

RC2: '[Comment on wes-2026-94](#)', Anonymous Referee #2, 21 Aug 2026

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

August 20, 2026

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.

[Reply:](#) We wish to thank the referee for their time and effort in reviewing our paper and for providing constructive feedback and helpful comments that have helped us improve the quality of the manuscript.

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”

[Reply:](#) Thank you for this helpful suggestion. We have updated the title as suggested to more accurately reflect the main focus of the manuscript on beam elevation.

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.

Reply: Thank you for raising this issue. The terminology used for elevation angle errors versus the static elevation offset was indeed unclear. The manuscript is primarily concerned with quantifying the elevation angle error as opposed to the static offset/error. Originally, the term *elevation offset* was used to describe the *elevation angle error* ($= \Phi_{\text{lidar}} - \Phi_{\text{ref}}$; Section 2.1) and these two terms were used interchangeably throughout the text which may have caused additional confusion. We have therefore replaced any previous reference to “elevation offset” with “elevation error”. We have also clearly defined in the text the difference between static elevation offset and elevation error, see Lines 89-91.

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.

Reply: Thank you for highlighting these very important points. We believe that some confusion has been caused by conflation of different terminology, including the point raised in previous comment #2. To help resolve this we have renamed the sea surface method applied as the Sea Surface Method, rather than Sea Surface Levelling. The aim is to use sea surface scans to estimate the lidar elevation angle error from the range of the sea surface obtained from RHI/PPI scans and was inspired by the SSL approaches in [1] and [3]. The sea surface range estimation methodology applied (i.e. determining the distance from the lidar to the sea surface via application of a sigmoid fit to the CNR return) is consistent with previous studies (e.g. [1],[2],[3]), although we have altered the method slightly based on our dataset. The altered version(s) of the sea surface range (r_{ss}) acquisition method is primarily the following:

- 1) For a given scan azimuth the SSL measurements were aggregated to 15-min to match the temporal resolution of the tidal data.
- 2) min/max normalisation of CNR between 0 and 1 was applied to optimise (i.e. speed up) the sigmoid curve fitting approach due to the large volume of data processed. This markedly speeds up the computation time by a factor of 1.5-2 times, but possibly more if parallel processing is implemented.
- 3) Using the range of the sea surface determined by the sigmoid function ([1],[2],[3]), obtain the elevation error (difference between lidar elevation and expected elevation angle) from a single scan, using the height of the lidar above the sea determined using the tidal gauge.

We have reordered section 2.3.3 to help clarify these steps. To provide evidence that the modifications 1)-3) do not change the determined range of the sea surface r_{ss} , we have included examples of the sigmoid fits in the appendix and a comparison of the curve fitting results from the standard CNR approaches (including linearised CNR in [2]) to our approach using min/max normalised CNR (Figure A5).

While the extended SSL method by Gramitzky et al., [1] can also be used, it relies upon accurately determining tilt, height and elevation offset from a curve fitting procedure at multiple azimuths and elevation angles. In this method ideally the azimuth range needs to be sufficiently large in order to resolve the tilt variations. In the original manuscript, we implemented the extended SSL method by [1], but on further investigation while addressing the comments raised by the reviewer, we noticed that the limited azimuth ranges of the respective RHI and PPI scans made it difficult to obtain reliable estimates of tilt and offset from the curve fitting approach. This was why a constraint was forced on the pitch and roll of the fit in the original manuscript since we observed from the internal inclinometer that the tilt variations during the test were very small. However, as the reviewer helpfully pointed out in later comments, the

inclinometer is uncalibrated and likely has an error/offset in the readings based on the amplitude of the sine curves determined from the hard target method. Based on these issues, we believe that the scan setup used in the experiment is not a fair test for the (extended) SSL method, and therefore have removed this method from the results.

We have added the following statement in reference to this in the Sea Surface Method Section 2.3.3

Note that this method is different to the simplified extended SSL method which may also be used to estimate Φ_e based on determine tilt (pitch/roll), lidar height and the static elevation offset by applying a curve fitting approach to r_{ss} measurements across multiple θ and Φ (Gramitzky et al., 2026). In the case of the scan setup employed in this test, it was not possible to reliably implement this method due to the limited ranges in θ of the PPI and RHI scans performed during the test, due to site constraints on the scan geometry. The new method employed in this study both removes the need for curve fitting, and allows estimates of the elevation error to be determined when the azimuthal field of view of the device is limited.

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].

