Reply on RC1

General comments:

The paper introduces a method for the calculation of wind profiles from RHI scans compares the retrieved wind with wind retrieved from DBS scans and surface-near in-situ measurements. A focus is on turbulence parameters. The authors implement a retrieval for vertical wind profiles of horizontal wind and horizontal TKE. The overall procedure is clearly described. However, I think there is a lack of justification for design decisions at some crucial points. I recommend to either use the methods established in the literature, or indicate clearly why established methods fail or are at a disadvantage for your purpose. DBS has advantages and disadvantages, but is not generally at a disadvantage. Perhaps you can specify the situations/use cases your method is designed for. Please distinguish between the scan patterns and the retrieval techniques. Some retrievals might be specific for a scan pattern. I am not aware about the retrieval methods the manufacturers use for DBS evaluations, but most likely they do not use the retrieval method you assumed for DBS as there are more robust methods that can be generally applied for measurements at the same height (or with height binning). However, they are usually not able to make turbulence estimations. This can justify your method for turbulence estimations. The blind zone can justify using RHI scans if the wind close to the ground is under investigation. In case you consider just wind profiling, established methods (with binning) would also work; in case you also consider TKE, they won't work. When you make comparisons between two methods (or your method and a reference method), please ensure that the application case is clear.

First of all, we would like to thank the reviewer for his/her thorough assessment of our paper and for all the relevant comments that helped us improve the manuscript a lot.

We initially used the old wind reconstruction method for the DBS because it is still implemented in the software of the Leosphere/Vaisala lidars, and there are still many end-users who do not reprocess the observations, because they are not willing or not skilled enough to implement an optimal estimation (OE) wind reconstruction method, or familiarize with the software packages that have recently become available. However, we understand the reviewer's remark and agree that the method we present should be compared with a state-of-the-art DBS reconstruction method. We therefore reprocessed the DBS observations using an OE method, replotted the figures and modified the text accordingly (the postdoc who took care of the reprocessing was also added as a co-author). The OE wind reconstruction, however, did not bring any improvement for the Paris dataset, as the DBS measurement periods on this site were too short. Paris remains interesting, however, to have a continental hilly site for the TKE discussion.

About the application case that led us to use this peculiar wind reconstruction method, the choice was made in the framework of our studies about turbulent coherent structures in the surface layer, for which we need both to fill-in the blind zone near the ground and get the TKE profile. Indeed, we detect the turbulent coherent structures using quasi-horizontal PPI scans (Cheliotis et al., 2020, in Paris; Maynard et al., 2025 in Dunkerque), so that we need a wind and TKE profile starting from the lidar altitude. Another reason to use a method than can both fill-in the blind zone and provide the TKE profile is that observations from this same lidar are also used to work on power/fatigue predictions for wind turbines and on low-level jets, which can be located at very low altitude in Dunkerque. We exposed more clearly this application case (no blind zone plus TKE profile) in the abstract, the short summary, and the introduction.

About distinguishing the scan pattern and the wind/TKE reconstruction method, neither of them had been properly christened – for instance, Bonin et al. (2017) just entitled the section about this method "RHI scans and vertical stares". Therefore, we propose to use the name we introduced ("cross-RHI") for the scan pattern, and call the wind/TKE reconstruction method *(tilted) Layers Velocity Averaging (LVA)*. This name has been introduced wherever necessary in the text and figures' legends.

Detailed comments:

Line 14, 91, 169: From my point of view it is not clear why DBS is generally more noisy than RHI. If you have a longer accumulation time and fixed beam positions, I assume measurements are less noisy than in continuous RHI measurements.

End-users who do not want to or cannot perform the wind reconstruction themselves will use the pre-programmed DBS cycle in the lidar software. In the Leosphere/Vaisala lidars, the default configuration is to use short accumulation times (1 s per beam) and rotate the optical head at full speed to record the five beams of the DBS cycle ; several consecutive cycles can then be programmed if some time-averaging is needed. In this manner, a lot of observation time is lost in reorienting the optical head. It is of course possible to use a custom measurement configuration with longer accumulation times, but we are not sure this is what most end-users do. We made the sentence more specific (line 101):

"DBS wind profiles recorded and processed using the manufacturers' software may be noisy"

Line 18: Please clearly indicate what the (new) method is doing before you describe what it takes into account.

In the abstract (lines 15-16), we replaced the statement about noise by a clearer definition of the application case for this method, i.e. TKE profile plus no blind zone:

"However, the DBS scan leaves a blind zone near the ground that makes it difficult to observe very low-altitude phenomena such as certain low-level jets, and it does not allow to retrieve the turbulent kinetic energy (TKE) profile. When the absence of a blind zone and the TKE profile are both required, observations from two vertical sweeps of the Range-Height Indicator (RHI) type can be used…"

Section 1: The information you provide about radars might not apply for all types of radars. Please add the type of radar.

Indeed, the range of Doppler radars is very variable, and depends a lot on the frequency band used. We removed the range values on former line 53 and modified the sentence on lines 51-53 (formerly lines 49-52) to mention this point:

"Doppler radars […] allow to "see" through the clouds and up to the free troposphere (sometimes even reaching the lower stratosphere, depending on the frequency band), while Doppler lidars […]. Doppler sodars [have] a range typically limited to the PBL."

There is a general ambiguity in the usage of the "VAD" term in the literature. Sometimes it is related to the scan type, sometimes to the retrieval technique, sometimes combinations. Please ensure the clarity of what is meant when you're using the "VAD" term.

