Round 2: Review of 'Estimation of the height of turbulent mixing layer from data of Doppler lidar measurements using conical scanning by a probe beam' by Banakh et al.

December 22, 2020

The revised version of the manuscript is much improved and the authors considered many of the reviewers' comments. I particularly appreciate that the authors now use more data for their analysis. I still have some comments and suggestions which should be considered, before I can recommend the manuscript to be accepted for publication.

1 Specific comments

1. Abstract: I suggest giving the number of days on which the results are based in the abstract. It should be made clear that the comparison of the mixing layer height with the Richardson number estimates are from another measurement campaign.
2. p. 1, l.21-22: Stronger turbulence does not necessarily always lead to a deeper mixing layer height. Other factors such as the inversion strength and vertical motion at the top of the ABL impact the mixing layer height as well (see e.g. Eq. 6.13 in Garratt (1994)). The statement needs to be rephrased.
3. p. 2, l. 5: Specify what r, u, v, w are.
4. p. 2, l. 20: 'MLH h_{mix} is double. One abbreviation is enough. Also see my comment about the usage of abbreviations from round 1.
5. p. 2, l. 30-31: Please specify in the manuscript what is meant by 'sufficiently high SNR'. In the response to my comment from the first review round, the authors explained it nicely: 'We assume that the SNR is sufficiently high if the relative error in estimating the dissipation rate does not exceed 30%.' This statement needs to be included in the manuscript.
6. p. 3, l. 10-12: Like in the abstract, it needs to be made clear that the comparison is from another experiment.
7. p. 4, l. 9: Wind by itself is not a process. The wind field may be stationary.
8. p. 4, l. 20: Add that D_l is a function of R_k.
9. p. 5, l. 12-18: The threshold for radial velocity should be given here as well and the reasoning why this thresholds are chosen should be included in the manuscript, like stated in the authors' response to the reviewers' comments in round 1: 'For estimation of the MLH from the dissipation rate profiles we the

threshold equal to 10-4 m2/s3. In the same time for estimation of the MLH from the radial velocity variance profiles we the threshold equal to 0.1 m2/s2. According to the calculation using Eq.(1) in the paper by Banakh and Smalikho (2019), at such threshold values ($\epsilon = 10^{-4}$ m^2/s^3 and $\overline{\sigma_r}^2 = 0.1$ m^2/s^2), the integral scale of turbulence L_V is approximately 200 m in the case of lidar measurement at elevation angle of 60°. Such L_V is quite consistent with the results of our measurements in the daytime at heights of 200 - 600 m. Therefore, we used this threshold (0.1 ^{2}/s^2) for the radial velocity variance.'

10. p. 6, l. 9ff: Refer to Fig. 5 here, as it gives an overview of the atmospheric conditions.
11. p. 6, l. 16-17: 'Estimates of wind turbulence parameters from the data obtained at this SNR [-15 dB] have a relative error exceeding 30%'. Here the authors state that -15 dB are enough. In the abstract and other places in the manuscript they state -16 dB. This needs to be consistent.
12. p. 6, l. 18-20: Give some examples when cloud or fog were present, e.g. morning of July 21, July 22 and July 25.
13. p. 6, l. 25: The explanation what $\Delta\tau$ means is given in the authors' response in round 1. For clarification, this information needs to be included in the manuscript as well.
14. p. 7, l. 14-17: It is confusing to have the same color for missing data and for data outside the colorbar range. Please change.
15. p. 7, l. 27-28: 'The minimum height ...' contradicts with the statement on p. 8, l. 1-2. Also, if MLH at the minimum is not longer given in the plots, this sentence needs to be removed here.
16. p. 8, l. 13ff: Add MLH to time-height sections of Fig. 5, to allow for an easier comparison of conditions and detected MLH. E.g. when describing the MLH minimum at 15 h on July 21.
17. p. 8, l. 16: Although the issue of the accuracy of estimating the dissipation rate in the presence of clouds is not specifically considered in the manuscript, it needs at least to be mentioned that clouds may impact the mixing layer height estimates and the heights strongly depend on how far the lidar beam penetrates into the cloud.
18. p. 8, l. 18: 'changes in the wind with height': What changes? Wind direction, wind speed or both? 'which is apparently the reason': What do the authors mean by that? Is a different air mass advected? Do these changes lead to a decay of turbulence? Maybe rephrase to 'changes in wind direction and speed coincide with a minimum in MLH' or similar.
19. p. 8. l, 19: What is the reason for the low SNR in all layers between noon on July 22 and noon July 24? Is this caused by lidar settings (jump in SNR below 500 m on July 22) or is it physical? In either case, please explain this very prominent feature.
20. p. 8, l. 22, Fig. 6: I recommend to put all six days in one plot? This would make it much easier to compare (like e.g. p. 8, l. 30).
21. p. 8, l. 23: It looks like the highest values on July 20 and 23 occur when the upper most measurement level is taken as mixing layer height. This should be mentioned.
22. p. 8, l. 28-29: The explanation about the difference between probing volume and range gate length given in the authors' response in round 1 should be included in the manuscript.
23. p. 9, l. 1-2: I understand that there are no radiosoundings for verification of the method. However, the agreement between cloud base and mixing layer height is not enough of a justification that the method is correct. The agreement strongly suggests that the method works (in the presented cases),

