Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-50

Anonymous Referee #2

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-RC2, 2021

Review of "A Twenty-Year Analysis of Winds in California for Offshore Wind Energy Production Using WRF V4.1.2" by Alex Rybchuk et al., GMD-2021-50

The manuscript describes a new model-generated dataset of wind resources for the California coast and compares it to an older dataset used by the wind industry. The manuscript is well written and well organized. The manuscript presents many interesting statistics of the wind climatology in these two datasets, e.g., the climatology of wind shear, wind veer, and "wind droughts". There are no observational datasets of wind in this region above the surface (buoys); thus, the comparison is purely made between two model-generated datasets. One could probably argue that the most recent one is more accurate, but the manuscript shows no evidence that this is true. The appendix contains a short evaluation against buoy data. But the authors well know that this is insufficient because the different surface and PBL scheme could give very different wind profiles (see, e.g., Draxl et al. 2014). My usual question for this type of manuscript is: "what new information does the manuscript provide that will help the scientific community in future investigations?" I cannot find any. Two datasets are compared, they are different in many aspects, but they cannot guide future WRF simulations for wind resource assessment. The information is perhaps valuable for wind farm developers and policymakers, but in my opinion, not to the reader of GMD.

Therefore, my recommendation is that the manuscript is rejected for publication in GMD but perhaps transferred to Wind Energy Science.

Also, I have a few more editorial comments:

- Please revise the figure captions. Most figure captions need further clarification. Many of them lack information on the averaging period. Also, please add (a), (b) labels to all

sub-panels. These labels are a requirement from GMD.

- Abstract L5-6: "The data set predicts a significantly larger wind resource (0.25–1.75 m s−1 stronger)," Since the units are m s-1. This is wind speed, not wind resource.
- Page 4, the bottom of page: "CA20 applies spectral nudging on a 6-km domain every 6 hours" This statement is incorrect. In WRF, the nudging terms are applied at every time step. But the tendencies used to compute this term could come to 6 hourly data.
- Page 10, L180. Is the MYNN simulation used as the basis for the stability classes? I think this needs to be clarified.
- L210: You write, "The updated product contains higher horizontal resolution (31 km vs 79 km), higher temporal resolution (1 hour vs 6 hours)". But I understand that you use the 6-hour ERA5 data for the nudging, so this fact is inconsequential.
- I don't see the point of including section 3.4.6. The different factors are clearly interrelated and nonlinear. So why analyze their sum?

References:

Draxl et al. 2014: Evaluating winds and vertical wind shear from Weather Research and Forecasting model forecasts using seven planetary boundary layer schemes. Wind Energy https://doi.org/10.1002/we.1555