Review of the manuscript wes-2021-16, entitled "Modelling the wind turbine inflow with a reduced order model based on SpinnerLidar Measurements", by A.K. Sekar, M.F. van Dooren, A. Rott, M. Kuhn.

This manuscript deals with the POD analysis of synthetic wind data, specifically lidar radial velocity, obtained with the virtual lidar technique applied to a single LES dataset. A truncated POD base is then used to approximate the time-series of wind parameters typically used for wind turbine control.

The authors have nicely shown how they master the use of POD on wind synthetic data; however, I have some comments on this work:

A. How this study is representative for the broad range of atmospheric and wind conditions experienced by a wind turbine? Specifically, the daily cycle of atmospheric stability leads to significant variations in velocity integral length scale, and energy distribution across scales and heights. The POD modes will vary significantly for the different conditions, and different POD modes may dominate specific conditions. Furthermore, a different number of POD modes might be needed to reconstruct a certain wind condition. Therefore, I am not sure about the applicability of this approach for real wind energy applications.

B. I am not sure about the predictive capabilities of this POD approach. In practice, the authors have carried out post-processing of wind data without providing any prediction for next-time occurrences. The authors mentioned that they plan to use this technique "on-the-fly". Even assuming that this would be computationally feasible, why you want to approximate the wind parameters with POD when you can already estimate them from the actual lidar data, and maybe with less computational costs?

I would also add that writing should be significantly improved throughout the manuscript. Several sentences should be rephrased and there is a large number of typos. Please find below some comments, which might help for further revisions.

Comments:

1. L 11, "*...we find that a 10 mode ROM could accurately describe most spatio-temporal variations in the inflow*". Can you comment on how this statement can be generalized for different wind/atmospheric conditions, and to rotors with different diameters and, thus, affected by structures with different sizes?

2. L 12-14, "*The reduced order modelling was accomplished using the inherent volume averaging property of lidar devices that attenuates high frequency turbulence with lower importance for the overall turbine response thus allowing significant data compression*". I have two comments on this statement: a) I am not sure how the spatial averaging of the lidar is connected with the ROM accuracy, at least not from this statement; 2) Recent works on lidar spatial averaging, see e.g. Cheynet *et al.*, Remote Sens., 2027; Puccioni & Iungo, AMT 2021, have shown that the variance of the radial velocity can be even halved from its actual value depending on lidar range gate, wind conditions and sampling height. Therefore, I would disagree that under-estimation of wind turbulence due to the lidar spatial averaging is of lower importance for the turbine response. Please comment.

3. L 18, “*have attracted greater attention*”, add some representative references. Similarly at L 21 “*feed-forward lidar-assisted control*”
4. L 22, maybe fiber-based.
5. L 31, “*with high spatial and temporal resolutions*”, provide some reference values and references.
6. L108, “*very high spatial and temporal*”. Quantify these lidar features and provide references.
7. L123, “for small yaw misalignment and tilt angles”. 30 degrees is not a small angle. I understand the simplification of neglecting *v* and *w*; however, remove the statement that this is doable based on the small lidar angles. It’s only an approximation with a certain error.
8. Fig. 4, Please report figure coordinates in non-dimensional fashion with *D*.
9. L 161, you mentioned that this LES case corresponds to an unstable atmospheric regime. Then, I assume you imposed and/or quantified the respective Richardson number, Obukhov length, and surface heat flux at the terrain. Please provide these specifications of the LES.
10. Sect. 2.2, I believe that important details on the sampling of the spinner lidar from the LES dataset are missing. I believe that the spinner lidar samples much faster than for the LES sampling frequency of 5 Hz. How did you deal with the different sampling frequencies for the various lidar beams? Furthermore, how the lidar spatial averaging is implemented in the virtual lidar?
11. L 186, the square of the norm of *V* is not its TKE, rather the square absolute value. You should first define the velocity mean according to the Reynolds-averaging approach, as you are doing next with *V’*
12. L 187, Please provide details on how you define the mean velocity field.
13. L 274-275, again, this is not TKE. If you remove the mean of the flow, you will see that the energy captured by the first 10 POD modes will be much smaller than 96.6%.
14. L 360, What is the projected longitudinal wind speed, and why it is connected with the rotor speed?
15. L 390 – 394, You are suggesting using POD in real-time “on-the-fly” while collecting lidar data. So my question is, why do you want to approximate the wind parameters (u_{eff}, s_v, $delta_h$) with POD when you can estimate them directly from the lidar data, and with less computational costs? Am I missing something?

References

Cheynet, E., Jakobsen, J., Snæbjörnsson, J., Mann, J., Court- ney, M., Lea, G., and Svardal, B.: Measurements of surface-layer turbulence in a wide Norwegian fjord using synchronized long-range Doppler wind lidars, Remote Sens., 9, 977, https://doi.org/10.3390/rs9100977, 2017.

Puccioni, M., Iungo, G.V., Spectral correction of turbulent energy damping on wind LiDAR measurements due to spatial averaging, Atmos. Meas. Tech., 14, 1457-1474, 2021.