Review of the article

"The NASA Aerosol Wind Profiler Doppler Wind Lidar, Part II: Airborne Flight Demonstration"

submitted by Bedka et al. (https://doi.org/10.5194/egusphere-2026-6111)

for publication in AMT (Received: 08 Oct 2026 – Discussion started: 12 Oct 2026)

Accompanying Part I describing the wind lidar technology, measurement and retrieval concepts, the manuscript "**The NASA Aerosol Wind Profiler Doppler Wind Lidar, Part II: Airborne Flight Demonstration**" by K. Bedka et al. covers the analysis of 3D wind profiles, measured with an airborne heterodyne-detection lidar during several flights in 2024. It focuses on the wind measurement performance including its comparison to validating reference data from drop sondes, AMVs and NWP models, with the goal to demonstrate the potential space borne DWL performance. Remarkable lidar measurement coverage and quality is shown from several flights, that is worth to be presented to the public, but with a bit less detail on individual flight examples than discussed in part II. The manuscript is well written and understandable. However, the publication would significantly profit from (minor) modifications of the structure and (major) more detailed results on the introduced goals (AWP validation and validity for a space DWL), as suggested in the general and specific sections below.

Proposed corrections that are already mentioned in RC1 to this manuscript (https://doi.org/10.5194/egusphere-2026-6111-RC1) are not repeated here. As the manuscript is Part II of a series, also the preprint of Part I including its available discussions were digested as prerequisite for this review.

After the proposed modifications, I would recommend to publish the manuscript. It will be a valuable contribution to the scientific community and a strong justification for sampling global wind fields with such a technology from space.

General comments

a) Bedka et al. 2025 already covers most of the topics, however with too much detail for a scientific publication. The part I-II series condenses the work to make it available for the community. As the AWP-validation is understood as one of the main add-ons to the part-I manuscript, I would propose to change the title in "...,Part II: Airborne Validation and Flight Demonstration".

b) While the presently reviewed manuscript of part II describes the objectives of the flown campaigns and results, the overarching objectives to both demonstrate/validate the DWL airborne performance and to extrapolate it towards a space system should be covered in more depths. This is deemed necessary to provide an added value to the scientific community, beyond what is covered in Bedka et al. 2025. Thus, it is suggested to remove one or two of the four detailed flight examples (or move them to the Appendix, e.g. 31 Oct.; part I already shows another example for 15 Oct.), to allow an expanded discussion on the performance validation incl. the zero-wind ground signal, coverage vs. aerosol and water vapor (from HALO data) and its relevance for space CDWL (see specific comments below).
Considering that some of the reviewer I comments probably also lead to additional content, and the already long manuscript of Part II, the shortening of the flight examples presented in the main body unfortunately appears unavoidable.

c) As the performance demonstration with respect to a space-based DWL is one of the main goals, the extrapolation of the demonstrated airborne performance to the

anticipated performance of an orbital system is under-represented and should be possible based on the acquired campaign data. With reference to part I, this is suggested to be additionally related to the different TFOMs of earlier DWL systems listed therein and elaborating on integration strategies relevant for the airborne and spaceborne cases. As the correlation TFOM vs. coverage is a long-standing and crucial debate within the community, based on the presented data, this aspect can and should be expanded in this dedicated manuscript.

d) The zero-wind validation from AWP ground-returns should be added to the validation section.
e) Apart from the validation of the AWP wind speed performance vs. drop sondes, the validation of the wind direction should be elaborated with more detail in light of the listed goal to show the strength/weakness of AWP. Given the "only" two LOS directions left and right of the aircraft, this is suggested to be done with respect to wind speed (as shortly presented in Bedka 2025) and depending on distance, i.e. horizontal separation between the LOS range bins.
f) For the flights crossing continental US, the geo-reference in the text is mostly referring to the overflown states as in Bedka 2025. For the international scientific community, please consider to instead either use lat/lon (or flight time reference) or to add the state names to the maps in the figures.

Although I suppose, that the proposed modification would cause substantial extra work, I hope the authors agree to the necessity and motivation suggested in this review. Given the demonstrated value a space DWL can deliver to the societies, and taking the perspective of an international cooperation based on several DWLs potentially materializing in the next decade(s), hopefully including a US system, the presented work will be a major contribution. The shown AWP data quality and coverage looks remarkably impressive and one has to congratulate the team on what they have achieved.

