Reply on RC2
Charlotte Rahlves et al.

Author comment on "Scan strategies for wind profiling with Doppler lidar - An LES-based evaluation" by Charlotte Rahlves et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-417-AC2, 2022

Dear Reviewer,

First of all, we thank the reviewer very much for agreeing to act as a reviewer and for investing their time to review this manuscript. The reviewer’s constructive critique is much appreciated and their comments are very valuable to us. Some excellent points have been raised that we did consider in the revision process. You also made some interesting and valuable suggestions for further analysis of our LES results and for additional studies that might be based on our results. We sincerely hope that you may accept that we did not follow all these suggestions within the frame of the present manuscript. We feel that our text already contains quite a number of relevant results and messages for the reader and simply do not want to overload it too much. Please find our detailed responses on your comments below.

Response to specific comments and questions:

The uncertainty for each retrieval configuration is given as a single value for the whole profile. How does the uncertainty vary with altitude, and particularly with scanning angle? The vertical profile of the uncertainty arising from the impact of turbulence is of clear interest, especially if there is a relationship between altitude and the size of the coherent turbulent structures being generated. Figure 2 shows that the larger scanning circles may match or exceed the coherent turbulent structure scales by 500 m altitude, but not at low altitudes (for this example). Figures 3 and 5 also suggest that the impact may vary with altitude substantially, particularly with respect to the zenith angle used. Since the profiles have already been generated, including such results (and adding a figure or two) would greatly enhance the impact of these results.

Quantifying any vertical variation in the uncertainty is important because a major goal of the wind profiling community is to provide an estimate of the overall retrieval uncertainty (i.e. combine these uncertainties with the instrument measurement uncertainties, which are likely to vary with range).
Variation of the retrieval error with altitude: The reviewer made a very good point here, it is absolutely true that knowledge of the error behavior with respect to altitude is essential for operational use and we agree that investigating this is the next step to take. We did actually start investigating the variation of the retrieval error with altitude. We did, however, come to the conclusion that this is an aspect that needs more thorough analysis, which we believe should be done on the basis of comprehensive ensemble simulations. We also believe that the manuscript, as it is, is already quite long and we feel that including a comprehensive analysis of the retrieval error variation with altitude would overboard this study. However, we decided to add a first exemplary analysis on this aspect as an appendix.

This study generates the averaged wind profiles by averaging the radial measurements first from all scans within the averaging period, before the wind retrieval is performed. What is the impact if the wind retrieval is performed per scan, and the retrieved wind profiles then averaged (in vector form)? Does this make a difference to the conclusions? This may be important in operational situations where rapid update is requested (e.g. a wind retrieval is provided within five minutes for aviation forecasters or for data assimilation); waiting for 30 minutes may not be an option for these users.

Averaging procedure: This is two questions in one: (1) Changing the sequence of averaging vs. wind retrieval and (2) rapid output of mean values.

(1) For the averaging procedure, we followed the procedure most commonly applied in operational use.

(2) The averaging procedure allows for different averaging intervals to be taken into account simultaneously. The lidar completes multiple revolutions. The number of revolutions that are used for averaging depends on the desired averaging time. Therefore, a 10-min average can be obtained, while later the 30-min average can be obtained, as well.

The difference in performance between the 6-beam VAD and 24-beam VAD is a surprising result, although the authors do provide a reasonable suggestion for why this is the case. As clearly stated in the manuscript, this study was only intended to quantify the impact of turbulence on wind retrievals and not to attempt to create a fully-featured Doppler lidar simulator. However, all retrievals presented here implicitly assume that the radial velocity measurement has no instrument measurement uncertainty. What would be the impact on the retrievals if a random uncertainty with a standard deviation of 0.1 m s\(^{-1}\) (for example) was added to each radial velocity?

Instrument measurement uncertainty: As the reviewer said and acknowledged, it was not the aim to build a fully-featured Doppler-lidar simulator or to provide a fully comprehensive error analysis. We explicitly focused on the scanning strategies and the measurement uncertainty arising due to the violation of the homogeneity assumption. To add a measurement uncertainty would go beyond the scope of the study and overload the manuscript.

Response to technical comments:

Line 2: Suggest using ‘Doppler lidar scanning’ rather than ‘Lidar scan’.
Adapted.

