List of Manuscript Changes in Response to Reviewers

1. Response (1) to the anonymous reviewer was implemented on Line 542 of the original manuscript.
2. Response (3) to the anonymous reviewer was implement on Line 159 of the original manuscript as a correction to the spelling of the cited author's name and as an insert before Line 1228 of the original manuscript to add the missing citation for O'Neill et al.
3. Response (5) to the anonymous reviewer was implemented as the replacement of "zeta" with the corresponding Greek character in Line 277.
4. Response (6) to the anonymous reviewer was implemented as a reformatting the opening and closing parentheses to be non-italic in Lines 431 and 432 of the original manuscript.
5. Response (7) to the anonymous reviewer was implemented by replacing the Borvarán citation beginning at Line 912 in the original manuscript with the revised cited provided in our response.
6. Our first implementation of a response to Dr. Banta's comments was at Line 82 of the original manuscript: after "(Strobach et al. 2018)…" We also added a missing space between "al." and "2018" in the Strobach et al. reference at that location, and we inserted the Banta et al. 2013 reference necessitated by the text change ahead of Line 899 in the original manuscript.
7. Our second implementation of a response to Dr. Banta's comments was the addition at the end of Line 125 of the original manuscript.
8. Our third and final change to the manuscript based on Dr. Banta's comments was the rewording involving the phrase "refactoring of existing codebases" at Line 566 in the original manuscript. The rewording is as we expressed it in our response.
9. Comments from Dr. Emeis did not appear to require a change to the manuscript at this time.

In addition to the manuscript changes above that respond to reviewer comments, we also corrected a few typographical errors and corrected three references as follows:

1. On Line 703 of the original manuscript, we removed the extra space between "systems" and "and".
2. Beginning at Line 963 of the original manuscript, the two Deskos et al. references were incorrect and have been replaced by the correct ones.
3. In the Pringle and Kotamarthi citation beginning at Line 1257 of the original manuscript, the URL for the article has been change to the correct version.

Response to Reviewers for Reference, as previously uploaded to WES:

Response to Emeis comments

We appreciate the encouraging review provided by Dr. Emeis.

He first noted that the paper draws illustrations and perspective from conditions found around America. We acknowledge that this is the case as we stated at the outset of the paper. We felt that this approach was justified because, as the paper notes, there are several features involving western boundary currents and deep ocean upwelling that extend the challenges addressed by the substantial body of work already carried out in Europe.

Dr. Emeis second noted that wind farm parameterizations were not addressed. This was not an oversight. This paper is one of ten that are submitted or are nearing submission to Wind Energy Science that as a group will substantially expand the concepts in

> Veers, P., Dykes K., Lantz, E., et al.: Grand challenges in the science of wind energy, Science, doi:10.1126/science.aau2027, 2019.

One of those papers, which is nearing submission, focuses specifically on the interactions of wind plants with the boundary layer and on wind farm parameterizations. Since it is not yet submitted, we cannot explicitly cite it. We will consult with the WES editor regarding whether to mention this in our manuscript.

Response to anonymous reviewer

We thank the second reviewer for the constructive review and specific comments. We respond to the specific comments as follows (review comments in quotation marks; our responses immediately follow):

1. "The authors described relations between dT (temperature - SST) and vertical wind speed profiles in Section 3. The wind profile can be definitely affected by SST. Meanwhile, strong winds (e.g., with tropical cyclones) can also decrease SST because of vertical convections in the sea surface. It could be worth adding a description mutuality and complexity of the relation."

   The perspective the reviewer is suggesting is implicit in the coupled-model discussion in Section 5.2.2. To make it more clear, we will add the sentence "…Wu et al. 2020). Such coupled models explicitly account for SST evolution from ocean mixing driven by marine ABL winds. They…"

2. "On page 2, line 47, I would prefer to move the annotation 1 to the reference list."

   We added this as a footnote since it is a web address rather than citable, refereed literature. If the WES editors prefer, we will be happy to make the suggested change in the final version of the paper.

3. "On page 6, line 159, the quote O'Neil 2012 is missing in the reference list."

   We thank the reviewer for catching this. We will add the following citation to the bibliography and correct the spelling of the author's name:

   > O'Neill, L., Chelton, D. B., and Esbensen, S. K.: Covariability of surface wind and stress responses to sea surface temperature fronts, J. Clim. 25(17), 5916–5942, doi:10.1175/JCLI-D-11-00230.1, 2012

4. "On page 7, Figure 2 would be more reader friendly if heading symbols (e.g., a), b),…) were used for figures, respectively."