Specific comments

1. Line 57: GOES and AMV acronyms should be introduced here instead of later in text (line 395)
2. Lines 81 – 88: Please update the clarified procedure according to the discussion and potential modifications of the part I manuscript.
3. Line 88: Please add a sentence specifying which of the flights will be discussed in more detail in the remainder of the manuscript, why these were picked and why they are presented in this order (non-chronologically). Please highlight these flight dates and tracks in table 1 and fig. 1, respectively.
4. Line 132: shows should read show.
5. Line 161: The wind direction chart of fig. 4 shows regions with strong shear. However, when compared to the wind speed, these regions often show close to 0 m/s wind speed, where the wind direction might be not clearly measurable (also taking into account that the single LOS intersections are around 7 km apart). Please add a comment on this effect in this manuscript section (later repeated in line 416) and exclude under-defined wind directions measurements, if appropriate (e.g. for < x m/s) in the respective plots of all (incl. in the Appendix) examples in the manuscript (e.g. justified by table 2, showing a 30° error in the 0-2.5 m/s fraction). Please add the cloud detection contours of fig. 3 to the fig. 4 (and even fig. 8) plots for reference to the scene. This should provide the potential for expanding on the capabilities of the airborne vs. a spaceborne DWL in terms of coverage and winds around clouds, which is stated as a main capability for the PBL-focused heterodyne DWL.

6. For better comparison, it is proposed to add Fig. 8 after Fig. 4 and below Fig. 6, preferably again adding HALO data with the same zoom, before showing Fig. 5 and 7 at the end of the series. For both the relation to the DWL coverage (humidity, hygroscopic aerosol) and to the wind dynamics (latent heat), it is further suggested to add the HALO water vapor and aerosol (backscatter at 1 µm, HSRL 532 nm) channel results curtains and a short discussion on remarkable features in this context. Concerning the DWL coverage addressed in the manuscript, humidity is often strongly correlated and the parallel operation with the HALO system provides the rare opportunity to expand on this point, important for the coverage topic.
7. Line 169: Please also limit the wind direction color bar of fig. 6 to better reveal the wind shear details. The font size of the color bars is at the lower limit. Please consider an increased size, for improved readability.
8. Line 195-200: It is suggested to again show the HALO backscatter and water vapor curtains in this section and to discuss the cirrus features in the lidar-synoptic context. The collocated measurement bare great potential to constrain speculations and to demonstrate the individual strengths of the DWL (and the HALO) lidar technology.
9. Line 212: It is worth mentioning the remarkable coverage of the strong wind speed features despite the often-low SNR conditions, because these can be important contributions of a space DWL to NWP.
10. Line 222: It might be worth emphasizing the high wind speeds in the cirrus outflow, connected to the strong convective activity.
11. Line 223: You might either want to expand the color bar to 2 m/s in the respective plot of fig.11 or add a statement “exceeding the plotted range” in the text.
12. Line 236: Fig. 10 would profit from again adding cloud detection contours like in fig. 3. Please also add the averaging time as in fig. 3. **Both is proposed as a standard for all the curtains also from the other flight examples (incl. in the Appendix), because of its relevance for section 3.5.**
13. Line 244: Please add the contours of Fig. 10 (see previous comment) also to these plots (and potentially to added HALO plots).
14. Line 287, 317, 323, 336: See comment 5. How much confidence is in the wind shear close to the ground with very low wind speeds?
15. Line 287, 336 (+ Appendix): Is the surface contour individually plotted for the three LOSs ground intersection points? Please clarify in the text and correct in the plots, if necessary. The strongly varying terrain sections of the flight examples could lead to occasions, where one off-nadir LOS hits the ground earlier than the other side as they are a few km displaced when measuring from high altitude. In such cases there should be data outages for wind speed and direction close to the ground, which are not visible in the plots. Could you please add an explanation to the text.
16. Line 346: Section 3.5 As this topic is of high relevance for the technological DWL approach towards a space-based system, and it is off the flight examples section, I propose to raise it to a section (section 4). It is also proposed to expand on the topic as discussed in some coverage-related comments above, preferably in relation to the parallel aerosol and water vapor measurements of HALO. Also, the numbers of clear vs. cloudy bins are of relevance in the coverage context. Could you please also specify the number of profiles contributing to the different fractions and the horizontal and vertical integration/averaging for the contributing profiles (which was varied during the campaign flights) to fig. 18. In fig. 10 of part I you expand on the influence of the integration time on the detectability. Would you mind adding a figure and discussion on the success fraction for different integration times, i.e. measurement resolution (hor./vert.)? I think this would be an important contribution to the manuscript and the motivation.

