Estimation of turbulence parameters from Doppler wind lidars and in-situ instrumentation in the Perdigão 2017 campaign

Norman Wildmann[1], Nicola Bodini[2], Julie K. Lundquist[2,3], Ludovic Bariteau[4], and Johannes Wagner[1]

[1]Deutsches Zentrum für Luft- und Raumfahrt e.V., Institut für Physik der Atmosphäre, Oberpfaffenhofen, Germany
[2]Department of Atmospheric and Oceanic Sciences, University of Colorado Boulder, Boulder, Colorado, USA
[3]National Renewable Energy Laboratory, Golden, Colorado, USA
[4]Cooperative Institute for Research in the Environmental Sciences, University of Colorado Boulder, Boulder, Colorado, USA

Correspondence: Norman Wildmann (norman.wildmann@dlr.de)

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 RC2, General Comments

1. *Equation 19 should include the effects of Lidar Instrumental noise in this analysis. This has shown to significantly corrupt the Lidar data in many instances (Frehlich et al., 2006, Newsom et al., 2017). Please take a look at Lenschow et al., 2000 and address that issue in your estimates from Lidar data. This could explain a lot of the variability the authors are seeing in low dissipation rate estimates.*

   We agree that noise is critical, especially for the low turbulence regimes. For the RHI retrieval, two different kinds of lidar noise can be considered in fact. First, lidar instrumental noise can be included as E for the spectral width determination in Eq. 14. However, following the example by Smalikho et al. (2005) and introducing a noise threshold n_{th} to the processing of the Doppler spectra, we assumed this contribution to be negligible. Next, following the example of the vertical stare retrieval, we did include in the revised manuscript another term in Eq. 19 to consider the noise contribution to the LOS variance, following the method of Pearson et al. (2009). From the results, which are presented in the revised manuscript, we cannot see a significant change in the large variability at low dissipation rates estimates. This is probably because other sources of uncertainty, connected to the atmospheric variability and the limits of the assumptions made with regards to turbulence theory, especially in complex terrain, outweigh the instrumental noise contribution. In addition, since the estimation of the noise contribution to the variance itself is not straightforward and relies on assumptions as well (see Lenschow et al. (2000)), we think it is important to provide an uncertainty estimation as shown in Sect. 3. For the revised manuscript we have significantly sharpened the filtering for the RHI retrieval, excluding all data with uncertainties larger than the actual value of ε. In the future, refinements of the method with improved error models might be possible, but we do not think that the Perdigão dataset is an appropriate one for this kind of analysis due to the large uncertainties connected to the complexity of the atmospheric flow.

2. *Equation 19 should also include the covariance between the measured variance and turbulent broadening of the spectra. They are related and it needs to be accounted for in the equations. Please take that into account in your analysis.*
   Eq. 19 follows directly from Eqs. 20 and 21. The theory is consistent and not missing a covariance term in that context.

3. *Also, show the length scale estimates from the RHI Lidar retrievals compared to Sonic measurements. In low dissipation rate conditions, the uncertainty of this type of retrieval is high and this has a lot to do with proper length scale estimation.*
   Yes, this is definitely true and we agree that it makes sense to show integral length scale estimates, since the dissipation rates are derived from them. We have calculated the integral time scale from the sonics using the variance of horizontal velocity and dissipation rate estimates according to Eq. 33 of the manuscript in order to be consistent with the theory for the lidar retrievals:

$$L_v = \frac{1.972}{C_k^{3/2}} \frac{\sigma_v^3}{\varepsilon} \quad . \tag{1}$$

   Figure 1 compares the results of L_v and ε for the whole period from 9 to 15 June. As the reviewer suggests, length scale estimation is naturally prone to larger uncertainties, which manifest in large variations at short timescales even in the sonic anemometer estimates. Integral length scale retrievals for corresponding dissipation rate estimates smaller than $10^{-4}\mathrm{m}^2\,\mathrm{s}^{-3}$ have been removed because of the large errors introduced through Eq. 1 at low values of ε and are excluded from the lidar retrieval already through the applied filters.

   In general, the diurnal cycle of turbulence and the corresponding length scales are well-captured by both the lidar and the sonic anemometer; however, some large variations emerge. We will include this plot in the revised manuscript and discuss it accordingly.

