Response to referee 1

Dear referee,

Thank you for your general comments on our work, which we consider very important in helping us to improve the manuscript. Here is our response to each of your comments. Comments from the reviewer are reported in black and followed by our answers in blue.

Best regards,
The authors

Main comments:

(1). Although the manuscript is clearly structured (theoretical background, methodology containing "numerical simulations" and field measurements, and a results section), I found it rather difficult to assess its main takeaway messages. In particular, the results section is a mere juxtaposition of results (in the form of numerous plots) and their descriptions in the main text without providing further background or even highlighting their significance. E.g., the authors find in Fig. 9 that the introduction of an additional measurement point (from Fig. 7 (f) to (g)) significantly improves the estimation and uncertainty of the Reynolds stresses. This certainly seems to be a relevant result and would therefore deserve further discussion, which could be - to some extent - even speculative: Why is this additional center point leading to substantially better results? Does the additional center better grasp certain aspects of the turbulent fluctuations or only the shear profile? In the context of homogeneous isotropic turbulence, for instance, it was shown [R Stresing and J Peinke 2010 New J. Phys. 12 103046] that the inclusion of a point of reference has a considerable influence on the statistics of turbulent fluctuations.

Adding a central beam to the 50-beam lidar is beneficial for computing the u, v, w velocity variances because the opening angle of the central beam is different from other beams in the circular scanning pattern. As we explained in Section 3.2 in the revised manuscript, "if the nacelle lidar has only one opening angle, some of the six equations from the least-square procedure will be linearly dependent, and we have fewer knowns than the unknowns, which leads to infinite solutions". Also, we added more explanation in Section 4.2: "In principle, adding an extra beam in any different opening angle than the others in the 50-beam scanning pattern will improve the estimations. The central beam is the best option for improving the estimation of $\langle u'u' \rangle$ since the beam aligns with the along-wind velocity component and can fully capture its variation when the probe volume is neglected."

To improve the results section, we added some background and highlighted the significance of the results in some places when needed.

(2). On a more general note, results shown in the current version of the manuscript are restricted to single point quantities such as the Reynolds stress tensor in Eq. (11) and its determination by different methods and the results are quite impressive. Many important statistical features of atmospheric turbulence, however, are actually contained in spatio-temporal correlations (correlation lengths, scale-dependent anisotropies, even the validity of Taylor's hypothesis used by the authors) and even higher-order moments of the velocity field (intermittency or non-Gaussianity of turbulent fluctuations), see e.g., [Liu et al. Boundary-Layer Meteorol 134— 243–255 (2010)] or [Böttcher, F., Barth, S. & Peinke, J.

Stoch Environ Res Ris Assess 21, 299–308 (2007)]. From the proposed measurement setup, which contains multiple spatial points, it should also be possible to at least determine spatial correlations between two points. I would appreciate the authors commenting on these issues, perhaps even in the outlook section of the manuscript.

We added some text in the outlook "In this work, the single-point turbulence statistics are estimated using the least-square procedure, which assumes homogeneity over the lidar scanning area. Wind turbines nowadays are often operating inside a wind farm or have large spans over the swept area. The assumption of homogeneous turbulence can be violated under those conditions. Therefore, further studies on the optimized lidar scanning strategy for turbulence estimation should consider the inhomogeneity of the inflow. Additionally, the proposed nacelle lidar scanning strategies can be used to study the wind evolutions, the spatial correlations of turbulence and estimate multi-point statistics, which better characterize the inflow that interacts with the turbine than the hub height ones. The wind field reconstruction of the inhomogeneous wind fields can benefit from constrained simulations, which incorporate lidar measurements into three-dimensional turbulence wind fields. Future works could also consider the non-Gaussianity of turbulence (Liu et al., 2010; Schottler et al., 2017) and the scale-dependent anisotropy of wind fluctuations (Syed et al., 2023.)"

Apart from the outlook, we also mentioned wind evolution at the end of Section 4.1 "For the 5-beam lidar, adding measurement planes only slightly reduces the uncertainty of the $\langle u'u' \rangle$ and $\langle u'w' \rangle$ components. This lack of sensitivity is partly due to Taylor's frozen hypothesis, as we do not account for evolution in the turbulence fields.", and in discussion "Taylor's frozen turbulence hypothesis (and homogeneity) is assumed throughout our numerical simulations, because the wind evolution is not very relevant to turbulence statistics, but more to the rotor-effective wind speed estimations (Chen et al., 2021)."

