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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Extreme Wind Shear Events in US Offshore Wind Energy Areas and the Role of Induced Stratification" *by* Mithu Debnath et al.

Anonymous Referee #2

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General impression

This manuscript describes the occurrence of high wind shear events off the US east coast, based on recent floating LIDAR observations. This is a highly relevant topic for wind energy applications, and since such measurements are quite scarce, this new dataset is very valuable. The analysis is decent, the presentation mostly clear with good quality figures. I like how the authors present their material in a way that is relevant and accessible for engineering applications, without sacrificing too much meteorological rigor. In this review I will focus mostly on the meteorological rigor.

The authors state that their "goal is to characterize these events and understand the

physical mechanisms governing their onset and dissipation". They did a great job in characterizing the events. When it comes to understanding, however, there are some points that require attention:

1. The authors focus exclusively on the US east coast, not only in their results, but also in their attribution of previous work. However, there are many more studies that have thoroughly analyzed similar events in other parts of the world, and provided much insight into their mechanisms. Disregarding these studies not only hampers the stated objective of understanding the phenomena observed in this particular location, but also feels a bit disrespectful. This feeling is reinforced by the notion that the authors coin a new term ("very low-level jets") for something that's been described many times before under the common name "low-level jet".

2. The authors explicitly state that they don't expect diurnal effects in the 'offshore' environment, which suggests that they have not considered the role of coastal mesoscale dynamics at all. By contrast, many of the earlier studies emphasize the role of the coastline in establishing a horizontal temperature contrast between land and sea, which gives rise to a plethora of mesoscale phenomena such as sea breezes, thermal low pressure areas, and baroclinically forced coastal jets. While many of the results presented in this manuscript are consistent with these theories, these theories are not discussed, the hypotheses not falsified. This makes it hard to confide in the overall rationale, even if it is quite sound in many places.

3. I think the manuscript does not accurately depict the role of atmospheric stability. First of all, the metric used to quantify 'atmospheric stability' (the difference between 2m air temperature and sea-surface temperature) is not actually a measure of temperature stratification in the atmosphere. Rather, it is a proxy for the buoyancy forcing at the bottom boundary, which is much more an 'external forcing' than the 'internal parameter' that is atmospheric stability. While the authors seem to be quite careful not to use these terms interchangeably, the presentation in its current form is prone to misinterpretation. Secondly, the authors often refer to stability as the 'driver' or 'cause' of

enhanced wind shear. Moreover, stability, wind shear and turbulence are sometimes discussed as if they are separate quantities, while in fact they are tightly linked: in a stably stratified atmosphere, buoyancy acts to suppress turbulence, while wind shear acts to enhance is. The result is a very delicate balance. If the shear term 'wins', turbulence will be produced, and will act to dissolve both the wind speed and temperature gradients. If the buoyancy term 'wins', turbulence will be suppressed, which 'permits' or 'enables' the vertical gradients to grow. But then the competition starts all over again. So it doesn't really make sense to speak of stability as a cause or driver, because it is very much part of the phenomenon itself.

4. I'm not sure if I understand the jet detection criteria, I think the authors may have made a small mistake here. It would be great if they could share their code, because that would help me to verify the method and reproduce the results.

5. Directional wind shear ('veer') is not discussed. I wonder how much of the 'wind speed shear' discussed herein can be attributed to changes of wind direction with height.

More details to clarify these concerns may be found in my specific comments.

Specific remarks

P2 L38: This statement is too easy. I understand that the authors focus on the US east coast, but when it comes to understanding the low-level jets, I think it would be fair to also acknowledge some studies focusing on different areas as well. For example, Burk and Thompson (1996) and Parish (2000) studied similar events on the US west coast, and they provide a much more complete physical interpretation. In addition to static stability, they argue that the coastal discontinuity introduces a horizontal temperature contrast, and that the associated baroclinity and geostrophic adjustment play an important role in the establishment of the low-level jets. Colle and Novak (2010), which have been cited by the authors, state that they find 'qualitative agreement' with these findings. Furthermore, there are numerous

