Wind Energ. Sci. Discuss., doi:10.5194/wes-2016-60-RC2, 2017

Interactive comment on

“An analysis of offshore wind farm SCADA measurements to identify key parameters influencing the magnitude of wake effects” by Niko Mittelmeier et al.

Answers to comments from anonymous Referee #2 by

Niko Mittelmeier et al. April 25, 2017

Dear Referee,
Thank you very much for reviewing our paper. Your comments helped us to understand where certainly more explanations is needed and we hope that we could add clarity and additional content to answer your questions sufficiently. You are right, when you point out, that it’s a big step from SCADA data to stability classification and that strong simplifications have been made. For this reason we have accessed more data to close the gap between meteorological stability classification, TI, Shear and SCADA signals. We also want to be more precise on the purpose of this work. The main aim is to find turbine signals which can describe the magnitude of wake effects that are varying with different environmental conditions. With these signals it should be possible to fine tune wake models for more accurate predictions.
Our responses to your comments are marked as ***/ Response /***.

This paper presents a new parameterization of stability classes for the prediction of single and multiple wake effects based on met mast and LiDAR data. After reviewing the paper, I am fairly convinced that this line of reasoning is worth pursuing. However, there are some issues to be addressed before the paper can be recommended for publication. These are enumerated below.

Specific comments

Page 2, line 12 ‘rotordiameter’ is missing a space.

/ changed to “rotor diameter” /

Page 3, fig 2 caption ‘cycles’ should be ‘circles’.

/ changed to “circles” /

Page 4, lines 11 and 12 There are two instances where ‘is’ should be ‘are’.

/both instances changed to “are” /

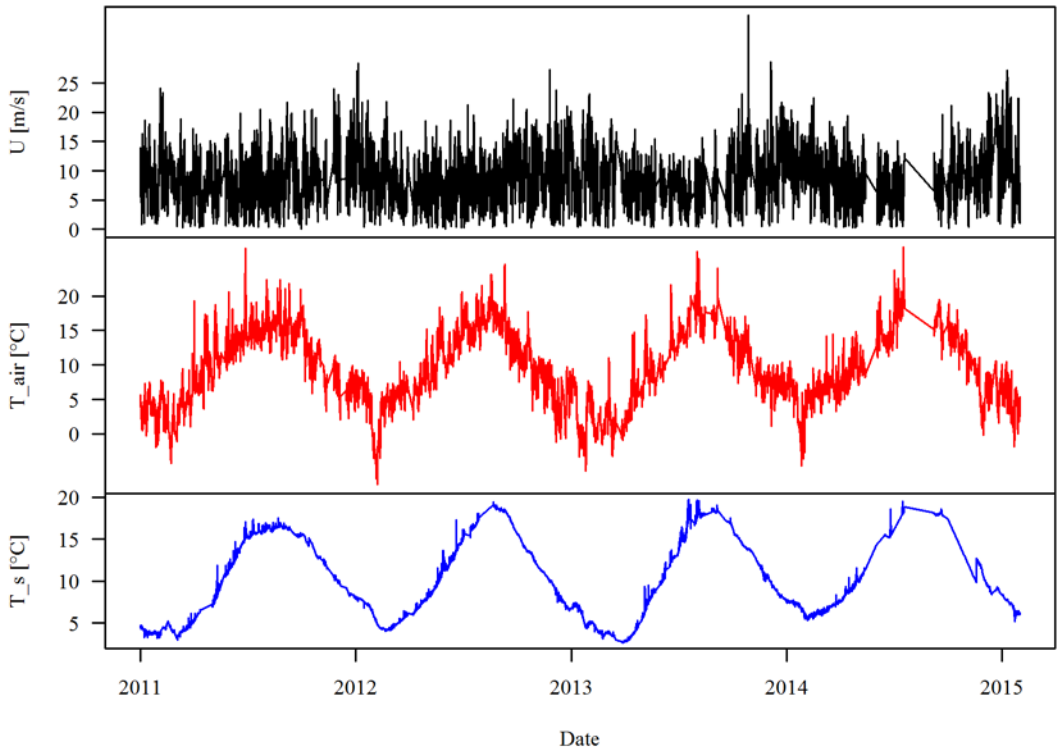
Page 4, lines 17 and 18 There are two spelling errors: ‘allowes’ and ‘includs’. Also the possessive is not necessary for ‘turbine’ and ‘nacelle’.

/ Changed to “allows” and “includes” and possessives deleted/

Page 6, section 3.1 It is surprising that a study specifically considering stability effects is relying on a simplified classification technique. This introduces a considerable amount of unnecessary uncertainty as an independent variable (i.e. the stability) is not directly measured.