but it is no proof. The ABL may be deeper. Please rephrase the statement 'The agreement between cloud base and mixing layer height confirms the correctness of the MLH time series assessment.'

24. p. 9, l. 3: It is not clear what is meant by that? Not contradict in what way? Please clarify. How do they compare?

25. p. 10, l. 2: Here and at other places in the manuscript, mix between present and past tense.

26. p. 10, l. 16-17: The purpose of this closed numerical experiments as given in the authors' response in round 1 should be included here.

27. p. 10, l. 25: What is meant by 'atmospheric experiment'? Data from the measurements during the forest fires? Please clarify.

28. p. 13, l. 15, Fig. 11: White curves in Fig. 11 are hard to see. Please change color. I suggest extending the color scale for the gradient Richardson number to negative values, to allow to distinguish between dynamically and statically unstable conditions.

29. p. 13, l. 23-24: The MLH obtained from the gradient Richardson number using the objective threshold method needs to be included in Fig. 11. At the moment it is not clear where this MLH is located and how it relates to MLH from the dissipation rate.

30. p. 13, l. 25ff: The description of MLH April on 10 is unnecessarily detailed. The authors describe the relatives deviations of MLH for different days. This is the first time the talk about relative deviations. I assume the mean the error in the mixing layer estimates σ_h? The terminology needs to be consistent. The information on MLH uncertainty described in the text is not at all visible in Fig. 11. I highly recommend including the uncertainty in the plots. At the moment it is not clear which periods suffer from a higher uncertainty and which not and it would help to interpret the comparison between MLH from dissipation rate and gradient Richardson number.

31. p. 14 l. 6-7: As MLH from gradient Richardson number is not indicated in Fig. 11, it is not clear how the 22% differences are calculated. In line 25, the authors state relative deviations not exceeding 25%. Which value is correct? Please clarify.

32. p. 14, l. 11ff: Strong wind alone does not lead to low gradient Richardson numbers. It is necessary to have strong wind shear. I cannot follow the examples given by the authors. Between 0 and 6 h on April 10, I see l low gradient Richardson number in the layer between around 250-700 m. Are these the layers with significant shear? Was there a low-level jet? How does that relate to the wind profiles? Same on May 1, I don't see how high wind speed between 150 and 650 m and strong shear between 75 and 200 m links to the observed gradient Richardson number distributions with high values between 200 and 400 m.

33. p. 14, l. 23-24: Add information that data from a second experiment are also used.

34. p. p.14, l. 32: Why does MLH depend on wind? Please clarify.

35. Summary: In their response in round 1, the authors state 'The method described here can be applied to data measured by a high-power pulsed coherent Doppler lidar under normal conditions (with a background aerosol), which will enable a full-fledged statistical analysis.' This is a very valuable information for the reader and I highly recommend including this in the summary or introduction.

36. Fig. 1: Change 'velocity' to 'speed' in label of (f).

37. Fig. 2: I can see differences between both plots, even with this color scale (one color per order of magnitude). Differences might be even be more visible if a finer color scale (like for radial velocity variance in Fig. 3) was used. A difference plot would help to see differences between dissipation rate and radial velocity variance from both elevation angles more clearly and support the statement that the differences for radial velocity variance are larger.

38. Fig. 4: Like in Fig. 6 and 10, the results obtained when the specified threshold exceeds the dissipation rate at an height of 60 m should not be shown.

39. Fig. 5: Change 'wind velocity' to 'wind speed'.

40. Fig. 9: Make clear in the caption that these are 4 examples of random realizations.

References

Garratt, J. R.: The atmospheric boundary layer, Cambridge atmospheric and space science series, Cambridge University Press, 316 pp, Cambridge, 1994.