Paper Review - WES-2024-154

Title: Understanding Cluster Wake-Induced Energy Losses off the U.S. East Coast

September 17, 2025

In this paper, the authors use a GPU-accelerated version of WRF to investigate the impact of cluster wakes off the US East Coast. In particular, the authors consider both existing and planned wind farms using available (where possible) and derived data regarding project capacity, capacity density, wind turbine rating and layout. The authors run one year simulations, with and without wind farms, assembling the most representative months among 24 years of atmospheric conditions. The authors introduce the concept of "wake shadow", by transforming wind into power losses by means of a typical turbine power curve. Using this metric and the same percent thresholds as for wind losses, the authors show that the area affected by wind farm wakes may increase by up to 30% compared to the standard approach that solely focuses on wind speed. Additional aspects such as the effect of stability on wake shadow and inconsistencies in calculating metrics among different studies are also discussed. The main key findings of this study (claimed by the authors and identified in the manuscript) are:

1. large wind speed deficits do not necessarily translate into significant energy losses
2. power losses are most pronounced under stable conditions and below rated conditions
3. given a threshold, the area affected by wakes is highly correlated to the capacity density (this is independent of the adopted metric).

While I think this study focuses on a very relevant topic for the wind energy community, it features very little novelty in my opinion, at least in its current framing. Regarding the first of the key findings, I believe that it is common knowledge that wind turbines do not harvest all available power above rated conditions. So it is quite obvious to me that, while wakes may be strong in these cases, power deficits are zero or close to zero. On the other side, I must recognize that there is a large tendency in literature to show wind deficit maps instead of power deficit maps, so the metric in itself is justified and useful. In my opinion, also the second key finding is almost common knowledge at this point, as it is very easy to find studies (both using WRF or micro-scale models such as LES) where stronger wake deficits and persistence are observed under stable conditions. Notably, when these studies investigate the physical reasons which dictate this behavior, they are still very valid and welcomed in my view, but unfortunately this is not the case for the present manuscript. The third finding is the greatest novelty of the study in my opinion, and I am surprised it is not even mentioned in the abstract.

For the reasons above, I personally don't recommend the publication of this study in the Wind Energy Science Journal, at least in its current form and framing. In order to be eligible for publication, the authors should in my opinion expand some of the interesting thematic areas that are briefly touched throughout the paper and shift the focus towards an assessment of different metrics and guidelines for their use in addressing the magnitude of cluster wake effects, which is in my opinion novel and of interest. Specifically, the authors are encouraged to expand on the following thematic areas:

1 try to differentiate between intra-farm and cluster wakes, producing estimates regarding the fraction of each of these effect on the total loss, which is what the authors solely considered. This would probably require to run additional simulations where the north, central or south lease areas are removed (or simulations of existing vs existing + new lease areas).

2 try to expand the analysis on stability, introducing additional metrics such as the Obukhov length L and/or the lapse rate inside the ABL (which gives a clear measure of stability). In fact, Ri and L are a proxy for the exact stability conditions throughout the entire ABL and are the only available measure when performing observations (in absence of a thermodynamic profiler, for example). In this case the authors have exact information on the stability, so why not use it.

3 try to restructure the paper narrowing focus on the comparison of different metrics to assess wake effects (thereby introducing the valuable "wake shadow" metric), and assessing their performance under different stability conditions, capacity density and lease areas. The authors should use these metrics to offer some insights on whether, using a fixed total capacity, it is better to have smaller and denser or a larger and less dense lease area. In my opinion this is not an easy question. Maybe the actual "affected area" is very similar in both cases, so the small area is convenient due to lower maintenance and installation costs. Also this point would probably require to run additional simulations using different capacity densities, making the manuscript less "realistic" but more insightful.

These three comments are expanded in Sec. 1, while specific and technical remarks are outlined in Sec. 2.

1 General Comments

1.1 Comment 1

In the abstract – as well as in the very title – the authors claim that they want to assess cluster wake effects (line 1: this study seeks to advance our understanding of energy losses caused by wind farm cluster wakes). This is further highlighted throughout the paper, but the modality used to carry out the study is not in line with the objective of understanding cluster wake effects. In fact, by looking for example at line 74, it is misleading to refer to "magnitude of the energy deficit for downstream wind farms", as the authors only consider WF (wind farms) and NWF (no wind farms) situations. Notably, energy deficits of a downstream wind farm in the form proposed by the authors are affected by both itself (the downstream wind farm) and upstream clusters. A deficit of 30%, claimed by the authors and observed inside some wind farms (line 271: under this scenario, power generation for wind farms like Community Wind, Attentive Energy, and Leading Light Wind could experience a greater than 30% reduction in power output)

