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Atmospheric Measurement Techniques Discussions

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Interactive comment on "Estimation of turbulence parameters from scanning lidars and in-situ instrumentation in the Perdigão 2017 campaign" *by* Norman Wildmann et al.

Anonymous Referee #1

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The paper describes a new methodology for retrieving dissipation rate estimates from scanning Doppler Lidar RHI scans. The results are compared against a sonic anemometer, another vertical staring Lidar and TLS measurements. Overall the paper is well written and provide results that are consistent with theory, but there are some aspects which needs more clarity and further analysis. The reviewer has given some major and minor comments for the authors to consider in revising their article.

Major 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

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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. 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. 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. 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. 5. What time period of data was used for the Sonic calibration? 6. Please also state the expected performance of this algorithm in orthogonal wind directions. Looks like those were the cases, the measurements diverged significantly? 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? 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? 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. 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 range-gate 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. 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.

Minor Comments: 1. Distance between Lidar data & Sonic measurements? I think within 20 m, but maybe mention it in the article. 2. Page 12 Line 16: Remove the double dots after “Lv = 3” and mention “Lv = 3 to 1000 m”. 3. Page 18 Line 3: Remove the double dots after “At 0400” 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. 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. 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.

Further references for authors to consider adding and review: âĂć Frehlich, R., Meillier, Y., Jensen, M. L., Balsley, B., & Sharman, R. (2006). Measurements of boundary layer profiles in an urban environment. Journal of applied meteorology and climatology, 45(6), 821-837. âĂć Shupe, M. D., Brooks, I. M., and Canut, G.: Evaluation of turbulent dissipation rate retrievals from Doppler Cloud Radar, Atmos. Meas. Tech., 5, 1375-1385, https://doi.org/10.5194/amt-5-1375-2012, 2012. âĂć Newsom R.K., W.A. Brewer, J.M. Wilczak, D. Wolfe, S.P. Oncley, and J.K. Lundquist. 2017. "Validating Precision Estimates in Horizontal Wind Measurements from a Doppler Lidar." Atmospheric Measurement Techniques 10, no. 3:1229-1240. PNNL-SA-121097. doi:10.5194/amt-10-1229-2017 âĂć Wilczak, J. M., Stoelinga, M., Berg, L. K., Sharp, J., Draxl, C., McCaffrey, K., ... & Muradyan, P. (2019). The Second Wind Forecast Improvement

Project (WFIP2): Observational Field Campaign. Bulletin of the American Meteorological Society, (2019).

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