Review for wes-2026-94 ”How accurate is the beam position of scanning lidars? An intercomparison” by Young et al. 2026

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1 Summary

The manuscript by Young et al. 2026 presents a comprehensive intercomparison of three different methods classes to determine the elevation offset of scanning wind lidars in coastal environments. The accurate determination of the scanner elevation and associated offsets is essential for accurately positioned lidar measurement volumes at larger ranges. Therefore, the work is of very high relevance for the wind energy industry and lidar community.

The three compared method classes are namely the static hard targeting (SHT) by mapping given and known hard targets around the lidar position, dynamic hard targeting (DHT) by mapping a drone with an accurate RKT-corrected position, and sea surface levelling (SSL) using the sea surface around the lidar as a reference. They demonstrate all methods for two different scanning lidars from the same manufacturer at the test site C-TEST in the UK. As a reference to evaluate the methods, they use the position of a cup anemometer at a 103 m high met mast located 5 and 7 kilometres offshore, respectively. Next to a detailed description of the method and measurement setup with suitable figures and tables, they also provide analytical or Monte Carlo-based uncertainty propagation for all methods.

The authors conclude that the DHT method provides the lowest uncertainties with high accuracy, whereas the SSL method is associated with the highest uncertainties.

Overall, the manuscript is well written and structured. Most figures are clear and support the findings of the authors. However, the manuscript is lengthy and could be shortened in some aspects, and some figures/tables could be moved e.g., to an appendix. Further, some used methods are not clearly explained in full detail and are partly not implemented as proposed by previous publications. Missing shared code and data consequently limits the reproducibility of the results for the readers. In the discussion, the results are not put into the context of other related work. Therefore, we suggest publication after major revisions. Detailed comments are listed below.

2 Major comments

1. The title is dealing with the ”beam position”, however, azimuthal or range uncertainties are not considered (although of similar importance when doing e.g. spatially aligned and synchronized dual-Doppler measurements). It is mentioned in line 132+, but it should also be reflected in the title already. Suggestion: ”How accurate is the beam elevation of scanning lidars? An intercomparison”

2. In the abstract (line 10) the authors mention static elevation angle error = elevation offset. However, in the manuscript they treat the elevation offset as a function of the lidar azimuth (e.g. Fig. 6), which is, to our understanding, not static but the combined internal (static errors) and external misalignments (tilt of the device). Please define the terminology ”elevation offset” clearly in the manuscript. See also the definitions of [1] and [2], who use elevation offset as static offset, and elevation error as static offset + azimuth dependent errors due to device tilt.

3. The authors use altered versions of the SSL method introduced by Rott *et al.* [3] and the simplified extended SSL method by Gramitzky *et al.* [1] still using the original naming. This could be misinterpreted by the reader. Further, the separation between the used SSL methods is not clear throughout the manuscript despite the significant difference in the used methods (e.g. line 632). The naming needs to be adjusted to account for the differences with the original methods. Further, a more clear definition of the used SSL methods and the associated assumptions should be included in the methods section (similar to Table 10 in the results section). The discussion and conclusions need to more clearly separate between the different SSL methods and highlight the differences to the original methods (see comment below). Please name the reason for the applied changes to the original methods.

4. As mentioned in the last comment, the authors use the SSL methods introduced by [3] and [1] but strongly simplify and change them. The device tilt is neglected or limited in some cases. This assumption is unrealistic and contradicts the results e.g. in Figure 9 (amplitude of sinusoidal curve is approx. 0.15 degree). Further, the height of the lidar above sea surface is derived using a distant buoy measurement. This value is prone to errors. The original method by [1] and [2] has no need for the lidar height as input but obtains it from the fitting procedure. Please elaborate on the physical reasoning for these assumptions and modifications. We suggest using the original SSL method as implemented by [2].

5. The drone-based hard target method is a pragmatic approach, and the authors show that it is associated with the lowest uncertainties. However, the authors do not put their results in context with the work by [4], who have provided a comprehensive uncertainty investigation for the DHT method. Despite the fact that [4] is not a peer-reviewed document, we suggest the authors discuss their findings with them. Also, the authors do not refer to their own work [5] in the discussion. In general, the

results are not well discussed with previous results on the same topic (e.g. [4], [5], [2], [1] and others).

6. In total, the manuscript is quite lengthy and could be shortened in some aspects. Many sentences span more than three rows and are difficult to read (e.g. line 62+). Also, some figures and tables could be moved to the appendix for better readability (e.g. Fig. 4,5,7, Table 7).

