Review of "Annual variability of wake impacts on Mid-Atlantic offshore wind plant developments" by Rosencrans, Lundquist, Optis, Rybchuk, Bodini, and Rossol, submitted for publication in Wind Energy Science

This is a WRF modeling study of the wakes of future offshore wind energy areas planned along the US east coast. It considers three scenarios of offshore wind development: Vineyard Wind only, lease areas in the Mid-Atlantic, and lease + call areas in the Mid-Atlantic. An impressive modeling effort was undertaken, with nested high-resolution WRF runs for one year repeated for the three scenarios above, plus the control case with no farms, plus runs with 0% and 100% TKE added. The team had also numerical noise issues, as seems to be the norm when the wind farm parameterization is turned on, and therefore had to deal with several additional runs to take care of it. The paper is definitely worth publishing as it includes interesting and valuable results. It is exceptionally well written. It was a pleasure to read such a good paper!

I have two somewhat major issues to recommend addressing prior to publication and several minor comments.

Major issues

1. The study focuses too much on added TKE

The paper promises to study annual variability of wake impacts (see comment #4 below about how it is monthly and diurnal, not annual). Such a study should have a main run with certain fixed parameters, perhaps a few case studies of special interest or a validation effort, and a few case studies to assess the sensitivity to some of the parameters. Instead, in this study a lot of effort was put on the sensitivity. The parameter of focus is the amount of added TKE, which is 25% in the default settings of the WRF model, and which was found here to have a relatively small impact on the power ouput (<5%). One would expect that the main run would be with 25% TKE and then a few cases (perhaps one week in each season) would be run with 0% and 50% and 100% TKE, in addition to 25% TKE, to assess sensitivity. The main results reported in the abstract and in the conclusions would be obtained with the default 25% TKE and a sentence or two would address sensitivity to TKE.
Instead, on one hand the study focuses excessively on the sensitivity to added TKE, because all the runs have been repeated entirely for 0% and 100% TKE, when a few weeks would have been sufficient. Of the 6 figures in the paper that describe modeling results (Figures 8-13), <u>all</u> of them are doubled to show 0% and 100% TKE. This would be understandable if TKE had a large impact on power output, but it did not (at most 5%).
On the other hand, the team did not perform a year run with the recommended value of 25% TKE. Thus in principle every value that they report in the abstract should be a range, but it is not. Plus, no TKE results are shown (I would like to see the equivalent of Figure 8 but for added TKE).
In summary, this study focuses excessively and at the same not enough on TKE. An obvious recommendation would be to ask the team to conduct a new one-year run with 25% TKE and

rewrite the paper to focus on those results and reduce the sensitivity analysis. But I think that this would be an excessive request, plus there is already a lot of value in the current runs.

My first recommendation is therefore that the results in the abstract and conclusions, which do not report a range (i.e., the range of results with 0% and 100% TKE) but are presented without explanation as one value (see comment below), be modified by either reporting always the range, or by using an interpolation based on the few days of 25% TKE results that the authors have already run (Fig. A1). What I mean is that the team could obtain a relationship between average power output (or wind speed deficit or whatever the parameter of interest is) with 0%, 25%, and 100% TKE from the few simulated days. This relationship does not seem to be linear from Fig. A1. An example of this relationship might be something like: the power at 25% TKE is the mean of that at 0% and 100% TKE, on average. Then use that relationship to report one value (per parameter) in the abstract and conclusions, that "fitted" to 25% TKE.

The second recommendation is that the authors add a figure and discussion on the TKE distribution in the wakes with 0% and 100% TKE, like Fig. 8.

2. The calculation of the losses from external wakes may be incorrect

From the abstract, the effects of internal wakes are reported to be -27.4% and the effects of external wakes are -14.1%. The sum of the two is -41.5%. However, the combined effect is reported to be -35.9%. This is problematic. At first sight, this discrepancy may be the result of the non-linearity of the wake processes. If so, all the authors need to change is to reverse the order of two sentences and add a few words in the abstract to explain it: "Internal wakes <u>alone</u> cause greater year-long power losses (27.1%) compared to external wakes (14.1%). When both are present, however, the mean year-long wake impacts reduce power output by 35.9%, which is lower than the sum of the two due to non-linear processes."
However, I suspect that there might be a design issue in the way the power losses are calculated in Eq. (9) and (10). Aside from the unclear notation (see comment below), the denominator of the two equations is not the same and that may be why the discrepancy arises. Eq. (10) is correct because there is no double counting: there are no losses in the denominator and the internal losses are only in the numerator. In Eq. (9), however, there are internal losses in both the numerator and the denominator, and they are not equal. The internal losses are not equal in the VW and CA cases because, as upstream conditions change due to external wakes in the CA case, the internal wakes change too and therefore the internal wake losses do not "cancel out", there is still some influence from the internal wake losses. As such, the ratio in Eq. (9) does not quantify just external losses because it still contains the effect of internal losses; it quantifies a mix of internal and external losses.
I suggest that the authors report Eq. 10 first (Loss_internal). Then, they should replace P_VW at the denominator with P_NWF in Eq. 9, to obtain the total effect from internal and external wakes due to the CA areas (call it Loss_total). We know this value: it should be -35.9% (from l. 384). The effect of the external wakes then is the difference between the total losses and the value from Loss_internal:

Loss_external = Loss_total – Loss_internal = -35.9% - (-27.4%) = -8.5% (Eq. 11)

This way the denominator is the same and the individual values for external and internal sum up to the correct total.

