a) *Check the tense of verbs to be consistent.*

   (b) *The acronyms of the Doppler lidars in Upper Silesia region are misleading: for the research aircraft you use the acronym “DLR” (e.g. in the abstract but also on line 8 of page 4) and for Doppler lidars you have introduced the acronym “DWL”. Why do you introduce here another acronym for DWL, i.e., “DLR”? Please, use only one acronym for Doppler wind lidars throughout the paper.*

We change the naming of the lidars in the CoMet campaign to DWL#1-DWL#3 in the revised manuscript. The tense of the verb are all changed to past tense.

7. *Page4, lines 23-25: "a case study on 5 June 2017, on which D-FDLR was performing long straight and level legs between 800 m and 1600 m as indicated in the flight path in Fig. 2." This is the first time you introduce the research aircraft data and it is somewhat vague. Please, provide more information on why did you choose this data set, how did you select this specific period, what kind of instrumentation there was onboard, how accurate are the turbulence measurements from the aircraft in general, etc?*
   More information with the relevant reference is given in this Section in the revised manuscript.

8. *Page 6, line 7: "the values are calculated for 2-minute intervals and then averaged over half-hour periods.". Include here information about the distribution of values calculated for 2-minute intervals, to show that the average is (or is not) a representative parameter for the population of values (see also the major comment).*
   See above answer to major comment.

9. *Page 7, line 19: Should it read Eq.2 instead of Eq.3?*
   It is actually Eq. 3, but the way this is written is confusing and we rephrase in the revised manuscript.

10. *Page 7, line 20: Why does Ψ_l change to Ψ_1 here? Please, explain what it means that l=1?*
    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.

11. *Page 7, line 25: Typo: “Kolmogorov-Obhukov spectrum”*
ok.

12. *Page 8, Equation (15): It is not clear how this Equation is derived from Eq. 13 and 8: what happens to σ_e^2 that was in Equation 8?*
By taking the difference of $D_a(\psi_l) - D_a(\psi_1)$, σ_e^2 gets eliminated. We rephrase slightly to be clearer: "Using Eq. 13 and 8, ε can be retrieved from $D_a(\psi_l) - D_a(\psi_1)$:"

13. *Page 9, line 8: “In (Smalikho and Banakh, 2017)” change to “In Smalikho and Banakh (2017)”*
Ok.

14. *Page 10, Equation 20: There is no index j in the equation (which is included in the summation). Moreover, are there some parenthesis missing?*
The index j is a mistake, it should be m. There are no paranthesis missing.

15. *Page 12, Section 3.3: Again, it is not clear why research aircraft data is used: is it used a) because it gives more reliable results than DWLs and therefore can be used for validation of DWL data or b) is it used because it would be interesting to know how good the research aircraft data is compared to DWLs (and sonics)?*
We consider turbulence measurements with flow probes on research aircraft a well-established method which provides more reliable measurements than a DWL since it does not depend on many assumptions except the Taylor’s hypothesis.

16. *Page 14, Figure 5: It is not possible to see the dotted line in panel (a). Moreover, in the caption, could you provide the Equation numbers for the averaged variance and total variance methods in order to strengthen the link between the theory and the results.*
Since the lines for the both methods lie almost on top of each other and the dotted line is thus hard to see we include a subplot showing the difference between the

two methods for the presented time series (see Fig. 3). The references to the equations are added in the revised manuscript.

17. *Page 14, line 5: "modified version W19 introduced in this study", maybe you should use acronym W20 for the method introduced in this paper?*
    We changed the acronym throughout the manuscript.

18. *Page 15, Figure 6: Does the biases in (b) and (e) include all points or only those after the advection filter?*
    All points except the grey points below the threshold are used. We will clarify in the caption. We think it is important to use all points to evaluate the effect of the advection correction.

19. *Page 17, Figure 8: Why there is an oscillatory pattern in TKE bias as a function of horizontal wind speed? The oscillatory pattern is more significant than the differences between the methods, and therefore it should be discussed. Could you also provide here the amount of data in each bin, maybe by adding another y-axis for that?*
    There is no physical reason for an oscillatory pattern in TKE vs. horizontal wind speed. The shape of the curve is merely a specialty of the dataset where more values of certain bins fall in nighttime hours with lower TKE and thus also lower absolute error in TKE. We add the number of data points in the bin in Figure 8 of the manuscript as shown here in Fig. 4.

20. *Page 18, line 9: "Here, it shows that the difference between S17 and W19 only occurs at the very lowest level" - this cannot really be seen from Figure 12.*
    It shows that for DLR1, there is a small difference between the two methods, but it is not very well visible. We change the discussion of this figure in the revised manuscript.

21. *Page 19, Figure 11: Another horizontal axis with a km scale would be nice, because in the text you give the length of the flight path in kilometers.*
We change the plot to give the x-axis in kilometers instead.