Reply: The main aim of the sea surface method applied is to estimate the total elevation angle error of the device. Since the lidars are deployed on static/non-moving surfaces, we assume *variations* in device tilt are small and therefore do not attempt to distinguish between the elevation angle error due to tilt versus that due to internal misalignment.

We acknowledge that the height of the lidar above the sea surface is associated with uncertainties (e.g. due to tidal gauge) and this is why they have been specifically accounted for in analytical uncertainty formulation and budget (see Section 2.4, Table 4).

As mentioned in response to previous comment #3 we have removed the results from the extended SSL method [1] from the manuscript.

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).

Reply: We have added more discussion of the results from the drone method wrt [4], [5], [2] and [1].

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).

Reply: Thank you for the suggestion. To help reduce the length of the manuscript, we have replaced Figures 4 and 5 with a single Figure 4, and reduced the content in Figure 7. Some of the original subfigures that have been removed and are now provided for reference in the Appendix. We have removed Table 3 (see response to Specific comment #13 below). We have moved Table 7 to the appendix. We have also re-written some long sentences and removed some sentences with unnecessary information.

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.

Reply: To more appropriately describe and distinguish the different approaches, we have changed the naming to “*calibration methods... to determine and correct for beam elevation errors*” instead of grouping all the methods under the label “*hard target testing*”.

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.

Reply: We have corrected the statement accordingly:

Provided external misalignment of the device is small, Φ_e can be described by a sinusoidal function to determine the angular error as a function of θ .

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.

Reply: Thank you for highlighting this. We have corrected this statement to the following:

Sea surface scans were initially employed to estimate device height and tilt on the transition piece of offshore wind turbines, hence defined as “Sea Surface Levelling” (SSL; Rott et al., 2022). However, the SSL method has been further extended to additionally estimate the lidar static elevation angle offset, showing promising results (Gramitsky et al., 2026, Meyer et al., 2026).

4. Line 89: First demonstrations of the SSL from a coastal lidar installation were published; see the work by Meyer et al. [2].

Reply: Thank you for highlighting this valuable study. We have now referenced Meyer et al., (2026) in the previous paragraph and modified the beginning of the original paragraph on Line 89 to the following

While SSL methods show promising results, their performance in a coastal environment faces a number of challenges.

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?

Reply: It is expected that any additional surface movement / external tilting to the device will increase the uncertainty in all methods. The sentence has been modified as below:

Therefore, the absence of significant device tilt is expected to partially reduce the uncertainties of both sea surface and other beam calibration methods.

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.

Reply: This is something we have not been able to test due to the limited temporal measurement frequency of the internal inclinometer. We have removed the sentence.

7. Line 127+: Are really the precise coordinates in a global coordinate system required, or are the relative distances Δx , Δy , Δz or even only the polar coordinates (ϕ_{ref} , $\theta - \text{ref}$) required? If the range is not of importance, the polar coordinates should be enough?

Reply: For full reproducibility, ideally either the coordinates in global coordinate system or the Δx , Δy , Δz of each target relative to lidar coordinates are optimal, since all relative hard target locations and therefore polar coordinates can be calculated from this. Polar coordinates are also enough but since these exclude the range which is important although not a focus of our study we present the

full global coordinates. We have simplified the sentence here in context of describing polar coordinates only since these are the main relevance for this section. The survey method is described in Section 2.2.

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.

Reply: The introduction sentence to the compared methods has been removed from "Experiment Setup" and instead used to introduce subsection 2.3 "Calibration Methods".

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.

Reply: The settings were chosen to be as uniform as where possible, given the different generations of the lidar systems which limited the probe length settings, and depending on measurement range of the tests. Different probe length settings were tested for SL South and showed no clear influence on the drone method results, with the angular resolution being more important in obtaining the angular position of the drone within the lidar reference frame. We have added the following text discussing this.

The scan settings employed throughout the experiment were chosen based on the settings available for the two different lidar systems and depending on the target ranges of the different methodologies. The shortest available probe length setting was 75m for SL North (generation v2.0) and 25m for SL South (generation v2.1). For the majority of drone flights executed for the DM (Table 4), the shortest probe length of 25m was applied to SL South due to the near-range of the drone to the lidar (500m). However, for Flight #4, the probe length was increased to 100m in order to simultaneously scan both the drone and NOAA reference target during a single flight. For Flights #8 and #9, the probe length was increased to 150m to match the probe length setting used for SL North, due to the far-range of the drone from this lidar. Despite these probe length changes, no clear deviations were observed between the DHT results with the angular resolution being the dominant factor in determining the drone position in the lidar reference frame.

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])?

