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Interactive comment on "Atmospheric turbulence affects wind turbine nacelle transfer functions" *by* Clara M. St. Martin et al.

Anonymous Referee #1

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General comments:

This manuscript quantitatively (statistically) analyzes the influence of different stability classes and turbulence regimes (obtained through the bulk Richardson number, turbulence intensity, etc.) on the wind turbine nacelle transfer functions. The authors analyzed data from one wind turbine and two sets of data from an upwind position from the wind turbine (mast and wind scanner data). The paper is well written and within the scope of Wind Energy Science.

The manuscript addresses an interesting subject that might have both practical and scientific applications in wind energy sector. However, the manuscript requires a number of clarifications throughout the text. Most of my questions are regarding the methodology and data, but I don't ask additional analyses to be conducted at this point. Namely,

it is not clear how the authors calculated some of the atmospheric quantities (e.g. bulk Richardson number). Interpretation of the results could also be better. Please see my specific comments below.

I recommend minor to moderate revisions for this manuscript before it can meet the publishing standards of Wind Energy Science.

Specific comments:

1. Anemometer and wind vane are not visible in Figure 1. The purpose of this figure (according to the text) is to show these instruments, but they are not visible. I advise the authors to add Figure 1b in which the anemometer and wind vane will be zoomed in (i.e. visible). The current Figure 1 can be Figure 1a.

2. The last paragraph in Introduction contains too many "as well as" phrases. Please reformulate these sentences in order to increase the readability of the text.

3. The last paragraph in Sect. 2.1 starts with "Further". I would suggest starting it with "Lastly."

4. Line 112. What do you mean by "simple, built-in transfer function" and how would this function modify the measured data? Please clarify as this might have importance for your results.

5. Lines 120-125. You estimated Weibull distribution parameters from the 2.5 months of data and then assumed that these parameters are representative for the whole year; am I right? Assuming that, you calculated the annual energy production. Can you please compare these calculated parameters against the parameters obtained from the data that actually cover one full year at that site, so we can see the uncertainty of your assumption and analysis?

6. Line 130. You are talking about near-surface flux measurements at 15 m and humidity measurements interpolated to 15 m, but in Sect. 2.1 (Meteorological and turbine data) you didn't mention any flux and/or relative humidity measurements. How/from

where did you obtain this data? Also, what kind of interpolation did you apply to get relative humidity at 15 m?

7. Similar to the previous comment, how did you calculate the virtual temperatures (absolute and potential) in order to obtain the bulk Richardson number values? That is, did you measure/calculate specific humidity or the mixing ration or the wet-bulb temperature? Please clarify.

8. I suggest you merge the last paragraph in Sect. 2.3 (Line 149) with the previous paragraph.

9. The caption for Fig. 3 can be simplified. You can say it's the same as Fig.2, but using second-order polynomial fit.

10. Line 220. If the nacelle anemometer underestimates the upwind winds, how is it possible that AEP based on the data from this anemometer is higher than using the upwind data? You provided an explanation, but I do not understand it. Please clarify.

11. The bottom row in Table 2 says "% difference from tower winds." If that's the name you choose, then the values are not accurately corresponding to that name. It indicates that AEP_upwind is 100% different from itself. Please simplify/rename and clarify.

12. The size of error bars and circles in Fig. 6 are not (very well) visible at 100% zoom. Please try to make these figures bigger as the interested reader is not able to actually estimate the errors from this graph.

13. The size and scaling of Fig 6. (bars, lines, points, etc.) are inadequate to develop the discussion that starts at Line 235 and ends at Line 247. Looking at Fig. 6a, I am not able to see any difference between the stable and unstable conditions and the arrows don't help much. Some discrepancies between the lines are visible at around 400% zoom.

14. Line 244. You believe that unstable conditions amplify the blockage effect and you carefully used the words "we speculate", "might be", "could be", etc., which I like.

However, can you provide some physical reasoning behind this speculation? Namely, why would the interaction between turbulent eddies and turbine augment the blockage effect and not diminish it? Your results show an augmentation (not very visible in Fig. 6 as it is now, but nevertheless show it), but what is the physics behind it?

15. References. Sometimes you used abbreviations for journal names and sometimes full names. Please be consistent.

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