***/ You are right. An acceptable representation of stability is needed and therefore we have accessed new data that has been just recently published on the BSH Fino Server. We will also develop our arguments slightly different. A new reproducible classification (see your comment from page 8) based on the magnitude of wake effects will be used and predictability with the different measured signals ($\zeta = z/L$, turbulence intensity and Turbine SCADA) is studied.

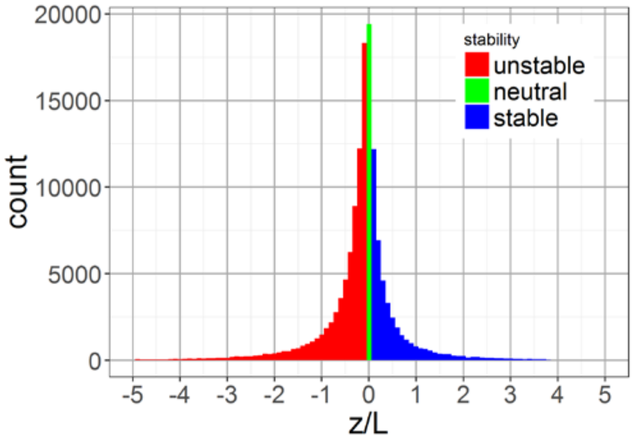
With latest calibrated temperature data from DEWI (Richard Fruehmann) we were able to follow the approach suggested by Ott and Nielsen (2014) and calculated the dimensionless $\zeta = \frac{z}{L}$ for T_air at 33m. The plot below shows the data availability for the selected period.



We decided to keep the number of classes at three (“unstable”, “neutral” and “stable”) based on the following table: (We will add more description of the methodology for the estimation of thresholds in Section 3.3, see also your comment for page 8) The estimated thresholds have also been proposed by Rajewski et al. (2013).

Category	Range
Unstable	$\zeta < -0.05$
Neutral	$-0.05 \leq \zeta \leq 0.05$
Stable	$\zeta > 0.05$

This leads to the following histogram for the three classes.



References:

Ott, S. and Nielsen, M.: Developments of the offshore wind turbine wake model Fuga, E-0046 Report 2014, DTU Wind Energy, Lyngby, Denmark., 2014.

Rajewski, D. A., Takle, E. S., Lundquist, J. K., Oncley, S., Prueger, J. H., Horst, T. W., Rhodes, M. E., Pfeiffer, R., Hatfield, J. L., Spoth, K. K., Doorenbos, R. K., Rajewski, D. A., Takle, E. S., Lundquist, J. K., Oncley, S., Prueger, J. H., Horst, T. W., Rhodes, M. E., Pfeiffer, R., Hatfield, J. L., Spoth, K. K. and Doorenbos, R. K.: Crop Wind Energy Experiment (CWEX): Observations of Surface-Layer, Boundary Layer, and Mesoscale Interactions with a Wind Farm, Bull. Am. Meteorol. Soc., 94(5), 655–672, doi:10.1175/BAMS-D-11-00240.1, 2013.

/***

Page 7, table 1 More discussion for the boundary values for TI is needed. How exactly were these values assigned to unstable, neutral, and stable?

***/ The presented values are taken from Dörenkämper (2015). There is no description how exactly these values have been selected. In earlier studies (Dörenkämper et al., 2012, 2014) the authors used 1min averaged data and therefore also different thresholds than in his 2015 studies.
With your later comment, requesting a more solid method to reproduce the assessment of the thresholds we will use our new classification method (See Comment for Page 8) .

References:
Dörenkämper, M., Tambke, J., Steinfeld, G., Heinemann, D. and Kühn, M.: Influence of marine boundary layer characteristics on power curves of multi megawatt offshore wind turbines, in Proceedings of 11th German Wind Energy Conference, Bremen, Germany, 7-8 November., 2012.

Dörenkämper, M., Tambke, J., Steinfeld, G., Heinemann, D. and Kühn, M.: Atmospheric Impacts on Power Curves of Multi-Megawatt Offshore Wind Turbines, J. Phys. Conf. Ser., 555(1), 12029, doi:10.1088/1742-6596/555/1/012029, 2014.

