

Review egusphere-2026-4872:

The article *The NASA Aerosol Wind Profiler Doppler Wind Lidar, Part I: Instrument Description and Airborne Demonstration* by Kristopher Bedka et al. comprehensively presents the latest developments in coherent Doppler wind lidar at NASA Langley Research Center, which led to the development of the Aerosol Wind Profiler (AWP) lidar. The paper provides a detailed description of the instrument, including the laser modules, electronics, alignment, and mechanical systems.

It also provides valuable insight into the data-processing chain and the lidar configuration used to reconstruct 2D and 3D wind fields from an aircraft. The discussion of how different parameters can be adjusted to obtain higher-resolution or less noisy measurements depending on atmospheric conditions is particularly useful.

The analysis of a flight conducted in October 2024 is very interesting. It illustrates the type of measurements provided by the lidar while also showing observations of several atmospheric phenomena, including the jet stream, tropopause variations, and gravity waves. The lidar results are compared with independent measurements, including dropsonde profiling systems and satellite water-vapor brightness imagery, with very good agreement. Comparisons with atmospheric model results are also presented.

Overall, I found the article very complete and informative. It provides useful context, detailed technical information, and representative measurements from an airborne coherent Doppler wind lidar. I believe it will be valuable to the scientific community, particularly for future projects involving spaceborne coherent Doppler wind lidar.

I therefore recommend publication of the article. I would nevertheless appreciate it if the authors could address the following comments and questions, which are mainly intended to clarify certain points and further improve the clarity of the manuscript.

Questions and comments:

Line 42: The acronym NOAA is not defined in the text.

Line 66: CDWLs are not always capable of measuring through clouds. This depends, among other factors, on cloud optical thickness and pulse energy. Could this statement be nuanced accordingly?

Line 116: The authors state that high wind-speed precision can be achieved with a single lidar pulse. This may be possible under very favorable conditions, such as constructive speckle, high backscatter, and sufficiently high pulse energy. Could the statement be qualified to reflect these conditions?

Line 211: Can you also provide the spectral width? I assume the pulse is close to transform-limited.

Line 230: The word “crystal” appears twice in close succession.

Line 241: Could you explain the reason for placing the QWP before the beam expander? Is this related to size constraints?

Line 256: Is the stated 12 W average power?

Line 289: Am I correct in understanding that α is selected based on the probability of successful signal-peak detection?

Line 330: It may be worth mentioning that the lidar return also depends strongly on the backscatter coefficient.

Line 513: I do not recall the pulse shape being specified earlier in the manuscript. If the pulse is a square 250 ns pulse, could 128 data samples be used to improve spatial resolution ?

Line 522: Could the authors provide more details on the interpolation method used in addition to zero-padding? Besides interpolation and peak searching, are other processing methods used to determine the frequency shift, such as a barycenter calculation or Gaussian fitting?

Lines 557–560: Since the variance calculation involves different averaging times for the off-nadir (~ 6 s) and nadir (~ 1 s) beams, does this generally result in higher SNR and a higher probability of successful peak detection for the off-nadir measurements, even though the relative detection threshold is higher for the nadir measurements? If so, does this imply that vertical winds are measured with higher accuracy?

Line 562: Could the authors provide an approximate false alarm rate for the given integration time and peak-search bandwidth?

Line 637: The values on the horizontal axis of the left panel of Figure 9 appear to be missing.

Line 639: In the manuscript, does S refer to the total signal power or to the peak signal power? When calculating the SNR, is S integrated over the signal spectral width?

Line 639: In the figure legend, σ is referred to as the standard deviation, whereas above it appears to be used to denote the variance. Could this notation be clarified? For a single pulse without averaging, is the mean noise floor value equal to its standard deviation?

Line 665: There appears to be a grammar error in the expression “where which”

Line 760: The figure legend states that mountain waves are evident. They are clearly visible in the horizontal and vertical wind speed measurements, but less so in the SNR panel. Could the contrast in the SNR figure be increased to make these features more visible?

Line 781: The authors state that backscatter intensity is being monitored, whereas only SNR is displayed. Although the two quantities are related, relating SNR to backscatter intensity would require accounting for the lidar’s spatial response and calibration. Is the lidar beam collimated? I think it would be useful to discuss this point, since only SNR is currently presented.

Other comments:

- For clarity, I would suggest placing Section 2.4 before Section 2.3, since several of the points discussed in the LEM module require the low power module to be introduced first.

- Direct detection wind lidars and previous demonstrations are discussed in the manuscript. However, there have also been efforts to develop airborne wind lidars based on fiber amplifiers. It may be useful to mention these developments for completeness.

- I understand that Part II of the paper will address measurement accuracy and precision. As such, I assume that a more complete assessment of measurement precision will be provided there, since the results presented in the current paper do not allow this aspect to be fully assessed.

Best regards,