Reply: We have added the following text to explain the reasoning behind the probe length settings and adjustment applied to the sea surface method which is in line with previous studies, including [2].

For SS scans, the probe lengths implemented are similar to those used in other studies (e.g. Gramitsky et al., 2026; Meyer et al., 2026). A probe length of 75m was used for SL North since this was the shortest that could be set in the system. For SL South, a shorter 50m probe length was used as a trade-off between the shortest available setting (25m) yet similar in magnitude to SL North. While investigating the sensitivity of probe length is not the focus of this study, we apply a recommended probe length correction to account for potential sensitivities of the determined sea surface range to the probe length which has been shown to be suitable for probe lengths of 75m (Gramitsky et al., 2026) and 50m (Meyer et al., 2026).

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.

Reply: Yes, this was primarily due to field of view constraints. We have added text explaining the limitations of the sector choice.

For sea surface scans, azimuthal sectors spanning 75° were used based on the availability of an unobstructed field of view due to nearby buildings and objects.

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

[Reply:](#) Fig. 3 was updated accordingly with scale and 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?

[Reply:](#) All hard targets were observed from a single total station position at the north site. At the south site, the total station required two setups in order to capture all hard targets, due to obstructions. Each setup was independently resected onto RTK GNSS-surveyed control points visible from that specific location. Because all control points share a common absolute reference frame, this approach avoids error propagation between setups, ensuring all captured hard targets are consistent in terms of coordinate system and accuracy. We have added the following details in Section 2.2 to clarify this:

We setup the total station using the resection from three known positions (Ghilani and Wolf., 2012) determined from the global-navigation-satellite-system (GNSS) corrected by real-time kinematics positioning (RTK).

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.

[Reply:](#) To address this and also help cut down on the length of the manuscript, the scanning lidar locations are now specified in UTM coordinates alongside the height and unit details in Table 2. Since all target coordinates can be determined using these coordinates in combination with θ_{ref} and ϕ_{ref} and the target distances/range provided in Table 6 (now Table 5) we have removed Table 3 from the manuscript for brevity (also to help address request of major comment #6).

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

[Reply:](#) To address this a new section 2.3.4 *Scan Setup* has been added to Section 2 explaining the measurement setup and is now separate from the methods.

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].

[Reply:](#) Thank you for bringing [2] to our attention. To our knowledge this was published at the end of May when our manuscript was submitted (29 May) and therefore was not possible for us to consider at the time. It is a very informative and valuable study; we now refer to it throughout the manuscript and present a sensitivity analysis of the sea surface range determined from our approach alongside the different methods presented by [2] in the Appendix B1, Figure B2. Note that in [2] the sea surface range was only validated at relatively short measurement ranges (400-500m) and limited time period. In summary, Figure B2 shows some variations among the methods, however we have applied the recommended correction for probe length ($-1/2$ probe length) by Gramitsky et al., (2026) which has been verified and shown adequate in [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.

[Reply:](#) To help clarify the approach and distinguish it from Sea Surface levelling methods presented by [1],[2],[3], we have renamed the method (now Sea Surface Method) and more clearly separated

the naming between it and SSL methods. As in response to Major Comment #3, the limited azimuthal coverage from the PPI and RHI scans due to the site constraints made it difficult to obtain robust parameter estimates from the SSL method [1], and therefore this method has been removed.

For the sea surface method presented, the uncertainties in $h_{\text{lidar,amsl}}$ and h_{tide} have already been acknowledged and considered in the analytical uncertainty formulation for the method, as explained in Section 2.4 (also see Table 4). $h_{\text{lidar,amsl}}$ is determined from the site survey (GNSS antenna), and has a small uncertainty of 0.1m. Typically this is obtained from a site survey with total station which is generally performed when setting up a scanning lidar device. As for h_{tide} , we acknowledge that this is potentially subject to higher uncertainties due to the location of the tidal gauge ($\pm 0.51\text{m}$, Table 4) and furthermore may not be available in all cases. An alternative is to use a tidal model to estimate the tidal height. This is mentioned in the discussion section:

If in-situ tidal measurements are unavailable, the SS method could also be combined with a tidal model to account for tidal height particularly for near-shore deployments with a large tidal range.

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.

Reply: We assume that since the lidars are installed on a static surface as opposed to, for example, a turbine transition piece, variations in the device tilt are expected to be small. We have added the following text after equation 7 (now equation 9) to clarify:

Consequently, the estimates of Φ_e from this method describe the total elevation angle error due to both the external misalignment and internal misalignment of the device.