Dörenkämper, M.: An investigation of the atmospheric influence on spatial and temporal power fluctuations in offshore wind farms, PhD Thesis, University of Oldenburg, Oldenburg., 2015.
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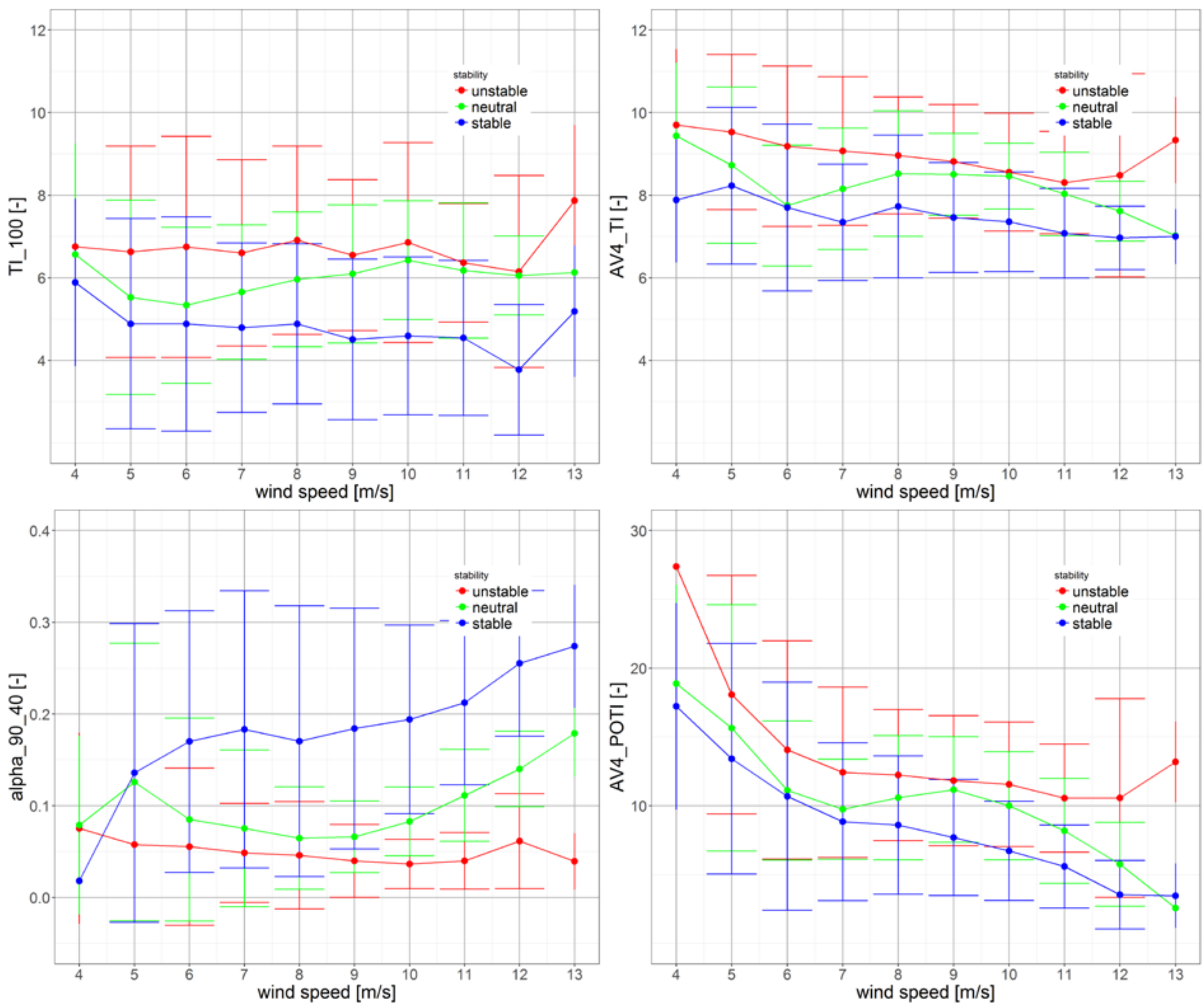
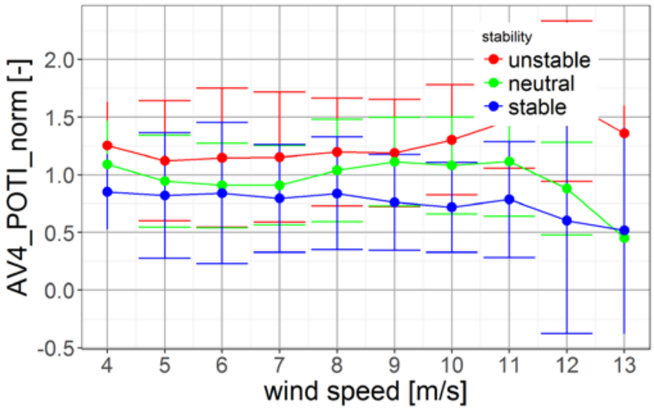
Page 7, eq 4 I suspect, but cannot verify, that the decent correlation between the two may be (in part) happenstance. Atmospheric turbulence intensity decreases with wind speed as a result of flow physics. The standard deviation of output power to power also decreases with wind speed but largely because the turbine controller plays an increasingly active role. The authors allude to this later in the paper but more discussion of why eq 4 might be a suitable proxy for eq 1 would be of interest.

