

Wind Energ. Sci. Discuss., referee comment RC3
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Comment on wes-2021-109

Anonymous Referee #3

Referee comment on "Detecting and characterizing simulated sea breezes over the US northeastern coast with implications for offshore wind energy" by Geng Xia et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-109-RC3>, 2021

The paper described a method to identify three types of sea breezes – pure, backdoor, and corkscrew – from WRF model simulations conducted over the US Northeast. The simulations cover only one year (September 2019 – August 2020) and used two-way nested domains of 6 and 2 km resolution. Statistics of the results are presented, as well as a sensitivity analysis to the values of the thresholds adopted to identify the sea breezes and a comparison of the typical wind power production to be expected for each sea breeze type.

The paper is well written and the figures are clear, but there are so many issues with the paper that, in my opinion, it should be rejected for publication in this journal, although it might be suitable somewhere else.

I'll focus on major issues first.

The first major issue is that it is not clear why this paper is relevant for wind energy. The authors focus on sea breezes along the US East Coast and only at the end of the Discussion section present two figures somewhat relevant to offshore wind to presumably

show that the power output depends on the sea breeze type. How innovative or useful is this type of information? Why was the turbine placed in the center of domain 2? How would a developer or wind farm operator benefit from this information? I suggest that the authors choose a different journal, one perhaps focused on climatological or meteorological aspects.

The second issue is that the simulations cover only one year, therefore they are not long enough to produce meaningful statistics, climatologically speaking. The paper does not explain why the period was chosen. I would understand if the authors had collected observations over that period and wanted to validate the model results, but they did not, which is in fact my third issue. There is no model validation and we are left with no convincing evidence that the two-step method indeed finds sea breeze events correctly.

The fourth issue is methodological. The paper uses averages and means abundantly. I am particularly troubled by the use of average wind directions. Since the wind is a vector, the sum of vectors is not an average. What's the average of a northerly (0°) and southerly (180°) wind? A wind from the east (90°)? It does not make any physical sense. As such, Figures 4, 6-8, and 11 are not acceptable because they show "average" wind vectors. In addition, even taking the averages over each sea breeze type at each hour is at least questionable. The authors make the implicit assumption that each sea breeze type evolves exactly the same at each hour and therefore taking the average at hour, say, 11 LT is meaningful. But this is not true, differences occur at 11 LT due to the season, due to the position of the sea breeze front, due to the background wind flow, to list a few. Aside from vectors, this is especially troublesome with convergence and divergence fields used to identify the average position of the sea breeze front, because averaging a positive and a zero or negative value at a grid cell, for example, could dilute the signal of the sea breeze location. The authors need to find alternative methods to characterize the statistics of the sea breezes, for example using median values or some pattern recognition techniques.

The last major issue is probably just a matter of explaining things better. There must be a sub-region or location of focus of the study, otherwise how can there be one sea breeze type for the entire domain 2? If the sea breeze is affecting New Jersey, it must be east-to-west, but in the northern shores of Long Island is it north-to-south? If it's a pure sea breeze in New York City, could it be corkscrew somewhere else? I suspect that the issue is partly due to Figure 3, which I find very obscure. Where is the land? Is the prevailing wind the geostrophic wind? Where is north and south? Which way is the sea breeze flow?

Minor issues

- Why were the 4 quadrants introduced? I don't understand their purpose as they are not used. The text near line 130 talks about "mean ... for each individual quadrant", is this an area average over all grid points in the quadrant? Then a "dominant wind regime" for that day is obtained. What does dominant mean? How many hours out of 24? What if different quadrants had different sea breeze types?
- Table 2: It suggests that 246 days had sea breezes in a year, which seems too many. Again, maybe the sea breeze types are not mutually exclusive, but then I do not understand how the averages are even calculated.
- 185: here it seems that only 61 days were identified, but Table 2 is not consistent.
- 205: again, I am confused about the averaging, are you averaging over all 3 types of pure sea breeze here? If so, it seems even more questionable to average over such a broad range of wind directions.
- Figure 6: even given the fourth major issue above, it seems to me that the only location with a sea breeze here is Long Island.
- Figure 8: The corkscrew sea breeze seems to be in New Jersey only.
- Figure 9: because these figures are averages, the dynamic evolution is basically lost. There is no meaningful difference between the fields at 12, 13, and 14 for the pure sea breeze case, for example. Averaging out conv/div, the signal is diluted and the front is less distinguishable.
- Line 260: I disagree that "Overall, the results indicate ..." There is no evidence that the method works!
- Line 292: which 10-MW turbine?
- Line 293: why was this location chosen? Is it where a lease area is proposed? Please explain.
- Line 319: Your methodology of averaging out everything washes out the details of the timing and evolution of the sea breezes, that is why your results are not consistent with past studies.