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 by doing so?

Reply: Our method was inspired by the SSL methods [1], [2], [3] which determine several unknown parameters (static offset, pitch, roll, lidar height above sea surface) based on the range of the sea surface. As in response to previous comments we have changed the naming of the method to distinguish it from the SSL methods. The method employed removes the need for curve fitting to estimate these parameters and allows the estimates of the elevation error to be determined when the azimuthal field of view of the device is limited. We have added this in explanation of the method in Section 2.3.3:

The new method employed in this study both removes the need for curve fitting and allows estimates of the elevation error to be determined when the azimuthal field of view of the device is limited.

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 $\approx 0.125^\circ$ (amplitude of the sine curve). The internal inclinometers (uncalibrated?) are expected not to provide accurate readings.

Reply: Thank you for bringing this to our attention. As per response to Major comment #3 the 0.05° pitch/roll constraint was originally implemented as the internal inclinometers suggested these levels, and since the azimuthal range of the scans were relatively limited, this was used to help determine the elevation error from the SSL method. However, we agree with the reviewer that the sine curve indicates that the device is tilted. Since the internal inclinometers are indeed uncalibrated and the limited azimuthal range of the scans penalises the extended SSL method, we have removed the method from the paper.

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.

Reply: As explained previously in response to comment #16, we have implemented Meyer et al. 2026 method to determine the sea-surface range as a sensitivity study and show the result in Appendix B1, Figure B2.

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.

Reply: -2° to -1° was a typo in the sentence on Line 280+ and has now been corrected to the correct angle range of -2° to $+1^\circ$ degree which were used for the RHIs (c.f. Table 2, now Table 3). This angle range was selected based on visual inspection of CNR decay with range across different elevation angles during test scans. Angles steeper than -2° were excluded due to local beam obstruction. Because of the positive elevation angle errors inferred for both devices from the NOAH hard target, an upper limit $+1^\circ$ was set for both lidars for consistency, although data were filtered for the range -1.5° to 0° based on curve fit parameter distributions shown in Figure 9. Our interpretation of Figure 6a from Gramitzky et al., (2026) and associated descriptions is that the sensitivity analysis presented is of the error in the modelled *static elevation offset* Φ_{offset} as a function of elevation angle sector range, for a constant range error of $\frac{1}{2}$ probe length and fixed lidar height of 20m. Their result suggests that lower errors in Φ_{offset} are present for sectors spanning shallower elevation angles (see their lower error for RHI1: -1.5° to -0.3° , versus higher error for RHI2: -3° to -1.5°) and not necessarily across a larger elevation angle sector size. The RHI setup presented in our study covers the elevation angle range associated with the lowest errors in their Figure 6a. In the updated manuscript, based on the parameter distributions in Figure 9 (originally Figure 10) we also further filter the sea surface estimates of r_{ss} and the resulting elevation error Φ_e estimates based on a valid ranges of $-1.5^\circ \leq \Phi_{\text{lidar}} \leq 0^\circ$ (SL South) and $-1.5^\circ \leq \Phi_{\text{lidar}} \leq 0^\circ$ (SL North) since across this range r_{ss} and Φ_e have smaller uncertainties.

We have added the following text describing the elevation angle ranges chosen.

Section 2.3.3: Based on these distributions, valid elevation angle ranges of $-1.5^\circ \leq \Phi_{\text{lidar}} \leq 0^\circ$ for SL South and $-1.5^\circ \leq \Phi_{\text{lidar}} \leq 0^\circ$ for SL North associated with lowest uncertainties in r_{ss} and Φ_e are used to further filter the scan data when presenting final results and statistics of Φ_e . A similar elevation angle range has also been shown to be less sensitive to r_{ss} errors and the effects of waves (Gramitzky et al., 2026).

Section 2.3.4: An elevation angle range of $-2^\circ \leq \Phi_{\text{lidar}} \leq 1^\circ$ were used for the RHI sea surface scans selected based on visual inspection of CNR decay with range across different elevation angles during test scans. Angles steeper than -2° were excluded due to local beam obstruction. Because of the positive elevation angle errors inferred for both devices from the NOAH hard target, an upper limit $+1^\circ$ was set in both lidars for consistency.

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.

Reply: Thank you for bringing this to our attention. We agree this was misleading and we have changed our terminology throughout the manuscript by avoiding conflation of the terms static offset ϕ_{off} and elevation error ϕ_e . The original sentence on Line 281 has been removed while re-arranging the method section to distinguish between the measurement setup and methods as per the request previous in comment #15.