This sentence was changed to make it clear that PPI refers to the scan pattern and VAD to the wind reconstruction method (lines 76-77):

"Originally, wind profiles were reconstructed using the Velocity Azimuth Display (VAD) method, using observations from a Plan-Position Indicator (PPI) scan, where the beam performs a conical sweep at a fixed elevation from the ground"

Line 87: Why must the Doppler lidars be positioned on a rooftop? Are 75° DBS scans not possible? Is it related to RHI scans from 0°?

As said in the answer to the general comments, observations from the Doppler lidar in Dunkerque are used for different applications, among which studies about turbulent coherent structures in the surface layer. The structure detection relies on quasi-horizontal PPI scans, hence the need for a clear view over several kilometers around the lidar, which is only possible by raising the instrument. From the wind/TKE profiling perspective, if the objective is to eliminate the blind zone near the ground, then it is necessary to start the RHI scans at 0° elevation, which again supposes to raise the instrument.

Sometimes you use "single-Doppler cross-RHI", sometimes "cross-RHI". Why is it called single-Doppler? Just because it is not dual-Doppler, which is a special application?

This "single-Doppler" prefix was added to make the transition with the previous paragraph, where dual-Doppler was also evoked, but we removed it as it may be confusing. We also introduced the distinction between the scan pattern and the wind reconstruction method (lines 124-125):

"In this article, we consider the first approach, that we can name cross-RHI for the scan pattern and Layer Velocity Averaging (LVA) for the associated wind reconstruction method."

Steinheuer et al. (doi:10.5194/amt-15-3243-2022) discuss some more scan patterns for similar application cases.

We modified the sentence about optimal estimation (lines 85-89) to precise this method can apply to DBS or PPI scans with a varied number of beams:

"Over the last decade, more performing wind reconstruction methods based on least-square fitting using optimal estimation (OE) have been introduced, which can be applied to PPI scans with varied azimuthal resolution, from the simple DBS to the full conical sweep (Baidar et al., 2023; Steinheuer et al., 2022). Aside from minimizing the retrieval error, OE methods include a more efficient filtering of the observations, enabling a more accurate reconstruction of the wind in turbulent conditions and an increase of the wind profile vertical range."

Line 120: Please explain this issue. Why can vertical wind be neglected at 45° elevation angle?

As the radial wind is u or v*cos(elevation)+w*sin(elevation), it is clear that neglecting the w term supposes to have an elevation angle that is not too large. However, none of the previous papers (Banta et al., 2002; Bonin et al., 2017; Pichugina et al., 2008) does define what "too large" means so they do not detail why this precise 45° threshold was chosen, e.g. using orders of magnitudes on u, v and w. We added this point to the sentence (now lines 131-133):

"These studies limited the elevation of the RHI scans to ~45° to safely neglect the contribution of the vertical wind component to the radial wind, which limited the extension of the reconstructed profile to ~600 m, though the choice of this precise threshold value was not justified."

In our case, the 0.2 threshold on the sine value is empirical, and possibly site-dependent, as it was set by observing the time-height cross-sections u and v computed while neglecting the w contribution (such as Fig. S2b or S5a,b). If the exclusion cone is not wide enough, then u or v near the zenith will exhibit deviations from their average in the quasi-horizontal layer.

Line 180: Is the -27dB threshold recommended for both instrument types? You could validate that with a CNR to radial-wind-speed distribution like carried out by e.g. Zentek et al. 2018 (doi:10.5194/amt-11-5781-2018) in AMT (Fig. 5).

The scatter plot of CNR-*vs*-radial wind speed values that we get for our Leosphere/Vaisala lidar (new Fig. S1) is very different from the one that Steinheuer et al. (2022) or Zentek et al. (2018) get for their HaloPhotonics instruments. The Leosphere lidar has a discontinuous CNR distribution, with a clear separation between a cloud of good-quality measurement points above −27 dB, and a very dispersed cloud of bad quality data points below this threshold. Flexible threshold methods do not add data points to the retrieval in our case, which is why we sticked to the threshold prescribed by Leosphere. We added that information and some references for discussion in Sec. 2.2 (line 193-199):

"For all types of sweeps, individual data points were first filtered using a threshold on the Carrier-to-Noise Ratio (CNR). The chosen threshold value depends strongly on the measurement environment, particularly the atmospheric aerosol load at the observation site. This variability is reflected in previous studies: for example, Pearson et al. (2009) applied a threshold of −23 dB, whereas Päschke et al. (2015) used a more conservative cutoff at −18.2 dB. In the present study, a CNR threshold analysis was performed using the Dunkerque DBS dataset, representing a total of 579,600 measurements points. The CNR-vs-radial wind speed density plot (Fig. S1) show that there was a clear gap between the "good" and "bad quality" clouds of points, that respectively fell above and below a CNR threshold of −27 dB, that is the value advised by the lidar manufacturer."

Line 188: How do you define good-quality observations?

It means points that are above the CNR threshold, below the radial wind dispersion threshold, and within the radial wind speed range allowed by the manufacturer. We modified the sentence to clearly state that the 2D filter is applied after individual point filtering (now lines 206-208):

"After this first filtering step on individual data points, a two-dimensional filter was applied to the RHI scans to remove isolated points in the sweep: only points having at least three neighbors with observations passing the individual data-quality filters were retained"

Line 194: A more detailed explanation about the range-fold echoes (e.g. in the caption of S1) would help to follow in the figure. Maybe you can support the understanding by showing the associated CNR or Doppler spectrum width plot if there are significant differences between regular measurements and range-folded measurements. I understand that filtering these echoes is very difficult and sometimes maybe not possible. However, I do not understand how the filter applied addresses the range-folded echoes. You apply a filter removing measurements where not enough valid measurements (according to the CNR?) are in the surroundings. It probably does not work for range-folded echoes at high elevation angles, since the echo will not appear in this "whiskers" and more neighbouring measurements are flagged as valid. Additionally, it does not work, if the range-fold echoes are mapped to areas, where also real measurements are available. In Fig. S1, range fold measurements are only filtered if there was no measurement from the neighbour beams in the surrounding. When highlighting the removal in these areas, you should also discuss the other cases. A further step follows in line 233.