is completely misleading, some of this deficit is there just because those wind farms are generating power, hence is not a power reduction of the wind farm. The research question, which the authors should try to address, is what part of this 30% reduction is due to cluster wakes effects.
This uncovers a limitation of the "wake shadow" approach, in my opinion, where velocity deficits are transformed into energy deficits. This metric measures the energy that can be extracted from any given region of the flow if a specific wind turbine is placed at this location. If one constructs the percent difference of this metric – like the authors do – by comparing the NWF and WF cases, the resulting map makes little sense in the context of addressing cluster wake effects, as it represents the energy deficit w.r.t. the case where each turbine behaves as if it operated in isolation. As a consequence, the deficit the authors are looking at contains intra- and extra-farm wakes, as well as blockage effects. To isolate the effect of neighboring clusters one should substitute the NWF case with the case where only the cluster of interest is present. Only at this point the metric provides the energy loss with respect to an ideal case where no wake effects from neighboring clusters are present. I believe that this is a crucial limitation of this study and of the "wake shadow" metric, and it should be highlighted. In an operational setting, where one wants to place a new wind farm within a new lease, the procedure to address cluster wake effects should be the following in my opinion. First, run a simulation with the new cluster only and second, include all existing neighboring wind farms. Third, use the "wake shadow" to calculate energy losses due to neighboring wind farms (cluster wake effects). NWF simulations make little sense to me in this context.

1.2 Comment 2

The authors use the Richardson number, which provides the ratio of buoyancy vs shear forces within the flow. While this number is able to tell if the flow is buoyancy or shear dominated, it does not provide a clear distinction between stable, unstable or neutral static stability, which is what affects the wake. A better metric to use would have been the mean lapse rate inside of the boundary layer, which provides exact information regarding the flow stability. Flows with Richardson number close to one (both positive and negative) can be either statically stable or unstable. It would have been interesting to see how Ri and L relate to this (and with each other, see for example Basu et al., 2014). Also, it would have been extremely interesting, downstream of the analysis proposed in Comment 1, to directly relate cluster wake losses to Ri, L and γ (the lapse rate) on a diagram, instead of a table, to investigate if any clear dependency is present.

1.3 Comment 3

While the paper aims to "advance our understanding of energy losses caused by wind farm cluster wakes", I think it does very little in this sense. I suggest the authors to restructure the paper with the objectives of

- understanding the **sole** impact of cluster wakes on new lease areas (by running these areas individually and with neighboring wind farms).
- addressing the effect of stability on these losses by using all available stability metrics (Ri, L, γ).
- relating different metrics used to compute losses (wind deficit and energy deficit using both formulas 7 and 8 in both cases) to the actual power deficit experienced by the planned wind farms in the isolated vs waked conditions. Make considerations on capacity density vs affected area with fixed total capacity.

if this framing (or similar) will be used for the revised manuscript and the analysis will be expanded, I will agree to suggest consideration of this manuscript for publication in the Wind Energy Science Journal.

2 Specific Comments

Please provide information on how the WRF simulations have been run, e.g. in batches of how many days, did the authors use spectral nudging, what is the update-frequency of ERA5 data used to provide boundary conditions.

line 40: "largest discrepancies" add percent value as for the neutral and unstable conditions.

line 42: "high-resolution" specify the meaning of this (the cited authors run with 1 km and 500 m resolution).

line 44: "they often fail to capture cluster wake effects across the entire farm, likely due to misrepresentation of internal wake dynamics" I would add that high resolution is certainly good in order to reduce the turbine accumulation within the same cells, with under or over prediction of wake effects, but it breaks the assumptions that are made in most PBL schemes. Even for the 3D PBL scheme, increasing the grid seems to produce even worse results from a wake-evolution perspective. I think this research gap in current literature needs to be mentioned, and it is especially important when looking at wake effects.

line 64: "marine spatial planning" specify in the US.

line 84, 92 (and other parts): the link in the reference Veer, 2023 is broken. This is quite an important reference, as it is cited also later as a report that describes the capabilities of the GPU-accelerated WRF. This reference needs to be accessible and the link to the report needs to be active.

line 110: I don't understand, shouldn't the authors avoid point 4 and only look at the months? So the best January from all Januaries, best February from all Februaries etc? This should automatically provide the year, for each of the selected months.

table 2: months and years are swapped.

line 113: "more meticulous" this is AI-preferred terminology (and also not very meaningful), please change to "alternative".

line 115: "a more complete picture of the meteorological situation is created" this is true only up to a certain extent. The authors are not checking for e.g. statistics of ABL height (not mentioning the geostrophic wind and lapse rate, which also impact power performance). I would arguably say that the picture is complete just enough.

equations 1,2,3: dimensional analysis of these equations is not correct. Also, P_{ijk} is not a power per-se, as it is normalized by density.

line 138: "The thrust coefficient is a dimensionless quantity representing the thrust force" not exactly correct, it rather represent the fraction of local flow momentum applied by the turbine to the flow.

