RESPONSES POINT BY POINT TO REFEREE N°3 OF WIND ENERGY SCIENCE DISCUSSIONS
Brief communication: Enhanced representation of the power spectra of wind speed in Convection-Permitting Models
Nathalia Correa-Sánchez, Xiaoli Guo Larsén, Giorgia Fosser, Eleonora Dallan, Marco Borga, and Francesco Marra

Dear Referee No.3,

We thank you for your review work and the valuable comments, which helped to improve our paper. Our responses are reported in blue, and all the modified or new text is reported in *italics and red*. Line numbering refers to the original version of the paper that was available for the open discussion.

General comments

Dear authors, thanks for a short, interesting and well-written manuscript!

See my comments in the pdf attached.

I'd like you to review existing, recent works already published which adress the same topic and use CPM models. This is not to question to novelty of your work, but instead to bring you closer to the small community of CPM modellers with an interest for Wind Engineering applications (not only Wind Energy, but also Wind Hazards in general).

All the best

Rémi Gandoin, C2Wind, Denmark.

Thank you very much for your positive thoughts about our work. We will respond to each of your specific comments in the following section.

Specific comments

Comment #1. Consider adding "mean", i.e. "mean annual exceedance probability"

Thank you for this suggestion. However, we insist that *annual exceedance probability* is the correct term in extreme value statistics literature. Perhaps the reviewer meant that the return period is an average recurrence interval.

Comment #2. In Line 50: Please consider reviewing:

We reviewed the suggested references in the background section. Since this is a brief communication, the journal sets guidelines regarding the maximum number of references to be used, so we had to prioritise.

Chun-Hsu Su's work with the BARRA-C and BARRA-C2, and soon BARRA-3 suite of regional reanalysis https://doi.org/10.5194/gmd-14-4357-2021, their work include wind speed.

Thank you for your recommendation. However, after reviewing the suggested paper, we found that although the document is a relevant work on convection-permitting datasets and evaluates wind, it does not address the spectral properties of wind at turbine height or its direct application to 'wind energy' in detail. Therefore, it does not support the statement we made in line 50 that these areas of knowledge have not been explored.

This paper evaluates the wind speed at 10 m and the surface properties that may influence its estimation. It analyses metrics such as root mean square difference, Pearson correlation, additive bias and variance bias for wind speed. However, the paper does not discuss the spectral properties of wind, its energy spectrum or turbulence characteristics for this reanalysis. Furthermore, although it makes a general mention of the potential of reanalyses for renewable energy applications, it does not specifically explore BARRA-C's ability to reproduce wind properties in the context of wind energy applications within its own assessment. Finally, the study evaluates the BARRA-C reanalysis by focusing specifically on four mid-latitude subregions in Australia, not in central Europe.

Considering these points and the prioritisation of references that we must make within the Brief communication, we do not believe that this work offers a relevant contribution to our study. However, it does present the evaluation of a convective-scale reanalysis, which, although related, does not directly influence our scope.

Similar (and carried out in collbaration with the above authors) with the NZRA regional reanalysis (includes explicit deep convection as BARRA-C2)

https://www.data-assimilation.riken.jp/isda2024/files/pdf/p1-16.pdf
https://doi.org/10.2307/27226715

Thank you for this suggestion. However, this work, as you mention, is along the same lines as the previous one, but for New Zealand this time. Here, the study compares the performance of the NZRA for wind speed at 10 m and wind gusts, demonstrating that the NZRA better fits wind speed observations, even at higher percentile thresholds, and outperforms other reanalyses in estimating strong winds. Furthermore, the research also suggests that the knowledge generated by NZRA can contribute to various disciplines where wind energy and wind risk assessment fit perfectly. However, the study does not address the ability of CPMs to reproduce the 'spectral properties of wind'. The evaluations focus on performance metrics such as percentiles, correlation, time series, extreme event frequencies, and biases for mean wind speed and gusts.

Although this is an excellent example of the use of CPMs in meteorology and climatology to assess wind for risk and energy purposes, demonstrating significant added value in the prediction of strong winds and gusts, it does not address the spectral properties of wind and therefore its contribution focuses on the accuracy of wind magnitudes and the frequency of extremes in New Zealand, not on spectral analysis.

The following works:

https://doi.org/10.1016/j.jweia.2024.105844

Thank you for this interesting contribution. However, the source provides partial support and contextual background for the statement, but does not directly support the statement in its entirety.

The reference confirms the growing use of CPMs in meteorology and climatology and their application in the field of wind energy. It also details the limitations of these models in simulating small-scale phenomena such as gusts or grid-scale turbulence, which are aspects of wind spectral properties. However, it does not explicitly address or refer to the ability of these models to reproduce wind spectral properties in wind energy applications. Instead, it describes sub-grid scale variability as an inherent limitation of the models.

https://doi.org/10.1007/s00382-023-06803-w

After reviewing this interesting source, the conclusion is that the work of Adinolfi et al. (2023) does not fully support the claim in line 48. While it provides support for part of it, it lacks crucial information for the rest. For example, it does not evaluate or discuss the spectral properties of the wind or the ability of the VHR-REA_IT model (the same one presented in Raffa et al. (2021)) to reproduce them. Furthermore, the study focuses exclusively on the evaluation of temperature at 2 m and precipitation, not winds at any height, rather it is limited to describing how turbulent flows are parameterised in the model, but does not evaluate its performance in reproducing wind properties.