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.

Reply: The range resolution was limited by the maximum possible number of range gates (319) for the probe length settings implemented, while ensuring at the same time the range extended far enough to hit the sea surface at shallower elevation angles. We agree that this adds additional uncertainty in determining inflection point, particularly at near ranges (see SL North). However, we specifically account for this by excluding sigmoid fit data where sea surface range error ($\sigma_{r_{ss}}$) from the curve fit exceeds 20m and including this uncertainty in the analytical uncertainty formulation using a constant sea surface range error of 20m (see Table 4). We also relax this constant range error to directly use the varying uncertainty per r_{ss} ($\sigma_{r_{ss}}$) estimated from the curve fit (see Table 7). We have added the following text to describe these aspects.

Section 2.3.4 Scan Setup: A range resolution of 20m was chosen for the SS scans based on the maximum possible number of range gates (319) for the probe length settings implemented, while ensuring at the same time the range extended far enough to hit the sea surface at shallower elevation angles. Uncertainties in the sea surface range determined by the method due to the 20 m range resolution are accounted for by excluding sigmoid curve fit data where the sea surface range error $\sigma_{r_{ss}} \geq 20$ m and incorporating a fixed sea surface range error of 20 m in the analytical uncertainty formulation (see Section 2.4 and Table 4).

Section 3.3 Sea Surface: In contrast, the mean analytical uncertainty estimates u_{ϕ_e} are larger with mean values of 0.05° (PPI) to 0.09° (RHI, SL South). These estimates are higher than values of approximately 0.03° reported for uncertainty estimates of the static elevation offset using the SSL approach offshore (Gramitzky et al., 2026) and at a coastal site under controlled conditions with minimal tidal effects (Meyer et al., 2026a). However, they are attributable to the conservative $u_{r_{ss}} = 20$ m fixed across all scans to account for the scan range resolution setting of 20 m, which therefore has a proportionally larger impact at shorter r_{ss} at steeper negative elevation angles. If this fixed constraint is relaxed to vary by $u_{r_{ss}} = \sigma_{r_{ss}}$ calculated from the sigmoid curve fit for each estimated r_{ss} , the mean uncertainty estimates reduce most notably for the RHI scans from 0.09° to 0.07° for SL South, and from 0.07° to 0.06° for SL North.

Discussion: It is acknowledged that implementing a shorter range resolution for sea surface scans compared to the resolution of 20 m used in this experiment may reduce the uncertainty of the method particularly at near range.

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.

Reply: The source of the uncertainty estimate for the SSL Ext + Tilt method was originally described on lines 349-351, i.e. it was given by the standard error of ϕ_{off} estimated from the covariance matrix of the curve fit i.e. the fit to equation (8). We have however now removed this method (and uncertainty) from the results.

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

Reply: Temporal variability in the external lidar levels means variations in the external lidar level (pitch and roll) with time. However, we have removed reference to “temporal variability” to simplify and avoid causing any confusion. The sentence now reads

In general, $u_{\phi_{lidar}}$ is dependent on the stability of the scanner head, driven by the external lidar levels and the internal alignment of the scanner head.

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

Reply: The drone hover stability was not additionally verified by the total station due to the difficulty in continuously measuring the same drone position throughout a flight using the total station scope. Applying the standard deviation to the position determined by the RTK, in addition to other RTK positioning errors, including manufacturer specification, is expected to sufficiently capture these uncertainties.

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.

Reply: We have combined the hard target maps from Figs 4 and 5 into a single Figure A1 in the appendix. In the main manuscript body Fig. 4 now only show hard target maps of NOAH derived from both lidars.

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

Reply: To help cut down on the length of the main body this table has been 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.

Reply: As explained in the text, the fit results in Fig 6 have a high uncertainty for SL North due to the limited azimuthal distribution of the available hard targets. In this case it is not recommended to infer tilt from the amplitude. However, if NOAH is included in the fit for SL North the tilt is much lower ($A=-0.03^\circ$). The fit is more reliable for SL south and does indeed indicate some tilt to the advice. This is now discussed in Section 3.4, also in response to comment #45.

31. Fig. 6: It seems for SL South the hard target S1 at $\theta_{\text{lidar}} \approx 300^\circ$, 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?

Reply: Could the reviewer be referring to a different hard target? The offset for target S1 is of similar order of magnitude to the adjacent points S2 and S3 for which the elevation offset decreases clockwise with increasing azimuth, in-line with the sinusoidal variation across all targets.

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.