   We elected not to give the panels individual letters because all elements are common to both panels and there is no discussion in the manuscript that addresses either panel individually. We

felt that the addition of letters might therefore clutter the figure without assisting the reader.

5. “On pages 11 and 14, Figures 3 and 5 would be better if heading symbols were used as same as below.”

   For Figure 3, we will change “zeta” in line 277 to be the symbol “ς” to match the figure. We did not try to adjust the figure, since it is reproduced with permission from the Patton et al. article.

   For Figure 5, it is not clear what symbols the reviewer is referring to, since the only symbol in the figure is “z” for height, and the text does not include additional symbols.

6. “On page 17, lines 431 and 432, brackets should be changed from italic to normal fonts.”

   This change will be made.

7. “On page 32, line 913, information of the article should be updated. It was published already.

   We thank the reviewer for catching this. The citation will be changed to

   > Borvarán, D., Peña, A., and Gandoin, R.: Characterization of offshore vertical wind shear conditions in Southern New England, Wind Energy, 24(5), 465–480, doi:10.1002/we.2583, 2021.

Response to Banta Comments

We are grateful for the substantial and constructive comments that Dr. Banta has provided. We also acknowledge his broad criticisms of the paper. These may be generally summarized as follows:

1. Synoptic-scale and mesoscale systems are the primary drivers for inflow into wind plants, and the inflow is then modulated by the processes on which we have focused in out manuscript. He has suggested that the paper would benefit from an expanded discussion of these larger-scale processes.
2. He has also suggested that the paper needs to address the primary forcing mechanisms of the synoptic-scale and mesoscale systems and consequently to provide guidance on field measurement strategies that will illuminate NWP errors and their causes so that the models can be improved.
3. Dr. Banta has also suggested that the paper prioritize focus on various atmospheric processes based on their importance to wind energy and their levels of uncertainty.

With respect to Point 1 above, the scope of such a discussion can be vast, and the current manuscript is already near the maximum length of what Wind Energy Science can support as one paper in a set of 10 that expand on Veers et al. (2019). We had extensive discussions regarding inclusion of issues associated with the representation of synoptic and mesoscale phenomena in numerical weather prediction models, and we believe that the paper represents a fair balance. We do discuss sea breezes and low-level jets in the paper, but for these mesoscale flows we feel that it is most important to emphasize the lack of validating observations in offshore wind energy areas, especially since most of what is known about sea

breezes, low-level jets, and other phenomena of daily meteorology comes from measurements made over land. This is also reflected in the references that Dr. Banta provided. We note the BAMS 2018 article that we originally cited in a narrower context has some broad discussion of the phenomena that Dr. Banta suggested that we discuss. Perhaps the most efficient at least partial solution is to direct readers to that discussion early in the paper 2018 BAMS paper.

We will therefore insert the following at line 82:

> "(Strobach et al.2018). From a terrestrial perspective, Banta et al. (2013, 2018) have provided descriptions of these kinds of circulations as they relate to wind energy together with suggested observational strategies for better observing them. Offshore…"

We will also add the following to the end of Section 1 of the manuscript beginning in line 125:

> "While synoptic-scale weather systems are important for driving wind plant inflows in the boundary layer and remain an active area of forecasting challenge and research, the structure and forcing mechanisms of these systems are beyond the scope of this paper."

Points 2 and 3 are related. The general forcing mechanisms of diurnal meteorological flows are well known, but the ability of models to capture details of what modulates that forcing, especially offshore, is still poorly validated. As a result, prioritization will likely be an iterative process as more observations become available to validate more model scales under a broader range of conditions. In the interim, some focus is provided by workshops such as the one held in 2019 with input both from industry and researchers.

Regarding Dr. Banta's comments on h vs Zi as the appropriate variable indicating ABL depth, the authorship team discussed this at some length as we were drafting the manuscript. We decided that Zi was also appropriate because turbulent mixing, even in a stably stratified ABL, will always create a nearer-neutral profile of potential temperature than will be present immediately above the height where the mixing stops. Also, since the temperature profile is often not truly inverted (increasing with height) above a convective ABL (even though potential temperature increases with height), we felt that this choice is acceptable. It was also convenient, since the figures we reproduced for this paper were labeled with Zi as the height of the mixing layer.

Finally, on line 566, we will change "is the refactoring of existing codebases in order to be ported to…" to "is the optimization of the design and the structure of existing numerical model codebases in order to be adapted to…"