Wind Energ. Sci. Discuss., referee comment RC2
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Comment on wes-2022-33

Anonymous Referee #2

Referee comment on "Evaluating the mesoscale spatio-temporal variability in simulated wind speed time series over northern Europe" by Graziela Luzia et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-33-RC2, 2022

Review of Evaluating the mesoscale spatio-temporal variability in simulated wind speed time series over Northern Europe

This is an interesting study which evaluates the ability of the Weather Research and Forecasting (WRF) model to accurately simulate near-surface wind speed timeseries. A range of model configurations (e.g. different nesting setups or boundary forcing) are compared to global reanalysis data using a number of statistics relevant to someone who might want to simulate timeseries of wind power. The paper is well written and provides clear and useful results. The experiment setup is very well thought out and I liked the choice of metrics to validate the models against. I have a few minor comments below for the authors to consider before publication. The main theme of my comments is that given energy-meteorology is a very interdisciplinary field, a bit more information and clarification of some of the modelling steps may increase the readability and uptake of the research.

Comments:

L32: Can you comment on how ‘extra information can be added by combining microscale with mesoscale data’ ?

L34: Can you be more specific on the definition of short-term in this context?

L35-45: Can you comment on the datasets which performed best from these studies?

L46: This is a long sentence which is hard to understand in its current form, consider breaking into two?

L55: [The results] – is this in reference to all three of the previously mentioned papers?

L63: This work [focuses] on …

L70: It is probably worth adding in a comment that your study region is Europe in this paragraph.

L78: Can you highlight the key updates between the two WRF versions used that might influence the results here?

L79: A couple more details would be useful, e.g. the frequency of the nudging. This is potentially of relevance when thinking about the correlations.

L80: A quick summary of the WRF validation would be useful to confirm that the model captures the right phenomena for the right reasons, and any previous comments on things of relevance WRF may struggle with.

L99: Can you mention the model levels that WRF is outputting data and what you have from ERA5? This would give a sense of the importance of the logarithmic extrapolation. Also does the logarithmic extrapolation make assumptions about atmospheric stability? If so can you comment on this, or if it might be more appropriate at certain times of the year?

L112 and in L303: Is there anything remarkable about the year 2009 that may influence your results? I appreciate it isn't possible to do multi-year simulations due to computational requirements and lack of validation data, but this does give you a very limited sample. Some comments on your chosen year (notable extreme events, or lack of these) would be useful for context.

Section 2.3: I like the metrics. Can you comment somewhere that because of shapes of power curves errors at certain parts of the distributions matter more than others? For example errors below the cut-in speed are less important than in turbine ramping regions.

L144-155 are a bit repetitive of the metric descriptions so these could be combined.

Fig 2: Define EMD in the caption.

L 163: either [have higher] or [has highest] might be better?

L166: 'It reveals the difficulties of mesoscale models in simulating the effects of the

forest on the flow dynamics,' – but surely ERA5 isn't doing a very good job of this? Can you clarify any other reasons for why ERA5 is highest?

L175: Are the autocorrelations a lot worse than the correlations for ERA5 due to issues in simulating the variance of the data? It might help to show a short timeseries of the data to illustrate some of these issues?

L180: 'The reason for larger errors in coastal and offshore sites can be due to the difficulties of mesoscale models in simulating turbulence over and close to the sea' – This could be a reason, but you haven't explicitly tested for this. Is there a reference you can put to support this comment?

L199: I think this not-shown result is of high relevance. It would be interesting to break these results down into different regions of a power curve e.g. before cut-in, ramping, rated, and above cut speeds out to see if different datasets have larger errors in different regions. As this may be useful in deciding a particularly model setup. This is most relevant for the EMD metric.

L216: 'Also, the "ERA5" has the advantage of data assimilation, which periodically adjusts and approximates the simulation to the Observations' – but is this information not then passed through to the WRF simulations? Can you clarify?

L278: Can you consider rephrasing this sentence as it's currently a bit confusing.

L289: 'appears to be' rather than ' will appear to be'

L305-315 These are very nice key points. Can you maybe bring these headline results to the start of the conclusions and then unpack them afterwards? This is the key information the reader will come to this section looking for first.