Review of ”Estimation of turbulence parameters from scanning lidars and in-situ instrumenation in the Perdigão 2017 campaign” by Wildmann et al. 2019 (amt-2019-171)

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In this study, the authors propose a new approach to estimate TKE from RHI scans performed with Doppler lidars and compare the results to TKE retrieved from a vertical staring Doppler lidar and in situ measurements on towers and a tethered lifting system. The method is applied to data from the Perdigão field campaign which was conducted in Portugal in 2017. Finally, the authors present an example on how TKE from different instruments allows to investigate the wake of a wind turbine.

With the recent enhancements in remote sensing technology, undreamed-of possibilities to study flow structures and turbulence in the ABL appear. To my knowledge, this nice study presents a novel technique related to this topic and falls into the scope of AMT. The manuscript is overall well written and carefully prepared, relevant literature is cited and figures are of good quality. Nevertheless, I have several specific and two general comments - one is about the presentation of the technique to retrieve the dissipation rate from RHI scans and the other one is about the structure of the result sections. I believe that the manuscript is suitable for publication provided that the authors prepare a revised version of the manuscript considering my comments and suggestions.

1 General comments

1. Sect. 3.2.4: The method to derive dissipation rate from RHI is based on the idea of Smalikho et al. (2005). Currently, I find it hard to to distinguish between results of Smalikho et al. (2005) and modifications done by the authors. Where does the modification start? On p. 10, l. 17? I suggest making this much more clear in the text.

2. Sect. 4 and 5: The structure of these two sections is not straightforward and clear in my opinion. The authors start with some statistics of point-to-point comparison in Sect. 4.1,

but already introduce the case study which they will discuss later on (p. 16, l. 9ff). In Sect. 4.2 they first compare profiles from this specific case followed by some statistical comparison of profiles on p. 18, l. 9. In Sect. 5, they finally explain the wake during the case study in detail. I suggest re-arranging these two sections. For example, the authors could start with a section on the statistical point-to-point and profile comparisons for all available days (so basically the results of Fig. 6, 10 and 11. In a second section they could focus on the case study, first starting with the evolution of conditions throughout the night (Fig. 8) followed by the analysis of the wake. When re-arranging many of the figures could (should) be combined. For example, the profiles in Figs. 8 and 14, RHI scans of dissipation rates in Figs. 12 and 16 and coplanar wind in Figs. 7 and 13. This would also reduce the quite large number of figures. Also, the authors should consider to show corresponding times. For example, it would be interesting to see Fig. 13 at the same times as Fig. 12 and 16.

2 Specific comments

1. Title: As the authors focus on the dissipation rate, turbulence parameters could be replaced by dissipation rate.

2. Abstract: The abbreviations TKE, ABL, TLS, RHI, DSW, LOS and LLJ are not necessary in the abstract as they are not reused in the abstract.

3. Introduction: The structure of the introduction is not clear. In the current version, the authors start with turbulence over complex terrain, followed by model considerations and observations in general. While the proposed method can be applied to all kind of terrains, the data shown here are for complex terrain which imposes additional complexity. The authors could rearrange the introduction as follows: start with the challenges in models (wrong parameterization, impact on wind, importance for wind energy sector), continue with the methods to derive turbulence from observations and end with the particular issues related to complex terrain (which is presented in this study). This would then be more from general to specific. Also, the research objectives and motivation should be more clearly formulated and specified.

4. p. 2, l. 10: What does XPIA stand for?

5. p. 2, l. 15-16: This is especially true above the surface layer when few large convective cells can dominate the spectrum. A relevant study to cite in this context is Maurer et al. (2016).

6. p. 2, l. 20: RHI scans have been used as well to derive turbulence characteristics (Bonin et al., 2017).

7. p. 2, l. 26: Remove turbulence before TKE.

8. p. 2, l. 34: The content of Sect. 6 is not mentioned here.

