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Interactive comment on "Wind tunnel experiments on wind turbine wakes in yaw: Effects of inflow turbulence and shear" *by* Jan Bartl et al.

Anonymous Referee #1

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This paper provides a very useful study of wakes in yaw through a detailed and careful wind tunnel campaign. Presenting the results from the wind tunnel is valuable and confirm some assertions made by LES studies. The separation of inflows into low and high TI, and with shear, provides useful into the relationship between inflow and wake behavior. The comparisons of the wind tunnel experiments with well-known models is also a helpful analysis. Overall I found the paper to be well-written, the figures clear the arguments well structured and the contributions important.

Overall Comments:

Symmetry: Sometimes I became a little confused about discussions on symmetry. At some points (page 12 for example) the focus was on the shape of curl, but on bottom page 14, I had the impression symmetry here meant a difference in the effectiveness

of positive versus negative yaw. Maybe this could be further clarified.

Further, if I understand, both asymmetries are explained as being explained by interaction with the tower. This made sense to me in the discussion of the symmetry of the wake itself, but I had some doubts if it could fully explain the asymmetry in +/- effectiveness. For example, some LES codes show this asymmetry while not including any tower model in the flow (for example ALM, or ADM codes which have essentially only the rotor modeled). Wouldn't this imply some other mechanisms could also be responsible?

A final point on this discussion, could you include some discussion of the proximity of the rotor to the ceiling and the floor? I was thinking a source of discrepancy might be that LES/field data will have only a ground, and as a result only one of the vortices experiences ground effects. Is this a consideration?

A second overall comment, the authors point out that is difficult to reduce wake deflection to a single value, and can complicate interpretation of results such as Fig 8-9. Since you already employ the method of available power, I believe an interesting additional comparison between the collected data and the models would be to compare the power output of an imaginary turbine located at x/D=6 and z/D=0 (and perhaps z/D = +/- 0.5). This could represent an interesting assessment of do the models correctly predict the change in power obtained through wake steering for a given arrangement.

Specific comments:

1) The introduction is well done, with a good review of the literature to date. Useful to read it summarized in this way.

2) The selection of y as vertical and z as cross-wise was surprising to me, although since you provide a coordinate system in Fig 4., not too confusing. But is there a reason for this? FAST and Bladed for example both have z directed upward

3) Page 6, cos cubed is found for power-loss function. Anecdotally, this would be high

for a utility-scale turbine I believe (although it fits the theoretical value). Is this a function of the scaling?

4) Fig 11: I didn't understand why for the lower plots, two different methods of fitting are used. It had the impact on me, to reemphasize the difference in value of the points, since on the right the higher points are outliers to the fit.

Connection to the companion paper:

I also could use a little more explanation of which material has been put into which paper and why. For example, the companion paper is focused on changes in TKE, and wind speed variability. Does it make sense to also discuss TI in this paper? To be clear, I am fine with the current division, but it would be helpful to understand a little more the distinction between the papers, if they both include profiles of turbulence for example. Perhaps one additional paragraph more explicitly delineating the papers, to be added to both?

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