Reply: On line 337, the drone hover stability (and positioning uncertainty) is referring to the flight stability determined throughout the flights calculated from the 1s RTK positional output. The reference azimuth standard deviation quoted in Line 396 (0.2°) was the upper value of the reference azimuth standard deviations given in Table 7. The reference azimuth standard deviations quoted in Table 7 are the standard deviations of the N reference azimuth angles obtained from the N independent drone acquisitions by the lidar for a given location and height(s). These therefore indicate variations in the drone position for a given location (D1/D2/D3) across all flights as opposed to the stability of the drone during a flight. To help clarify this we have changed reference to flight 'stability' here to 'variation'. In general, for most acquisitions this value is $<0.1^\circ$ and values above this occurred if the drone position at the time of lidar acquisition varied slightly between flights. We have re-written the sentence (now in Appendix A alongside the Table based on comment # 29) to explain this.

The standard deviations of the reference azimuth, σ_{ref} and reference elevation, $\sigma_{\phi_{\text{ref}}}$ indicate variation in the horizontal and vertical position of the drone between each N independent lidar acquisitions of the drone at a given flight location/height across all flights. Note therefore that these values are

reflective of variations between snapshot acquisitions of the drone and do not indicate the continuous hover stability of the drone, which was determined to be ± 0.1 m (Table 4).

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

Reply: Fig. 7a,b,c have been moved to Appendix.

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

Reply: Thanks for noticing - this was a typo in the table; $\sigma_{\phi_{\text{off}}}$ for SL South at Ref (NOAH) is 0.01° and the table has been corrected.

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 $\phi_{\text{lidar}} = -2^\circ$, $r_{\text{ss}} \approx 300\text{m}$, whereas at $\phi_{\text{lidar}} = -0.2^\circ$, $r_{\text{ss}} \approx 1000\text{m}$, 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 $\phi_{\text{lidar}} = -2^\circ$ and $\phi_{\text{lidar}} = -0.2^\circ$) and show why the R^2 is so inherently different.

Reply: Thank you for noticing and highlighting this problem which was overlooked. To solve this, we re-processed the full dataset changing the fit R-squared to be calculated only across data up to the inflection point + 100m. We added 100m to this to include the full drop of the sigmoid, but not enough to introduce increased variance in the data from the noise after the drop. In doing so, the r-squared variation with ϕ_{lidar} is now much higher from -2° then begins to reduce as the beam becomes near-horizontal (see new version of Fig 10, now Fig. 9 in revised manuscript).

We have also updated the text originally on Line 446 so that it is clearer that the distributions of R^2 and fit σ_{rss} are being used to examine the fit quality and therefore the elevation angle range over which the r_{ss} is reliable for determining ϕ_e :

Overall, the distributions of the goodness-of-fit parameters R^2 and fit σ_{rss} indicate the elevation angle range at which the r_{ss} obtained from the sigmoid curve fit method is reliable for estimating ϕ_e .

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

Reply: This appears to have been an artefact of calculating R-squared out to a very large range after the inflection point of the sigmoid. In the updated results (see response to comment #35 above) this behaviour has disappeared.

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 σ_{rss} is misleading.

Reply: Assuming this comment is in reference to Fig 10 panels c) and d), the median fit σ_{rss} plotted is not the actual standard deviation of r_{ss} , but is the median value of the uncertainty in r_{ss} (σ_{rss}) output from every sigmoid curve fit in the dataset. The median value of this variable as function of elevation angle across all curve fits in entire dataset is shown to illustrate that in general fits become more uncertain at near-horizontal elevation angles. A threshold of 20m is applied to the curve fit uncertainty of r_{ss} (σ_{rss}) to remove measurements of r_{ss} with a higher uncertainty in the data.

The mean +/- 1 standard deviation of the r_{ss} obtained from all curve fits at a given elevation angle is shown as shading in Fig 10 g) and h) to transparently show variation in r_{ss} across the different

elevation angles. At near-horizontal elevation angles there is a larger variation which can be attributed due lower data population.

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?

Reply: This is possible however we were able to check this by increasing the PPI sample sizes by including data from all PPI line-of-sights within 1 degree azimuth of NOAA in the figure (now Figure 9). There are still sample size variations between PPI and RHI for both SL South and North, but now there are more PPI data. The difference in elevation offset for PPI vs RHI is so systematic at SL South that we expect it is associated with the measurement as opposed to sampling variations. If the issue was solely due to sampling differences, we would expect to see a similar variability between the results from the two scan types from both lidars.

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

Reply: Figure updated with a larger fontsize.