22. *Page 20, Figure 12: Different DWL lines are extremely difficult to see in both panels. Consider using different colors for the lines and rescaling of the figures.*
We rescaled the figure and changed the color of the greyscale for the S17-method slightly for better readability of the plot.

23. *Page 20, lines 16-18: "The advection effects are most relevant at the lowest measurement heights where the spatial separation of lidar beams along the VAD cone Δy are small." This is true, but what could perhaps be also mentioned is that the advection speed increases with height because of the logarithmic wind profile.*
We add this thought to the discussion. In our observation, the effect of increasing wind speed with height is however not as strong as the effect of a counteracting larger Δy.

24. *Page 21, lines 1-3: "dissipation rates of values smaller than $10^{-3} m^2 s^{-3}$ are underestimated by the lidars, likely because the small scale fluctuations that are carrying much of the energy in these cases, cannot be resolved any more." This can be true, but you should still check the method to retrieve dissipation rates from sonic anemometers as mentioned in the previous comments.*
As shown above we have evaluated the sonic anemometer retrieval and agree that there can be differences between different methods to obtain a representative value of ε within the half-hour period. However, the strong underestimation of lidar measurements below $10^{-3} m^2 s^{-3}$ is much more significant and found in any case.

25. *Page 21, lines 8-10: this information should be provided much earlier in the manuscript. This is not just a result but also the motivation to use research air-*

craft data in the first place.

More information on the motivation to use aircraft data is added to Section 2.2. in the revised manuscript.

References

Muñoz-Esparza, D., Sharman, R. D., and Lundquist, J. K.: Turbulence Dissipation Rate in the Atmospheric Boundary Layer: Observations and WRF Mesoscale Modeling during the XPIA Field Campaign, Monthly Weather Review, 146, 351–371, 10.1175/MWR-D-17-0186.1, 2018.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-8, 2020.

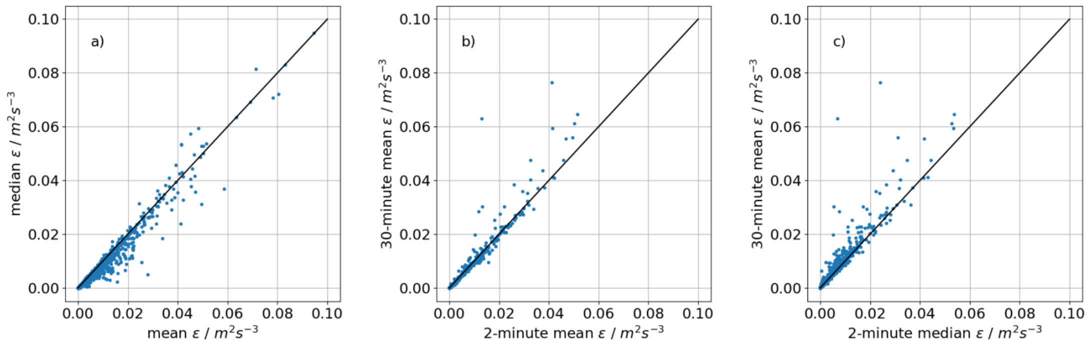

Fig. 1. Comparison of different estimations of dissipation rate from sonic anemometers