***/ This is a very valid point. Therefore we have investigated more on the influence of wind speed. In the plot below, we have used the new stability classification based on $\zeta = z/L$ (See comment for Page 6). Bin averaged turbulence intensity (TI_100) measured at the met mast and at the nacelle (AV4_TI) as well as met mast shear (alpha_90_40) and AV4_POTI are plotted for each class as function of the wind speed. The selected bin of 8 ± 1m/s is quite well distinguishable with constant thresholds for all the provided variables. Whereas turbulence intensity from the met mast is fairly constant for the selected wind speed range, shear and POTI are showing a strong dependency on wind speed.



For this reason we propose to develop Eq. 5 which takes wind speed measured at the nacelle also into account.

We are now proposing a correction for POTI to substitute the wind speed dependency. This can be done by normalizing POTI with a third order polynomial. The resulting plot is shown below.



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Page 7, line 26 Why use the median instead of the mean?

***/ In our data example we obtained a mean = 0.516 and a median = 0.5108 which is very close together (0.0052). We decided to use the median because the mean was effected by some outliers. A deeper analysis of these outliers revealed that an additional filter criteria for the data is needed. The new filter removes 10-min intervals when one of the turbines has had a downtime in the interval before. In this way the flow throw the wind farm gets another 10-min time to develop. Additionally data with a power ratio > 1 meaning that the turbine in the wake center (±5°) produces more than a free flow turbine has been deleted (only two values). After removing these outliers, mean and median have now a difference of 0.0015. We agree that it is more appropriate to use the mean when enough care for outliers has been taken.

We will describe the new filtering in 2.4 and change 3.3 to "mean" instead of median.
/***

Page 8, line 2 '...the thresholds are selected to achieve the best distinction between the three data sets.' As these thresholds are central to the stability classification (and this work in general), a mathematical definition of best distinction must be included. Currently, this work is unreproducible by a third party.

***/ This is a very valid point. We will describe the methodology in more detail:

At first we select the normalized power (waked turbine, normalized by the power of a free flow turbine) for a small sector (10°) in the full wake for the relevant wind speed range (8 ± 1 m/s) (Fig 1a). Secondly we eliminate the dependency on wind direction by normalizing the normalized power for each wind direction bin (binwidth = 2°) with its mean value (Fig. 1b).

Fig. 1a

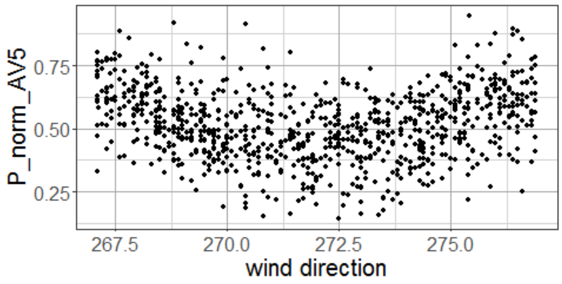
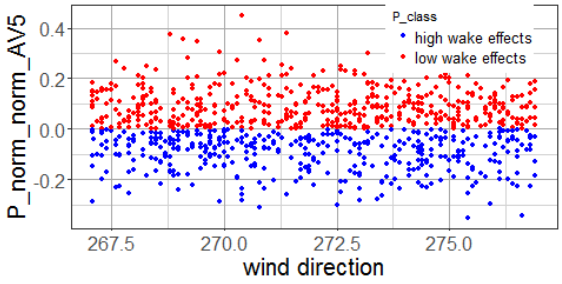


Fig. 1b



The third step divides the data set into high wake effects (values < 0) and low wake effects (values >= 0) and the density distribution of the variable of interest is plotted for these two data sets (Fig 2). We use the median for each density distribution to allocate the thresholds.