4. *Since Turbulence is more a statistical quantity, instantaneous snapshots of turbulence are not extremely helpful in decoding the trends within the atmosphere. So please show the below two plots in your analysis (See Shupe et al., 2012) a. Spectra of the Lidar and sonic measurements needs to show -5/3 and that you are able to resolve the inertial subrange with your measurements. b. Please show distributions of percent error between Lidar and tower measurements, since turbulence is a statistical quantity. Its important to understand how well the Lidar is doing for all conditions.*
   We agree that turbulence is a statistical quantity, however we are not presenting snapshots, but the results from half-hour statistics, as common for turbulence measurements and representation in the ABL. Spectra of lidar and sonic measurements are shown in Appendix C, Fig. C1. We make a very detailed analysis of the uncertainties of the lidar retrieval and give scatterplots of the comparison of all systems to show how well the retrievals work in different conditions.

5. *What time period of data was used for the Sonic calibration?*
   All available data from 9 June through 15 June was used for the calibration in order to get the best possible statistics over all occurring situations.

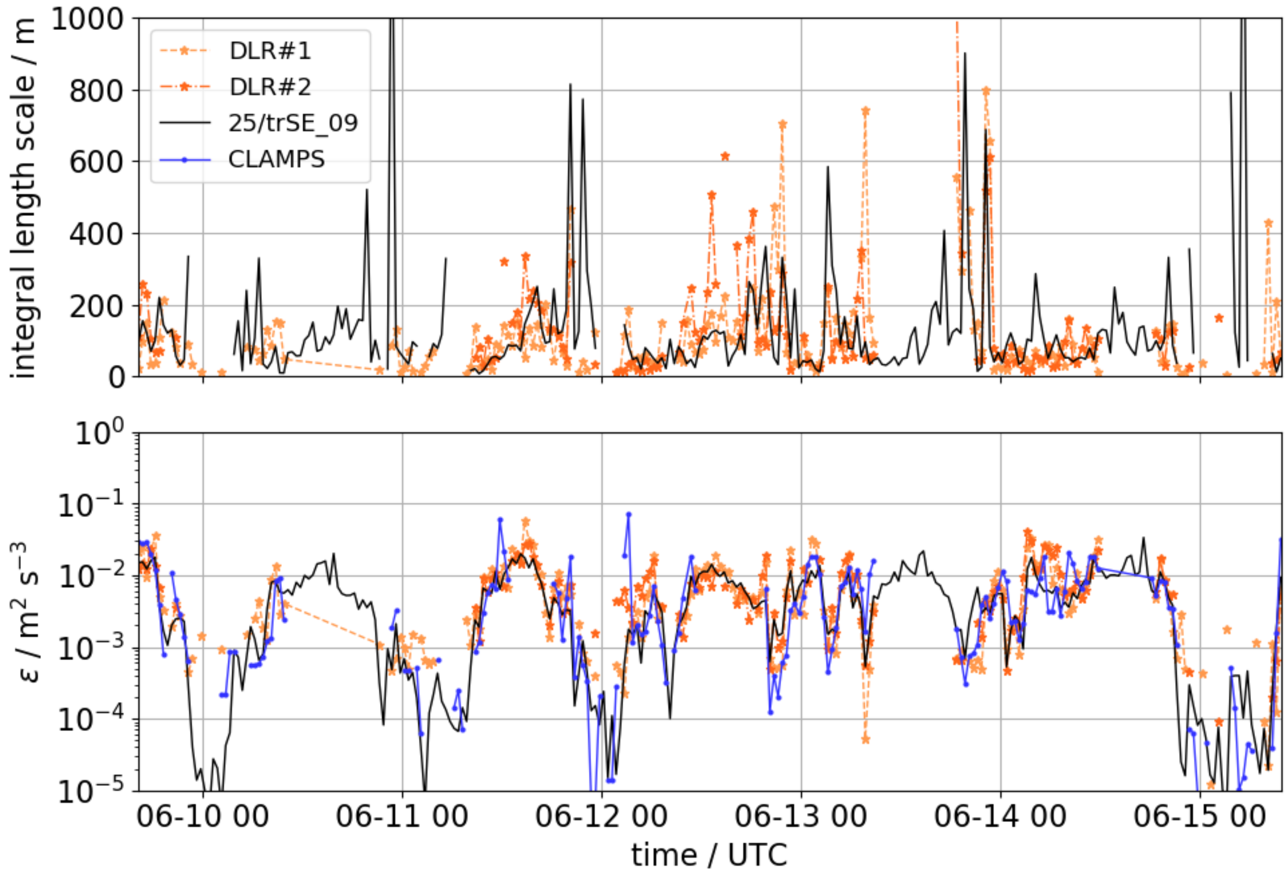

Figure 1. Comparison of time series of L_v (a) and ε (b) for the period form 9 to 15 June 2017 for the two RHI lidars DLR#1 and DLR#2 and the sonic anemometer at 100 m on tower 25/trSE_09 in the valley.