studies in Europe, e.g. at the Hovsore measurement site in Denmark, the North- and Baltic seas, or the Iberian Peninsula, for example: - https://doi.org/10.1175/1520-0493(1996)124%3C0668:TSLLJA%3E2.0.CO;2 - https://doi.org/10.1175/1520-0450(2000)039%3C2421:FOTSLL%3E2.0.CO;2 - https://doi.org/10.1002/qj.2386 - https://doi.org/10.3402/tellusa.v66.22377 - https://doi.org/10.1002/joc.5303 - https://www.jstor.org/stable/43749611?seq=1 - https://doi.org/10.5194/wes-4-193-2019 - https://doi.org/10.3390/en13143670

P2 L38: It would further increase the relevance of this paper for the audience of WES if it was put in context to the global occurrence of the phenomena. In addition to the references mentioned above, the global climatology of coastal jets in Ranjha (2013) and Lima (2018) might also be a relevant sources. - https://doi.org/10.3402/tellusa.v65i0.20412 - https://doi.org/10.1175/JCLI-D-17-0395.1

P3 L45: It would be good to also discuss the use and limitations of reanalysis data. Especially the most recent datasets with unprecedented resolution have shown at least some skill in reproducing coastal stratification and low-level jets, and they can overcome the spatial limitations of observational studies. See e.g. the global climatology by Lima and the study by Kalverla mentioned above.

P3 L58: While I agree that these datasets are important and perhaps unprecedented, I think the authors are overstating their comprehensiveness and relevance. I don't think it's justified to claim these measurements are representative for all lease areas in figure 1. The coastline in this figure stretches over 8 degrees (!) latitude. Also, the buoys are placed quite for offshore, which is definitely valuable, but the coastal morphology has been shown to significantly impact the structure of the boundary layer closer to the coast. A more nuanced statement would be that they enable a first order characterization of the overall/larger scale situation.

P3 L62: A wind speed maximum at 100 m has been reported in many previous studies under the term low-level jet (as opposed to very-low-level jet). I don't think this event is unique and/or different from those previous studies. Admittedly, there are also studies that describe low-level jets in the lowest 500 or even 1000m. But if the authors choose to coin this new term, it would be good to further expand on its precise definition. What sets it apart from (some) other studies, and which other studies actually report on the same phenomenon under the conventional name?

P3 L64: This reviewer doesn't understand why many authors are so keen to report power-law exponents in situations where they don't apply. The core assumption for this type of fit is that the wind speed increases monotonically with height. This condition is obviously violated in the case of a low-level jet event. One could argue that the profile can be fitted up to the wind speed maximum, but from the description it appears that the authors used a fixed range between 40 and 160 m. Consequently, a power-law fit would underestimate the wind shear for wind speed maxima below 160 m. Of course, the power-law is deeply embedded in engineering standards and practices, and it is good to communicate in some sort of common vocabulary. But in a scientific text it seems inappropriate to present this metric without any discussion of its shortcomings. PS. I'm happy to see such a discussion near the end of section 2. Perhaps the authors can add a short note here in anticipation of this discussion?

P5 L95: I don't really understand criterion 1. It doesn't necessarily identify jets, right? The highest shear will almost always occur near the surface, also for 'conventional' power-law or log-law profiles, and shear will generally decrease as you go upwards. Even in a low-level jet situation, the 'height of maximum shear' doesn't necessarily coincide with the jet nose. Didn't the authors just mean to refer to the height of the maximum wind speed? That would also help to understand criterion 3, where the (height of) the maximum wind speed are used.

P5 L95: Criterion 2 refers to the 'maximum shear' across the rotor layer, which is confusing. Is it the shear over the (entire) rotor layer, or is it the maximum over the shear computed over smaller height increments, such as currently described for criterion 1?

P6 L105: what if no local minimum is detected? Is the lowest wind speed in measurement range used instead?

P6 L107: I'm happy to see the reference to Baas et al., but I think a bit more discussion on how this algorithm differs (or not) from, and credit for, previous studies is appropriate.