We do not save the Doppler spectra on a routine basis, as this generates a huge amount of data, so instead of adding a figure, we extended the explanation of the range-folded echo phenomenon. As said above, we precised that the 2D filter is applied after the individual-point data-quality filtering. And you are right, the 2D filter works very well for middle elevation beams, while cloud echoes for high-elevation beams have to be filtered at another step, and we also added a precision about that. The new explanation is now at lines 211-222:

"A range-folded echo appears when an elevated cloud is located beyond the lidar maximal range, which is limited by the pulse repetition frequency of the laser: the light emitted during a given laser pulse and backscattered by the cloud returns to the lidar after the next pulse has been emitted. In that case, the signal from the elevated cloud adds to the signal from a lower-altitude air layer, so that a peak corresponding to the speed of each air layer appears in the Doppler spectrum. As the cloud backscatters much more light, the associated peak in the spectrum is more marked and is naturally selected by the instrument software (which assumes that all returning light comes from the last emitted pulse). As the wind speed values existing in the upper free troposphere are often much higher than in the PBL, range-folded echoes tend to produce abnormally large wind values in the PBL. Besides, as the beam moves away from the zenith, the cloud is located ever farther beyond the lidar maximal range, so that the fold signal from a horizontal cloud layer will form a 'whiskers' pattern of outliers on the RHI sweep image (Fig. S3.a of the supplement). The isolated-point filter removed the cloud signal for cases when it appeared in a layer devoid of clear-air signal, i.e. for middle elevation beams (Fig. S3.b). For beams close to the zenith, the cloud echo falls at low altitudes and the corresponding points are not isolated, so a second step of filtering is performed at the layer averaging step (Sec. 2.3.2)."

Line 205: What does it mean "the contribution of the vertical wind is canceled"? With at least three beams from DBS pointing in different directions, the 3D wind vector can be calculated. What is the DBS retrieval? Is it the proprietary software developed by the manufacturer? Even though the implementation is unknown, I would expect they calculate the 3D wind vector with a least squares fit like many others (e.g. Päschke et al. 2015 doi:10.5194/amt-8-2251-2015). Later, the term "usually" is used. Please clarify, what typical retrieval technique you refer to.

According to the technical documentation, the Leosphere/Vaisala software still uses the linear beam combination reconstruction to process the DBS, which we confirmed by comparing the wind profiles from the lidar software and from lab-made reconstructions (linear beams combination or OE). We are not aware of recent developments regarding the software of HaloPhotonics lidars, or of other manufacturers, but there are still a lot of Leosphere lidars in use.

As explained in the answer to the general comment, our original choice was to use the wind reconstruction that is implemented in the lidar software, but we changed it for an OE retrieval method, at least in Dunkerque where there was enough data available for this method to properly work. The new DBS dataset is described on lines 223-230 (see also the answer to the comment on Section 3). The sentence "the contribution of the vertical wind is canceled" thus referred to the old beam linear combination DBS retrieval, and has been modified as (lines 245-249):

"To process the DBS, but also the PPI sweeps, all wind reconstruction methods suppose that the wind field is horizontally homogeneous during the scanning period. Under this assumption, the projection of the vertical wind along the beam direction is the same for all beams at the same elevation, so that it can be fitted (for the new OE methods) or cancelled by computing the difference between beams recorded in opposite directions (for the ancient DBS reconstruction method)."

Line 209: I do not understand, why a DBS retrieval should produce noisy u and v in case of high w, if not all beams are close to the vertical.

This comment concerns the old DBS retrieval method, which we precised in the text. Also, when we wrote "apparently 'noisy' results", we meant that the wind profiles *look noisy* because of the atmospheric fluctuations caused by turbulence, not that they are noisy in terms of instrumental noise or retrieval uncertainty. We therefore rewrote this part of the text (lines 252-258):

"In the ancient DBS reconstruction method, w was thus poorly corrected in the convective boundary layer, which caused fluctuations in the retrieved u and v. In the OE reconstruction methods, atmospheric fluctuations caused by turbulence increase the fit residual, thus the uncertainty on u and v, but the fit process still allows a more accurate retrieval of the large-scale wind. Whatever the wind reconstruction method, time averaging is used to compensate for the effects of turbulence, by recording series of consecutive DBS scans or by increasing the accumulation time for each beam. However, this possibility is limited when the measurement scenario has to include other types of sweeps, for instance horizontal scans to observe turbulent structures."

Section 2.3.1: I am wondering why you deviate from the standard least-squares fit wind vector retrieval method using pseudoinverse/svd submatrices. Perhaps, the reason is your space averaging instead of time averaging, which is introduced very shortly in line 220. If that is the reason why you are using another method, this should be indicated more early. Additionally, the advantages and disadvantages compared to the state of the art should be discussed. From my point of view, calculating a 3D wind vector least-squares fit works as well for the layer binning displayed in Fig. 1. Maybe the turbulence estimation have additional requirements. If such requirements exist and are essential for the wind vector calculation, these requirements should be introduced before.