6. *Please also state the expected performance of this algorithm in orthogonal wind directions. Looks like those were the cases, the measurements diverged significantly?*

   Isotropy of turbulence is part of the assumptions that are made for the lidar retrievals, as well as for the in-situ retrievals. Proving or quantifying its validity is a challenging problem which we can not fully address with the available dataset. We can however see from the comparison between the RHI retrievals of the two lidars that are pointing at the same point with different beam angles that there is no systematic difference between the two systems.

7. *Figure 15 needs some imagination to confer with authors view, as the result is mostly noisy. I would recommend removing that figure and probably show a vertical profile of wind direction within the valley from one of the remote sensors?*

   We agree that the figure is disputable in its present form. This is mainly due to the weak performance of lidar DLR#3 cause by some technical issues. For this reason we will remove the figure from the manuscript. Wind direction profiles in the valley are given in Fig. 14 of the manuscript for all available instruments.

8. *Figures 12 & 16, although show the dissipation rate within the wake and the trapping of the turbulence within the valley as authors suggest but is extremely choppy. Maybe the height of the measurements can be limited to 200 m AGL for some clarity?*

   In Fig. 12 we believe that it is important to show the heights up to 800 m to be able to see the upper part of the LLJ and the increased turbulence there. For Fig. 16 and the analysis of the wind turbine wake we agree that a limit to 200 m makes a lot of sense and will change the figures in the revised manuscript accordingly.

9. *Since the authors had done Wake tracking in an earlier paper, can they provide a plot showing the decay of wake induced dissipation rate downwind from the turbine from these results? That would really add value to the paper.*

   For the case that we analyze here, plotting the decay of dissipation rate downwind of the wind turbine in the lidar RHI plane would be misleading. Our analysis shows that wind direction at the turbine location is not aligned with the RHI plane, as only in the valley winds veer towards the RHI plane. Given this complex wind field and the complex shape of the wake (as indicated in Fig. 15 of the manuscript), showing dissipation rate from the RHI plane as a function of distance to the wind turbine would not show the actual decay of turbulence in the wake. Given the methodology that we develop and validate in this study our plan is to look more closely at many occurrences of wind turbine wakes that are more aligned with the RHI plane throughout the campaign in the future. This is however out of the scope of this manuscript.

10. *Page 24: It is important to note, that the general remarks about turbulence retrievals with Doppler Lidars, especially second point about resolving length scales smaller than the rangegate is incorrect. Please see Frehlich et al., 2006, where Length scales smaller than the range-gate size can be estimated using the azimuth structure function method.*

    We explicitly say that the restriction is there for vertical stare and RHI measurements as they are processed in this manuscript. The lead author has also worked with VAD methods using the azimuth structure function as described in Stephan et al. (2018b) and referencing Smalikho and Banakh (2017). Despite the theoretical possibility to retrieve turbulence parameters for $L_v < \Delta z$, these studies show that the sensing volume does influence the azimuthal structure function and it should be considered in the used turbulence model to reduce the systematic error. If these corrections are applied, L_v needs to be larger than Δz for theory to hold. We believe that it is important to emphasize the limitations of lidar measurements with the specific methods that are applied for turbulence retrievals. In our study, these limitations yield high uncertainties in low turbulence conditions that are inherent to the limitations of the resolution of the lidars. We will rephrase the statement on page 24 and also mention Frehlich et al. (2006) accordingly.

11. *There is a risk of the paper being too long, so I would recommend the authors to use the supplement section wisely to transfer some information into that section for brevity of the paper. Since most of the math is very similar to Smalikho et al, 2005, it would be recommended to have most of the equations relating to that in the supplemental section.*

In many papers about lidar retrievals, equations are spread over many references and are comparably hard to follow. In this manuscript we wanted to give a complete and still concise description of the full methodology for the interested reader. We are afraid that pushing many of the equations to the supplement would contradict this goal. We also think that in the final, two-column layout of the manuscript, the equations will not take as much space.