3 Specific comments

1. Line 6ff.: it seems the authors include the sea surface leveling in their definition of the "Hard Target Testing", whereas the detection of the sea surface is inherently different compared to actual hard targets.
2. Line 69ff.: Note that the error between the pointing in the lidar reference frame and the true pointing is not exactly described by a sinusoidal function. For small misalignment, it is assumed to be so. Please specify.
3. Line 86ff.: The lidar height is already a parameter that can be obtained from the original SSL model by Rott et al. [3], see their Figure 11.
4. Line 89: First demonstrations of the SSL from a coastal lidar installation were published; see the work by Meyer et al. [2].
5. Line 95ff.: Is this only the case for SSL or also for the DHT/SHT methods, where device tilting will lead also to a variation in the elevation of the laser beam and thus higher uncertainties?
6. Line 96ff.: An indication of the magnitude of this effect by e.g. internal inclinometer data for the scan head at different azimuth angles would be beneficial to support the argument. Suggestion: Remove this sentence to shorten the introduction.
7. Line 127+: Are really the precise coordinates in a global coordinate system required, or are the relative distances $\Delta x, \Delta y, \Delta z$ or even only the polar coordinates ($\phi_{\mathrm{ref}}, \theta -$ ref) required? If the range is not of importance, the polar coordinates should be enough?
8. Line 135: The subsection "Experimental Setup" starts with an introduction to the compared methods. This is misleading and a clear structure should be kept.
9. Table 2: Why did the authors decide not to use uniform settings throughout the experiment? Why was the probe length for DHT and SL South chosen to be so small? Did the authors see any difference in the results due to the different settings? The influence of the different settings (probe length) should be better discussed.

10. Table 2: Why was the probe length for the SSL not chosen to be smaller to remove the dependency of the different definitions of the actual inflection point (see [2])?

11. Table 2: for SSL PPI and RHI only small azimuth sectors (75°) were chosen, probably due to site constraints. Please elaborate on this aspect. Regarding Fig. 3 wider sectors seem possible.

12. Fig. 3: Please include a map scale as well as axis labels.

13. Line 182: Was the total station able to capture all hard targets from a single position, or has it been replaced using an accurate (i.e. RTK corrected) GPS?

14. Table 3: Think about providing more significant digits to the DD.dddd representation of the Lat/Lon coordinates (or alternatively UTM coordinates) as this representation provides an (unknown) uncertainty of a few meters at your location.

15. Section 2.3 In general: It is often mixed between method and actual measurement setup description, this could be made more clear.

16. The authors mention, that for the SSL method still some uncertainty in the sea surface range exists, which, however, has been covered and validated in a recent publications [2].

17. Line 241: See also the major comments 3 and 4. The described approach of predefining the vertical height above current sea level ($h_{\text{eff}} = h_{\text{lidar,amsl}} - h_{\text{tide}}$) is new and not covered in Rott et al. or Gramitzky et al. 2026 and describes a new methodology. This could be made more clear as this can be confusing for the reader. The knowledge about $h_{\text{lidar,amsl}}$ and h_{tide} is also associated with significant uncertainties, which must be considered. Further, these two parameters might not be available in many applications. Please explain why the original method deriving the lidar height from multi-elevation scans is not used.

18. Eq. 7: Do the authors assume, that there is no device tilt? They mention this in line 251, but it could improve readability, if it was mentioned again just before Eq. 7.

19. Section 2.3.3: In general, it is not clear why another method for the Sea Surface Leveling is introduced. Why do the authors want to reduce the number of unknowns? Is there an improvement in the results results by doing so?

20. Line 253: Is the limitation of pitch and roll to be less than 0.05° a good approximation? From the SHT results in Figure 6 for SL South it looks like the device is tilted by ≈0.125° (amplitude of the sine curve). The internal inclinometers (uncalibrated?) are expected not to provide accurate readings.

21. Line 272+: The authors mention, that i_p in the inverse sigmoid might not describe the distance to the water surface but somehow a position within the immersed

pulse. Please consider the recent work by Meyer et al. [2], who show that the inflection point is a good approximation of the distance to the water surface, when fitting the inverse sigmoid in the linear scale and not the (logarithmic) decibel scale. Also, see their suggested method for the lidar-sea surface range determination, based on a more physically based approach. Please elaborate why this method is not used.