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Discussion paper

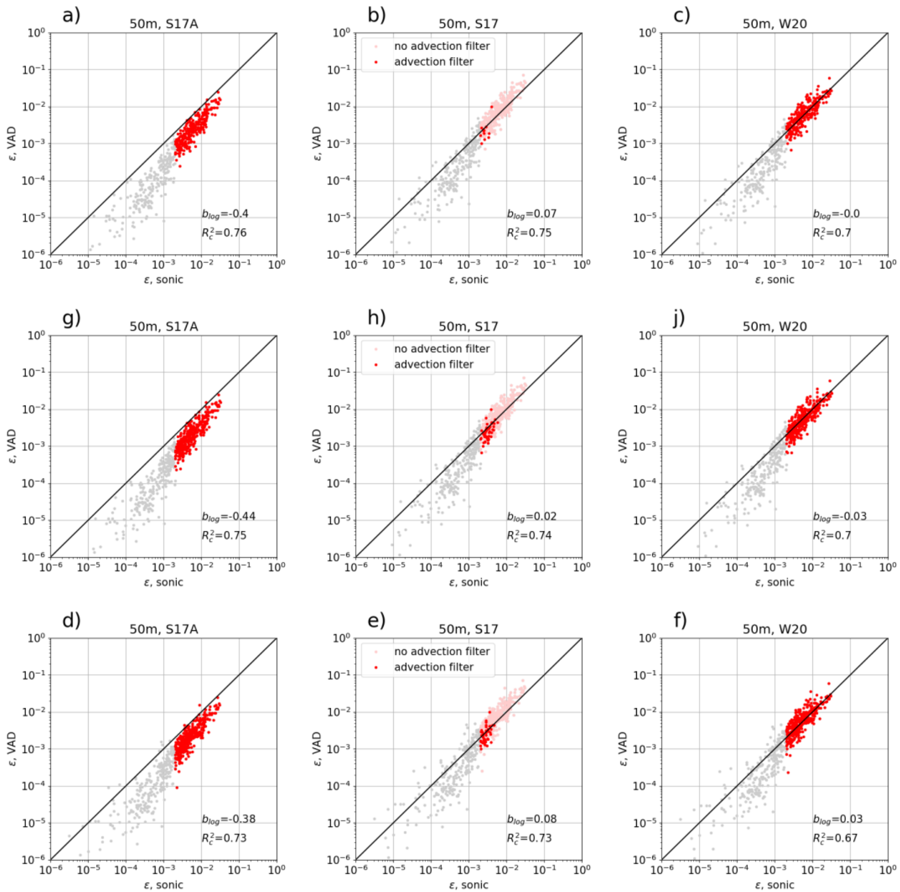

Fig. 2. Scatter plot of lidar dissipation rate retrieval against sonic anemometer dissipation rate, calculated with different methods.

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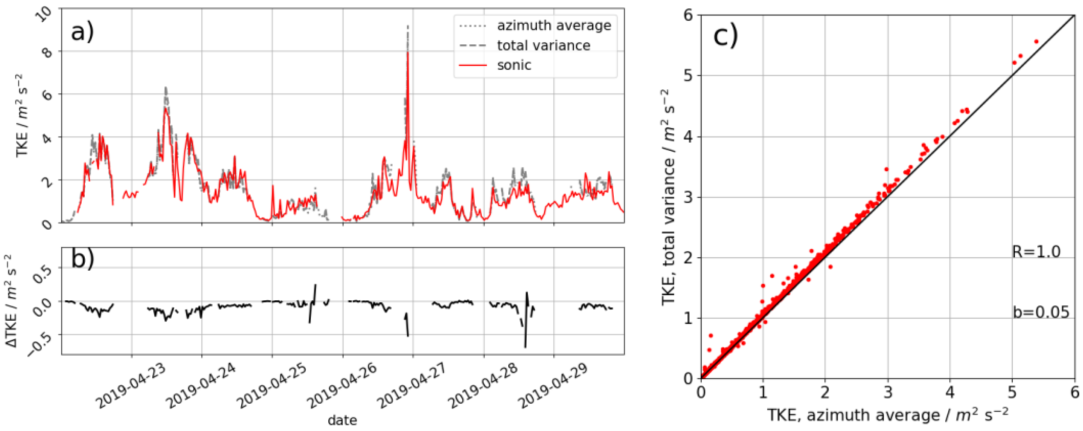

Fig. 3. Modified Figure 5 of the manuscript.

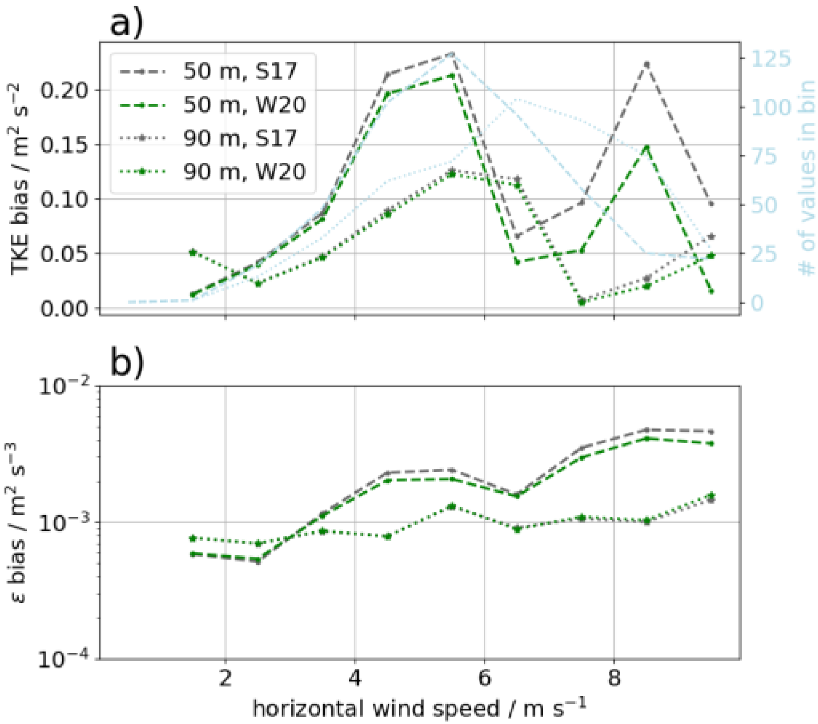

Fig. 4. Modified Figure 8 of the manuscript.