Reply on RC2

While the authors have compiled a substantial six-month observational dataset across two distinct urban environments, the manuscript cannot be recommended for publication in its current form. The recommendation for rejection is based on two clear, foundational flaws that, in the opinion of the reviewer, cannot be resolved without a complete restructuring and re-submission:

1) Total absence of a mathematical framework

For a technical methodology paper introducing a wind field reconstruction method and a novel optimization routine, the text contains zero formal equations. The geometric projections and the 2D tilt optimization cost functions are treated as a complete "black box," preventing independent verification and undermining the core scientific principle of reproducibility. While providing the underlying source code satisfies open science protocols, it does not substitute for documenting the governing physical and mathematical theory within the manuscript text itself.

We agree that articles must include a sufficient mathematical framework to make it possible for other researchers to reproduce the method and verify the results. However, the equations describing the radial wind geometric projection for RHI sweeps have been given in several articles before (e.g. Bonin et al., 2017), as have been the formulas relative to the Reynolds decomposition of the wind. We also chose not to include these formulas because they can be inferred simply from the scheme in Fig. 1, nevertheless we added them in the supplement (Sec. S3.1 and introduction of Sec. S4).

Besides, the tilt angle is determined separately for each RHI sweep, which means this is not a 2D optimization problem, but two independent 1D optimizations. One-dimensional optimization is not a mathematically complex problem, and there are many other optimization functions that could have be used successfully, the only stake here being the starting point and the computing efficiency. The computation of the 2D inclination angle from the two 1D tilt angles is only a question of geometry that was already detailed in the supplement.

Finally, we remind that the data for the Paris site and the codes have been shared on Zenodo.

2) Circular validation baseline

The manuscript relies on a direct statistical comparison against standard Doppler Beam Swinging (DBS) observations to validate the proposed cross-RHI technique. However, the paper's own introduction and results explicitly establish that the DBS baseline is compromised by horizontal inhomogeneities in the wind field. Validating a new profiling method against a similarly flawed operational baseline creates an unresolved logical loop. When the profiles inevitably diverge, it is physically impossible to determine which method is correct without an absolute, uncompromised independent reference across the vertical column—such as a tall meteorological mast with sonic anemometers or a multi-Doppler virtual tower setup.

We agree that, in a purely metrological perspective, i.e. for establishing ISO norms for the wind energy industry, a comparison with the reference technique that is sonic anemometry is necessary. However, we do think that there is some space left for papers proposing earlier-stage contributions about the wind and TKE reconstruction methods, even if their authors did not have the means to push to a normalized validation. Another practical reason is that meteorological masts are limited to a height of about 300 m while we intended to validate the wind reconstruction for full-height wind profiles, meaning that only remote sensing observations can be used as a reference.

Besides, the processing of the DBS observations – that are used as reference – has been greatly improved, following the suggestion from the other reviewer. In the first version of the article, we had chosen to stick to the old DBS wind reconstruction method (beams' linear combination), because it is implemented in the software of commercial instruments, and because many end-users use this method as it is, notably in the wind energy industry. However, this second version of the article relies on a state-of-the-art wind reconstruction method for the DBS observations (least-square fit optimal estimation, following Steinheuer et al., 2022)). This method aggregates more data, thus providing much better results in terms of filtering the range-folded echoes from clouds and smoothing the atmospheric fluctuations caused by turbulence.

As these two main flaws alone undermine the scientific integrity and reproducibility of the methodology, the manuscript is structurally unresolvable in its current form, and a rejection is recommended.

We disagree with this conclusion, for all the reasons mentioned above, and hope that the reviewer will give the new version of the manuscript a thorough read and acknowledge its merits.

References

Steinheuer, J., Detring, C., Beyrich, F., Löhnert, U., Friederichs, P., and Fiedler, S.: A new scanning scheme and flexible retrieval for mean winds and gusts from Doppler lidar measurements, Atmos. Meas. Tech., 15, 3243–3260, https://doi.org/10.5194/amt-15-3243-2022, 2022.

Bonin, T. A., Newman, J. F., Klein, P. M., Chilson, P. B., and Wharton, S.: Improvement of vertical velocity statistics measured by a Doppler lidar through comparison with sonic anemometer observations, Atmospheric Measurement Techniques, 9, 5833–5852, https://doi.org/10.5194/amt-9-5833-2016, 2016.