

Reviewer 1 Comments and Responses: Debnath et al. wes-2020-103

We thank the reviewer for the valuable comment throughout the review process. We believe that the quality of the article has been improved a lot over the review process. In this document, the reviewer's comments are in black and the author's responses are in blue.

In the revised version of the manuscript all of my comments has been satisfactorily dealt with. I have a few remarks that easily can be dealt with:

Line 46 – Floors et al 2013 is mentioned twice

Thank you for pointing out the mistake. We have corrected it.

Line 326 something is missing in the sentence

Thank you for finding this; the LaTeX command of "citep" was used instead of "citet." We have fixed this error and the sentence now reads, "[a] similar synoptic environment is found in a case study within Nunalee and Basu (2014) where daily low-level jets formed in coastal New Jersey under an area of high-pressure centered over the mid-Atlantic states."

Reviewer 2 Comments and Responses: Debnath et al. wes-2020-103

We thank the reviewer for the valuable comment throughout the review process. We believe that the quality of the article has been improved a lot over the review process. In this document, the reviewer's comments are in black and the author's responses are in blue.

Fig 6, 10, 11, 12. I find these overlaid bar plots rather difficult to read. Red is much more pronounced than blue. The (irrelevant) vertical lines on the bars clutter the figure and distract from the important parts. Perhaps consider changing the plot type to line/scatter?

We agree with the reviewer that red bars are more pronounced than blue. Therefore, we have decreased the thickness of the red bars. We have also tried the scatter and line plots suggested by the Reviewer. After checking the three versions of the figure, we decided to keep the updated bar plot.

P9 L170: "but it may" instead of "but may"?

We thank the reviewer for the suggestion and have changed the part of the sentence to read, "but it may provide some indication of atmospheric stability".

Fig 9f: why did you choose to show the shear exponent on the x-axis, rather than the wind speed gradient itself?

This is a good point. We have checked the plot with wind speed gradient. As the wind direction is calculated with the ratio of wind speed components, wind direction is a non-dimensional parameter of wind speed. But wind speed gradient takes care of the wind speed magnitude and shows a different relationship with wind veer in low and high wind speed conditions. We have added the below sentence to clarify this point.

"Note that, as the wind direction is calculated with the ratio of wind speed components, wind shear exponent is better suited than wind speed gradient to show the relationship between the wind veer and wind shear."

P14 L222: "... reasons for having events being observed" please consider rewriting this sentence.

We thank the reviewer for pointing out the unclarity of the sentence. The sentence now reads, "In this section, we briefly explore potential reasons for having 13% additional events at the SW buoy over the NE buoy."

P14 Section 3.4: The content is fine, but the readability of this paragraph is a bit less pleasant than the rest of the manuscript.

We thank the reviewer for the comment. We have read this section and made minor corrections to improve the readability. Without clear guidance into what the reviewer found unclear, we are not sure we have addressed the concerns.

Fig 11a: maybe add a note that this figure may not be entirely representative for the climatology, since inter-annual variability can be substantial?

We agree with the Reviewer. It is hard to say anything definitive about climatology, however, we are reporting what we are observing based on a year of data. The below sentence is added to the section. "It should be noted that this study uses a year of observational data, but multi-year data would be more useful to investigate the seasonal variability and climatology."

Fig 11b: I find it a bit confusing that a polar plot is used here. So far that's only been used for directions. If the reason is that time is circular, then 11a should also be a polar plot. But I think a normal Cartesian plot is easier to interpret. If you decide keep the polar plot, consider rotating it such that it aligns with an analogue clock.

We understand the reviewer's concern and have rotated the plot to represent the analogue clock format.

Fig 12a: The legend doesn't correspond with the line types shown in the figure

It does. The legend concisely shows that solid, red is for SW Buoy; dashed, blue for NE buoy; thin line is for monotonic shear; thick line is for LLJ.

Fig 12: VLLJ -> LLJ in legend of a, y-axis of b, and x-axis of c. Also fig 13 legend.

Thank you for catching the typos.

Fig 12b: why is there no KDE fit here?

Because we are looking at six discrete bins.

P16 L266: "monotonic shear profiles not included". Why not? So do I interpret this correctly that the "no-shear" and "LLJ" do not add up to 100%, because the monotonic high shear profiles are excluded? Or is it "LLJ" vs "everything else"? The latter seems to be implied in L264.

It is LLJ vs. no shear as the beginning of the paragraph explains.

P17 L278: "comprise of" is this grammatically correct?

We agree that this sentence used questionable grammar. We have changed the sentence to read "...events generally include a surface low-pressure..."

P18 L301: “Expanding to consider ... it may not be a good characterization of all events”. What does “it” refer to? The entire synoptic setup with a low and high pressure system, or the additional strengthening and eastward propagation? If it is the latter, please clarify the sentence. If it is the former, it seems to contradict previous statements about 75% of the events.