Wang et al. (2015) indeed applied a least-square fitting OE method to partial RHI scans, but this method does not provide the TKE, which was a requirement for our other studies. Also, the method by Wang et al. (2015) requires RHI scans towards at least 3 azimuths (they even used 7, closely spaced), with a rapid repetition of the cycle of RHI scans, so as to have a sufficient number of scans in each azimuth during the 10-minute averaging period. Using so much time for the vertical profiling was not compatible with the other types of scan we needed to include in our measurement scenario. We explained that in the introduction after explaining the LVA method (lines 136-140):

"Finally, Wang et al. (2015) adapted the least-square fit OE method to RHI sweeps, but their technique relies on rapidly repeated partial scans in at least three azimuths. As a consequence, RHI scans have to be limited to low elevations and will occupy a large part of the instrument observation time. For this reason, and because methods from the OE family do not provide the TKE, the LVA wind reconstruction method was preferred in Bonin et al. (2017) and in this study."

Line 234: radial wind speeds from range-fold echoes are in many cases higher than the other observations, but not generally.

We changed the sentence, replacing "generally" by "often" and stating that it allows to filter "most of" the cloud echoes (lines 279-281):

"This made it possible to filter most of the range-folded echoes for which the spurious points were located amongst good-quality wind observations (Fig. S3.c), based on the fact that the wind values from the fold signal are often much larger as they correspond to the free troposphere."

Line 239: Is it on average really close to 230? In the figure caption you wrote, the maximum is 230.

230 points in each layer is the theoretical maximum value, given the RHI configuration. In practice, the aerosol load on both sites was most of the time sufficient for the effective lidar range to be larger than the RHI maximum range, so that 230 points was also the effective maximum number of points. This value is the color scale upper bound on Fig. S10(b) and S11(f), and we added the corresponding tick to the color bars. The sentence in the article body was modified accordingly (now lines 284-285):

"The typical number of good-quality data points was close to its maximal theoretical value of 230 in the layers below 0.5 km, except for the lowest and shallower layer, where it was around 150"

Line 245: Is there a reference for the random errors from scattering particles' movement? I would expect already an averaging effect due to many laser pulses covering differences in the movement. However, moving scatterers are essential for the Doppler lidar measurement principle.

Frehlich et al. (1998) showed using high temporal resolution lidar data that the scatterers' random movement added a background level to the radial wind spectrum in the high-frequency region, compared to the Kolmogorov spectrum, leading to a small overestimation of the wind variance. These results were obtained for a high-energy single-pulse lidar, but authors working with low-energy high pulse-repetition frequency lidars also correct for this background level in the radial wind spectrum when trying to retrieve the eddy dissipation rate (Bodini et al., 2018). However, this correction is possible for DBS or six-beam scans, for which there is a large number of beams recorded with the same orientation, not for RHI sweeps.

Line 247: What was the estimation/result of Eberhard et al. (1989)?

Eberhard et al. (1989) estimated the instrument random error variance to be around 0.4 m^2/s^2 from fixed shots on a sandpaper target, for radial wind variances of the order of 2 m^2/s^2 during the observation period presented in their paper. This value, however, cannot be transposed to another instrument, and it would probably be significantly lower for current lidars. The point was more to illustrate a simple way to estimate experimentally the instrument random error variance, while more recent studies (e.g. Bonin et al., 2016; Wang et al., 2016) use the autocovariance method introduced by Lenschow et al. (2000), i.e. a statistical processing of the lidar data.

We realized that the discussion about errors was split between the last paragraphs of Sec. 2.3.2 (layer averaging) and 2.3.4 (TKE). For clarity, we have gathered all information in the TKE section, and completely rewritten this part. We tried to better present the different types of errors, also focusing on the more recent methods used to mitigate them (which excludes de facto Eberhard et al. (1989)), and explaining why these methods do not apply to RHI sweeps and why there is this need for a CNR threshold. This discussion corresponds to lines 309 to 340 of the revised manuscript:

"The TKE retrieved from a remote sensing lidar instrument differs from the one measured using an in-situ ultrasonic anemometer for several reasons, which can be classified in three categories. The first two categories concerns errors existing independently of the measurement geometry, thus affecting even the radial wind variance in a fixed beam direction, while the third category corresponds to errors arising when combining beams in different directions during the reconstruction process. The first category gathers errors that lead to underestimate the wind variance by missing the high-frequency part of the turbulence spectrum. It includes volume averaging (Doppler lidars do not measure the wind at a precise point, but within the air volume occupied by the laser pulse, which makes eddies smaller than the range resolution indiscernible) and limited sampling frequency (fluctuations at time scales shorter than the beam accumulation time or than the period of the measurement cycle are not resolved). The second category includes instrumental random errors affecting the Doppler shift retrieval, such as an imperfect laser frequency control, or in low-CNR cases, the 'speckle' effect due to the scattering particles' random movement. These errors have a zero mean but their variance adds to the 'true' wind variance, leading to an overestimation of the wind variance. Finally, the third category of errors corresponds to beam recombination effects, which occur when the air motion during the scan results in sampling the same air parcel with beams of different azimuths (Sathe and Mann, 2012).