17. Line 371: In the upper altitudes the R^2 dominated decay of the probability is visible. This can be extrapolated to a space DWL performance from e.g. a 400 km orbit. Please elaborate on this important motivated topic in the manuscript (e.g. in relation to fig. 5 right in Henderson 2022, https://beyondphotonics.com/wp-content/themes/beyondphotonics/img/CLRC_2022-Wind-SP_Tech.pdf or page 20 in Henderson 2025 https://beyondphotonics.com/wp-content/themes/beyondphotonics/img/3D_Winds_2025-Coherent_Lidar_Tech_Advances.pdf).
18. Line 376, section 4: As the validation is one of the main motivations for part II, I would propose to rephrase the section to "AWP validation" (in general) and add important validation results and details that are not addressed in the current manuscript. Please also provide a validation vs. ground (land/ocean), as this is the established procedure in the airborne downward-looking configuration to show the stability of the data retrieval chain, incl. INS/GPS-based Doppler correction (shown in fig. 8 of part I).
    Please add information on how the chirp in the pulse is potentially affecting measurements and treated in the date retrieval?
19. Line 385: Please specify the uncertainty (RMS?).
20. Line 391: Please provide a comment on how this weighting procedure is represented in the literature for similar drop sonde validations of DWL (e.g. for DAWN, Bedka, Greco)? How does this affect the retrieved DWL random error (it is not clear, if this issue is tackled with the statement in line 407)? This could be demonstrated by applying the same averaging procedure to the zero-wind ground-echo-based validation (see previous comment).
21. Lines 402 – 413: The validation for AWP by the comparison vs. AVAPS is influenced from representativity (different sampling), AVAPS error (discussed, but not corrected for) and the AWP instrument error characteristics (single LOS error plus sampling representativity error), plus else (?). Please add a few sentences on how you address these issues and what their contribution is when interpreting the bias and RMSD values given in the plots and table 2. E.g. concerning the comparisons in fig. 20, there are different error contributions from both AWP and AVAPS introduced on the wind vector speed, the u and v components, when projected onto the respective directions.
    When comparing with DAWN that has more than two LOSs contributing to a wind profile, the LOSs averaged per profile are different (on top of potential averaging). Please discuss in the text, how this is addressed for a fair comparison.
    Please add the AWP random error dependency on the averaging (e.g. pulse integration duration) in the AVAPS and/or ground speed validation.
22. Line 422, fig. 19: Please increase the font size of the plots to a level, readable in the printed version.
    There seems to be a vertical averaging filter applied to the AWP data. The averaging strategy (during measurement and/or post processing, e.g. pulse integration duration) that is relevant for these plots, fig. 20 and table 2 is not totally clear to the reader at this point (at least to me), because there are several statements distributed in part I and Part II. In the validation section, it is thus proposed to repeat/summarize in one or two sentences what is the relevant averaging applied for these results. Please clarify in the text or caption what the AWP vertical and horizontal averaging is or at least point to the respective manuscript section(s).
23. Line 426, fig. 20: This is a central fig. for the AWP performance evaluation. To better characterize the data-set, please also show plots on the wind speed distribution (only partially provided in table 2), the wind direction distribution and histograms on the wind direction and speed differences to the AVAPS winds (similar to fig. 25 and Bedka 2021-fig. 15). The latter would also be part of the important single LOS validation based on

ground returns (see comment 18). The single LOS speed validation could also be done with the AVAPS data for the atmospheric channels. Please comment on why is this not done (if not).
Please restrict the number of useful digits like in table 2 (see comment below).
Fig. 20 caption: (top) should read (left).

24. Line 428: As the comparison to AMVs was introduced as one of the goals for the project, a direct comparison like in fig.20 for the dropsondes is suggested (as also a comparison to NWP, see following comment).
25. Line 434, table 2: The direction bias error is not smallest for the 0-5 km fraction, but for 10-15 km. Please comment on this in the validation section. Is this coincidence or an effect of the two LOS sampling of AWP, i.e. retrieving wind direction from measurements which are up to several km away from each other (expanding on what is stated in line 418)? Showing the height dependency of error and AVAPS distance could reveal that.
In the table, please restrict the number of useful digits (e.g. 7° +/- 33° instead of 6.46° +/- 32.62° and 1.1 m/s instead of 1.13 m/s).
26. Line 435: Section 4.2. This is understood like a "validation with and of NWP" (depending on the forecast lead time) and therefore could be split accordingly. Please add a wind speed comparison to the 0h lead time model winds (like presented for AWP vs. AVAPS winds) or expand, why it is not appropriate in this validation context.
This section could also be limited to the validation aspect with the 0h NWP data. The longer lead time comparisons are a validation of the NWP model prediction quality rather than of the DWL performance and could be left for the proposed future paper (lines 400 and 499), to safe space and to focus on the main purpose of the publication.
27. Line 464, fig. 22: Wind direction errors vs. the model are high (reddish, blueish regions in 0 and 72 h fcst), where the wind speed is low. Also here, these regions should be excluded (like discussed above) or additionally addressed in the discussion.
Please increase font sizes and change numbers to UTC for the NWP plots (18:30 instead of 18.5).
28. Line 470, fig. 24: Same as previous comment. Wind direction error applicable to a few winds close to the ground in the center and right of the scene.
29. Line 476, Summary: This section might need adaptation depending on the additional results introduced in the review process. Apart from that I would propose to again make the connection to the overarching goal (as presented in section 5.2 of part I) in this summary (and in the abstract of part II).
30. Line 477: Compared to part I it should read "…describes and validates…CDWL performance and…".
31. Line 487: Please specify "high" altitudes.
32. Line 495: Should read 0.71 m/s.
33. Line 538: Remove double "development"
34. Line 615, Appendix: Please also here increase font size for the plots to a level, readable in the printed version.
35. Line 647: Discussed in "Section 3" of Part I, not in Part II. Please correct.

Technical corrections:

36. Line 230: Correct spacing
37. For AMT m/s should be formatted m*s^-1
38. The color bars might need adjustments to meet color-blindness readability.