In conclusion, although this reference is very valuable because it confirms the growing interest and use of CPMs in meteorology and climatology, which we recognise due to their advantages in representing local-scale phenomena, especially temperature and precipitation, it does not provide any information or evaluation on the ability of these models to reproduce wind spectral properties, nor does it focus on the specific context of wind energy applications.

https://journals.ametsoc.org/view/journals/apme/60/10/JAMC-D-21-0029.1.xml (NORA3)

Thank you for this other interesting work. Although the purpose of the study is to demonstrate that NORA3 (a reanalysis covering mainly Norway and other regions of northern Europe) significantly improves the wind field compared to previous reanalyses such as ERA5 and NORA10, especially in mountainous areas and along coastlines with enhanced grid resolution which si very relevant for wind energy; as in the previous recommendations, the article does not explicitly discuss or evaluate the spectral properties of wind, which are the fundamental objective of our Brief Communication and what we refer to. Therefore, we do not consider that this work, although very valuable for other related topics, supports the idea we want to convey in line 49, to which the comment refers.

I believe all the above references uses CPM for wind-related analyses.

Thank you very much for your suggestions. It is indeed very interesting literature that we enjoy reviewing, but in the case of the statement in question (line 49), while we do not consider it to contribute significantly as a back ground in the spectral characteristics of CPM simulations of wind speeds at 100 m. Moreover, we needed to consider the limit on the references allowed in WES for a Brief Communication. However, based on your comment, we have included two of the most recent references you suggested that support the use of wind fields from CPM datasets to feed the first part of the sentence, although they do not cover spectral characteristics. That is why we will change line 49 as follows:

"…Despite the increasing use of CPMs in meteorology and climatology (Pirooz et al., 2023; Raffaele et al., 2024)*, their ability to reproduce wind spectral properties in the context of wind energy applications have not yet been explored in detail…*"

Comment #3. In th observational data subsection: A reference to a document describing the measurements (type of sensors, mounting etc) needs to be provided.

Thank you. We will add the reference *"Kohler et al. (2018)"* of the scientific paper describing the mast observation in Line 80.

REFERENCE: Kohler, M., Metzger, J., & Kalthoff, N. (2018). Trends in temperature and wind speed from 40 years of observations at a 200-m high meteorological tower in Southwest Germany. *International Journal of Climatology*, *38*(1), 23-34.

Comment #4. I think you can find others, with measurements closer to the surface, such as

W1M3A http://www.w1m3a.cnr.it/OI1/modules/site_pages/about.php

You could discuss why not using measurements closer to the surface, and easier to find; possibly w references to the two Italian papers I mentioned in my earlier comment, one of them uses measurements from 21 stations.

We appreciate the reviewer's suggestion regarding W1M3A and other surface measurement networks. We chose the KIT mast dataset after a thorough an exhaustive search for open-access wind measurements that met strict criteria: (1) spatial coverage within our study area, (2) at least 10 years of data for solid spectral analysis, (3) hourly resolution to match our model outputs, and (4) measurements at 100m height to avoid vertical extrapolation errors.

After an examination of the W1M3A observatory, we noticed that meteorological measurements are taken at about 7-15 meters above sea level on the upper mast of the ODAS Italia 1 spar buoy. This height is much lower than our study's target of 100m. Moreover, W1M3A is located offshore in the Ligurian Sea, around 80 km from the coast.

Extrapolating measurements from around 10 m in a marine setting to 100m would lead to several systematic uncertainties. These include assumptions about vertical extrapolation, the differences in atmospheric stability over the ocean compared to land, and the boundary layer characteristics. Additionally, the transition from marine to terrestrial conditions would add more bias when comparing with our mainly terrestrial CPM grid points.

Our method focuses on eliminating factors that could be misinterpreted as differences in CPM spectral performance. Using measurements taken at the model native output height of 100 m allows for the cleanest evaluation of spectral accuracy without the errors associated with extrapolation or differences in environmental conditions.

While the W1M3A and Italian networks are important meteorological resources, they do not meet our specific needs for an unbiased CPM spectral evaluation at wind turbine hub heights.

Comment #5. (in Line 98) Other studies such as https://iopscience.iop.org/article/10.1088/1742-6596/2151/1/012009 looked into this, consider referring to them.

If you end up quoting the NORA3 paper, see this study where they look at both wind speed and precipiation spectra https://doi.org/10.1016/j.rineng.2024.102010, it would be good to refer to it.

Thank you for your recommendation. Like Bastine et al. (2018), which we already included in the text, Meyer et al. (2022) also cover and apply spectral corrections to NEWA data (and ERA5 data). Both articles use and evaluate the spectral correction applied to NEWA data with the aim of obtaining more accurate estimates of extreme winds. The relevance for us would be that, as with Bastine et al. (2018), this work also recognises that mesoscale simulations such as NEWA tend to smooth out high-frequency wind fluctuations. To this end, we have changed the current sentence in line 98:

"…However, this evaluation did not specifically address the spectral characteristics or the representation of high-frequency variability that we examine in this study…"

For the following sentence:

"…However, Bastine et al. (2018) and Meyer et al. (2022) *applied spectral corrections to address the smoothing effect and correct the underestimation of extreme winds, since they detected that NEWA tends to smooth out high-frequency wind fluctuations…"*

Comment #6. About ERA5: I suggest to mention that it used as forcing for NEWA.

Thank you very much for bringing this important detail to our attention. Yes, we will indeed add the following to line 109 to close that paragraph:

"...and wind energy applications. Furthermore, ERA5 was the main source of initial and boundary conditions for the NEWA simulations."

Technical corrections

Thank you for the detailed review and for correcting the typos and spelling mistakes. We will correct all the ones you have pointed out, and we will also review the rest of the manuscript once again to ensure that everything is spelt correctly.