

Atmos. Meas. Tech. Discuss., referee comment RC1  
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## Comment on amt-2022-73

Anonymous Referee #1

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Referee comment on "Behavior and mechanisms of Doppler wind lidar error in varying stability regimes" by Rachel Robey and Julie K. Lundquist, Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-73-RC1>, 2022

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### General comments:

This is an interesting study that tackles a fundamental problem for the Doppler wind lidar observations which is the better understanding of the measurements' error. The authors used large-eddy simulations for different atmospheric stability conditions and analyzed which mechanisms drive the errors under each condition. This study can be very useful for researchers studying turbulence as observed by a Doppler wind lidar. It could help them understand possible error biases or the cause of large errors. It also shows the importance of multiple scans so that the observations can be averaged over time to decrease the error. I believe this study is worth publishing. Nevertheless, the authors should make several changes in the manuscript in order to improve its structure. In the current state, it is difficult to read through. Introduction, methodology, results and conclusions are mixed, the authors should set apart these chapters to improve the flow of the manuscript. In the current version, parts of the introduction are presented in the methodology, whereas parts of the methodology are introduced in the results and some results are presented but discussed in a later section. In the conclusions, a summary of the work with the main points of the results should be presented, however the authors make a comparison of their results with previous studies and furthermore they introduce new references. Regarding the abstract, the results should be supported by numbers so it will be evident that these are the results of the current study. Some terms should be explained better throughout the study, so it is clear to the reader what is the magnitude of terms such as the "strong winds" or the "large structures". The figures and their captions need corrections as well. All the important information should be included in the plot with the description written in the caption, rather than important information, such as the colour of the lines, only explained in the caption. The language in the manuscript is fluent. Please find my comments below.

### Scientific comments:

Page 2 Line 27-29: The authors mention that lidar data offer an indirect representation of

the flow field. Although it is true that the wind lidar observations most likely need extra steps to extract the wind components compared to sonic anemometers, it is possible to directly observe the wind components if the beams are aligned with the wind direction e.g. horizontal beams with no elevation angle alongside the wind direction or vertical beams for the vertical wind.

P2 L45-47: Similarly to my previous comment. In the phrase "Questions about measurements of vertical velocities"; do the authors mean vertical velocities variances? Because vertical velocity can be directly measured by the Doppler wind lidar and in fact even the vertical velocity variance is the "easiest" turbulence parameter that can be estimated using wind lidar observations, see Bonin et al. 2016: "Improvement of vertical velocity statistics measured by a Doppler lidar through comparison with sonic anemometer observations".

P2-3 L59-62: The sentence "Compared to field studies of instrument accuracy, studies with virtual instruments in LES ...." should be supported by some relevant references for such studies.

P3 L67: The PALM model was developed by Raasch and Schroter, 2001: "A large-eddy simulation model performing on massively parallel computers". The reference should be added in this sentence.

P4 L78-79: The Skamarock, 2008: "A description of the Advanced Research WRF version" reference should be included in this sentence.

P4 L82: The Chow et al., 2005 is not relevant here as they do not mention the WRF model in their study.

P10-11 L248-270: The authors state that they selected cases with "strong" and "weak" convective boundary layers. In line 262, these cases are characterized well-mixed layers. However, in Figure Table 1 we can see that the for the weak cbl the abl height is 525 m. This value seems to correspond to a developing boundary layer and not a well mixed. The authors should comment on that and whether these values can occur only in an ideal case of the simulation with a flat, homogeneous terrain etc. Moreover, the vertical range of the Windcube is portrayed in Figure 3 but it is not mentioned in the text and even in Table 2 it is not directly shown. This information should be included along with an explanation of this selection. The value is higher than the ABL height under stable conditions (170 m). Wouldn't this affect the comparison? See also my comment regarding Figure 4.

P11 L266-267: The authors mention the limitation of the lidar range to include the entrainment zone. For the instrument, it is practically difficult to measure at this height due to the scarcity of aerosol in the zone. Do the authors refer to the instrument or the

simulations? What are the limitations for a simulation?

P14: In Figure 4 panel (c), for the lower altitudes the median of the curve is not at zero as it also stated by the authors in P15 L356. The authors should explain at this point of the manuscript why this occur only at these particular levels of the SBL. It is also apparent from panel (c) that for the levels above the ABL height (170 m) the distribution becomes similar to the one near the surface. Any comments regarding this? Was this something the authors expected?

P15 L355: What could be the cause for overestimation during convective conditions?

P24 L506-507: The terms strong winds and strong shear are vague. Can you quantify these parameters? Is the underestimation expected above a specific threshold? Additionally, the argument that surface shear is one of the cause of the underestimation should be supported by some results in the form of Figures. Maybe the authors could add a secondary y axis with the wind speed and wind shear values at the different given heights in Figures 9, 10, 11. The estimation of the wind shear can be tricky but this claim should be supported by results.