9. p. 3, l. 6: It would be good to mention how far the ridges are apart.
10. p. 3, l. 23: The title reads scanning lidars. While this is true it is misleading, as the Halo Streamline is not used in scanning mode. I suggest rephrasing the title to Doppler lidars.
11. p. 3, l. 32: What is meant by flexible multi-Doppler lidar measurements?
12. Fig. 1: The rotation of the map is confusing in combination with the wind directions discussed in the result section. It would be good to add a small map to show where in Portugal the measurements were done. The contour labels are hard to read and the grey structures are not very clear to see. For #3, 4 RHI directions are given. Please indicate which ones are used in the analysis (Fig. 15). In general, the abbreviations used for the different sites are not consistent throughout the manuscript. For example, trSE_04 is named 20/trSE_04 and CU TLS is just labeled TLS in the text. The OU CLAMPS lidar is referred to as Halo Sreamline lidar in the caption of Table 2. This is unnecessarily confusing.
13. Fig. 2: I am confused by the position of lidar DLR #2. From Fig. 1 I have the impression that it is in the valley while in Fig. 2 it seems like it is on the slope. "lidar CLAMPS" should be labelled the same way as in Fig. 1.
14. Table 1: Please comment why the range date distances were chosen differently and give the physical resolution which results from the pulse length.
15. Table 2: What is the physical resolution of the Halo lidar?
16. p. 5, l. 3 and l. 10: Please comment a little more on the chosen specifications for the lidars. How continnous were the vertical stare measurements of the OU CLAMPS lidar? Where they only interrupted by the VADs every 15 min? You should also mention here that the Doppler spectra are stored during operation as they were needed for the analysis. How are the data filtered? Did the authors apply a SNR filter to detect erroneous radial velocity data before the LOS variance is calculated (Eq. 13)?
17. p. 7, l. 9: Please clarify what is meant by ensemble average in this context.
18. p. 7, l. 18: What does the integral length scale describe?
19. Sect 3.2: The order in this section (sonic, TLS, lidars) does not agree with the order in Sect. 2.2 (lidar, TLS, sonic).
20. p. 8, l. 7: Why not include the inertial dissipation technique in Sect 3.1?
21. p. 8, l. 15: No brackets around O'Connor et al. and Bodini et al.
22. p. 8, l. 17: How is the range of the inertial subrange determined.
23. p. 8, l. 23: Which height is used for U? One value for all heights or height dependent? It could be mentioned that the horizontal wind speed is not necessarily the propagation speed of

cells and the turbulence characteristics such as the integral length scale may be very sensitive to the choice of U. A recent study dealing with this topic is e.g. Adler et al. (2019).

24. p. 9, l. 1: "..measured lidar spectrum.."

25. p. 9, l. 17: How many different points are in one square sub-area?

26. p. 9, l. 20-21: Although 30 min are a common averaging intervals it might be too short and contributions by larger cells might be missed, in particular above the surface layer.

27. p. 11, l. 2: Eq. 13 and Eq. 20 both give expression for the measured velocity variance. Which one is used?

28. p. 11, l. 4: Please explain a little more what is shown in Fig. 3.

29. p. 11, l. 5: Please refer to Eq. 8 for the von Kármán model.

30. p. 12, l. 1: Under which assumptions is the equation for H_p simplified?

31. p. 12, l. 5-9: Are these equations from Smalikho et al. (2005)? Why Δ_z and not Δz in Eq. 29 and 30?

32. p. 12, l. 14: Why $\hat{\mathrm{L}}_v$ and not L_v?

33. p. 12, l. 20: Why not σ_v^2? I thought the hat denotes measured variables?

34. Sect. 3.3.2 and Sect. 3.3.3: Are these methods new or have they been done before? Please cite appropriate literature or make it clear that this is novel.

35. p. 15, l. 12: ".. measurements are taken (Fig. 1)."

36. p. 15, l. 14: How is the interruption by VAD every 15 min affecting the retrieval of dissipation rate from vertial stare mode for 30 min intervals?

37. p. 16, l. 1-3: Here, the authors say that the scatter is related to the spatial separation. On p. 15, l. 12-13, they state that they expect a similar behavior. Please rephrase.

38. p. 16, l. 3: Where is shown that the uncertainty of the retrievals for vertical stare mode increases for weaker turbulence?

39. Fig. 6: How many data are used for the scatter plots? The squares and circles for DLR #2 and DLR #1 are hard to distinguish and the color range for low probability density is hard to see as well. Please change.

40. p. 16, l. 9 - p. 18, l. 7: This better fits to the case study section (see major comment 2).

41. Fig. 7: It would be more helpful for the analysis to show the same time periods as in Fig. 9.

42. p. 17, l. 5: Better say "..retrieved values of ϵ..."

43. Fig. 8: It would be helpful to indicate the time period of the profiles and of the RHI scans shown in the other figures for the case study in the time series plot. Which bin is shown for the RHI scans and vertical stare? The ones closest to the tower measurement height? The plots acutally start at 21 UTC and not at 00 UTC as indicated in the caption.

44. p. 18, l. 3: "... at 0400-0430.."

45. p. 18, l. 6: I mainly see a better agreement above 400 m and not above the ridge.

