Reviewer 2:

The authors investigate the interaction between wind farms and intermittency in the stable atmospheric boundary layer under weakly and strongly stable conditions, corresponding to weak and strong surface cooling rates. Two turbulence modeling approaches, the dynamic reconstruction model and the TKE-1.5 closure, are first compared. The dynamic reconstruction model is ultimately selected due to its better computational efficiency and its ability to capture backscatter, i.e., energy transfer from small to large scales. Intermittency is defined following the methodology of Coulter and Doran (2002), based on the sorted cumulative probability distribution of TKE (in this manuscript). Although the overall results suggest that intermittency has a little impact on mean wind-farm power production (primarily increasing power variability rather than the mean) this study provides a useful numerical framework for future investigations into the interaction between wind-farm performance and different intermittency mechanisms in turbulent flows.

> We thank the reviewer for the time spent reviewing our work and for the supportive suggestions and comments. Our replies are inserted below into the reviewer's comments.

The manuscript is generally well written, however, several critical issues arise concerning the treatment and interpretation of backscatter.

(1) In Figure 5, the total mean does not appear to equal the probability-weighted sum of the conditional means. For example, panel (a) at z=50 m is incorrect, whereas panel (c) at z = 80 m is correct…

> See response to (2) below.

(2) Also in figure 5, why conditioning on forward cascade is less smooth than conditioning on backscatter. I would expect the opposite due to much more samples for forward events. Any convergence study?

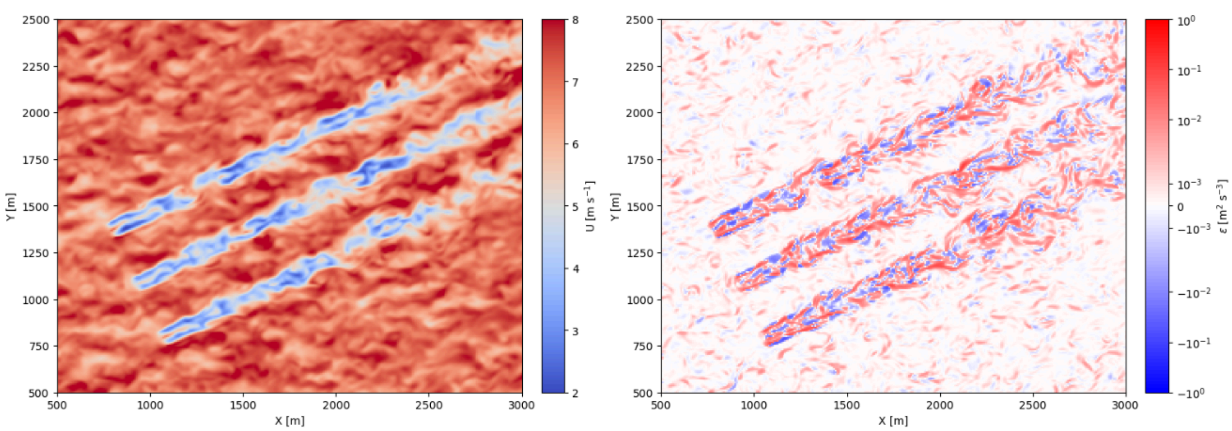
> Thank you for bringing these points up regarding Fig. 5. Upon review of the post-processing code, we found a bug where the forward cascade and backscatter lines were swapped. Figure 5 has been corrected and the associated discussion has also been adjusted.
>
> Considering that there are fewer samples during backscatter events, it now makes sense that the backscatter line is less smooth compared to the forward cascade line.

(3) It would be valuable/interesting to extend the discussion to the wind-farm region itself, including conditional statistics based on forward cascade and backscatter events in the presence of wind turbines.