1.2 RC2, Specific Comments

1. *Distance between Lidar data & Sonic measurements? I think within 20 m, but maybe mention it in the article.*
   We mention the distance of the sonic anemometers from the RHI plane and CLAMPS in Sect. 4.1 and it can also be seen from Fig. 1, but we will additionally mention it in Sect. 2.2.3 in the revised manuscript. The distance is 150 m. We are aware that this is a comparatively large distance, but assess and discuss it throughout the manuscript.

2. *Page 12 Line 16: Remove the double dots after "Lv = 3" and mention "Lv = 3 to 1000 m".*
   Although the style guide of Copernicus journals explicitly allows the notation with dots (three dots though...) we will change it in the revised manuscript.

3. *Page 18 Line 3: Remove the double dots after "At 0400"*
   As previous.

4. *Figure 11 can be moved to the supplemental section. The variance is too high, and probably the look directions are different which is causing the large spread in estimates.*
   We agree that a lot of scatter is shown in the plot. A main cause for this are bad estimates of LOS velocity and turbulent broadening that propagate into the turbulence retrievals. In many cases these are due to hard target reflections, for example by clouds in far distance that will corrupt the Doppler spectra. However, we think the plot is important to show that there are no systematic differences between the two lidars. In order to make the plot better we sharpened the filtering of data significantly for the revised manuscript. Estimates with uncertainties larger than the actual value of ε are removed and CNR-filters of $-25\ dB$ at the low end and $5\ dB$ at the high end are applied. For Fig. 11, we removed all occurrences with a probability density below 0.02 to remove remaining outliers and make the plot easier to read.

5. *Looks like there was a similar dissipation analysis comparison done in the recent WFIP2 study, Wilzack et al., 2019 and this should be mentioned in the article as both talk about complex terrain and Lidar comparison in the introduction.*
   The Wilczak et al. 2019 paper was published online just about at the same time that we submitted our paper. We will add it in the revised manuscript.

6. *The analysis in Shupe et al., 2012 is very similar, albeit for Cloud Radars, the authors are recommended to take a look at that article for some interesting details.*
   Thanks for the information. The method that is used in this article is very close to the method we use for the retrievals from vertical stare lidar measurements. We will reference the paper in the revised manuscript.

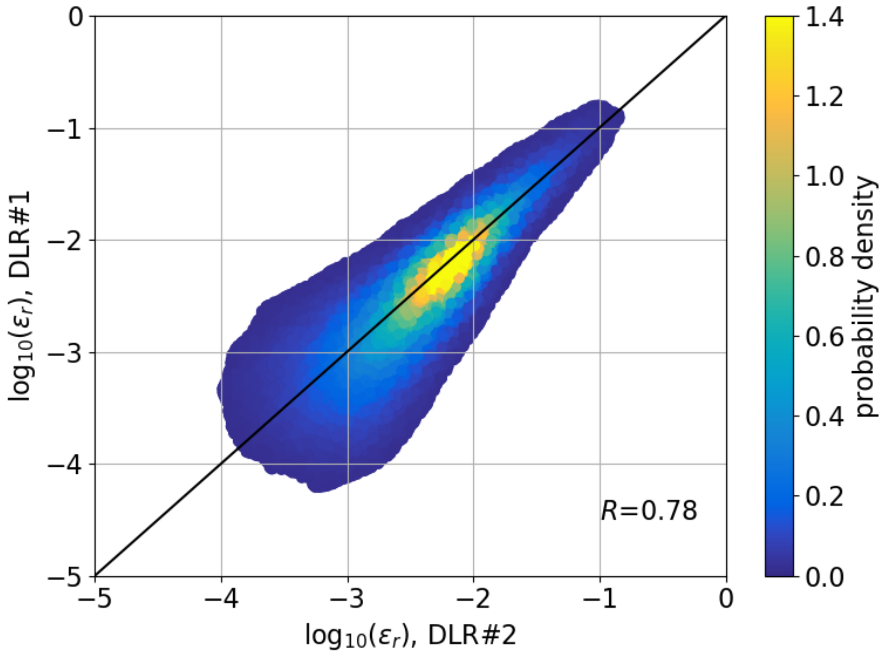

Figure 2. Comparison of all estimates of ε in the RHI plane from DLR#1 and DLR#2 from 9 June through 15 June. The color scale represents the probability of occurrence of a measurement point. The black line is the line of identity.

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