Referee report on "Dependence of turbulence estimations on nacelle-lidar scanning strategies" by Wei et al.

The manuscript assesses the impact of different scanning strategies of a nacelle-based Lidar on the accuracy of certain turbulence parameter estimations such as the components of the Reynolds stress tensor, which plays an important role in the characterization of inflow turbulence conditions as well as subsequent turbine load or power curve estimations. The results presented in the manuscript are based on atmospheric turbulence measurements from a SpinnerLidar and "numerical simulations" of inflow turbulence, in this case the well-known Mann wind field model. Model parameters as well as scanning strategies used in the synthetic wind field model were chosen in order to emulate inflow conditions and scanning strategies used in the SpinnerLidar measurement campaign. Furthermore, estimation of Reynolds stress tensor components by the various scanning strategies, which essentially differ in the number and spatial arrangement of points in the rotor plane (by systematically eliminating measurement points of the SpinnerLidar trajectories), are compared to measurements of a sonic anemometer at hub height (in case of the synthetic wind field, the time series extracted in the center of the box). Using a recently proposed least squares method that allows to determine Reynolds stresses from the actually measured radial velocity variances, it is found that at least six points in the rotor plane are needed for an accurate estimation of the six Reynolds stresses.

Overall, the manuscript addresses an important issue in wind energy, i.e., the accuracy of in-situ inflow turbulence measurements by Lidar and its limitations, especially in comparison to standard (but limited) anemometer measurements from a meteorological mast. It offers a rather comprehensive study of the advantages and disadvantages of different Lidar scanning strategies and might thus contribute to a better understanding of the statistical characterization of inflow wind fields and turbine loading. I especially appreciated the combination of theoretical modeling, by testing scanning strategies inside of a synthetic wind field, and the measurement campaign. Nonetheless, I find that certain aspects, e.g., the presentation of results from both the numerical as well as the measurement campaigns, could still be improved in a revised version of the manuscript.

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 extend - 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.

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.

Here, some further comments on the manuscript:

- Not every reader of Wind Energy Science might be familiar with the Reynolds decomposition of the velocity field $u_i(\mathbf{x}, t)$ into mean $\langle u_i(\mathbf{x}, t)\rangle$ and fluctuating parts $u'_i(\mathbf{x}, t)$. 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.
- Line 249: Possible typo? Only off-diagonal stresses, such as $\langle u'v'\rangle$ or $\langle u'w'\rangle$ can become negative.
- 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 $\langle u'u'\rangle$, $\langle v'v'\rangle$, and $\langle w'w'\rangle$ 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 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.
- 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.
- 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.