46. p. 18, l. 9: In the caption of Fig. 10 it is stated that the period from 9-15 June is shown and in the text is says 14 June.

47. p. 19, l. 1: How are the values interpolated? Linearly?

48. Fig. 10: What are the number of values used? Figs should not be in the middle of the page surrounded by text, but rather at the top or bottom.

49. p. 19, l. 3: What is meant by these directions differ significantly? As the azimuth is the same, do the authors refer to the elevation angle? Why do the authors say that the dissipation rate should not depend on the direction? If the elevation angles of the lidars are different, they average over different volumes even when volume center is identical. So, they can be (slightly) different.

50. Fig. 11: Why not show this comparison for all available days? This should then much better fit to the (proposed) statistic section.

51. Sect. 5.1: While in the previous section mainly time intervals (e.g. 0500-0530 UTC) were given, the authors now use the center (?) times of the intervals (e.g. 0515 UTC). Please homogenize.

52. p. 20, l. 18: The information that there were radiosoundings and information on the launch time, etc should be given much earlier in the manuscript.

53. Fig. 12: Why do the authors not include the dissipation rates from the towers in these figures? Indicate in the caption what the squares and circles are.

54. p.22, l. 1: Where is the Lower Orange Site? Indicate in Fig. 1 and use uniform namings.

55. p. 22, l. 3: What time was sunrise? Are there temperature profiles available throughout the night? Was the whole valley filled by an inversion or was there a capping inversion near ridge height?

56. p. 22, l. 6: ".. at 235° (Fig. 8)."

57. p. 22, l. 14: I do not know what the authors mean by ring of large σ_t^2. Please indicate in

Fig. 15.

58. p. 22, l. 19-20: I am not sure I can follow this: The inversion is about 100 m above ridge height (from launches in the valley center). The high dissipation rates are confined to a layer below the ridge height. So how can they be trapped under the inversion?

59. Fig. 15: This figure is difficult to understand. It is hard to imagine where the slices are placed. It would be good to at a sketch here or in Fig. 1 to show where the slices are. Maybe a 3D plot would help as well, where the shown slices are placed in the 3D orography. Maybe the authors could even combine several slices from DLR #3 or even with the slices from # 1 and #2 to give a better impression of the 3D conditions.

60. Sect. 6.1: As this section mainly deals with turbulence measurements by Doppler lidar this should be specified in the title.

61. p. 24, l. 7-12: Isn't the variability within the averaging volume partly considered in σ_t?

62. p. 24, l. 13-15: The large uncertainties for small dissipation rates are likely related to the accuracy of the Doppler lidars.

63. p. 24, l. 21: "... was sufficient to capture..". How was this tested?

64. p. 24, l. 24-25: This is certainly a limitation for the method. Very often there is no option to calibrate with in situ measurements. Can the authors comment on how useful the method is, when no calibration can be performed?

65. p. 25, l. 10: How far downstream in terms of m are three rotor diameters?

66. p. 25, l. 25: Strictly speaking, the authors analyzed only on period in detail and attributed the disagreement to the wake. However, there are several more periods, when the dissipation rates from the different instrument differ.

67. p. 25, l. 29: "Within its limitations (Sect. 6.1)..."

68. p. 27: The list in Appendix A is not complete. For example, σ_I, σ_e, $\hat{\mathrm{L}}_v$ are missing. Please complete.

69. Fig. B1: Why did the authors use WRF simulation for the large scale conditions when a radiosounding was available? What does "location: tower 20 mean" in the plot?

References

Adler, B., Kiseleva, O., Kalthoff, N., and Wieser, A.: Comparison of Convective Boundary-layer Characteristics from Aircraft and Wind Lidar Observations, Journal of Atmospheric and Oceanic Technology, doi:10.1175/JTECH-D-18-0118.1, 2019.

Bonin, T. A., Choukulkar, A., Brewer, W. A., Sandberg, S. P., Weickmann, A. M., Pichugina, Y. L., Banta, R. M., Oncley, S. P., and Wolfe, D. E.: Evaluation of turbulence measurement techniques from a single Doppler lidar, 10, 3021–3039, 2017.

Maurer, V., Kalthoff, N., Wieser, A., Kohler, M., Mauder, M., and Gantner, L.: Observed spatiotemporal variability of boundary-layer turbulence over flat, heterogeneous terrain, Atmos. Chem. Phys., 16, 1377–1400, 2016.

Smalikho, I., Köpp, F., and Rahm, S.: Measurement of atmospheric turbulence by 2-μ m Doppler lidar, J. Atmos. Oceanic Technol., 22, 1733–1747, 2005.