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Comment on wes-2021-51

Anonymous Referee #2

Referee comment on "On Turbulence Models and LiDAR Measurements for Wind Turbine Control" by Liang Dong et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-51-RC2, 2021

In this paper, the authors compare two different turbulence models (the Kaimal model and the Mann model) and two different LiDAR scanning protocols to assess their potential for LiDAR assisted control (LAC). The focus of this work is on larger rotor sizes. The comparisons are based on numerical implementations of the turbulence models and simulations of the LiDAR scanning protocols. The authors find a good agreement of the magnitude-squared coherence between simulated LiDAR measurements and the rotor effective wind speed with the theoretically expected ones.

Overall, the work is interesting, clearly structured, and well written. The main finding is that the asserted coherence depends strongly on the specific type of turbulence model. In fact, it is shown that the assertion of the benefit for LAC depends more on the turbulence model than on the specific LiDAR scanning pattern.

This brings me to my main question. Whether LAC is beneficial or not ultimately depends on the coherence of the atmospheric turbulence. This means that the major question should be which turbulence model is better suited to capture atmospheric turbulence. In that sense, sentences like "the value creation of LAC, evaluated using the Kaimal turbulence model, will be diminished if the Mann turbulence model is used instead." seem inappropriate to me since no direct conclusions for field measurements can be drawn. I think the authors should clarify their scope and in particular the limitations of their study.

Another question concerns the choice of parameters. Many of the parameters in this study are kept fixed, which makes me wonder how much the conclusions depend on the specific parameter choices. Can the authors comment on that?

Minor comments:

* Eq. (5): The argument of R_ij should be boldfaced.

* I don't understand the statement below eq. (7): "When the two indices i=j, then Delta_y=Delta_x=0..." Can't I vary i, j, Delta_x, and Delta_y independently? I think this statement needs clarification.

* Line 114: I think "the" in "especially for the Mann model" should be deleted.

* Eq. (9): The notation is a bit difficult to understand: Shouldn't all vectors be boldfaced? Perhaps it is worth checking the manuscript once more regarding the consistency of notation.

Once these aspects are addressed, I can recommend publication in WES.