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

Reply: This was an artefact of outliers associated with very low data population as the beam becomes very near horizontal, i.e. nearly no longer hitting the sea surface. When updating the filtering method (based on comment #35) to calculate R-squared using data up to just after the inflection point of the sigmoid (instead of the including the full tail), these data points are removed from the filters ($R^2 > 0.8$ and curve fit $\sigma_{r_{ss}} < 20$ m. The updated results (now Fig 9) now show no valid data points $\phi_{lidar} > 0^\circ$ for SL North. The noisier nature of the mean r_{ss} at ϕ_{lidar} just below 0° is due to very low data populations at this point. We also now improve the overall filtering by calculating final statistics in Fig 11 onwards only using an elevation angle ranges of $-1.5^\circ \leq \phi_{lidar} \leq 0.2^\circ$ for SL South and $-1.5^\circ \leq \phi_{lidar} \leq 0^\circ$ for SL North. See also response to comment #22.

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.

Reply: We agree that the continuous change in elevation may contribute to some scatter, but it is not clear from the figure if it is specifically due to RHI only since in the figure the variance in the ϕ_e scatter (identified by the error bars showing 1 standard deviation around the mean) is very similar for both methods (PPI/RHI). In theory a similar impact would be expected for PPI scans which are also scanning continually in azimuth. We have however included this explanation in discussing possible causes of the differences when referring to Fig 11 (now Fig 10):

One contribution to the variance in ϕ_e observed between the two scan types employed is the continuous variation in the device elevation (RHI) and azimuth (PPI) angles during the scan accumulation time, causing r_{ss} to vary with each pulse.

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.

Reply: We believe this was due to issues in reliably estimating the offset from measurement data over a narrow azimuth range which was limited by the scan geometry. As mentioned in previous comments, the result from these methods have been removed.

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.

Reply: The basis of the elevation angle ranges chosen, now explained in the new Section 2.3.4 Scan Setup, are similar to the starting and ending elevation angle range of Gramitzky et al. (2026). In the updated manuscript, we filter the RHI elevation angles to -1.5 to 0.2 degrees (SL South) and -1.5 to

0 degrees (SL North) based on the parameter distributions shown in Fig 9. Since the SSL method has now been removed from the manuscript, the sentence originally on Line 483 has been removed. See also response to comment #22.

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 rss 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.

Reply: The height of the deployment above mean sea level is a constant value whereas the effective height varies depending on the tidal/water levels. As mentioned in previous comments the deviations in the fits are due to the limited azimuthal range of the scans and we have therefore removed the SSL results from the manuscript. We have updated Figure 13 (now Figure 12) showing only the reference effective height calculated from combining the lidar height above mean sea level obtained from site survey with the sea level height from the nearby tidal gauge.

45. Line 512: Are the internal inclinometer readings calibrated so that they actually indicate a tilting to the horizontal plane, or are these the internal standard inclinometers coming with the devices? If so, why do they show $\approx 0^\circ$ pitch and $\approx -0.03^\circ$ 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 $|A| = 0.12^\circ$?

Reply: Thank you for highlighting this. The internal inclinometers are the standard inclinometers that come with the devices and therefore are uncalibrated. However, the internal inclinometers were used to level the devices during installation which is standard procedure for the devices. Based on the amplitude of the sine curve fit result in Fig. 6 (now Fig. 5), the device indeed may be tilted. We have included the following text describing the tilt in reference to Fig 12 (timeseries) and Fig 5 (sine curve).

Section 3.4: The internal inclinometer measurements from both lidars indicate that the level of the devices remain stable throughout the entire test period (pitch/roll $\approx 0^\circ$), however it should be highlighted that these measurements are uncalibrated. The sine curve amplitude $A = -0.11^\circ$ for SL South obtained from the HTM (Figure 5) suggests that SL South is slightly tilted. In contrast, the sine curve fit from HTM for SL North estimated $A = -0.20^\circ$ (Table A1). However, this fit is associated with high uncertainty due to the uneven azimuthal distribution of static hard targets available for the device. If the NOAA reference target is included in the estimation of the sine curve at SL North the result $A = -0.03^\circ$ (Table A1) suggests minor tilt for this device.

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?