Numerous studies since the late 1980's have proposed varied techniques to estimate and mitigate the different types of errors, but recent work have converged toward variants of the autocovariance method originally proposed by Lenschow et al. (2000). For instance, Bonin et al. (2016) applied this method to the determination of the vertical wind variance from fixed vertical shots and evaluated its performances by comparison with a sonic anemometer, concluding that it could both remove errors from the random instrumental noise, and from the volume averaging and sampling frequency limitation. However, the autocovariance method requires to have numerous series of radial wind observations with the same beam geometry, which is only possible when the lidar operates continuously in the same, short-duration scan pattern such as DBS or six-beams, and for periods of 10 to 30 minutes (Wang et al., 2016). The autocovariance method would be applicable to RHI sweeps only if partial scans limited to low-elevations were used, in order to ensure a short repetition period, but the choice made in this study was to aim for full-height profiles. Therefore, as stated by Bonin et al. (2017), who also used cross-RHI scans and the LVA method to retrieve TKE profiles, there is no straightforward way to remove the random error contribution in the LVA reconstruction.

Nevertheless, in order to minimize this contribution, it is necessary to exclude individual data points marred with a large uncertainty on the radial wind. This is done by imposing a threshold on the CNR, as the velocity uncertainty strongly decreases when the CNR increases (Pearson et al., 2009). With a lidar identical to the one used in Dunkerque, Bonin et al. (2017) concluded that the manufacturer's threshold at −27 dB was sufficient for the LVA reconstruction, which is supported by the error analysis presented in Sec 2.2. However, Bonin et al. (2017) observed that the TKE tended to be overestimated during low-turbulence periods, compared with ultrasonic anemometers. One of the possible explanations they gave was an artificial increase of the variances of the horizontal wind components due to the flow inclination, which will be addressed in the next section."

Line 248: Do you propose to "correct"/reduce the random error? Why is there a need, when it is stated that −27dB (the threshold you apply as well) ensures a sufficient data quality? Regarding the determination of the threshold, see above.

This point was dealt with while rewriting the discussion about errors (see the previous answer).

Line 254: Has the DBS wind reconstruction been introduced and you want to refer, or do you want to state that you use the same method for both?

We only meant that, once you have the horizontal wind components, whatever the reconstruction method they come from, the computation of the horizontal wind speed and direction is the same. We changed that sentence to make a better transition with the previous subsection (lines 289-290):

"The previous step provides the vertical profiles of the zonal and meridional wind components, which are then combined to compute the horizontal wind speed and direction"

Besides, we added the LVA reconstruction formulas in Sec. S3.1 of the supplement, as the other reviewer insisted we should have provided the mathematical basis, even though the latter has nothing complicated.

Supplement S2.1: Do you have only the horizontal component, i.e. elevation is 0°? From Sect. 2.3.1 (you are referring to) that is not clear. If so, do you calculate the horizontal component from the scan with azimuth in the opposite direction?

As we added the reconstruction formulas in the case of perpendicular RHI scans (Sec. S3.1), this part of the text (now in Sec. S3.2) should be clearer now.

Line 264: Is full TKE generally not possible without a series of vertical shots for at least one minute or in your specific application case?

More detailed explanation were provided on this point (now lines 301-305):

"w and its variance σ_w^2 cannot be retrieved using the LVA, as the vertical wind component is neglected in this wind reconstruction process. Besides, if the RHI beams close to the zenith (that were excluded from the LVA) can be used to estimate w, they do not provide enough information to retrieve σ_w^2. The vertical term in the TKE can only be retrieved if the lidar measurement scenario includes series of fixed vertical shots for a duration sufficient to capture the large scales in the turbulence spectrum, ideally 10 to 30 minutes (Larsén et al., 2016; Lenschow et al., 1994)."

Line 296: Why does the minimum not work and you need fminbnd? Is the presented curve usually not available and an algorithm has to figure out the minimum?

In order to plot the error curve as in Fig. S5(c,d), it is necessary to perform the wind and TKE reconstruction for an ensemble of tilt angles. This represents approximately 140 executions of the LVA reconstruction subroutine (0.1° step × 14°-wide window) while the optimization typically requires 14 iterations when it converges. Using an optimization function such as `fminbnd` is therefore 10 times faster, which we precised in the text (lines 340-342):

"The error curve, however was not plotted on a routine basis. Instead, the minimum was found using an optimization function, as this requires typically ten times less computation time."

Line 303: What are the consequences? Are these measurements rejected? Are they flagged?

These measurements are kept, but flagged so they can be removed later, if need be. These observations where kept for the wind comparison, but rejected when comparing the TKE with and without the tilt optimization. We mentioned this flagging lines 371-373:

"In this case, no optimization was performed […], but the observations were flagged so they could be filtered out later, if necessary."

Section 3: Clarify what the DBS reconstruction method is. Usually, three radial velocity measurements in three linearly independent directions are sufficient to retrieve a 3D wind vector (check line 324).

As explained in the answer to the general comments, the DBS observations in Dunkerque were reprocessed using a least-square fit OE wind reconstruction method implemented following Steinheuer et al. (2022). For the Paris site, however, the old linear-beam combination method was kept due to the insufficient amount of observations. These information have been added in Sec. 2.2 (lines 222-235):

"Regarding the DBS observations, the three wind components and their associated uncertainties were retrieved from the five-beam radial wind measurements using a least-squares fitting OE method following Steinheuer et al. (2022). Additional data filtering steps are included in this process: a point was considered as missing when the data availability at a given altitude was below 50 % within the 2.5-minute DBS measurement period, and a sigma filter with a threshold value of 1 was further applied to the data. Besides, outliers associated with abrupt changes in the DBS measurements were discarded using a filter based on the temporal wind speed absolute difference, with a threshold of 2 m s−1. In Dunkerque, with 10 consecutive DBS cycles during each measurement period, the uncertainty on the zonal and meridional wind components was typically 0.5 to 0.7 m s−1 (Fig. S2.a, b), and 0.1 to 0.2 m s−1 for the vertical wind (Fig. S2.c). In Paris, where only one measurement cycle was recorded, the least-square fit could only be performed on four points, so the OE retrieval did not provide a significant improvement compared to the beams' linear combination wind reconstruction method. The DBS in Paris was therefore processed using the old wind reconstruction method, and this site will have less weight than Dunkerque in the discussion about the wind profiles intercomparison (Sec. 3.1). The Paris observations remain interesting, however, to discuss the TKE retrieval in a continental and hilly site (Sec. 3.2)."