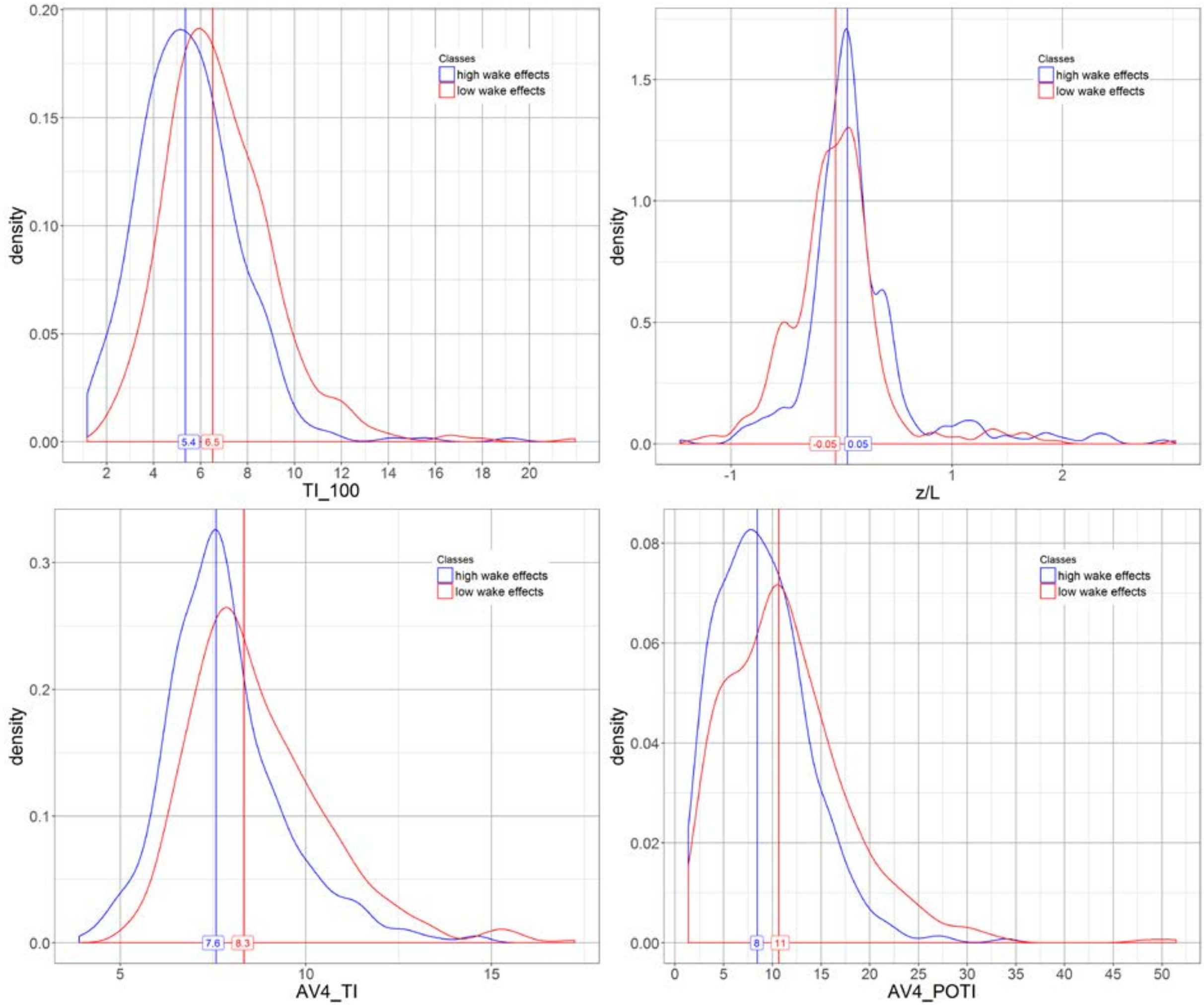
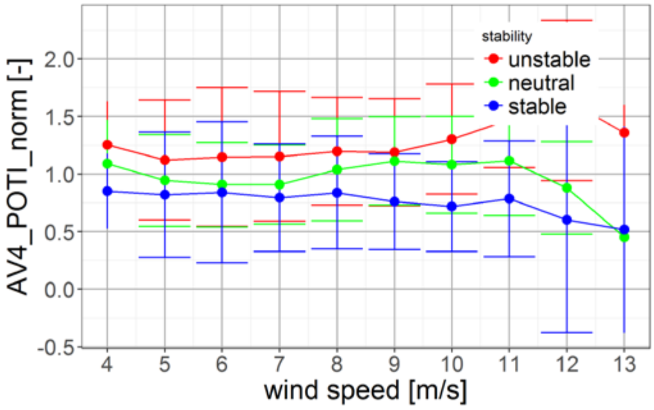


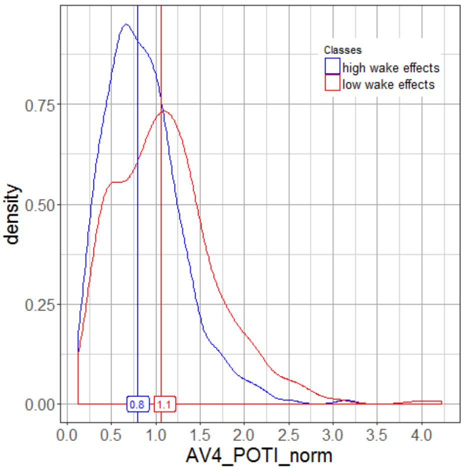
Fig 2. Data density for different variables based on low and high wake effects. The median for each distribution is highlighted with a vertical line. The data corresponds to a wind speed bin of 8 ± 1 m/s and a sector width of 10° around the full wake.

