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Comment on wes-2021-22

Matti Koivisto (Referee)

Referee comment on "The 3□km Norwegian reanalysis (NORA3) – a validation of offshore wind resources in the North Sea and the Norwegian Sea" by Ida Marie Solbrekke et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2021-22-RC1, 2021

In the paper "Norwegian hindcast archive (NORA3) - A validation of offshore wind resources in the North Sea and Norwegian Sea", the authors present a comparison of data from NORA3 to measured wind speeds. I find the paper to have limited novelty. Similar modelling has been presented before, e.g., in NEWA and GWA. The authors do not show the added value of their modelling compared to other models and data sets. I consider the comparison to measured data to have quite limited value without comparison to other models. Some of the statistics and statistical theory are not well presented and explained.

1) The authors do not convincingly show what is novel and/or more advanced in their model compared to others. E.g., from Table 1 I cannot see what is new/better compared to NEWA. The authors discuss that NEWA is not validated offshore; however, this is different from having something in the model which is novel/advanced compared to other models. If the authors could show that NORA3 fits measured data significantly better than competing models/datasets, it could be considered an advancement; however, this is not done.

2) The statements such as "…NORA3 data is rather well suited…", "…slightly conservative…" and "…slightly underestimated…" are very vague. What are they based on? E.g., "The model is relatively good…": relative to what?

3) The biggest issue I have with the presented results is that they are not compared to other models. I thus consider it impossible to judge whether the model results are good or bad (see also the previous comment related to this). As NEWA mesoscale data are available (https://map.neweuropeanwindatlas.eu/), they should be compared to the NORA3 model results. I also consider comparison to ERA5 to be required, as the downscaled data should be shown to outperform the data they are based on.

4) As a large portion of the paper is comparing wind speed distributions, I suggest the authors to compare the distribution results also to microscale resolution data, which are available both from NEWA and GWA (https://globalwindatlas.info/).

5) The authors write: "…underestimation of 3 percentage point in the capacity factor". This would seem to me a large error, e.g., when considering profitability of an offshore wind power plant, and does not seem to be in line with the other statements of the model being

quite good (see the comment 2) above). Please comment.

6) I do not understand what this sentence means: "The corresponding wind speed variability is given by the Weibull standard deviation (std). The Weibull std is used instead of the Gaussian std because of the shape of the wind speed distribution (see Fig. 2a for an example wind speed distribution)." Standard deviation (or variance) gives the 2nd moment information of any (well-defined continuous) distribution (https://en.wikipedia.org/wiki/Standard_deviation); it is not related to the shape of the distribution. I find the sentence very confusing, please clarify and give references to what a "Weibull std" is and why it should be used.

7) If you consider standard deviation to be an inadequate measure, why not use higher moment measures (such as kurtosis, skewness) and/or compare the entire PDFs/CDFs of the distributions?

8) Why only 2004 to 2018 are simulated? ERA5 data are available for tens of years; why not downscale more years? Would not using longer time series provide a better estimate especially of events occurring very rarely? (the authors focus on rare events in specific sections)

9) The authors write: "Hence, the observed wind speed is somewhat more intermittent and variable than the modeled wind speed, indicating that HARMONIEAROME is missing some of the high-frequency variability embedded in the wind field." Why do you say that especially high-frequency variability is missing? Variance (and thus standard deviation) is a sum of all spectral components (https://en.wikipedia.org/wiki/Spectral_density) and thus a mismatch in standard deviations could be caused by mismatch in the spectra at any frequencies.

10) I find it difficult to understand what exactly is presented and done in sections 2.6, 4.4 and 4.5. I understand that for the estimation of extreme (very high) wind speeds, extreme value theory may be needed to extrapolate beyond the limited time range of data (however, please note comment 8) above related to this). But I find it very confusing that two very different causes for "zero wind power events" are mixed: one being quite frequent (wind speed below cut-in speed), which does not seem to need extreme value theory for estimating the likelihood; and one being relatively rare (wind speed above cut-out speed). But even the latter should happen offshore many times during a multi-year dataset. Why does it need extreme value theory? I would consider extreme value theory to be needed to estimate values like 50-year maximum wind speed, which may be needed for the design of the turbine (but that is very different from wind speeds causing storm shutdowns).

11) I find sections 4.4. and 4.5 confusing. a) Why do you apply extreme value theory? (see the previous comment); b) How is the extreme value theory exactly used to give you estimate of how long the "zero-events" last? Why does this matter, e.g., for LCOE? (I think LCOE is impacted by the entire generation distribution, not just the "zero-event" likelihood). I do not understand what the last paragraph on 4.5 is saying, consider a thorough rewrite.

I have given above references to Wikipedia, but you can check the relevant parts also from statistics / signal processing books.