3. The stability classification is not adequate.

The authors use a very simple classification for stability based on the value of L (Eq. 8). Neutral conditions are those with abs(L)>1000 m. This is inconsistent with the published literature, e.g., Gryning et al. (2007) and Sathe et al. (2011) used 500 m, Wharton and Lundquist (2012) used 600 m, Rajewski et al. (2013) used 400 m, Archer et al. (2016) used 500 m. In fact, too few neutral cases were found here, less than 2.5% of the time (p. 29 l. 595). I am unsure what to recommend here because there is not an “accepted” value of L for neutral conditions, but the authors need to assess the sensitivity of their results to a few values, at a minimum 500 m. This could possibly help with the previous inconsistencies in the areal extent and wake length, as days that were actually neutrally stratified may have been mixed in with days with other stabilities to obfuscate some of the relationships.

Minor issues

4. The title needs improvements

The title suggest that the wake impacts “on” the wind farm development will be studied. This is somewhat inaccurate, as the study is about the wake impacts on offshore wind power production or output, not on the development. Development is choosing the number of turbines or their specs or their layout, which are all fixed in this study; or, development can be how the wind farm installations grow/change with time. Either way, the development here is given (3 scenarios), what changes is the power output.
Also, the title mentions the “Mid-Atlantic” as the focus area, but technically speaking the Mid-Atlantic stops as far north as New York state. From the U.S. perspective, the Vineyard Wind project is not in the Mid-Atlantic and neither are the northeastern lease areas of RI or MA. According to Wikipedia, the following states are included in the Mid-Atlantic: Delaware, Maryland, New Jersey, New York, Pennsylvania, Virginia, West Virginia, and Washington DC. To non-U.S. readers, “Mid-Atlantic” could be the Equatorial zone, as the Atlantic Ocean extends between the two Poles. I don’t have a good recommendation for an alternative, but perhaps “U.S.” should be added in the title because the study focuses on the U.S. offshore areas after all.
Last, “annual” variability suggests that many years were studied to understand how the production changes from one year to the next. Instead, only one year was simulated here. Thus the variability studied here is monthly/seasonal and diurnal, but not annual.

5. Simplify naming

There is no need to add “_only” to the name of the run with only the Vineyard Wind farm. Just call it “VW.”

6. Unclear notation in Eqs. (9)-(10)

These equations have already been discusses at comment #2, here I am focusing on the notation only. Eliminating "_only" will help (comment #5). P_WV_waked is not defined and uses a notation that differs from that of all other subscripts. All the other subscripts refer to a specific run, whereas "waked" refers to, I believe, a subset of grid points. But the same subset of grid points was used for all other denominators and numerators, thus the confusion. Plus the term P_VW_waked refers to run CA, I believe. I suggest something like (<u>not</u> including my recommendation from comment #2 above):

$$Loss_{ext} = \left(1 - \frac{P_{CA}}{P_{VW}}\right) \times 100\%$$

In the text below the equation then you specify that this equation is obtained from the grid cells over Vineyard Wind.

7. L. 318-320 ("While here … schemes"): this discussion is irrelevant and unnecessary here.

8. P. 17: some of these results are rather counter-intuitive.

if the TKE_100 runs produce weaker deficits and smaller wake areas, then the wakes should be shorter, whereas the authors report 58 km for TKE_100 and 55 km for TKE_0. The explanation provided is vague and unsupported (l. 346: "larger reduction in momentum aloft"??). The authors do not report exactly how the wake length was obtained. I suspect the method was somewhat empirical and in fact it is giving counter-intuitive results. I suggest that either the authors develop an objective and automated method for calculating the wake length and, if the inconsistency persists, they document and explain it; or that they remove any discussion of the wake length.

9. Improve Fig. A1.

Replace 10-m wind speed with 140-m wind speed in Fig. A1. In <u>all</u> panels (except c), add the results from NWF to appreciate the magnitude of the impacts.

10. P. 26-27: the discussion is unclear, the authors report "reductions" in several sentences, but it is unclear what is changing and what the reference is: are they discussing changes from TKE_0 to TKE_100 or from TKE_100 to TKE_0 or from NWF?

11. Fig. D1: need a legend for the colors. Also, are these wind speeds or wind speed deficits? The caption indicates wind speed.