Line 326: What could be the reason? Did the DBS scan have more accumulation time for a specific direction?

The end of this paragraph was rephrased to improve clarity (lines 393-395):

"the LVA method is less sensitive to missing radial wind data because the reconstruction of each wind component relies on up to 230 points in each layer, while the DBS observations with the configuration used in Dunkerque provided only 50 data points at each height level, all beams altogether.

Line 335: Where can we detect the noise?

Same as the comment on line 209, it is not noise in the instrumental term, but atmospheric fluctuations caused by turbulence. We modified the sentence accordingly (now lines 401-402):

"In the morning convective PBL, the LVA method produced smoother results, less impacted by turbulent fluctuations than the DBS method, especially in this measurement scenario in which the time reserved for DBS sweeps was limited."

Fig. 2: Could you also plot the wind speed difference between (a) and (b) to make it easier to detect the differences (e.g. in the supplement).

This figure has been added to the supplement (new Fig. S9).

Section 3.1.2: Could you provide the standard deviation or variance?

We assumed that the request was to provide the uncertainties on the slope and intercept values, which were added in the text (line 427-428). For the Dunkerque site with the OE DBS retrieval, the uncertainty was below 0.01 for the slope and below 0.03 m/s for the intercept.

Fig. 4: Please add labels for the axes or add the information in the caption. For very low wind speeds (e.g. below 3m/s), high direction differences could occur, even though the differences in the resulting wind vectors are small. I would suggest removing these measurements from the direction comparison.

We replotted these figures after removing the points for which the wind speed was below 3 m/s. In the discussion, the fractions of points for which the wind direction difference was below 5° or 10° were also modified using this speed threshold.

Section 3.2.1: Describe the objective of the optimization. What range of inclinations would you expect? Generally, you should specify where your messages apply to. For example, one can understand that DBS or VAD retrievals (line 415) cannot be applied in sloping or complex terrain. I would not agree with that in general. Your statement is probably related to the turbulence estimation.

We modified Sec. 2.4 to (i) indicate that the expected inclination values are of a few degrees and (ii) explain why not accounting for the flow inclination results in overestimating the TKE.

Lines 342-344: "RHI scans recorded in Paris, and to a lesser extent in Dunkerque, may exhibit a small, but visible inclination of the flow, i.e. atmospheric layers with a similar wind speed are not horizontal, but inclined by a few degrees. This is particularly visible in the presence of a strong vertical wind shear, for instance due to a low-level jet (Fig. S5.a)."

Line 351-352 (added sentence): "if the averaging layers are tilted from the flow direction […] This will not change the average wind value, but the wind variance, and thus the TKE will be overestimated in this case."

We also added a small introduction to Sec. 3.2 (lines 474-477):

"As previously explained, the inclination of the atmospheric layers does not impact the horizontal wind retrieval, but leads to an overestimation of the TKE if it is not corrected. Firstly, the results of the optimization process introduced in Sec. **Erreur ! Source du renvoi introuvable.** will be discussed in terms of ability to retrieve the flow tilt angle, and of the angle values obtained (Sec. **Erreur ! Source du renvoi introuvable.**). Secondly, the effect of the inclination correction on the TKE values will be presented (Sec. **Erreur ! Source du renvoi introuvable.**)."

Section 3.2.2: I'm struggling with the wording. I would expect a bias of TKE_h (not inclined), if you see the TKE_h of the inclined version as the (true) reference. The true value lies within your confidence interval with 95% probability, so you can refer to that interval (95% probability for the true value to be between the interval limits, that are very close to 1; e.g. similar to the formulation in line 455) but not due to the 1 is within the interval.

As we compared different configurations of the layers' inclination optimization, we decided to keep the non-tilted LVA as the reference, though the tilted LVA falls indeed closer from the true TKE (at least within the layer where the tilt was optimized). We precised the definition of the ratio η and added a sentence to explain that reference point (lines 516-520):

"the intercept b is simply equal to the average of the logarithmic ratio η=log_10(TKE_TLVA ⁄ TKE_LVA), with TKE_TLVA and TKE_LVA being respectively the horizontal TKE for the tilted and simple LVA (Sec. S7 of the supplement). In all rigor, the tilted LVA should be used as reference, as it provides an estimation of TKE_h that is closer to its true value. However, in order to compare more easily the results for the different parameterizations of the tilt optimization, the simple LVA was used as the reference. In the reverse case, a minus sign would be added to η and b."

Line 458: You compare optimizations for the intervals [0,0.5km], [0,1km], [0,1.5km]. If you have the optimal result (inclination angle) for [0,0.5], I would expect to have worse results when comparing larger intervals. Could it be an option to use non-overlapping intervals?

It would indeed be possible to optimize the tilt angle on different altitude bands, then assemble a synthetic TKE profile, provided that each altitude band contains enough profile levels with valid data. As we determined that a minimum of about 10 levels is necessary to 'smooth' the error curve and have a well-defined minimum, this means that 250-m altitude bands would theoretically be possible. In practice, this would work only in the lower part of the profile, where all profile levels have valid data, while wider altitude bands should be used in altitude. Concretely, the heights of the optimization bands should be adjusted depending on the effective vertical range of the lidar observations, which means that they would differ among the cross-RHI pairs. In order to make comparisons on the entire dataset, we thought it would be more relevant that all the profiles were optimized using the same altitude bands.

