Response to the anonymous referee #2:

Thanks a lot for the review. Here our response to the reviewer's comments. The response is given within XXX--- ---XXX

Regards,
The authors

The paper provides an interesting evaluation of the effect of proximity to the coast on offshore wind farm wake losses which is clearly a relevant and topical area, though there are some points to address:

1) Given that the paper acknowledges that roughness change is the main driver to the change in wind speed offshore, why did the authors not compare the use of WRF with a simple roughness change model to confirm this?

XXX--- Although we understand the reviewer's point as it is an interesting comparison, we think that this is out of the scope of the paper and that it will divert the attention of the paper, which is on wake modelling. It will also make the paper much longer and difficult to digest. But for the reviewer's sake we have performed such analysis using the roughness model of WAsP engineering (Astrup et al. 1996) and as observed (qualitatively from the figure below) the model seems to be capable to reproduce the wind speed gradients on the wind farm due to the land nearby. A comparison of WRF and the results of a RANS model are also given in van der Laan et al. (2017a) (this is also now stated when this paper is mentioned in the introduction) ---XXX

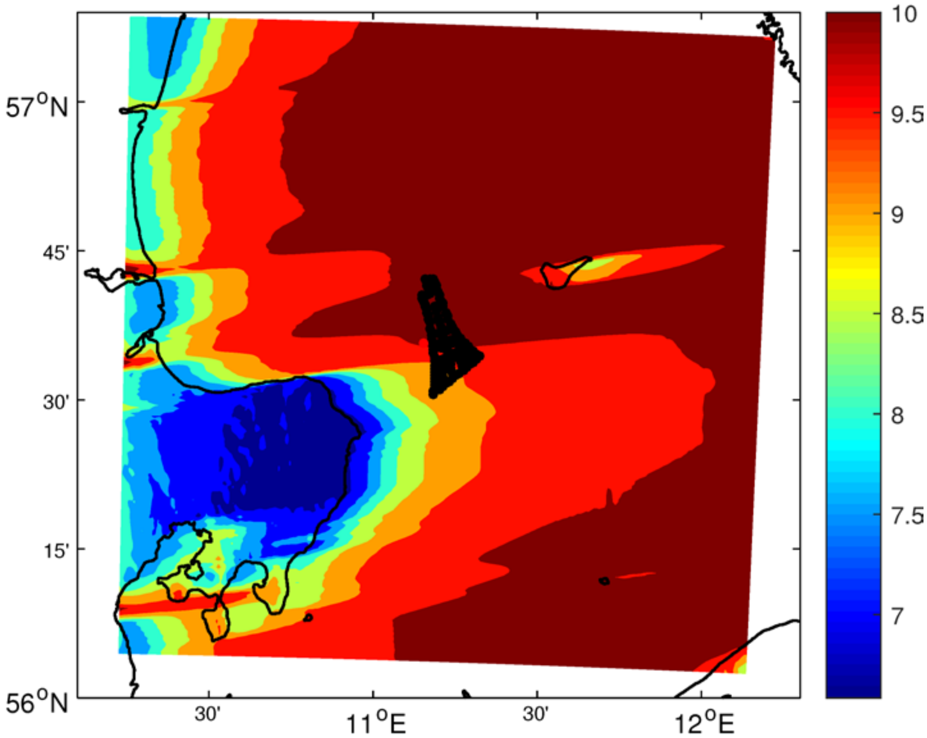

Figure. Wind speed simulated at hub height on the Anholt wind farm from a direction of 260 deg using WAsP Engineering.

2) It seems strange that a 'full' (non-linearised) RANS model was only used for the southerly flow case. Either such results should be shown for comparison in all cases or not at all.

XXX--- The RANS simulations are performed for the southerly flow case because, for this particular case, the SCADA does not match well the results of the “simple” wake models as we mention in the original submission (lines 10-12 Page 11). We wanted to investigate if this was because of the simplicity of the wake models. It is however too costly to perform 735 RANS simulations for the westerly flow cases since a single case takes about 3-4 hours using 153 CPUs (8 nodes). We think that is anyway valuable to show that the underestimation of the wake models is not due to the wake models per se but to the way that either data are treated or to conditions that we cannot extract from the SCADA such as atmospheric stability ---XXX

3) The discrepancy between the RANS model and the results in Fig. 7 was put down to a possible prevalence of stable conditions. It was stated that it was not possible to know this, but surely the WRF model results should have given enough information to at least estimate the stability conditions? Although not definitive, this could lend some weight to this hypothesis. Indeed, in all cases stability is likely to have played a role in wake recovery (and in the coastal transition), though this was not really commented on and would likely have affected the observed wake losses. Also, the RANS model could have been run under stable stratification (perhaps using a couple of z/L scenarios) to test whether a better fit was observed in this case.

