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.

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The study proposes a method for the determination of the mixing layer height based on profiles of dissipation rate estimated from scanning Doppler lidar measurements. The authors analyze several days from a period with smog related to forest fires. They give estimates of the uncertainty of the method and find that the relative error of the mixing layer height is less than 20 % when the signal-to-noise ratio is high enough and turbulence is sufficiently strong. Finally, the authors compare the mixing layer height to the Gradient Richardson number determined from Doppler lidar and microwave radiometer.

The determination of mixing layer height from dissipation rate itself is not new and has been published in other studies (e.g. Vakkari et al., 2015; Manninen et al., 2018), but the method described here is based on an estimation of dissipation rate developed by the same authors which has not been used to estimate mixing layer heights so far. In addition with the error estimate of mixing layer height this makes an interesting study. However, I have a major concern about the representativity and significance of the conclusions the authors draw. The conclusions are based on very few days only (4 days for the mixing layer height estimate, 2 days for the error estimate and 1 day for the comparison with temperature profiles). The authors mention that data are available for 10 days during a period with forest fires in 2019 and for nearly one month in spring 2020 and I wonder why they do not include data from all available days in their analysis. In my opinion, the results would be much more significant and relevant for the community if they were based on longer time periods and obtained in a objective and statistical way. The statement ”It is shown that for the estimation of the mixing layer height (MLH) with the acceptable relative error not exceeding 20 %, the 10 signal-to-noise ratio should be no less than -16 dB, when the relative error of lidar estimation of the dissipation rate does not exceed 30%.” is based on manual and subjective analysis of the individual days—at least this is the way it is presented in the manuscript. Besides this major concern, I have several other comments given below.

Based on my evaluation, I cannot recommend the manuscript for publication in AMT in its present state. However, I believe that the study could be suitable for publication, if the authors provided a major revision in which they base their conclusions on an objective and statistical analysis including all available days.

1 General comments

1. This comment relates to my major concern described above. Instead of basing the conclusions on error estimates and comparison with temperature profiles on manual evaluation of individual days, the authors should perform an objective and statistical analysis of all available days. This would e.g. make

their recommendation about what SNR values should be used to obtain reliable mixing layer heights estimate more convincing. Instead of showing timeseries and time height sections for individual days (which makes the number of figures unnecessarily long in my opinion) they could show scatter plots, e.g. relate SNR to the error of ϵ and calculate some statistical measures.

2. Sections 4 and 5 are quite long and confusing. It would be good to include some subsections, e.g. to distinguish the description of the method from the results in Sect. 5.

3. I recommend that the manuscript gets checked by a native speaker before publication.

2 Specific comments

1. p. 1, l. 18: "moisture, small gas constituents, pollutants, and heat"

2. p. 1 l. 20-21: The definition of the ABL is fundamental knowledge and one of the classical textbooks such as Stull (1988) or Garratt (1994) should be cited for that.

3. p. 1, l.24: Mixing layer height and ABL should be put into context.

4. p. 1, l. 28: and dissipation rate

5. p. 2, l. 6-7: The recent study of Manninen et al. (2018) should be mentioned here as well.

6. p. 2, l. 7-8: What is meant by vertical scanning? What is the difference compared to vertical stare mode and conical scanning?

7. p. 2, l. 17: Here and throughout the manuscript: The authors define abbreviatons e.g. h_{mix} or ϵ, but do not use them consistently throughout the manuscript. Instead the sometimes use the long name or both. Once an abbreviation is introduced it should be used consistently.

8. p. 3, l.3ff: The motivation and objectives of the study should be made clearer. What is new compared to previous work? How are the objectives addressed? What data are used?

9. p. 3, l. 23: Not clear what the scan number is. What is a scan? Full azimuth scan of 360 degree at a certain elevation angle?

10. p. 4, l. 12: Isn't D_L still a function of R_k?

11. p. 4, l. 21: What is y in $A(y)$?

12. p. 4, l. 22ff: Thus, $\overline{\sigma}_r^2$ depends on ϵ. How does that effect the comparison of mixing layer height determined from both quantities?

13. p. 5, l. 12-13: How strongly does this threshold vary in literature? Did the authors investigate how sensitive their results to the chosen threshold are?

14. p. 5, l. 16ff: Why did the authors choose the period during wild fires for the analysis? What impact on the ABL conditions and their method do they expect? What are the site characteristics, i.e. terrain, surface conditions, ...? Are there other instruments deployed simultaneously? Later they mention surface flux measurements.

15. p. 5, l. 22: Why are the accumulation numbers used for the different elevation angles different?

16. p. 6, l. 5ff: How is the threshold of -15 dB for SNR determined? How is the relative error of 30 % for turbulence parameters determined? How do clouds and fog affect the measurements in clear parts of the atmosphere? Please explain. Given that the whole time period encompasses 10 days only, it could be interesting to show 10-day time height sections of SNR and wind for the whole period to get an overview and information on the variety of atmospheric conditions.

17. p. 6, l. 12: What is meant by 'the elevation angle was alternated for $\Delta\tau \approx 1.5s$? Until here, I was assuming that consecutive full azimuth scans were done one after the other at each elevation angle.

18. p. 6, l. 23: An objective comparison e.g. by calculating RMSE and correlation coefficient, would be more meaningful than stating that there are practically no differences.