Line 466: Are slice-wise optimizations and different inclinations a possible solution?

Indeed (see the previous answer), we cited this as a possible improvement in the perspectives (lines 651-652).

Line 475: If you have perfect horizontal wind at some height with TKE_RHItilt = TKE_RHI, resulting in 1 for the fraction, the "bias" would be higher than the "bias" minimum detected, even though there would be no bias at the height of the perfect horizontal wind.

It is true that the bias values depends on the orography around the site. For a perfectly flat site, where the atmospheric layers would be expected to be fully horizontal, the tilt optimization would theoretically not be necessary, and should not reduce the TKE. However, even for the site of Dunkerque, that is bordered by the sea on one side and by a very flat plain on the other side, the 2D inclination angle is zero for only ~6.5% of the RHI pairs for which the optimization was successful (fig. S7.b). When the vertical wind shear is important, for instance hen there is a low-level jet, a small error in the inclination angle may change the wind variance by a large amount.

Figure 6: To identify the impact of the different inclinations, it is necessary to compare (a), (b), and (c), which have also different x axes. Is it possible to display it in one figure? More generally, it would be possible to see the impact of different inclination angles. Could you indicate the angles used in (a), (b), and (c)?

The three panels of the old Fig. 6 were merged in the new Fig. 6(b). Results from the Dunkerque site were replotted the same way and brought back from the supplementary as Fig. 6(a).

About the second part of the remark, there was not one single angle value used in each panel: Fig. 6 combines all the two/four months of data from each site and the optimized inclination angle was different for each RHI scan. What changed between the panels was the altitude band upon which the wind variance is minimized, i.e. the altitude band where the TKE correction was supposedly optimal.

About the effect of the inclination angle value on a single RHI, we added more detailed comments about the example error curve (now Fig. S5c) in the text (lines 359-362):

"Figure S5(c) presents the error curve corresponding to the RHI sweep of Fig. S5.a, with the result of the optimization process: the atmospheric layers were tilted by 1.65°, which reduced the average wind variance between 0 and 0.5 km altitude from 0.20 to 0.11 m2 s−2. In this case with strong vertical wind shear, not taking the flow inclination into account would have resulted in a large overestimation of the wind variance, and thus, of the TKE."

Line 478: Could you describe the temporal and spatial setup you compare for sonic and lidar?

We added in Sec. 2.1 (line 190) a reference to another open-access article including a map of the Dunkerque site (it was a different lidar instrument, but installed exactly at the same location). Precisions were added on lines 570 to 575:

"The anemometer was located within the zone covered by the RHI sweeps (at a distance of 1.4 km from the lidar, for a RHI range of 3.3 km), close to the North-South RHI track. No temporal interpolation was performed, and the lidar TKE was matched with the anemometer TKE of the 15-min averaging period during which the cross-RHI was recorded."

Line 496: Are the measurements considered at these low heights all of high CNR or are there possibly noisy measurements close to the −27dB threshold included? If so, does the CNR have an impact on the bias?

As said in the answer to the comment about former line 180, the distribution of the CNR values shows that Leosphere/Vaisala lidars exhibit a clear separation between points above and below the CNR threshold. Points around the thresholds are not numerous and should have a very limited impact on the reconstruction.

Figure 7: How would this comparison look like for TKI_RHI (without tilt)? Is there a significant difference? Maybe you can add this to the supplement.

We added this figure in supplement (new Fig. S16), along with a sentence in the text (lines 578-579): "without the optimization, the bias would have been much higher, reaching 34.7 %."

Summary and conclusions: You are highlighting some points, where it is not clear what is novel (new comparison/evaluation, advanced method) and what are general comments. For example, you write you provide "a more detailed description". Do you just describe another work in more detail, or did you extend/improve/adapt this work. The blind zone of DBS might not be relevant in many application cases. If you defined the scenario you are targeting at, it would be easier to follow the argumentation.

The first paragraph of the summary and conclusions was rewritten to better explain the application case (TKE profile + no-blind zone), in line with the changes made in the introduction (now lines 598-617):

"Scanning Doppler lidars are very versatile instruments in terms of observation strategy, but in order to retrieve the vertical profile of the horizontal wind, many users limit themselves to DBS scans with the old linear beam combination wind reconstruction method. Indeed, the latter is pre-implemented in the lidar instruments' software, so the DBS retrieval is performed online, enabling a real-time observation of the wind profile and eliminating the need for expertise in wind reconstruction algorithms. Recently, more performing wind reconstruction techniques have been developed to process the DBS observations, but these least-square fit optimal estimation (OE) methods do not provide the TKE profile, that is of high interest for turbulence or wind energy studies. Other scan patterns designed for turbulence retrieval have been proposed, such as the six-beam technique, but the DBS and six-beam scans leave a blind zone of about 50 m minimum near the ground, which can be detrimental when studying very low-altitude phenomena, for instance in urban or forest canopies. When both the TKE profile and the absence of a blind zone near the ground are required, it is possible to use another horizontal wind and TKE reconstruction method based on pairs of perpendicular vertical sweeps of the RHI type (cross-RHI scan). The reconstruction method, which consists in binning the radial wind values into horizontal layers, was called Layer Velocity Averaging (LVA). This technique was seldom reused since its introduction in the early 1990's, and always limited to short-range wind profiles and to relatively short observation periods. The first goal of this paper was therefore to validate the LVA reconstruction for full-height horizontal wind profiles, and over a longer period of time, by comparison with DBS wind profiles. Additionally, over sloppy or complex terrains, the atmospheric layers can be inclined, in which case the TKE values provided by the LVA will be overestimated, though no method had yet been proposed to correct for this effect. This paper thus provided a technique to account for the atmospheric flow inclination, which consist in tilting the binning layers used in the retrieval process, while optimizing the tilt angle so as to minimize the intra-layer wind speed variance, i.e. the horizontal components of the TKE, thus eliminating the TKE overestimation due to wind shear."