P26 L540 & 548-550: Similarly to my previous comment, the term "large coherent structures" is vague. It is evident from Figure 12a that there are structures, upward motions followed by downward motions, of approximately 1 km size. On the contrary, for the stable boundary layer (Figure 12b) the size of the structures seems to be equal to few hundred meters. The lifecycle of such structures should be different. Do the authors categorize both structures' sizes as large? The authors also mention larger scale structures above the boundary layer in the SBL. Do they mean from 170 m up to 350 m that is shown in Figure 12b or in higher altitudes? Either way it should be clear to the reader what are the sizes of the structures and these should correspond to the figures presented in the manuscript.

Technical comments:

P1 L1: Lidars instead of lidar.

P3: The full name for the abbreviation DBS should not be included in the caption of Figure 1, but rather in P4 L76.

In the first three instances Figure 1 is written with a capital F in the text, whereas in all the other instances figures in the text are written in parenthesis with a minor f. The citation of the figures should be consistent throughout the text.

P3 L72: The full name for the abbreviation SOWFA should be given here.

P3 L73: The full name for the abbreviation WRF-LES should be given here.

P7 L156: The parameter "c" is defined after the equation 7, although it is also part of the equations 5 and 6. In order to avoid confusion, I suggest to move the definition before or after the equation 5.

P7 L161: The sentence "Using parameters .... can be made concrete" needs rephrasing. It is confusing in its' current state.

P7 L167-170: The paragraph "The form of the pulsed lidar .... further distances by a pulsed lidar" is more suitable for the introductory section. The comparison of the RWF between pulsed and continuous lidar seems out of place in the methodology as only the pulsed lidar was used for this study.

P7 L172: Remove the word "found".

P7 L173: The "Spe" in the parenthesis is a missing reference?

P8 L185-188: The paragraph "Interpolation dominates ... subsequent developments" comments on the interpolation method and possible improvements in the data and methodology section. This paragraph is more suitable for a section like Conclusions/Discussion.

P8 L190-199: In the first paragraph of Section 2.1.2 the authors provide some general information regarding the lidar and the different scanning methods such as RHI and PPI. As these methods are not used in the particular study, they should not be mentioned in the data and method section. In my opinion, this paragraph should be removed entirely from the manuscript as it does not provide any valuable information for the study.

P10: In Figure 3 the explanation for the different lines (solid, dashed etc) representing the parameters is only included in the captions and not in the figures, hence it is not practical for the reader to study these figures, similarly for Figures 5, 6 etc.

P13 L303: The claim that the components of equation 10 are commonly used should be supported by some examples-references.

P13 L305-308: A figure showcasing the sign convention could be useful for the reader.

P14: The authors have introduced several parameters for the wind speed such as  $u_{\text{lidar}}$  or horizontal wind speed, therefore it should be clear in Figure 4 which one is shown including its' name as defined by the authors.

P14 L324: Remove "the".

P15-P16: The caption of the Figures 5 and 6 mention dashed and dotted lines but only dashed and solid lines are depicted. The figures should be corrected and additionally there should be a legend in the figure with this information.

P15: Figures should be easily readable even when separated from the rest of the text. In Figure 8, the caption is linked to the parameters of Figure 5 which should be corrected. The parameters should be included in the caption and the legend of Figure 8 independently from Figure 5.

P17 L374: Converge instead of converges.

P18 L401: The phrase "their respective height trends are similar to the previous section" should be accompanied by the respective values as a reminder for the reader.

P19-P33: In Chapter 4 the authors introduce several new equations. In my opinion, a manuscript flows better when all the equations and tools are presented in the data and methods chapter and subsequently the results are presented and discussed. So instead of presenting the figures in Chapter 3 and then using the equations to describe the results in Chapter 4, I believe it would be better if all the equations are already presented in Chapter 2 and the discussion of the results is moved to the corresponding figures. For example the explanation of the underestimation of the wind error although mentioned in Chapter 3 is explained much later in Chapter 4. By moving the equations to Chapter 2, the authors will also avoid repeating themselves.

P20: The caption of Figure 9 seems more like a part of the manuscript than a caption. It should be rephrased in a way to resemble a caption.

P21-22: It would be easier to interpret the results from Figures 9, 10, 11 if these figure were merged in multiple panels and thus it would be possible to use the same caption for all instead of linking to the caption of Figure 9 which makes the Figures unreadable independently.

P29 L616: The word "term" is used two times.

P30: The height of the wind speed is not mentioned in Figure 14. It should be included as part of the xaxis title.

P32: The legend showcasing the parameters that correspond to the colour lines is missing in Figure 16.

P32: The panels in Figure 16 are not numbered with (a), (b), (c) etc and the authors refer to the different plots as left and right panels. I believe it will be easier to use the numbering. Similarly the Figures 5, 6, 8, 9, 10, 11, 13, 17, 18 and the ones presented in Appendices A, D, E also miss the numbering.

P36-39: The comparison of the authors' results with previous studies should be included in the sections of the results and not in the conclusions. The authors should summarize the key points of their study and not include new information in this chapter. Although it is possible to include some previously mentioned references in the conclusions, it is not recommended to introduce new references such as Klaas and Emeis, 2021 and Teschke and Lehmann 2017. These references should be introduced earlier in the manuscript.

P38 L840: Add the word of - "The form of our error...."

P52 L1117: Remove typo "&thinsp;".

P52 L1119: Remove typo "&ndash".