P6 L116: While I agree that the bulk wind shear is more relevant than the power-law exponent, this parameter is also sensitive to the depth of the layer over which it is calculated. Especially in the case of low-level jets. I wonder what would happen when the 'maximum' shear from LLJ criterion 1 or 2 is used instead. Another relevant discussion may be found in https://doi.org/10.5194/wes-4-193-2019.

P7 Fig 4a: Very strong figure! I just wonder what the fitted line is supposed to represent. And what it would look like if the y-axis and color-axis are swapped. This is what I would do intuitively, but perhaps the message is stronger as it is now.

P7 Fig 4b: There seems to be a sort of kink near x = 0.02. Could there be any physical explanation for this, and if so, would it make sense to use this as a threshold instead of the 90th percentile?

P7 L127: I wonder if this is actually a significant difference. I would say it's the same order of magnitude. Looking at figure 5 it seems that there are more short-lived events on the SW buoy. Is there also a (significant) difference if the cumulative time is used instead of the absolute number of events?

P8 L140: "influence of local conditions . . . particularly, atmospheric stability" up to L145: "role . . . in driving". While it is true that stability is important (otherwise the wind speed gradient is quickly dissolved by turbulent mixing) and arguably a necessary condition, I would be careful not to overstate its importance as a 'driver'. One could also interpret stability and wind shear as two sides of the same coin, or manifestations of the same process. Whereas a 'driver' is more of an external force such as the advection of warm air over a cold (sea) surface, or the differential heating over land and sea.

P8 L142: Also in agreement with many of the other previous studies mentioned earlier in this review.

P8 L147: Note that Basu (2018) proposed an elegant method to estimate the Obukhov Length based on wind speed observations only: https://onlinelibrary.wiley.com/doi/full/10.1002/we.2203. It would be interesting to compare the two measures of atmospheric stability. Personally, I'm not a big fan of the Obukhov length for its inherent assumptions, and because delta T is a much more direct observation. But in this case the height difference is quite pronounced, so if you find that L and delta T show similar patterns, that would strengthen the analysis. At least don't state that you can't know it. Also note that, since you're comparing air and sea temperatures, this metric strictly represents the bottom forcing rather than the atmospheric temperature stratification. This is probably more relevant, but it has implications for the subsequent discussion, and is prone to misinterpretation.

P8 L149: Note that a similar reasoning goes for TI and the shear exponent: since it's normalized by the mean wind speed, you don't actually measure the relevant bursts. You could make a figure similar to figure 4a. Wouldn't the standard deviation be a better measure in this case?

P9 L156: Note that parallel to the coastline is also "aligned with the land-sea temperature contrast". Compare to literature about "thermal lows", and literature on baroclinic (low-level) jets (not necessarily offshore).

P9 L159: The hypothesis put forward in this paragraph aligns with the 'Blackadar' model of an inertial oscillation. Note that there is also the 'Holton' mechanism, which is also (maybe even more) consistent with the observations shown so far. These mechanisms are explained e.g. in 2 papers on the great plains LLJ: https://doi.org/10.1175/JAS-D-15-0307.1 and https://doi.org/10.1175/JAS-D-14-0060.1 and references therein. Especially this section would be less speculative if

it was presented in the context of these two mechanisms. Also related to my previous notes about 'causes' and 'drivers'.

P9 L167: These are two very nice example cases. Especially at the end of event 2, it strikes me that the temperature change is so abrupt. I wouldn't expect that if the wind was continuing to blow from the same direction with a very long fetch without any changes in surface properties. Such a change in advected air would only occur with a frontal passage and/or perhaps a change in wind direction (in which case that would be the cause, or driver, of the event). So it might be interesting to show wind direction as well. Furthermore, I'd like to put forward that an abrupt change of temperature at some fixed height can also be caused by turbulent mixing after a prolonged period of growing stratification. While I don't think that's what is happening here, I want to stress that temperature stratification is not really a driver of wind shear, but rather a result of the same process. Another interesting point to think about is the spatial dimension. Is it really the case that only temperature is advected, and stratification of wind and temperature build up as a result? Or is the air that is advected air already stratified, and does it just strengthen a bit over time? And vice versa at the end.