XXX--- For the southerly flow case, the WRF simulations show a wide range of atmospheric stability conditions and in `average’ the atmosphere is actually close to neutral following the derived stability estimates from WRF. As we now mention in the revised paper, the instantaneous estimations of stability from WRF are rather uncertain (Pena and Hahmann, 2012). We acknowledge that it would be great to include RANS results with stability. However, our RANS model has only been validated with measurements and LES for neutral conditions (van der Laan et al., 2015a; van der Laan et al., 2015b). We have two published (Koblitz et al., 2015; van der Laan et al., 2017b) and one unpublished methods to account for atmospheric stability in RANS, which are all not yet validated to be used for wakes. We are currently conducting this research in a dedicated paper ---XXX

4) The results for the capacity factor in 3.2 used the WRF gradient wind speed with wake models. Given that previously, results were presented with both a WRF wind speed gradient and a single representative WRF wind speed, why was this not presented here?

XXX--- Given that the capacity factor is directly related to the AEP, one can estimate the difference in capacity factors when using different types of wind information by using the differences in AEP shown in table 3 of the original submission ---XXX

5) The authors suggest that an extension to the work would be to infer the wind speed gradient directly from the SCADA data. It seems odd that this was not already included in this work as it was such an obvious thing to do compared to trying to estimate the effect using a model. I would suggest that it would make this work much stronger if it were included.

XXX--- We guess the reviewer refers here to the last two sentences of the first paragraph of the original Discussion. In fact, this is more complicated than it sounds because we need to find all instances where all undisturbed turbines are concurrently operating showing quality-checked and calibrated yaw positions, power and pitch values. Also, and probably more difficult is it to define exactly what is a turbine under undisturbed inflow conditions when you have directions of all 111 individual turbines. This is partly the reason of the use of WRF since we can extract all 111 wake-free wind climates. The results in Fig. 5-right show that for most westerly directions, where the horizontal wind-speed gradient is the highest, WRF seems to do a fairly good job compared to individually-derived wind speeds for the most westerly row. We have reformulated the sentences to clarify the aspects of such analysis and added a footnote to clarify that the wind-speed gradient cannot be infer from wake-affected turbines ---XXX

6) As suggested in other comments on this paper, the explanation of linear and quadratic wake addition would benefit from some equations and the order of ‘1’ and ‘2’ should be consistent between wake models.

XXX--- We have done this as suggested ---XXX

References

Astrup P., Jensen N.O. and Mikkelsen T. (1996) Surface roughness model for LINCOM. Risø-R-900(EN). Risø National Laboratory, Roskilde, Denmark

van der Laan M.P., Peña A., Volker P., Hansen K.S., Sørensen N.N., Ott S. and Hasager C.B. (2017a) Challenges in simulating coastal effects on an offshore wind farm. J. Phys.: Conf. Ser., 854, 012046

Peña A. and Hahmann A.N. (2012) Atmospheric stability and turbulence fluxes at Horns Rev – an intercomparison of sonic, bulk and WRF model data. Wind Energ. 15:717—731

van der Laan, M. P., Sørensen, N. N., Réthoré, P.-E., Mann, J., Kelly, M. C., Troldborg, N., Schepers, J. G., and Machefaux, E. (2015) An improved k-ε model applied to a wind turbine wake in atmospheric turbulence. *Wind Energy, 18*, 889-489

van der Laan, M. P., Sørensen, N. N., Réthoré, P.-E., Mann, J., Kelly, M. C., Troldborg, N., Hansen, K. S. and Murcia, J. P. (2015) The k-ε-fP model applied to wind farms. *Wind Energy, 18*, 2065-2084

Koblitz, T., Bechmann, A., Sogachev, A., Sørensen, N. and Réthoré, P.-E. (2015) Computational Fluid Dynamics model of stratified atmospheric boundary-layer flow. *Wind Energy, 18*, 75-89

van der Laan, M. P.; Sørensen, N. N. & Kelly, M. C. (2017b) A new k-epsilon model consistent with Monin-Obukhov similarity theory. *Wind Energy, 20*, 479-489