<u>Further comments:</u>

(1). Not every reader of Wind Energy Science might be familiar with the Reynolds decomposition of the velocity field ui(x, t) into mean ⟨ui(x, t)⟩ and fluctuating parts u′i(x, t). Please introduce the appropriate notation here; in the current version it is not clear that primed variables such as the ones in line 28 denote the fluctuating part of the velocity field.

In the revised manuscript, we introduce the decomposition of the velocity field in the introduction and also at the beginning of section 2.1.

(2). Line 249: Possible typo? Only off-diagonal stresses, such as ⟨u′v′⟩ or ⟨u′w′⟩ can become negative.

Theoretically, only off-diagonal stresses can be negative. However, we observed negative $\langle v'v' \rangle$ and $\langle w'w' \rangle$ estimations from field measurements using the least-square procedure, which was unexpected. This occurs rarely and randomly. The same results were shown and discussed in Fu et al. (2022a). As the authors stated in Fu et al. (2022a), the possible reason is that the radial velocity variances of the SpinnerLidar highly decrease with the increasing opening angles. In this case, the turbulence homogeneity assumption is violated.

(3). Uncertainties of the 6- and 50-beam measurements in Fig. 9 extend beyond the range that is plotted, which the authors explain by a vanishing determinant of the matrix in Eq. (15). Is it correct that the values for ⟨u′u′⟩, ⟨v′v′⟩, and ⟨w′w′⟩ for the 50-beam simulations in Fig. 9 (a) also lies outside of the range although that the corresponding measurements in Fig. 9 (b) still lie within this range and generally exhibit higher uncertainties?

In Fig.10 (Fig.9 in the preprint), the velocity variances estimations from 50-beam lidar get out of the plotted range, both from numerical simulations and measurements. This is due to the linear dependency of Eq. (15), which resulted in infinite solutions when the opening angles of all beams are identical. So the estimations from 50-beam lidar are random.

The estimations from the 6-beam lidar do not get out of the plotted range, as the matrix on the left side of Eq. (15) is not singular. Its Reynolds stresses estimations are reliable because the 6-beam lidar meets the requirements to solve the six Reynolds stresses from the least-square procedure, i.e., has at least six beams and two different opening angles.

(4). In the results section, it is quite confusing that the authors first present their least squares method (Fig. 9) and then directly move on to the estimation of σ_u with the three other methods discussed in Sec. 3.2. Please differentiate more clearly and explain that this is due to the fact that the least squares only works for more than six beams and that you have to rely on the methods discussed in line 155 otherwise.

In the revised version, we explained more about this in Section 3.2 and also at the beginning of Section 4.2.

(5). Concerning the model parameters in Sec. 3.3: Please explain the origin of the specific parameter values. As of now, it is not clear whether these model parameters were actually determined on the basis of the field measurements in Sec. 3.4 or not.

As explained in Section 3.3 in the revised manuscript, "The selected three parameters are adopted from Mann (1994) and characterize a neutral atmospheric stratification on a typical offshore site. The dissipation rate $\alpha\varepsilon^{2/3}$ is a scaling factor on the turbulence intensity". They were not determined based on the field measurements used in this work.

(6). The authors claim that accurate estimation of the Reynolds stresses requires at least six measurement points in the rotor plane. Is this due to the fact that the least square method is not working anymore or is it due to the fact that the measured radial velocity variances are not fully converged? Could the authors please be a bit more specific here.

We have modified section 3.2 to explain the reason more clearly. "To solve the six Reynolds stresses accurately from Eq. (15), two requirements of the lidar scanning pattern need to be fulfilled:

- the lidar has at least six beams or measures at six different locations within one full scan

- the lidar has at least two different opening angles.

If a lidar has less than six beams or only one opening angle and some of the six equations are linearly dependent, we have fewer knowns than unknowns in Eq. (15), which leads to infinite solutions. In that case, only the along-wind variance can be estimated well."

References:

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