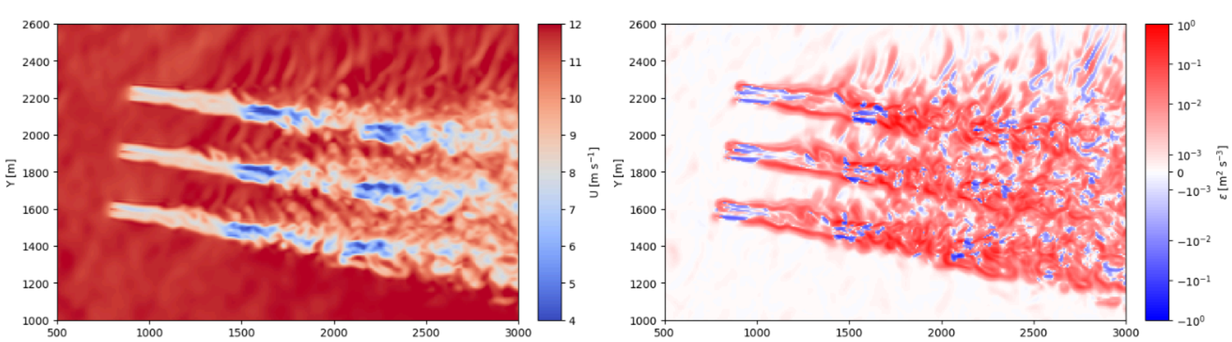
Thank you for this interesting comment. We did some additional analysis of backscatter in the wind-farm region, which is presented below. However, we decided not to include this analysis in the manuscript because we feel that it deserves a more complete treatment that is out of scope of the current study. Most notably, a more highly-resolved simulation with higher-fidelity turbine treatment (e.g., actuator line model) is likely necessary to connect turbine wake dynamics to backscatter.

Below are examples of instantaneous hub-height wind speed and turbulence dissipation rates during the weakly stable and strongly stable regimes. Backscatter (negative turbulence dissipation rate, blue) seems more prominent in the higher shear regions at the edge of the rotor and wake, as well as near the nacelle (although this is more difficult to see in the strongly stable case because of the high wind veer). To tie backscatter to the breakdown of blade tip vortices (or other wind turbine related structures) would likely require a higher fidelity model with finer grid spacing and an actuator line model.

Weakly stable case:

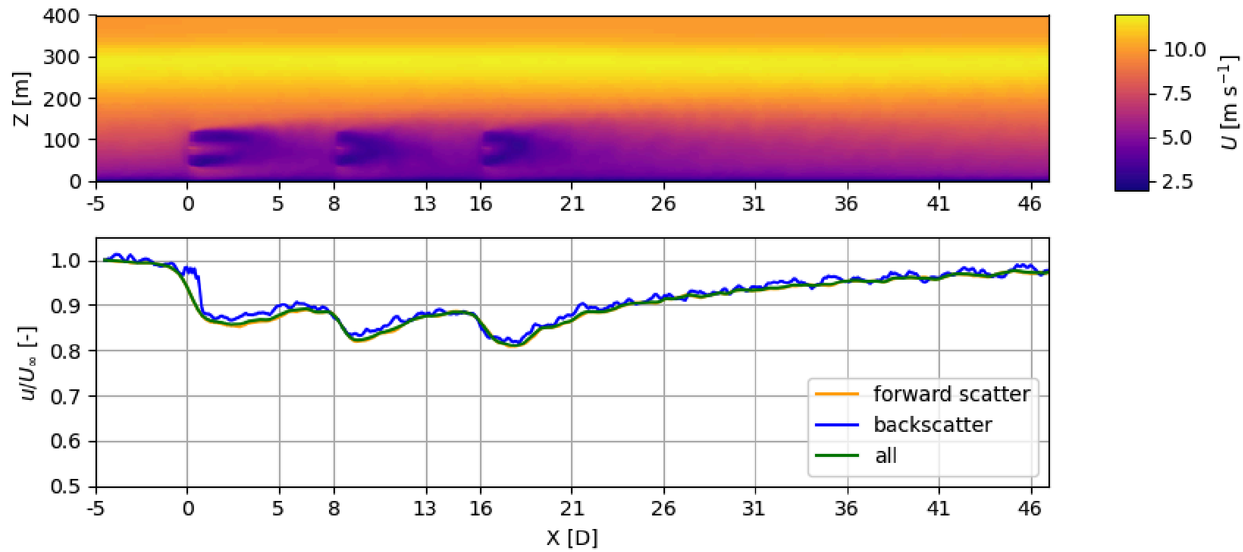


Strongly stable case:

