Review of 'The effect of tip-speed ratio and free-stream turbulence on the coupled wind turbine blade/wake dynamics'

This manuscript presents an experimental study of blade strain measurements on a wind turbine rotor using Rayleigh backscattering sensing (RBS). The authors investigate how gravity- and rotation-induced loads combine with aerodynamic loading, and propose a decomposition method to isolate centrifugal, gravitational, and aerodynamic strain contributions. Strain statistics are compared under quiescent background conditions and under controlled free-stream turbulence across a range of tip-speed ratios.

Overall, the manuscript is clearly written and the introduction provides a helpful overview of existing work and the remaining challenges in understanding the aerodynamics of rotating wind turbine blades. The experimental setup is impressive, and I believe the dataset itself could be valuable to the community. However, several key elements required to support the main conclusions are currently missing. In particular, the manuscript lacks adequate uncertainty quantification and sensor characterization, and a number of the subsequent data-processing assumptions appear insufficiently justified. As a result, some of the interpretations may not be supported by the presented evidence. I therefore recommend **major revisions** before the manuscript can be considered for publication in 'Wind Energy Science' journal.

Major points:

1. **Figure 1:** How homogeneous is the free-stream turbulence (FST) across the test section and across the rotor disc? Is the FST characterization reported in a separate publication? If so, this should be cited explicitly. Otherwise, additional details (mean velocity profile, turbulence intensity distribution, integral length scales, and spatial uniformity) should be provided, either in the main text or in an appendix.
2. **Section 2.2:** The uncertainty of the RBS strain measurements is not quantified. The measured strain can depend on several factors, including the bonding/gluing procedure, sensor placement, temperature effects, and surface curvature. In addition, since the study relies on time-resolved data, the dynamic response and bandwidth of the sensing chain should be documented (or cited from prior validation studies). Finally, potential asymmetry in sensor response under tensile versus compressive loading should be addressed.
3. **line 243:** *'…whereas the contribution of gravitational loads becomes negligible, potentially due to increased blade stiffening'*. This doesn't sound correct. Blade stiffening would be a valid hypothesis in case of bending moments. Here, the RBS sensor measures peak strain at \theta=\pi or 0 which implies that axial stretching of the blades due to gravity is measured. In case of such an axial loading, blade stiffening cannot reduce the \delta\epsilon due to gravity. I think the reason for such an observation is different. In the blade frame, gravity acts along the chordwise and spanwise directions as a sinusoidal force whose frequency is determined by the rotational speed. In total, gravity imposes a ~10 units of strain variation. But when the blade rotates faster, the system has less time to dynamically respond to a high-frequency forcing. Therefore, the gravity effect becomes invisible in the phase-averaged measurements beyond a certain rotation speed.
4. **Figure 9**: When comparing strain signatures across TSRs, changes in the effective structural response of the blade (including centrifugal pre-tension and any shift in modal properties) should be considered. At present, the role of blade structural dynamics is not discussed, despite being potentially important for interpreting off-design behavior.

5. **Figure 11**: It is difficult to draw strong conclusions from Figure 11 without comparing against an expected bending-moment/strain distribution under steady aerodynamic loading (e.g. BEM-based prediction). Such a reference could be used for normalizing the measurement data and render results from different sections comparable.
6. **line 357**: *'The decreased rate of increase for λ > λd reflects the aerodynamic stabilization of the blade'.* I don't understand what this means. The decreased rate is most probably because of the reduced bending moment close to the blade tip, which behaves like a free end of a cantilever beam (see the previous comment).
7. **line 364**: '*This suggests that at design conditions, the effects of FST on the time-averaged loads are mitigated by the operational conditions of the turbine.'* The observation that different FST levels have limited influence on time-averaged strain at design TSR is interesting and deserves deeper discussion. The current explanation is vague. I think, the authors should elaborate mechanistically.
8. **eq 11**: The proposed method for predicting FST-related RMS fluctuations assumes uncorrelated contributions. However, the manuscript also notes unsteadiness in rotational speed; in that case, strain fluctuations can be strongly correlated with speed variations and may not be separable by the proposed approach. Simultaneous analysis of rotation-speed fluctuations and strain (e.g. conditioning, coherence analysis, or decomposition techniques such as extended POD or conditional averaging) would be required to isolate the turbulence-driven component more convincingly. The possible influence of periodic forcing at blade-passing frequency (BPF) and its harmonics should also be addressed explicitly, and maybe eliminated priorly using notch filters.
9. **line 382**: *'TIP consistently exhibits the largest fluctuation levels across all operating conditions and FST cases.'* The statement that the tip consistently exhibits the largest fluctuation levels may be expected simply because the tangential velocity (and therefore sensitivity to rotation-rate variability) increases with radius. Normalization choices and sensitivity to RPM variations should be discussed before interpreting this result as purely aerodynamic.
10. **line 385**: *'…a marked increase in overall strain fluctuations at λ ≈ 3.5 is observed, potentially emphasizing the influence of the unstable regime of partially stalled to partially attached flow conditions'* I don't think there is enough evidence not enough to support this partial stall hypothesis. I find it more probable that there's a structural natural frequency of the blade close to the frequency associated with that rotational speed.
11. **line 398**: '*Moreover, at λ ≥ λd, conditions in which the flow is attached to the blade, increased TI consistently increases RMS(ε'a) across the three sections of the blade'.* Figure 12 doesn't support this observation.
12. **line 403**: *'These results suggest that, from an aerodynamic-induced fatigue damage perspective, it is preferable to maintain wind turbines operating at slightly above design conditions with the compromise of the increased contribution of centrifugal loads, rather than slightly-below design.*' The conclusion recommending operation slightly above design TSR from a fatigue perspective appears too general. If the observed RMS behavior is influenced by the blade's structural response and/or resonance proximity, it may not generalize across turbine designs.
13. **line 428:** *'The PDFs under quiescent background conditions follow a Gaussian-like profile consistent with the periodic impact of the combined centrifugal+gravitational loads acting on the blades.'* Periodicity alone does not imply a Gaussian distribution; a purely periodic signal sampled uniformly in phase typically yields a non-Gaussian PDF. The authors should clarify the processing used and the basis for expecting Gaussian statistics.

14. **line 459:** *'The profiles of E(ε'a) are estimated from E(ε')−E(ε'g+c).'* Once again, such a decomposition does not necessarily isolate aerodynamic loading if the components are correlated or if periodic contributions remain. This may explain why the spectra remain dominated by BPF in Figure 17. A more robust separation approach should be discussed.
15. **line 514:** '…ε'a and ε'g are uncorrelated'. This comment implies that the authors disregard the results shown in figure 17. Since both components can contain rotor-synchronous periodicity, the correlation assumption should be revisited.

Minor points:

- **line 308:** *'…and edgewise \epsilon_a^f…'* should be \epsilon_a^e, I guess.
- **line 421:** *'Figure 15 represents…'* I guess, it should be Figure 14.