*Line 5:* Suggest 'virtual Doppler lidar measurements' in place of 'virtual measurements'.
Adapted.

*Line 14:* Suggest 'measurable impact' instead of 'relevant effect'.
Adapted.

*Line 16:* Suggest 'monitoring air quality' instead of 'air quality control'.
Adapted.

*Lines 21-22:* Do you mean 'Long-range scanning Doppler lidar'? Not all Doppler lidars have this range. Is vertical resolution an important part of this statement? If so, give a typical resolution. Do you mean 'nearest 100 m in range' as the minimum height depends on both the minimum range and the choice of elevation angle.

We have modified the text as follows: “Profiling lidars can cover almost the entire vertical extent of the ABL with a reasonably high vertical resolution, except for the lowest 50 m to 100 m, depending on the scan elevation angle.” (now line 24-25 in the revised manuscript)

*Line 26:* Replace 'to expected' with 'to be expected'.
Corrected.

*Line 36:* Do you mean the scales of turbulent motion relevant for Doppler lidar? This may be true for DNS but for LES this would depend on the both the temporal and spatial configuration.

Yes, we mean the turbulent motion relevant with respect to Doppler-lidar. Of course this depends on the temporal and spatial separation, but here we say that in principle LES can do this job and we think that our set-up (with \( dx = 5 \) m and \( dt = 0.2 \) s) matches these goals.

*Line 40:* Replace 'like' with 'as'.
Corrected.
Line 41: Do you mean one scan sequence - i.e one VAD scan at one elevation?

In recent studies usually only one specific zenith angle configuration and/or one scanning scheme (VAD or DBS) has been investigated.

Line 44: Replace 'Those' with 'These'. Be clear here that you do not include the radial measurement (instrument) error.

Corrected.

Line 49: Replace 'such features as cellular structures, streaks or roll convection is' with 'features such as cellular structures, streaks or roll convection are'.

Corrected.

Line 64: Replace 'reference' with 'the reference profile in the LES simulation'.

Adapted.

Line 67: Replace 'confidence of' with 'confidence in'.

Corrected.

Lines 85-86: Suggest stating 'the vertical profile of the horizontal wind' rather than 'vertical wind profiles' here and elsewhere in the manuscript (e.g line 88).

Since it is not only the vertical profile of the horizontal wind, but rather the vertical profile of the horizontal, as well as, the vertical wind, we decided to use “the vertical profile of the three-dimensional wind vector” instead.

Line 93: Is it strictly necessary for the beam to rotate clockwise? Can state that the beam is then rotated in azimuth.

Adapted.

Line 103: Italic 'n'.

Corrected.

Line 114-115: Is this always the case? Could sample N-E-S-W for example (less time
spent scanning), or sample all 4 simultaneously. Also note that this describes only one version of the DBS scan, there are also 3-beam and five-beam variants.

Yes, the reviewer is right, this is one possible realization of a classical DBS scan and others with 3 or 5 beams are frequently used as well. We did add a note of this to the manuscript. And yes, N-E-S-W would need less time for a scan, however, to closer meet the stationarity assumptions inherently made in eq. (5)-(7) we decided to probe the opposite azimuth directions one after the other. Sampling the four beam simultaneously would not apply for a commercial lidar.

Line 122: Should u, v, w, denote the wind vectors rather than the components?

We changed this to “the three-dimensional wind vector”.

Line 183: Replace ‘truth’ with ‘reference’.

Adapted.

Line 196: Replace ‘buoyancy to shear driven’ with ‘buoyancy-driven to shear-driven’.

Corrected.

Line 199: Replace ‘literature’ with ‘the literature’.

Corrected.

Line 237: Replace ‘until a turbulent flow’ with ‘until turbulent flow’.

Corrected.

Line 240: Replace ‘domain averaged resolved-scale turbulence’ with ‘domain-averaged resolved-scale turbulent’.

Corrected.

Line 484: Only out of the zenith angles studied in the manuscript!

We changed the respective passage to: “The results of this study suggest that out of the zenith angle configurations investigated in this study, lidar configurations using a zenith angle of 54.7° yield most accurate results for measurements of horizontal wind speed in turbulent flow conditions.” (now line 499 ff in the revised manuscript)