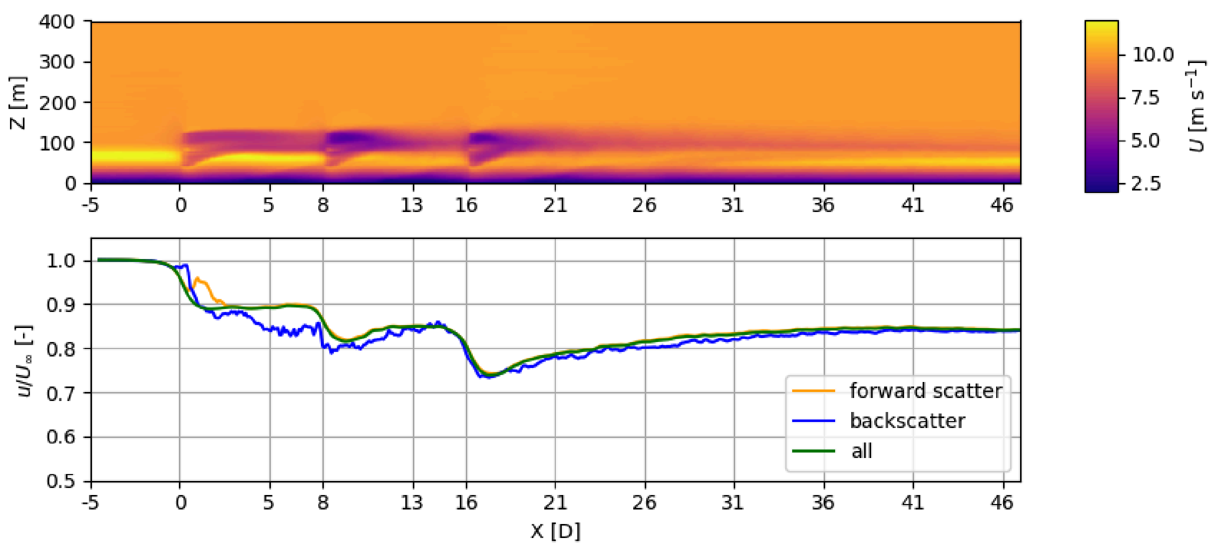


We also calculated conditional statistics on the hub-height velocity deficit. It is difficult to draw conclusions because of the smaller number of backscatter events within the wind farm region, although there is potentially a slower wake recovery in the strongly stable case during backscatter. We performed a similar analysis for the momentum fluxes and also found it difficult to determine the effect of backscatter on wake characteristics with the given setup. A study dedicated to this area could likely make more concrete conclusions.

Weakly stable case:



Strongly stable case:



Additional comments and suggestions are provided in the annotated PDF. Subject to satisfactory clarification and resolution of the issues outlined above, the manuscript could be recommended for publication.

Thank you for the annotated PDF. The comments from the PDF have been transferred to this document and responses are as follows.

Comments from the PDF

Line 28: There are several places where the full term is still used instead of the abbreviation, please check

Thank you for pointing this out. This has been fixed throughout the manuscript.

Line 49: Are there physical reasons for choosing TKE rather than heat flux, dissipation, or other quantities? In addition, the rationale for selecting the 50% threshold should be clearly explained.

Considering that the intermittent phenomena tends to be referred to as intermittent turbulence in the literature, it felt natural to use turbulence kinetic energy as it is commonly used to describe turbulence for a number of applications. We believe that in the Coulter and Doran (2005) study, they used heat flux because their stations were all relatively short and focused on near-surface characteristics. Since this study is focused more on wind turbine applications and heights that affect wind turbines and not necessarily the stability itself, the decision was made to use TKE as the main parameter of interest. However, we did also investigate heat flux, dissipation, and the Richardson number as metrics to define intermittent turbulence and found similar results since they tend to exhibit similar behaviors (see our response to later comments).

Our study is not meant to replicate Coulter and Doran, but more to utilize a method that they demonstrated for a similar phenomenon. As a result, the manuscript has been revised to the following:

“Here we extend their quantification method from turbulent heat flux to turbulence kinetic energy, which is the primary focus of our analysis and is a more common and accessible metric for assessing turbulence in the wind energy community.”

Line 259: It is unclear what the 5-degree value refers to. Please clarify.

The 5 degree value refers to veer across the rotor swept area. This has been clarified in the text.

Line 260: This paragraph is difficult to follow. Please clearly indicate which figure and which panels the discussion refers to.

> Thank you for bringing this to our attention. The text has been modified to include reference to Fig. 2a and 2b.

Line 263: figure 2 panel 3?

> This is correct. The text has been changed to Fig. 2(c).

Figure 5: The total mean does not appear to equal the probability-weighted sum of the conditional means. Please check

> Thank you for pointing this out. As noted above, the backscatter and forwardscatter lines were accidentally swapped in Fig. 5(a) and 5(c). They have been corrected and should appear to equal the probability-weighted some of the conditional means.

Line 308: Rephrase.

> This sentence has been rephrased to more closely follow the text from Holtslag and Niewstadt (1986) followed by how we ultimately implemented the definition. See lines 324-326 in the revised manuscript:
>
> "The boundary layer height can be determined using their definition, which is the height to which turbulence extends above the surface. Here, we calculate h by determining the height at which the heat flux goes to less than 1% of the surface value (see Fig. 3)."

