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Interactive comment on "Aero-elastic loads on a 10 MW turbine exposed to extreme events selected from a year-long Large-Eddy Simulation over the North Sea" *by* Gerard Schepers et al.

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Thank you for your contribution. This work touches upon a wealth of different challenging topics, all of which are important within the context of wind turbine loads.

Some general remarks:

- Although the work is very relevant, the way it is presented feels a little rough around the edges. This work could benefit from an editorial focussed review.
- The difference between the measurements of inflow fields and the reconstructed

fields still shows some reasonably large differences. How would you expect the difference between the measured and simulated fields to affect the loads?

- The authors touch three very big area's: high fidelity measurement campaign with lidar and met mast, BEM vs vortex models, extreme and fatigue loads for a range of inflow conditions. In between the authors also refer several times to a "sequal report". A more careful structuring of the text and avoiding referring to work that seem to be completed yet but is not included in the manuscript (why, due to space concerns?) might help avoiding the reader getting confused since so many different topics are already considered.
- I very much appreciate the efforts of the authors to identify various real and complex inflow conditions compared to the standards, and especially also the consideration that BEM with disk averaged induction needs to be considered carefully in that context.

Some specific comments:

- line 85: do you mean 365 simulations of 24 hours each, and as such arrive to 1 year?
- line 91: could you indicate how the azimuthal resolution compares to the standard IEC turbulence box resolution?
- line 91: could you also give an indication of how expensive these simulations are in terms of computational time and resources?
- Figure 3 and Figure 4 are not referred to in the text.
  - include reference to figure 2 also on line 101 and further around line 110?
  - should fig 3 and fig 4 be referred to around line 110-130?

- figure 6: to which grid size is the label GRASPref referring to?
- paragraph at line 180: I am not sure I understand this, are you referring to results from a study you did but have not published yet? Why not include those results, who show better agreement, here instead? That also begs the question, what causes some cases to match better than others?
- line 185: what is an Excel polynomial curve fitting? I assume you just have used an n-th order polynomial to fit something by minimizing a least-square cost function or something?
- lines 245-250: it is a very interesting teaser to this sequel report, but why not include the results here? As a reader I get the impression the authors already done the necessary analysis.
- figure 12 is confusing to me, it took me a while to figure out that TI, TKE, shear, ... on the titles of the upper row referred to the "TI: case at which extreme TI has been observed". At first this is not obvious because TI, TKE are the same labels used on the x-axis of the respective TI, TKE row plots.
- figure 13: it is clear that the observed 1Hz equivalent loads (EQL) are lower, but strictly speaking that is only half the story. The other half is the frequency/distribution at which all the different events occur and together they result in the life time equivalent load. You could consider pointing that out as well.

Regarding: 6. Comparison between aero-elastic loads at extreme events with loads from the reference spectrum:

I assume the extreme loads from DLC1.2 are based on at least 6 seeds per wind speed at 3 different yaw inflow angles (-8, 0 and 8 degrees)? In contrast, only 1x 10 minute realisation is used for the extreme events since they are directly based on

a reconstructed/measured inflow field. I understand that in this context the authors want to demonstrate that extreme inflow conditions based on observations/high-fidelity simulations does not necessarily lead to higher loads when compared to the standards. I agree that is an important observation. However, I think it remains plausible that you could in theory create very similar inflow conditions as you have observed leading to extreme loads exceeding the DLC1.2 reference values. Since there is so much uncertainty and variation in turbulence levels across the rotor plane with smaller local "bursts" driving an outlier event (for example: https://dx.doi.org/10.1002/we.497). The more simulations you consider, the higher the likelihood you bump into such an event. From that perspective comparing extremes is only sensible when considering many 10 minute realisations. You could consider stating even stronger in the conclusions that many more aspects require more analysis, and that this study contributes to that bigger picture.

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