19. p. 7, l. 2-3: 'sufficiently high signal-to-noise ratio': how is that determined? What is the criterion to distinguish between sufficiently high and low SNR?

20. p. 7, l. 4ff: Like above an objective comparison by calculating RMSE and correlation coefficient would be more meaningful. Also, the comparison should be done for the whole period to make the result more meaningful.

21. p. 7, l. 14-15: This is interesting. Do the authors have any hypothesis why TKE was similar while dissipation rate decreased with height?

22. p. 7, l. 17-18, Fig. 5: The profiles plot do not really show any new information compared to the time height sections. What is the purpose of including them?

23. p. 7, l. 22: Where does the threshold used for the radial velocity variance profiles come from? Is that based on other literature? Is it empirical?

24. p. 7, l. 25: In case the values of σ_r or ϵ are smaller than their corresponding threshold, the mixing layer height is set to 60 m. Why not set it too missing? It is well possible that no mixing layer exists at all.

25. p. 7, l. 28: similar like to the comment about the sufficiently high SNR above. Which objective criteria is used to determine low quality data?

26. p. 8, l. 3-4: An objective comparison between mixing layer height estimates for the different elevation angles could easily be done for all available days.

27. p. 8, l. 8ff: What is the purpose of showing all these examples with plots of dissipation rate and SNR profiles? Like outlined in my major comment, an objective and statistical comparison would make the results more meaningful. The example plots for the individual days could e.g. be put in a supplemental or appendix.

28. p. 8, l. 17-18: Can this change in SNR be related to a change in wind direction which could explain the enhanced advection of smog?

29. p. 8, l. 19-21: How reliable is ϵ calculated in the cloud layer? The mixing layer height is determined at the top of the layer with high SNR, i.e. somewhere in the lower part of the cloud. Depending on the cloud characteristics mixing may reach up much further. Thus, the estimated height does not necessarily agree with the true mixing layer height but is simply an affect of how deep the lidar beam penetrates into the cloud.

30. p. 9, l. 14: Please explain the difference between probing volume (30 m) and range gate length (18 m).

31. p. 9, l. 17-18: The fact that cloud base and the detected mixing layer coincide does not confirm the correctness of the method. As seen in the examples, mixing layer heights are detected at the top of the layer with maximum SNR, i.e. in the lower part of the cloud. Mixing layer heights may be deeper. The correctness of the method can only be confirmed by comparing it to independent reliable measurements, such as radiosonde profiles.

32. p. 9, l. 24: It would make it easier to understand how the relative error of ϵ is calculated, if the equation was given.

33. p. 9, l. 26: Why only look at the relative error at mixing layer height? What is the time height section of this error?

34. p. 9, l. 27ff, Fig. 11: In my opinion, Fig. 11 is not ideal and the explanation why the error is high or low could be much easier to follow if a scatter plot between the error and SNR was shown.

35. p. 10, l. 21ff: What is the purpose of the series of closed numerical experiments? It is not clear how they are done. What are the preset values of the mixing layer height?

36. p. 11, l. 1ff: What is the experimental error? On what assumption is the threshold of 30 % for the relative error based?

37. p. 11, l. 14ff: While reading this paragraph I was wondering how the calculations are done. This information is given in the following paragraph and I suggest changing the order.

38. p. 11, l. 25: What do the random realizations of ϵ look like? How much do they differ from the original profiles? It could be interesting to show some profiles.

39. p. 12, l. 4ff: the error for mixing layer height obtained with the described method is very small. A discussion of other uncertainties related to the mixing layer height, e.g. the sensitivity to the used threshold, should be included. To really assess how correct the determined mixing layer heights are, comparison with independent measurements such as radiosoundings would be necessary. It would be interesting to see if the mixing layer height determined from ϵ would agree with mixing layer heights determined from other instruments (Emeis et al., 2008) within the uncertainty range.

40. p. 12, l. 22: The Richardson number describes if turbulence can develop in a stably stratified atmosphere (e.g. Stull, 1988). The static stability is described by the temperature gradient.

41. p. 13, l. 4ff: It should be mentioned in the beginning that an additional data set from a period in 2020 is used. Also, if data from a microwave radiometer are used much more information on this instrument needs to be given. The temperature profile retrieved from the passive instrument are prone to uncertainties and errors and information on its accuracy and the used retrievals should be given. Microwave radiometers often struggle to resolve elevated inversion at the top of the ABL and thus the gradient Richardson number obtained from these instruments have to be used with care and it should not be taken as granted that the correctly detect the inversion at the top of the ABL and that they can be used to validate the mixing layer height detected from ϵ.

42. p. 13, l. 22ff: Like above, a statistical objective analysis of the whole period should be conducted and the conclusion that the mixing layer height derived from ϵ agrees well with the gradient Richardson number using a threshold of 0.5 should be based on the whole data set and not just on a single example day.

43. p. 14, l. 5: A mixing layer height of several hundred meters during the night must be shear driven. A discussion of the physical processes causing the mixing layer is missing and should be added to the pure description of the profiles.

44. p. 14, l. 20: Second period in 2020 should be mentioned.

45. p. 15, l. 13-14: It is not clear to me where the vertical gradient of ϵ is considered in the error estimate.

46. p. 15, l. 14: The result 'SNR should be no less than -16 dB' is not clear to me. On what analysis is that based?

47. p. 15, l. 16ff: A good way of showing this could be by plotting the error of the mixing layer height over ϵ.

References

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