P12 Section 3.4: This section is very short and quite superficial. It highlights some small differences in mostly atmospheric temperature between the two buoys, but doesn't proceed to explore why this might be the case. I also miss a discussion of the role of the distance to shore, which is quite different. I suspect this is the most important difference. If, as I have argued, the land-sea temperature difference plays a role in establishing the stratification, then the distance to shore is one of the key parameters. To make this clearer, the authors might also have a look at some literature on the extent of the sea breeze, which is a closely related phenomenon.

P13 L204: I disagree that the absence of a diurnal signature is expected. The distance to shore is probably not large enough to disregard the effect of mesoscale dynamics related to the presence of the coastline, which are largely driven by the diurnal cycle.

P15 L227: likely due to shear. I don't understand this reasoning. I think the more likely reason is given a few lines later, where the authors state that the low levels of turbulence suggest that there is stable stratification. But it is much more clear when you look at it from a different perspective: since stratification is a prerequisite for the formation of low-level jet events, one could state with near-certainty that stable stratification is present here. Subsequently, it makes sense that there is very little turbulence, because the stratification suppresses turbulent mixing. Stratification, high shear/LLJs, and low turbulence all go hand in hand.

P16 section 4: This is a very interesting section, and it connects very well with the use cases presented in figure 8. I would suggest to move this section to immediately after section 3.3. With respect, the sections 3.4 and 3.5 are much less relevant for understanding the events. I would have liked to have some closure on the mechanisms before continuing with some refinements and impact for wind energy.

P16 L232: 'caused by'. As should be clear by now, I think this statement is not accurate. I would suggest rephrasing it as "associated with".

P16 L235: 'generally consist of'. How did the authors analyze this? When it comes to understanding these events, this synoptic analysis is the most important part of the study, and it would deserve a more extensive explanation.

P16 L235: compare with previous studies on 'thermal lows'.

P18 Fig 14: How did the authors come up with this figure? Is it inspired by a general text book example explaining a low pressure system? Did the authors actually see the fronts in real cases? For if this is more like a thermal low, then I would not expect such fronts. I think both are plausible, but it is important to make clear whether we're looking at real data here, or a visualized hypothesis. Many readers will only remember the figure, and I'm not sure whether that take-home message adequately summarizes the discussion in section 4, which has quite a few loose ends.

P17 L267: Even though synoptic charts are published only 6-hourly, plotting the wind direction itself could provide some clues. Additionally, one could draw pressure maps based on recent reanalysis datasets or other published model data that are available at hourly resolution nowadays (e.g. ERA5).

P17 L272: I would expect that LLJs occur under more moderate conditions, as they require quite a subtle balance between processes. Would it be possible to distinguish between two types of events: one with a strong synoptic forcing, such as the winter storms, where the temperature stratification suppresses (to some extent) the strong mechanical turbulence and consequently one would observe strong but monotonic shear; and another type of event where, in the absence of strong synoptic forcing, the mesoscale/coastal dynamics play a much larger role?

Technical

P1 L10: "when when" P1 L15: "government retains … government retains" P3 L55: I'm happy to see a reference to the dataset. I noticed the download page also provides a citation statement (near the bottom). It would be good to add this to the reference entry. P7 L126: Difficult sentence due to … and … and … and P9 Fig 6c: the y-ticks are a bit strange since half event counts don't make sense. P10 Figure 7: "dependency … on": I'm not a native speaker, but this term to me suggests some sort of causal relationship, which would at this point be unjustified. "Dependence", according to the dictionary, is just a state of not being independent. But shouldn't this be 'dependence of a and b', or dependence between a and b' (like correlation between…)? P13 L186: Perhaps use 'difference' instead of 'change'? 'Change' to me suggests that there's a temporal component involved. P13 L203: 'frequent' instead of 'frequently' P16 L241: low turbulence

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