Geoscientific Model Development Discussions

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Comment on gmd-2021-50

Anonymous Referee #1

Referee comment on "A Twenty-Year Analysis of Winds in California for Offshore Wind Energy Production Using WRF v4.1.2" by Alex Rybchuk et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-50-RC1, 2021

General comments:

This paper deals with the differences in the modelled datasets between two different model setups. I would expect two different setups to produce different results, however, it is currently unclear which one of these setups is better, as the comparison with observations is not yet available. Regrettably, the performance of the PBL scheme near the surface (e.g. when compared to buoys) is not indicative of the performance at the hub heights. Moreover, even if we look at the verification results for buoys (Table A1) it is hard to argue that one setup is better than the other. The paper argues that the differences in wind-speed results from different PBL schemes can be explained by differences in frequency between different atmospheric stability classes. Do we know which of the PBL schemes provides a better (closer to observations) description of stability? Not at this point, regrettably. In summary, I am afraid that the lack of comparison with hub height observations diminishes the applicability of the conclusions carried out in this paper.

Specific comments (major)

- Changes in the MYNN PBL scheme: "The WIND Toolkit was developed using a 7-year (2007–2013) simulation with WRF 3.4.1. CA20 builds upon this by using WRF 4.1.2 across a 20-year period (2000–2019)." (Line 76-77). CA20 uses the MYNN parametrization scheme, the WIND Toolkit uses the YSU scheme (Lines 85-89). The

problem is that in WRF version 3.7 the MYNN scheme underwent significant changes, and indeed the authors acknowledge this (Line 192). The thing that I do not understand is why if the WRF version is one of the factors that is analyzed during the sensitivity study, why aren't the changes in the MYNN parametrization scheme also included in the sensitivity analysis (as a separate parameter)? There are no methodological difficulties, one would just need to run both WRF versions with the MYNN scheme, instead of the YSU scheme, as it was already done. The reason why I would like to see such analysis is that changes in the MYNN scheme can lead to significantly worse verification results at hub heights when compared to observations (see Figure 8, section 5.3 in Hahmann et al. 2020).

- Low-level jet: I am not a specialist in the wind climate of North America, but the coastal low-level jet along the coast of California seems to be a well-known phenomenon. Could it be associated with the large differences in results seen during May – July? I understand that investigation into meteorological processes is beyond the scope of this paper, but the link to the low-level jet (or lack thereof) could help interpreting the results, especially, as the low-level jet is linked to upwelling, which is already linked to the strongly stable atmosphere by the authors (Line 176). Furthermore, the presence of low-level jets and understanding of its typical height can help with the interpretation of shear results (Figure 13).

Specific comments (minor):

- Figures 2-5 show 100m winds. Figure 6 speaks about hub-height winds. Does "hub height" mean "100m" in Figure 6?
- "At all three sites, the relative frequency distribution of hub-height wind speeds bears resemblance to the Weibull distribution (Fig. 10)" (line 251). I would argue that these distributions, especially those seen in Figure 10a are very different from the Weibull distribution. The fact that the distributions differ from the Weibull distribution is not bad, that is just the feature of the region, but if the authors would like to claim closeness to the Weibull distribution, then I would ask them to fit the data to the Weibull distribution, estimate the coefficients, and show the fitted distribution in the figure.
- Figure 14 is very hard to interpret because the eye is drawn to the distribution of wind directions (wind rose) and it is hard to distinguish between α values inside each sector. Maybe the α distributions for only for certain key sectors can be shown, plotting them

the same way as in Figure 13?

- The results for the wind drought, especially in Humboldt, is quite counterintuitive. CA20 shows much higher windspeeds on average, but the number of wind droughts also seems larger, at least for droughts that are 6 – 12h long. Maybe the authors would like to comment on this?
- If the authors would like to stress the differences in model performance between different regions (description of Table A1), I would suggest plotting the biases on a map using a larger circle, colored according to the bias, for each station. That would help with comprehending of the results.