22. Line 280+: For the RHIs, only elevations of -1° to -2° are used, whereas Gramitzky et al. recommend a larger sector to reduce uncertainties (their figure 6). Please elaborate, why this has not been incorporated.

23. Line 281: Note, that in [1] and [2], the volumetric scan (multiple RHIs at multiple azimuths or multiple PPIs at varying elevations) are taken to fit the **static** elevation offset ϕ_{off} from all measurements and not from a single measurement. Therewith, the RHI scans do not provide variations in ϕ_{off} with elevation or azimuth. This is a deviation from the published methods and should be mentioned.

24. Please elaborate why a large range gate resolution of 20m was used for the SSL methods? With better resolution it is expected to improve the fit results. Especially, if the range gate length is short (50m). Small errors in the lidar-sea surface range can lead to significant errors in the elevation offset, see [2] (their Fig. 1). If a probe length of 50m is chosen with a range resolution of 20m, the drop in CNR is only resolved by few measurement points, which might not be sufficient for an appropriate fit in the most important region of the CNR curve, which is the drop at the inflection point.

25. Line 494: It is not clear, how the uncertainty is estimated for the SSL Ext + tilt methods. Also, the tidal variation only plays a role, when the water level is assumed to be constant.

26. Elaborate on what is meant by *temporal variability of the external lidar levels* in line 313?

27. Line 337: has the drone hover stability been verified additionally with the total station?

28. Fig. 4 and 5: To keep the manuscript short, we recommend only showing one of the hard target maps and moving the other plots into the appendix.

29. Table 7: Is quite comprehensive and could also be moved to the appendix.

30. Fig. 6: From the fit results, it seems that both devices were tilted by 0.11° and 0.21°, which is significant. Thus, a perfectly aligned lidar cannot be assumed for the SSL method. Probably, the internal inclinometer of the device by the manufacture show 0° but are also associated with uncertainties and errors.

31. Fig. 6: It seems for SL South the hard target S1 at $\theta_{\text{lidar}} \approx 300°$, has a major impact on the fit result and lowers the resulting ϕ_{offset}. Have the authors tried excluding this hard target due to potentially wrong coordinates or other reasons?

32. Line 397: Compare the resulting uncertainty in horizontal drone positioning from the RTK measurements (line 337+: 0.11m) to the results obtained from the lidar scans (line 396: 2m). Why is there such a big difference? Please elaborate.

33. Fig. 7: Also some of the subfigures could be moved to the appendix.

34. Table 8: Why is $\sigma_{\phi_{\text{off}}}$ so large for SL South and Ref(NOAH)?

35. Line 446: The authors conclude, that with the sigmoid fit accuracy, expressed by the R^2, is highly sensitive to the elevation angle of the beam. However, they do not explain why this is the case. If the same range resolution is used for the fits at different elevation angles, there is an associated error in the analysis. When the range min/max/delta = 150/6510/20m from Table 2 are used for all SSL scans at all elevations/azimuths, the following problem should be investigated. Imagine, at $\phi_{\text{lidar}} = -2°, r_{\text{ss}} \approx 300m$, whereas at $\phi_{\text{lidar}} = -0.2°, r_{\text{ss}} \approx 1000m$, the inverse sigmoid fit is done once on the drop and to a long range of noise in the CNR (which is not as smooth as the aerosol return CNR before the drop) and once fitting the smooth signal first and then only a little portion of the noise CNR. This is especially the case, if the CNR in (logarithmic) decibel scale is used. The noise here has larger fluctuations of a few dB, which leads to lower R^2. Therefore, the authors should remove the CNR after the drop has been detected or use the linear CNR [2]. So, please elaborate (show exemplary a fit of the inverse sigmoid at $\phi_{\text{lidar}} = -2°$ and $\phi_{\text{lidar}} = -0.2°$) and show why the R^2 is so inherently different.

36. Please also elaborate, why the R^2 and thus the frequency increases again for SL South at $\phi_{\text{lidar}} < -1.5°$

37. Fig. 10 when averaging the results over time, while there is a considerable variation in the water level, also the r_{ss} will change significantly, especially at close-to-horizontal elevation angles. Showing this as $\pm\sigma_{r_{\text{ss}}}$ is misleading.

38. Line 461: Can this deviation be explained by the fact that SL South has a different number of values from RHI and PPIs and a varying fit quality (R^2) compared to SL North?

39. Fig 10: The text size in this figure is a bit small and could be increased.

40. Fig 10.: Why does r_{ss} decay at SL north (at $\phi_{\text{lidar}} > 0°$)? We suggest improving the filtering, so that these values are removed.

