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# *Interactive comment on* "Field testing of a local wind inflow estimator and wake detector" *by* Johannes Schreiber et al.

**Anonymous Referee #3**

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The manuscript entitled "Field testing of a local wind inflow estimator and wake detector" deals with the full-scale experimental validation of estimator concepts based on the use of the rotor as a wind sensor. The methods are based on the processing of the blade load fluctuations, and particularly the blade out-of-plane bending moments. Since 2010, the research team lead by Bottasso developed, improved and validated the concept of using the rotor as a wind sensor and the present paper is in line with this continuous process. It reaches a new step, by performing the demonstration and partial validation of the concept at full scale, on utility-scale wind turbines. The main challenges are then to obtain statistically converged, reliable and exploitable results when the boundary conditions of the experiments are non-controllable and partially known (onsite environmental and atmospheric conditions) and when the propotype



is not specifically designed and equipped for R&D purpose experiments (utility-scale wind turbine). These aspects lead to the need for an extensive preparation of the database by using massive data pre-processing (ad-hoc calibrations and corrections, sample rejections, filtering, classification, etc.). In the present paper, these unavoidable pre-processing steps, as well as the actual data processing steps, are well argued, described and illustrated. The obtained results are on general, well explained and prove the feasibility of the "WT as a wind sensor" concept. On the other hand, a lack of information on the experimental set-up and on the site description affect sometimes the reliability of the results interpretation, leading the authors to use too frequently "likely", would", "seems to", could be due to". Mainly, a better knowledge of the site properties (terrain and micrometeorology) can help in some interpretations. This can be provided a posteriori using geographical and meteorological databases and it is essential to add them to the manuscript.

Major comments: - A thorough description of the experimental set-up must be added: measurement device (anemometers, strain gages, etc.) descriptions (type, brand, accuracy, cut-off frequency, etc.) - A thorough description of the site properties must be added: type of terrain surrounding the site (type of vegetation, associated roughness length), atmospheric boundary layer properties (wind rose, averaged power law and turbulence intensity at hub height for the studied wind directions, thermal stability encountered during the selected periods, etc.). If not findable by the measurement campaign itself, meteorological information can be extracted from global meteorology reanalysis database as MERRA2 or ERA5. - §3.3 Reference inflow & Figure 3 : it is written that the wind speed is measured at three different heights on the met-mast but two of them are located at 2m of each other. Therefore, one cannot consider that one has three distinct values to assess the power law exponent, but only two. What is the consequence on the accuracy of the obtained power law exponent? - Figures 7 and 8: The obtained values for the power law exponent (mainly between 0.2 and 0.4) are particularly high for such an open-field terrain, as it seems to be on the satellite picture. These values are usually encountered on rough to very rough terrains (forest or city). Again, a better description of the terrain fetch and of the local atmospheric boundary layer properties would help to justify the results reliability. - Page 12, lines 9-10: “This difference could possibly be caused by a non-ideal power law inflow profile, leading to a biased met-mast reference shear, although a definitive explanation of this mismatch could not be reached with the present data set.”. I would recommend to make a sensitivity analysis on the power law exponent to the number and position of the used anemometers - Pages 12-13: “Considering that all wind directions are for un-waked met-mast and turbine, these results suggest the presence of a spatial shear variation, probably caused by the local vegetation.” Again, a better description of the terrain fetch and of the local atmospheric boundary layer properties would help to justify this assumption. - - It is written on page 8, lines 4-5, “Measurements taken during yawing manoeuvres were also discarded, as additional induced loads can pollute the estimates”. On the other hand, on Figures 6 and 10, the wind direction progressively changes from 240° to 200° during 6 hours, and from 100° to 175] in 12 hours, respectively. Yaw manoeuvres should appear during these periods. It sounds in opposition of the first statement. Could you please add the wind turbine orientation time series to these plots and explain how you did the data analysis during these periods? - Figure 11 : would it be possible to classify the results considering the incoming wind speed category (and so the wind turbine operating point). One could expect that the wake is more or less intense, depending on the wind turbine operating point and that the wake detector is more or less efficient. - - Page 17, lines 10-12: “the wake is not developing exactly along the downstream direction. Indeed, the latter is a well-known phenomenon observed in vertically sheared flow (Vollmer et al., 2016).” Yes, it is true for yawed wind turbines, or for un-yawed ones in very stable atmospheric conditions but cannot be considered as a universal explanation for the bias in the present results. - Page 18, lines 24-25: “The larger fluctuations of the vertical shear compared to the horizontal one are probably caused by varying ambient inflow conditions.” It is not clear what this sentence means exactly. Could you elaborate more on these “varying ambient inflow conditions”? Again, a better knowledge of the local atmospheric boundary



layer properties can help to justify some results. - Conclusions: some conclusions are not new (i.e. “rotor-effective wind speed can be estimated from blade out-of-plane bending moments, with a quality that is nearly indistinguishable from the well-known torque-balance method”), since already drawn by previous papers from the same research team. What is new is to make the full-scale demonstration/validation of these different concepts.

Minor comments: -Page 3, line 17: remove A in the q formula - Page 4, line 3-4: “A rotor-effective wind speed can also be obtained from the blade-effective ones by simple averaging over all (three) blades”. One expects that the dynamics of the rotor-effective speed is quite low (cut-off frequency linked to the rotor diameter, whereas the dynamics of the blade-effective ones must be higher. Do you get the right rotor-effective speed dynamics by averaging the three blade-effective wind speeds? - page 4, line 17-18 “he smaller inertia and high damping of this degree of freedom makes this more sophisticated approach superfluous”: Please add a reference to prove this statement. - Page 5, figure 1: the reference framework (x,y,z) is not direct. Considering the naming convention in the downstream viewing direction, one assumes that x is in the downstream direction too. Then y, should be oriented on the left - Page 9, lines 7-8: add this information into the experimental set-up description - Figures 4& 5: should be written in the captions that it is after correction

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