Line 314: Additional explanation is needed for readers unfamiliar with this framework to clarify why h/L=19 is classified as an intermittent regime, while h/L=3 corresponds to continuous turbulence.

> We have made the following changes at lines 320-324 in the revised manuscript:
>
> "Their criteria depends on the boundary layer height h and the Obukhov length L, and defines when stability is strong relative to mechanical forcing, with large h/L indicating a very stable regime where turbulence becomes intermittent and difficult to sustain, especially aloft. For smaller h/L, buoyancy suppression is weaker relative to mechanical forcing resulting in sustained, continuous turbulence through much of the boundary layer."

Line 338: Again, why is heat flux not used for a direct comparison with the highlighted study?

Please see the above response.

Line 343: Not defined yet.

This has now been defined following the comment. Please see lines 371-372 of the revised manuscript:

“... with \sigma_U as the standard deviation of U.”

Figure 6: Not defined yet.

The two instances of CDF in the manuscript have been changed to just be cumulative distribution function.

Figure 6: There is no discussion of -5/3

We have shifted the -5/3 line to be closer to the spectra and added discussion. Please see lines 382-385 of the revised manuscript:

“In Fig. 6(d), as height increases, the width of the −5/3 inertial subrange decreases. The time series at 80~m is within the nose of the LLJ and the time series at 110~m is above the LLJ. At both of these heights, turbulence is very weak, so the spectra exhibit little to no inertial subrange, which is another reason there is little difference in the energy content between quiescent and turbulent periods at these heights is small.”

Line 367: TKE should be defined much earlier in the manuscript.

TKE was defined at line 321 of the original manuscript and line 349 XXX of the refined manuscript but is defined separately at the location of this comment. The reason for the differing definitions has to do with the available data output (we do not have high-frequency data at all points in space).

TKE when it is originally defined is now defined more clearly with equations and definitions in a similar style as the location of this comment.

Lines 349-351 in the revised manuscript:

"At the virtual tower, TKE is defined as 1/2*(u'^2 + v'^2 + w'^2), where u', v', and w' are the fluctuating components of the velocities obtained by removing a running 30 s mean, as we are focused on small-scale turbulence, and then averaged again over a 1-min interval to further smooth the data (shown in Figure 6(a) at various heights)."

Line 422: 25% is still comparable.

The term "minimal" when comparing the ambient turbulence to the wake turbulence has been reworded to "smaller".

Figure 11: add 50% reference lines as in figure 6 panel (b)

These have been added and we have included a short description of them in the figure caption.

Line 496: It would be helpful to show the TKE budget (along Z axis for example) earlier in the manuscript for verification and context.

Thank you for the suggestion. Unfortunately, we do not have all the outputs necessary to do a full TKE budget. However, we do have the high-frequency outputs for the gradient Richardson number, which demonstrates the balance of the shear and buoyancy forces. We have added two figures to the supplemental material for verification and context.

The first figure is a time-height contour of the gradient Richardson number during the analysis period and a time-series of gradient Richardson number at a height of 50 m (the height with the greatest intermittency or lowest intermittency fraction). In this figure, we can see the Ri number at 50 m periodically going above and below 0.25. We use 0.25 as the critical Richardson number, above which turbulence tends to be suppressed and below which turbulence is considered continuous (Stull 1986).

The second figures are 1 h time-averaged vertical profiles of shear, buoyancy, and the gradient Richardson number for the weakly and strongly stable regimes. For the strongly stable regime, we can see how buoyancy and shear balance on average to a critical value of Ri = 0.25 at approximately 50 m, which is where intermittency is the greatest.