We see that this sentence structure is confusing and have rewritten the sentences to now read, “Expanding to consider the 25 longest events (averaging 19 hours in duration) shows that only 12 exhibit this eastward propagation and deepening of the low-pressure system. This implies that while the advancing and strengthening low-pressure system is common in the longest events in this area, it may not be a good characterization of all events including those with a much shorter duration.”

P18 L306: “not shown is the wind shift”. But this has now been added to the figure, right? Maybe consider labeling the panels from a – h instead of 2 x (a – d). That would make it easier to refer to them.

We thank the reviewer for catching this error. We have adjusted the labels and sentence in question to read, “The wind shift from south-southwesterly to west-northwesterly is also shown (Fig. 8f) as would be expected during a typical cold frontal passage at this location.”

P18 L315: I’m not sure what “shortwave troughs” are. Can you add a reference or briefly explain?

We appreciate that shortwave troughs may not be known by readers without meteorology backgrounds and have provided the relevant impacts of shortwave troughs on the near-surface conditions within parenthesis that now reads, “... shortwave troughs (which are relatively small scale synoptic disturbances commonly associated with changes in wind direction near the surface but no, or slight, changes in temperature).”

P19 L324: I wasn’t too familiar with the “Mid-Atlantic” region. So this leads to Northerly shore-parallel winds at the offshore lease area? How does that work in terms of stability and warm air advection? It feels like a whole different mechanism. It would be helpful to also see a schematic of this setup. Could it be added in figure 14 (as this figure now only uses half the width of a page)? In my opinion, the distinction between two different 'types' of events (or at least their synoptic configuration) is a very important and new feature of this paper, and adding this setup to figure 14 would highlight that point.

We thank the reviewer for this comment and understand how this could read as confusing if not familiar with this region. We have adjusted the sentences to explain that the high pressure system is centered over the coastal Mid-Atlantic and produces weak synoptic flow (i.e., no, or little, advection) which allows for the diurnal processes to dominate the flow field. We agree that this type of event is important, however, the percentage of events with this setup is relatively small. Further, a case with this synoptic setup has been studied thoroughly in the cited paper by Nunalee and Basu (2014). Because of this, we feel that adding another figure to explain this relatively small subset of cases would be redundant from what is shown in Nunalee and Basu (2014) and distract from the synoptic setup that explains a much larger portion of events. The sentence now reads, “For the event days that did not display the setup

illustrated in Fig. 14 (roughly one quarter of event days), 13% displayed synoptic conditions with a surface high-pressure system over the coastal mid-Atlantic region and offshore lease area. This results in weak synoptic flow over the offshore lease area and conditions greatly subject to diurnal processes. A similar synoptic environment is found in a case study within Nunalee and Basu (2014) where daily low-level jets formed in coastal New Jersey under an area of high-pressure centered just offshore of the mid-Atlantic states.

P21 L368: This study is based on 1 year of measurements. It would be valuable to repeat it with more data. Will this dataset be extended?

We really appreciate the eagerness of the Reviewer for pressing the science needs. But most of the time the outcome and extension of the works do not depend on the science objectives but depend on the funding of the project and relevant commitment. Based on our commitments, we are happy that we could use a year of data. Hopefully, this article has created a good work to show that the collection and analysis of more offshore data will be needed and useful. We refer this suggestion to a future work.

Associated Editor Comments and Responses: Debnath et al. wes-2020-103

In general, the two reviewers are satisfied with the revisions that you have made to the manuscript. The authors should be congratulated for a job well done. However, a few minor issues remain to be corrected. Please consider the corrections requested by referee #2 and the few editorial points by referee #1.

Also, I have an extra comment. In L50-52: "Sloping terrain is also an important driver, where wind speeds closer to the surface accelerate faster than those aloft, producing a LLJ (Holton, 1967; Parish and Oolman, 2010; Shapiro et al., 2016; Du and Rotunno, 2014)." How is this relevant to LLJs offshore? Please explain. The issue of sloping terrain is again used in L190.

We thank the associated editor for this relevant comment. The thermal wind is a great driver of the low-level jets, and sloping terrain facilitates the occurrence of the thermal wind (Holton, 1967; Parish and Oolman, 2010; Shapiro et al., 2016; Du and Rotunno, 2014). In the Southern Great Plains, the gradual west-to-east terrain slope can create a horizontal temperature gradient on a daily basis (Whiteman et al. 1997, Parish et al. 1987) and facilitates the LLJ occurrence. The coastal topography is important for the offshore atmospheric boundary layer and marine phenomena (Beardsley 1987, Burk and Thompson 1996). The sea-level is at the minimum height compared to the shore or coastal topography. Considering the flow direction between the land and sea, there is a natural slope between the land and sea which is relevant to the sloping terrain mechanism described for onshore.