We also have to signal that we found a mistake in the text: we indicated that we used correlation coefficients (r^2), but it was actually determination coefficients (R^2), as stated in the scatter plots' annotations. This has been changed throughout the paper, and in fact strengthens the results as R^2 values are always smaller. Finally, to be exhaustive about the history of the LVA method, a reference to Gal-Chen et al. (1992) was added in the introduction.

Minor comments:

Please check if you want to use British or American English; you used both (BE would be centre, metre).

We checked that US English was used consistently in the whole manuscript.

Please check your prepositions for locations.

The two observation sites are cities, so we thought that writing "at Dunkerque/Paris" rather than "in Dunkerque/Paris" would be incorrect. Also, it seems awkward to use "at the Dunkeque/Paris site" everywhere. However, when appropriate, we replaced "in" by "for".

An object should be inserted between "allows" and "to" in "allows to", e.g. "allows the user to".

The corresponding sentences have been rephrased to use a noun instead of a verb after "allow" or "enable" (e.g. "allow the computation of" instead of "allow to compute"). Some occurrences have also been replaced by "make it possible to".

"m.s-1" should be without the dot in between.

This has been changed in the text.

Line 9: consider writing "measure wind" instead of "measure wind profiles" as the profiles are not directly measured

"measure" was changed to "retrieve".

Line 13: "plus" is informal

This sentence has been modified following another comment and the word "plus" does not appear anymore.

Line 29: What is the flow inclination angle?

This phrase was modified to "the two-dimensional inclination of the atmospheric layers".

Line 45: New sentence beginning with "Middle-income"

This was changed.

Line 55: Not all Doppler lidars have a 2 axis scanner

This sentence was changed to “Doppler lidars equipped with a two-axis scanning head also offer a great versatility in terms of observation strategy”.

Line 60: What is "virtually impossible"?

We changed this sentence, using “in practice” instead and precising this applies before the turbine is built (now lines 63-64): “using meteorological masts to obtain wind observations at hub height before the turbine installation has become impossible in practice”

Line 70: How can I imagine gas particle interpretation with Doppler lidars?

We made the sentence clearer by precising “to interpret gas or particle observations from in-situ instruments or from elastic or Raman lidars” (now lines 73-74).

Line 73: elevation angle

This was added.

Line 86, 170: deep?

We changed “deep” for “height”.

Line 97: Is this your experience or evidence?

We assumed the question bore on the duration of the averaging period for turbulence computations and added references (Larsén et al., 2016; Lenschow et al., 1994) (now lines 106-107).

Line 101: Too large at higher heights?

Indeed, we modified this.

Table 1: Consider using "scenario duration" or "iteration duration" instead of "total duration"

We changed the left cell to “Measurement cycle” and the second left cell to “scenario duration”.

Line 179: averaged

This word is not present on this line. We assumed you referred to line 177 and suggested that “averaged data” should be “average data”, which we corrected.

Line 204: singular or plural?

Singular. This sentence was changed into “The radial wind from a DBS or from a RHI sweep recorded in one of the cardinal directions” (now line 243).

Line 221: "too-vertical" is informal

This was changed into “beams too close to the zenith” (now line 261).

Line 272: Maybe you can find a formulation that is easier to understand line 275: that?

This sentence has been moved to Sec. 2.3.4 and rephrased into (now line 320-321):

“[…] beam recombination effects, which occur when the air motion during the scan results in sampling the same air parcel with beams of different azimuths”

Line 371: slope of the regression

We rephrase as “the slope values obtained from the regressions” (line 448).

Line 428: presents line 434: RH or RHI?

It was indeed RHI, now changed to LVA.

Line 428: connective??

“convective”, this was corrected.

Line 431: If you have a first step, do you have more steps?

This phrases echoes the one on former line 483, i.e. the second step is the comparison with the ultrasonic anemometer for the near-ground TKE. We rephrased this sentence as (now lines 511-513):

“As a start, the impact of the flow inclination optimization on the horizontal TKE retrieval was assessed by comparing values obtained with and without the inclination”

And replaced “step” by “phase” in the other sentence.

Line 508: vertical wind profile of the horizontal wind.

Corrected

Line 517: its

Corrected

Line 528: on average

Corrected

Supplement

Line 9: space missing before "denote"

Corrected.

Figure S7: Paros

Corrected! This one was not detected by the spell-checker, as there is a Paros island in Greece.

Figure S7, S9: Why do the subfigures have different resolutions?

In Paris, the total duration of the measurement scenario was 20 minutes, but it included two DBS cycles (one at the beginning, one in the middle of the scenario). There was therefore one DBS scan recorded every 10 minutes, and one pair of RHI sweeps recorded every 20 minutes. This was precised in the legend of the figure, but we moved it to the beginning of the descriptive text (lines 169-171).

Figure S9f: Is the colourbar label correct (compare to figure S8b)

Both color bars have the same bounds [1, 230], only the tick labels differ as the aspect ratio of the figures is not the same. We added a tick on the color bars to indicate the value 230.

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