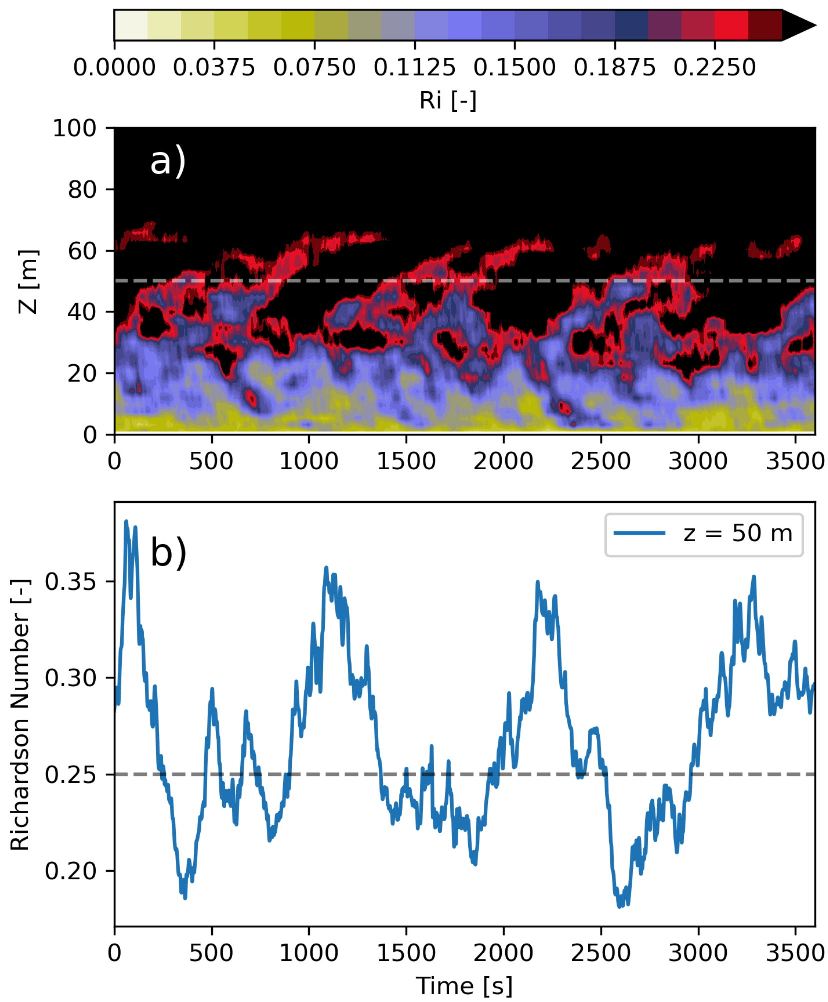


Figure B1: (a) Time-height contour of the gradient Richardson number (Ri) at 5D upwind of the wind farm on the inner domain during the analysis period emphasizing the critical Richardson number value of 0.25. (b) Time-series of Ri at z = 50 m with a dashed line at Ri = 0.25 corresponding to the critical Richardson number.

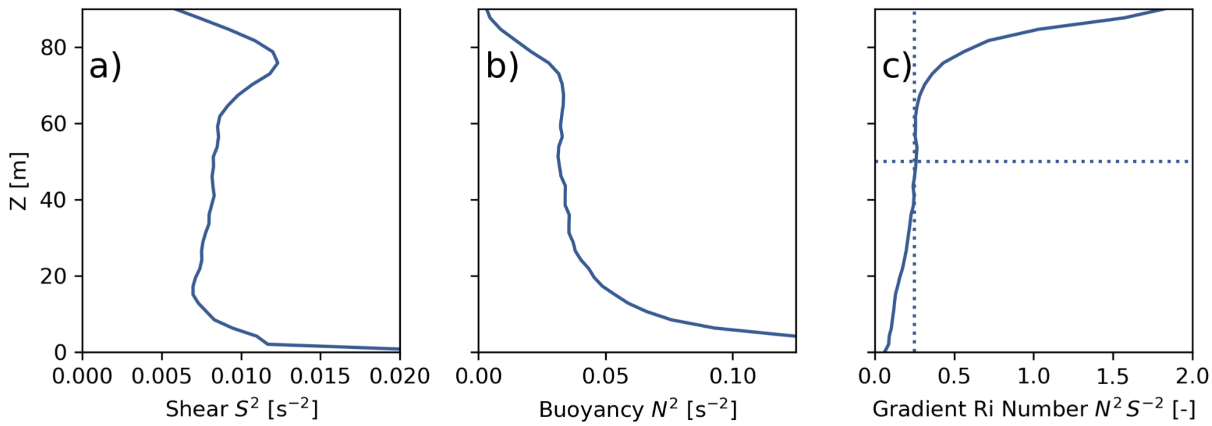


Figure B2: 1 h time-averaged vertical profiles of (a) shear, (b) buoyancy, and (c) Ri during the analysis period. The dotted lines in (c) correspond to the critical Richardson number of 0.25 and a height of 50 m.

Please note that these figures have been added as Appendix B in the revised manuscript.

Line 499: Turbulence is always intermittent. It may be more appropriate to use the term 'flow' instead.

The term flow is now used.

Line 514: Same here, it would be good to add TKE budget.

Please see the response to the comment two above.