line 158: clearly explain the need to consider peak shaving. Why not use the standard power curves? Do the authors expect peak shaving to have a non-negligible impact on wake deficits? It has already been showed that power output is not much affected, so why this approach given that the focus of this paper is not on loads. Also, layouts are approximated, so I really do not understand the need for this.

line 188: "around a lease are that where there" typo, please correct.

line 188: "wind farm cluster wakes" this study lacks distinction between intra- and extra effects, please see Comment 1.

line 207: "Additionally, we perform this analysis across three different wind lease regions (north, central, and south; Figure 1) to assess regional variations in atmospheric stability" if the authors are interested in stability, a valid choiche could also be to directly look at the mean lapse rate within the ABL (or at the hub height) γ. This is to rule out possible non-synoptic conditions due to specific atmospheric transients where, even if $Ri >> 1$ (stable) the static stability is neutral or slightly unstable (and vice versa). Also, the relation between Ri and L is of interest. It would be nice to see if the three parameters lead to the same statistics in terms of stable and unstable events.

Figure 5: the authors do not mention, in the entire paper, which curve they used to compute energy losses. This should be explicitly mentioned. The adopted power curve should be added to Fig. 3.

line 268: "the story" change to "these figures".

line 271/272: see Comment 1. Here intra- and extra farm wakes, as well as blockage effects, are being mixed.

line 273: "Large" make lowercase after ":".

line 274/275: "Significant power deficits are more often associated with below-rated wind speeds, regardless of atmospheric stability" this is very obvious and not a novel conclusion of the study. Rephrase to "this is in line with previous studies" or similar.

line 277: "Regions experiencing an annual energy loss of at least 5% are all situated near the wind lease areas. As the threshold lowers to 2%, the affected regions expand further" please try to avoid these kind of comments in a scientific paper. I think it is pretty obvious that wake effects decay moving away from the source. If I misinterpreted, then it was not really clear what the authors wanted to highlight with this comment. I suggest to rephrase or remove it at their convenience.

line 286: please try to avoid the term "wake expansion" which has a very specific meaning in the context of turbine (and also wind farm) wakes. I would suggest using something similar to "affected area", which I think renders the idea much better.

line 288: "CD in determining wake area size and underscores the need to establish an industry-acceptable energy loss threshold to maximize the area available for offshore wind development." this sentence is reductive of the problem, which is complex, in fact higher CD means either more power with equal lease area or the same power with a reduced area. It has to be seen if e.g. the lower $F_{wakearea}$ in low CD regions is compensated by the fact that the lease is larger to effectively produce the same power. There is also a cost problem, both capital and operational, is a larger lease more or less expensive? Installation cost is likely to be higher for a larger area, as vessels have to cover more distance, same goes for operations. So I don't quite get the point of the energy loss threshold, do the authors mean that energy loss has to be lower than a certain percentage outside of the lease by design? Anyways I agree that the wake shadow may be better than wind deficit as a parameter to look at, if calculated between isolated and waked wind farms cases.

table 4: specify that this table considers all atmospheric conditions in the caption (stable,

unstable, below rated etc).

line 297: see previous comment on "wake expansion". Please revise throughout the manuscript.

line 299: change "extend" to "extent", typo.

line 317: numbers (4.92, 3.23) are not matching with table 5, there is something off. Please clarify or correct.

line 341: "Future research should account for these methodological differences and exercise caution..." which formula is more physical in the authors' opinion? It would be nice to know how these formulae, applied to power/energy, compare to actual power/energy deficit calculated from turbine power variable in WRF.

line 362: "and and" typo.

line 376: see previous comment on "wake expansion". Please revise throughout the manuscript.

line 378: "industry-acceptable energy loss threshold to maximize the area available for offshore wind development" first, an analysis of waked and non-waked wind farm is required, to confirm what is the actual delta in loss w.r.t. the intra-wake-only case.

line 384: "accurate" please remove, accuracy was not measured. Energy metric is more "conservative", yes, but it was never demonstrated by the authors how to infer actual power losses from any of these metrics.

line 415: what PC stands for, project capacity? Please define it.

line 420: please define COD, commissioning date?

line 440: please define MA/RI.

line 449: change "specific" to "specifically", typo.

line 463: "Nevertheless, this correspondence is important, as it enables direct comparison of wake areas quantified using both wind speed deficit and energy loss expressed in equivalent percentages" I struggle to relate this sentence with what is stated above.

line 465: "Regarding the distinction between methodologies lies in the normalization of wind speed, Figure C2 further illustrate using data from Rosencrans et al. (2024)". Please check this sentence, it does not make much sense.

References

Basu, S., Holtslag, A., Caporaso, L., Riccio, A., and Steeneveld, G.: Observational Support for the Stability Dependence of the Bulk Richardson Number across the Stable Boundary Layer, Boundary-Layer Meteorology, 150, 515–523, https://doi.org/10.1007/s10546-013-9878-y, 2014.