41. Fig 11: SL North: please comment on the fact, that for the RHI there is a continuous change in the elevation during the accumulation time, where the r_{ss} will change with each pulse. This results in larger scatter for the RHI scan.

42. Table 9: The obtained standard deviation of the elevation offset for the Ext. SSL and Ext. SSL + tilt methods are very large, please elaborate, why.

43. Line 483: Please also comment on the chosen elevation ranges, as Gramitzky et al. 2026 provide an error propagation model for different starting and ending elevations.

44. Fig. 13 c) and d): Comparing SL South and SL North, it is clearly visible, that something went wrong in some fits for SL south when using the SSL method. The height obtained does not represent the actual height above mean sea level, indicating problems with the r_{ss} fit (see figure 10) or with the SSL fit itself. It would be helpful to provide an explanation and give exemplary plots of the fit results in the appendix. Also the pitch/roll fit for the period, that does not fit (2025-05-30 00:00) is deviating. Please elaborate, why.

45. Line 512: Are the internal inclinometer readings calibrated so that they actually indicate a tilting to the horizontal plane, or ar these the internal standard inclinometers coming with the devices? If so, why do they show $\approx 0°$ pitch and $\approx -0.03°$ roll for SL South, whereas from the fit results in Fig. 6 it is evident that there is a significant tilting of the device by $|A| = 0.12°$?

46. Line 555: For the discussion it would be beneficial if the authors could provide an approximate indication of the efforts needed in terms of working time and analysis. Can the methods be repeated remotely?

47. We suggest moving Figure 15 and the associated information to the methods section.

48. Line 567: can the authors comment on why they expect differences in the results due to the different scan head designs only for the SSL methods, but not the SHT/DHT methods?

49. Were the authors able, to check if a general range-offset exists for the lidars, by comparing the obtained ranges to the hard targets from DHT/SHT against the known coordinates?

50. Do the authors believe, that the elevation offset remains unchanged for high elevation angles (e.g. a 60° VAD)?

51. In general we are missing the consideration of backlash effects in the study. They are mentioned once for the SSL method only. Also see the work by Vasiljevic [6]

52. Line 587 ff: Since accuracy and uncertainty are sometimes treated equally, a short recap of the definition of both terminologies would be helpful in the methods.

References

[1] K. Gramitzky, F. Jäger, D. Callies, T. Hildebrand, J. K. Lundquist, and L. Pauscher, "Alignment of scanning lidars in offshore campaigns – an extension of the sea surface levelling method," *Wind Energy Science*, vol. 11, no. 3, pp. 861–882, 2026. DOI: 10.5194/wes-11-861-2026.

[2] P. J. Meyer, A. Rott, J. Schneemann, L. Pauscher, K. Gramitzky, and M. Kühn, "Experimental validation of the sea surface calibration for scanning lidar static elevation offset determination," *Journal of Physics: Conference Series*, vol. 3224, no. 2, p. 022 049, 2026. DOI: 10.1088/1742-6596/3224/2/022049.

[3] A. Rott, J. Schneemann, F. Theuer, J. J. Trujillo Quintero, and M. Kühn, "Alignment of scanning lidars in offshore wind farms," *Wind Energy Science*, vol. 7, no. 1, pp. 283–297, 2022. DOI: 10.5194/wes-7-283-2022.

[4] G. R. Thorsen, E. Simon, and E. H. Clausen, "Drone-based scanning lidar pointing calibration (d4.4)," DTU Wind and Energy Systems, Technical Report, 2023, Project: Wind and Energy Systems, DTU. [Online]. Available: https://backend.orbit.dtu.dk/ws/portalfiles/portal/410557394/Drone_based_scanning_lidar_pointing_calibration.pdf.

[5] A. Oldroyd, M. Young, M. Docherty, J. Bost, S. Redford, J. Haize, J. Royle, and J. Gottschall, "Comparison of classical and drone-based hard-target methodologies applied to scanning lidar for offshore wind," vol. 2875, p. 012 041, 2024. DOI: 10.1088/1742-6596/2875/1/012041.

[6] N. Vasiljević, G. Lea, M. Courtney, J.-P. Cariou, J. Mann, and T. Mikkelsen, "Long-range windscanner system," *Remote Sensing*, vol. 8, no. 11, 2016, ISSN: 2072-4292. DOI: 10.3390/rs8110896.