Dear referee,

Thanks for your general comments on our work. We are glad that you find it as interesting and strongly motivated. We are also thankful for your specific comments that certainly contribute to increase the value of the manuscript. Please, find our answers below. Comments from the reviewer are reported in bold and followed by our answers.

General comments

The manuscript entitled "A method to correct for the effect of blockage and wakes on power performance measurements" describes a strategy to account for blockage when a turbine is operating within a wind farm rather than in isolation. The work is interesting and the motivation is strong, as blockage effects can have a large impact on power performance. However, the proposed method suffers from limited practicality, as either multiple simulations or lidar measurements are required to apply it to a real wind farm. While I recommend this work for publication in Wind Energy Science, I suggest the authors propose some solution to these limitations (e.g., using simplified models or SCADA measurements) either within the body of the manuscript or as a direction for future research.

We agree with the reviewer upon the method requiring additional effort (either in the form of numerical simulation or short-range nacelle lidar measurements) compared to the current IEC standard. There are practical challenges, but we do not consider the applicability of the method to be poor. Multiple simulations are already conducted for calculating turbine interaction losses and energy production. In addition to that, the correcting method presented in this work only requires the simulation of a limited range of wind conditions and wind directions. Additionally, nacelle-mounted lidars are already a standard instrument for power performance tests, especially offshore. The additional measurements at 0.5D required for the method will not be a practical issue as most commercial lidars provide simultaneous measurements at different heights. Nevertheless, following the reviewer's suggestion, in the revised manuscript, we further discuss the applicability of the method and the expected direction of future studies including the evaluation of engineering wake models.

Specific comments

1. Page 2, lines 50-51: This sentence in the introduction is unclear to me: "Specifically, wind farm blockage appears to affect the wind speed relationship between the mast location and the rotor in these results." What is the relationship and how is it affected?

We agree that the sentence as written is not clear and will be changed in the revised manuscript to "Specifically, wind farm blockage appears to affect the ratio between the wind speeds at the mast location and the rotor".

2. Page 4, lines 106-107: Could you provide some support for the assumption that the blockage/induction remains constant with wind speed over the plateau of the thrust coefficient curve?

We state that the induction is constant over the plateau of the thrust coefficient for small changes in wind speed. This assumption is reasonable and further supported by the results of Fig.12, where it is shown how the correction is still accurate with a decrease of 0.5 m/s in wind speed.

T3. Page 5, lines 143-144: Can you comment on the effects of wind farm size and spacing?

There would probably be sensitivity to both wind farm size and spacing. It is not easy to estimate this without running some sensitivity studies, though higher spacing and less turbines would likely cause a reduction of the wind-farm-related correction factors. However, previous studies suggest that wind-farm-related correction factors would not disappear even in the simplest case of a single row of turbines, as it was shown that the power performance at such site is different from that of a single isolated turbine.

4. Page 9, lines 215-217: Why do the largest power losses occur at the edges of the farm rather than at the location of the largest velocity deficits?

This is due to the poor correlation between the power output and the wind speed retrieved at 2D from the rotor. The power deviations of Fig. 4-c are consequence of variations among the U_disk of the different rotor, while fig. 4-a and 4-b shows deviations in wind speed measurements retrieved at 2D in front of the rotors.

5. Page 13, section 5: Can you please elaborate on how this section ties into the rest of the manuscript?

A few introductory lines have been included in the beginning of the section in the revised manuscript.

6. Page 16, lines 324-327: Nacelle lidar measurements are also not trivial to obtain. Most turbines are still not equipped with lidars. Though it is interesting to propose measurements as an alternative to simulations, it does not significantly improve the practicality of this method.

We touched upon this point in our answer to the general comment.

7. Page 18, lines 336-337: Site-specific sensitivity is another strong limitation of this work. How much is this ratio expected to change under different conditions?

As mentioned in the manuscript, the possible variation of this ratio for different sites is a concern to investigate in the future extensions of this work. This could be done by looking at specific turbulence and stability conditions to characterize their effect on the ratio. However, we would like to point out that short-range lidar measurements would allow for the best correlation with the “disk” velocity independently of the site-specific conditions.

Technical corrections

1. Page 8, figure 3: Please put the axes in terms of D. Changed as suggested in the revised manuscript.

2. Page 11, figure 6: It is not clear how useful this plot is. The information shown seems redundant with figure 5. In addition, the variations shown are very small. If the authors choose to keep this figure, the y-axis should be labeled in terms of percent.

3. Page 12, figure 8: Once again, I am not sure how much this figure adds to the manuscript. The same information can be gleaned from figure 7. 4.

We followed the reviewer’s suggestion and removed both Fig. 6 and Fig. 8 from the revised manuscript.

4. Page 18, figure 14: The figure feels out of place here. It would make more sense to talk about the fidelity of the lidar measurements before talking about the relationship between lidar measurements and power (figure 13).

We decided not to change the order of the figures, as Fig. 14 provides the explanation for what we observe in Fig. 13, i.e. the good performance of the correcting method using the short-range nacelle lidar measurements (Fig. 13) is explained by the good correlation of such measurements with U_disk (Fig. 14). Therefore, we prefer to choose Fig. 13 before Fig. 14.