Reply: We have removed the reference in this sentence to “time intensive” as it is quite subjective. The time is highly dependent on the extent of drone testing required with respect to the drone flight duration capacity (i.e. battery), number of flights, lidar scans, and weather conditions, which also effect the maximum flight duration. In this study the drone testing was conducted over a period of ~ 3 days. However, combined with setup time, it is possible to perform 3 flights with multiple lidar acquisitions in half a day (e.g. Table 3). We suspect it may be possible to repeat the drone method remotely with a remote drone but we do not have experience with this. We have modified the sentence slightly with the following to elaborate:

The method is limited by site dependent flight restrictions, weather, drone flight duration, and requires more personnel (drone and lidar operators) to execute. However, under favourable conditions it is possible to execute the method with multiple flights and acquisitions within a single day (e.g. Flights #1–#3 and #4–#6, Table 2).

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

Reply: Thank you for this suggestion. But we disagree with doing so as the main aim of Figure 15 (now Figure 14) is to highlight in the discussion the impact of the elevation angle errors obtained from the main results on the error in measurement height and therefore horizontal wind speed measurements.

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?

Reply: This is a very good point and also raised by Reviewer 1. Although this specific sentence did not explicitly attribute the cause of the difference to be due to the different scan head designs, it was mentioned later on in Line 621-622 and also in the results section. The marked difference between the elevation errors determined from sea surface RHI versus PPI scans for SL South was unexpected. Since the methodology applied to both devices is the same, but the main difference between the two devices is the scan head design, one possibility is that there is an additional scan head control error in this device introduced during the sea surface scan sequence that was not accounted for by the recommended backlash settings. Unfortunately, we did not test consecutive RHI versus PPI scans on the static hard target and drone methods which may have helped provide an answer. For the static hard target and drone methods, multiple PPI scans at different elevation angles were used. These scans therefore effectively use the same scan strategy as the PPI sea surface scans (but scan through more elevation angles), but the resulting elevation error still falls in between the sea surface RHI and PPI scans and is not the same as the sea surface PPI scans, which would be more expected. For all PPI scans used for static hard targets, drone method, and sea surface method, the same redundant fixed scan was used to account for backlash.

We have elaborated on this after the sentence originally on Line 567 :

As shown in the results presented here, the SS method exhibited differences between the two different lidars tested, with SL South exhibiting a pronounced difference between the elevation error estimates obtained from RHI and PPI scans. Since the methodology and scan scenario applied to both devices was the same, one possible cause is that an additional scan head control error in SL South was introduced during the sea surface scan sequence that was not accounted for by the recommended backlash settings implemented. This may stem from the different type of scan head mechanism (e.g. belt driven for SL South versus gear driven for SL North), and firmware/software versions depicting scanner head control (Table 1). However, a control error was not observed while performing the multi-elevation PPI scans for both the hard target and drone methods. Since it was not possible to verify this further, the cause is unknown.

We have also changed the specific attribution originally on Line 621-622 to:

The source of this difference is unknown, although one possibility is that an additional control error was introduced during the sea surface scan sequence, stemming from the different scan head design and software/firmware of the system.

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?

Reply: We did not examine this in detail but during the SHT scans the range of both lidars with respect to the true target ranges did not indicate any apparent offsets. The range the drone acquired by SL South during the drone test also did not show any apparent offset.

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

Reply: We have not been able to estimate the elevation offset (error) at very high elevation angles such as 60° so would hesitate to comment on this. At high elevation angles the relative contribution of the elevation angle error to the error in the measurement location/position would be much

smaller. In the context of this study we are interested in quantifying it for very small angles, i.e. near-horizontal beams where it is critical for ensuring the device is measuring at the correct /desired height within the boundary layer.

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].

Reply: Thanks for raising this. It was not explicitly mentioned but backlash effects were also accounted for in HTT and drone method scans using the same approach applied to the scans implemented for SSL method. The text has been updated to the following to clarify this in the new Section 2.3.4 Scan Setup:

For all scan types and calibration methods the effects of backlash were accounted for by recommendation from the lidar manufacturer. Additional redundant fixed beam scans of 1 s duration with angular offsets of -0.5° for SL South and -0.1° for SL North applied to the initial θ_{lidar} and ϕ_{lidar} were programmed prior to the start of a given scan (PPI/RHI) to minimise the effect of mechanical backlash on the pointing stability of the respective scanner heads during the tests. The magnitudes of the angular offsets applied to each device differed due to the different scan head mechanisms of each device (Table 1).

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.

Reply: Thank you for highlighting this. By accuracy we mean how close the elevation angle of the lidar is to truth. We have added a clarification of these terms to Section 2.1:

By definition, θ_e and Φ_e measure the accuracy (i.e. closeness to truth) of θ_{lidar} and Φ_{lidar} with the uncertainty of these quantities dependent on the specific calibration methodology implemented to estimate them.

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