Note:
The TI and POTI thresholds have slightly changed compared to our first version of the paper. The difference in TI thresholds is due to the fact, that we have used the values from Dörenkämper (2015) and now we are suggesting this new methodology.
POTI thresholds have also slightly changed because the criteria was visual inspected and now we propose to use the median. In this way, the results should be reproducible now.

To overcome the shortfall of AV4_POTI signal having a strong dependency on wind speed, we propose a normalization of this signal with a third order polynomial.



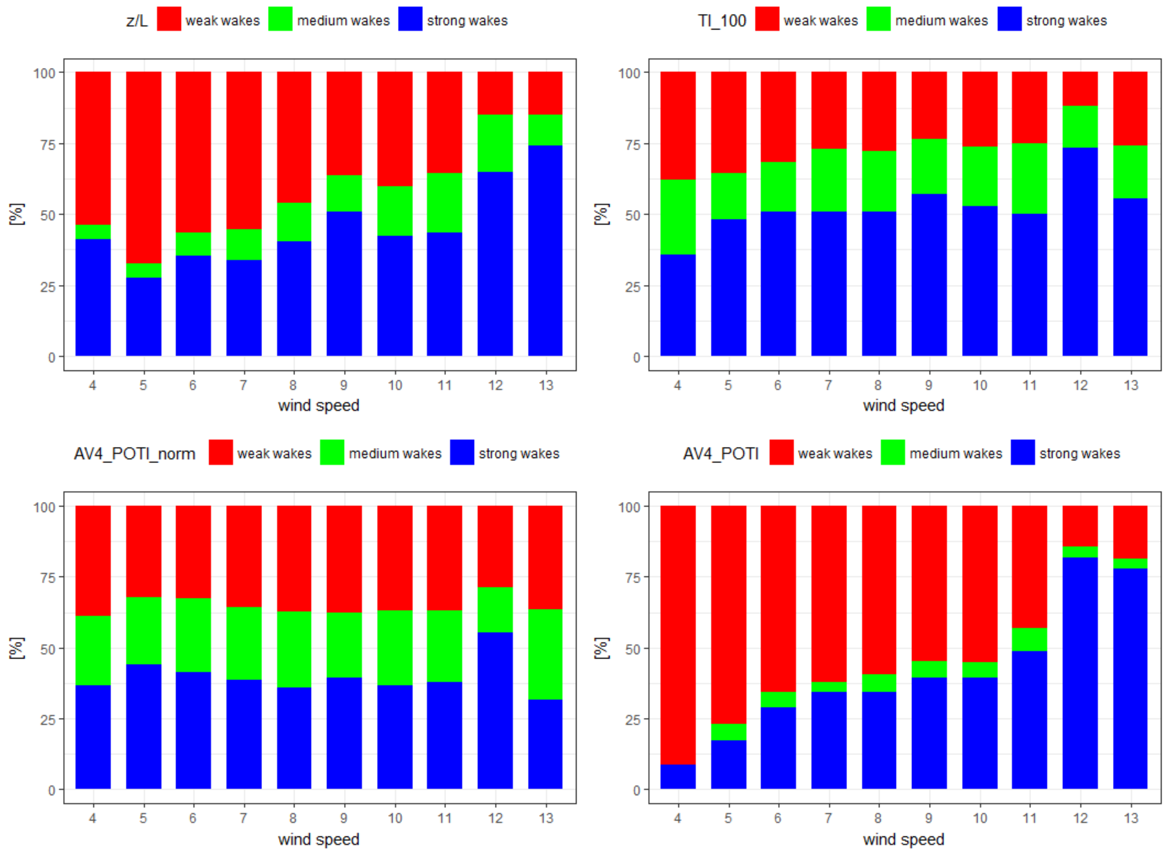
When applying the same methodology to AV4_POTI_norm as described above, we obtain a density distribution as below:



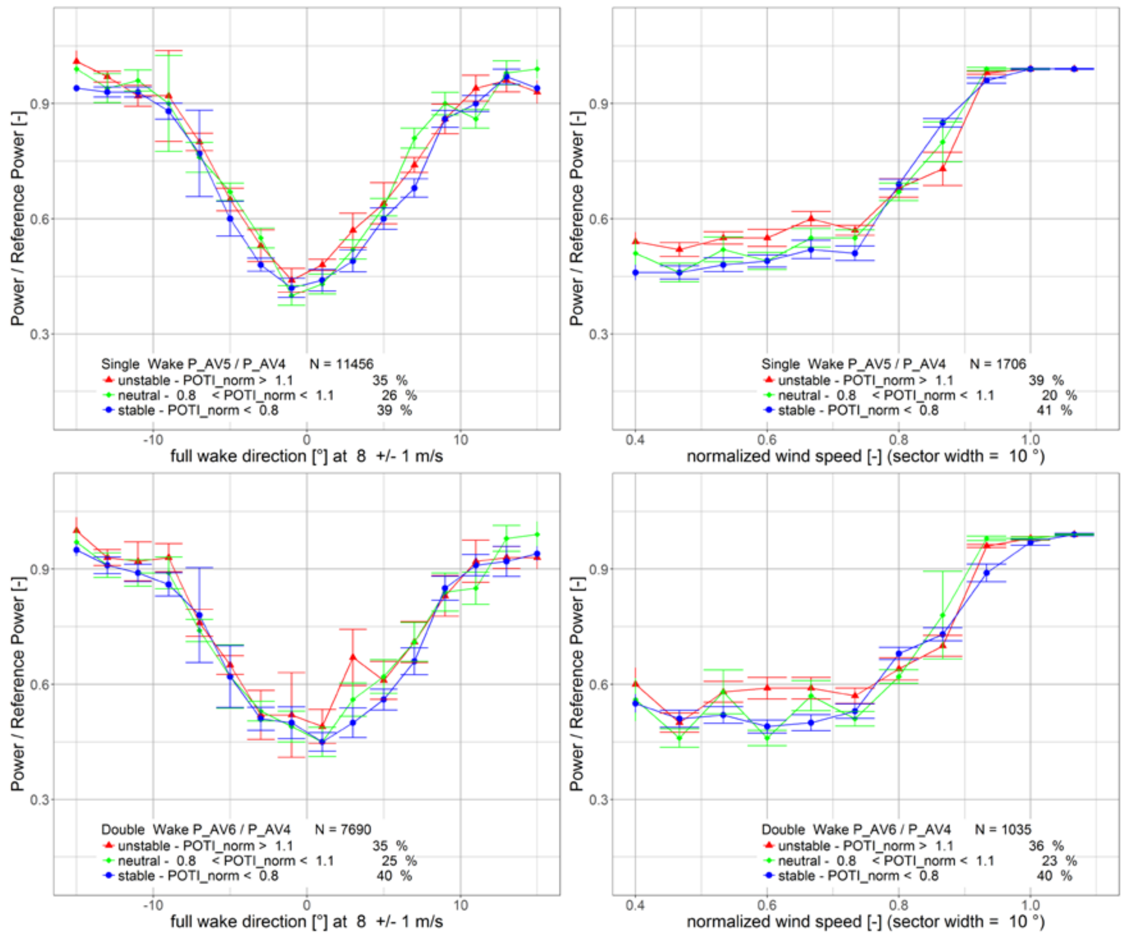
The table below summarizes different classes of interest:

Category	$\zeta = z/L$ **[-]**	**TI_100 [%]**	**AV4_POTI_norm [-]**
Weak wakes	$\zeta < -0.05$	TI_100 $< 5.4\%$	$POTI_{norm} < 0.8$
Medium wakes	$-0.05 \le \zeta \le 0.05$	$5.4 \le T_{100} \le 6.5$	$0.8 \le POTI_{norm} \le 1.1$
Strong wakes	$\zeta > 0.05$	$T_{100} > 6.5$	$POTI_{norm} > 1.1$

Looking at the distributions for each class, one can see an improvement from POTI to POTI_norm. Latter is much more comparable to the turbulence intensity measured at the met mast.
For z/L, weak wakes cases seem to become less frequent with increasing wind speed. POTI seams to overestimate this trend. TI_100 and AV4_POTI_norm provide similar results.



Using AV_POTI_norm as a classifier, we obtain the following wake plot:



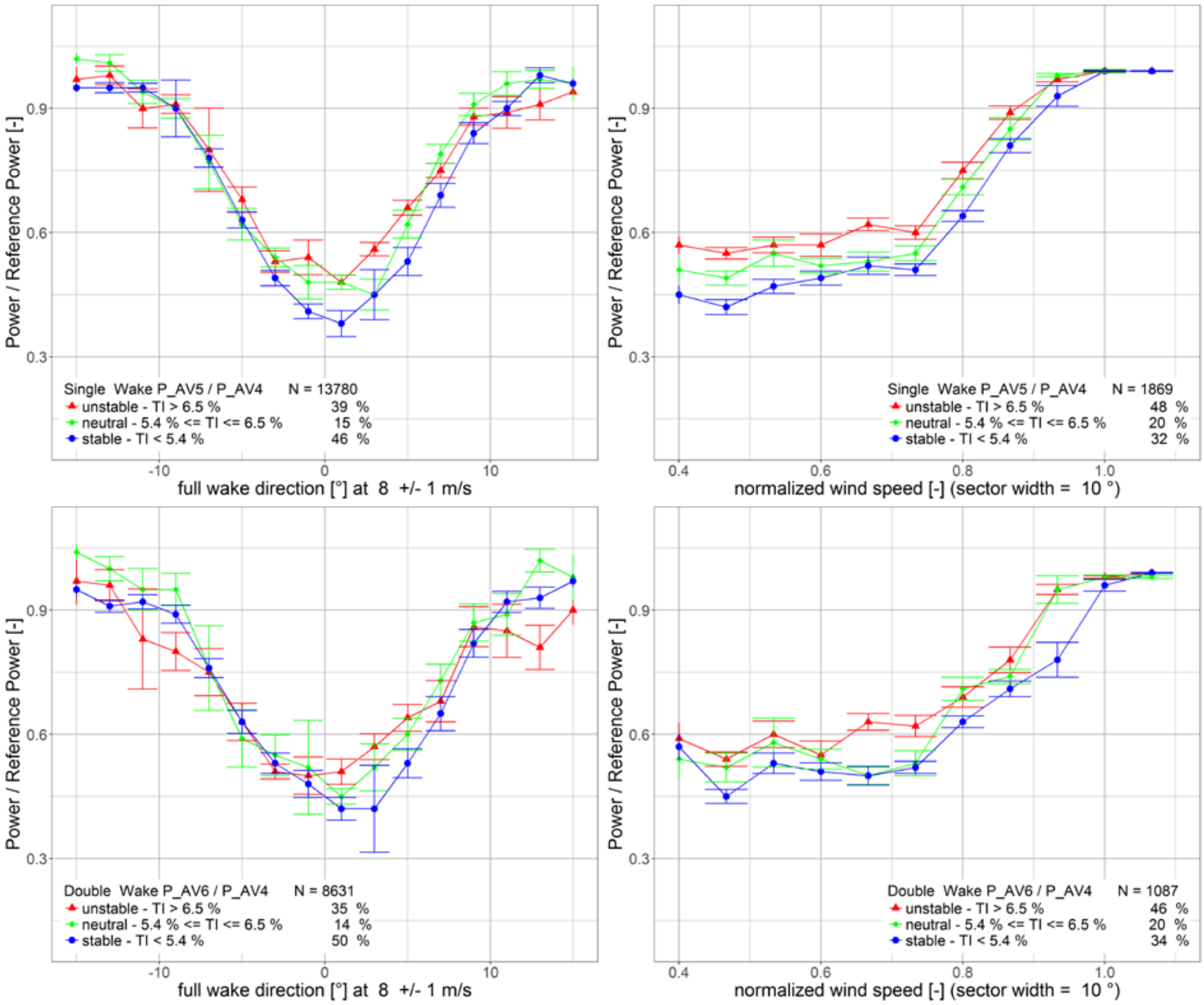
References:
Dörenkämper, M.: An investigation of the atmospheric influence on spatial and temporal power fluctuations in offshore wind farms, PhD Thesis, University of Oldenburg, Oldenburg., 2015.
/***
Page 8, line 7 Please avoid use of the word ‘prove’ in this context.

/ “prove” replaced by “shows”/

Page 9, fig 5, bottom left One point just right of the centre for the stable curve is clearly an outlier. Any comment? Tables 2–5 The large variation in thresholds suggests that the approach is not general and more details regarding how these thresholds are determined is needed.

***/ We have inspected the outlier and added two new filter criteria. The new filter removes 10-min intervals when one of the turbines has had a downtime in the interval before. In this way the flow throw the wind farm gets another 10-min time to develop. Additionally data with a power ratio > 1 meaning that the turbine in the full wake (wake center ±5°) produces more than a free flow turbine has been removed.

The new plot:



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Page 16, line 19 Why limit the data set to 7–9 m/s? Are these results representative?

/We will change the wording from “7-9 m/s” to “8 ± 1 m/s”. In this way it is consistent to the Fig. 5, Fig. 9 and Fig. 13. /

Page 17, line 1 ‘The difference in power production between stable and unstable cases is in the range of 10%.’ Stability is only inferred here from TI so the statement should preferably refer to differences between high and low TI conditions.

***/Agreed!
New wording:
“The difference in power production between high and low turbulence conditions is in the range of 10%, …/***

Page 17, line 8 The paper would be significantly more complete with this range of important wind speeds included in the analysis.

/ That’s true, we are working on it/

Page 18, line 7 Typo: ‘reviled’ should be ‘revealed’.

/changed to “revealed”/