Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-23-AC2, 2020

Geoscientific
Model Development
Discussions

Interactive comment on "The Making of the New European Wind Atlas – Part 2: Production and Evaluation" *by* Martin Dörenkämper et al.

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Received and published: 17 July 2020

1 Response to Referee #1

This manuscript presents the results of a major multi-institutional technical landmark project to develop a new European wind energy atlas. The work is of the highest caliber, and for the most part is completely and carefully described. The evaluation of the wind atlas results demonstrates that the atlas provides usefully accurate estimates of the annual wind energy resource in both areas of flat and complex terrain. Notably the manuscript highlights the assumptions, simplifications, and limitations that were necessary to construct the atlas. The paper, when published, will be extremely useful to all who make use of the new atlas.

Thank you for your positive feedback to our manuscript. We respond to all of your comments separately below.

1.1 Specific Comments

1. Pg. 8, line 26. "The effective horizontal resolution of the mesoscale atlas is several kilometers . . ." Is this referring to the effective resolution of the 3 km WRF runs, or of the final atlas after downscaling? If it is of the 3 km WRF simulations, then I would say "resolution of the finest grid WRF simulations is". Also the effective resolution according to Skamarock (2004) will be approximately 7 Δx, or $\approx$ 20 km, which is more than "several km".

   Good point. We suggest to avoid the term "resolution", and instead write: "The horizontal grid-spacing of the mesoscale atlas is three kilometres (in each direction)".

2. Pg. 9, lines 4–12. The procedure used here seems to be that WRF is run to simulate flow over topography, the effects of the topography then are removed using a complex procedure including the use of a simplified linear model, and then the effects of the topography are added back in using another linear model. The authors should motivate and describe in general terms why such a convoluted process is necessary. Also the link to the reference by Hahmann et al., 2014 that describes this methodology already gives "page not found".

   Thanks for spotting the wrong reference, which should have been Badger et al. 2014 [1]

   We agree that more motivation could be provided for readers outside the wind energy community. Due to the realtively coarse resolution of the WRF model topography, a downscaling with a microscale model is needed to provide more accurate wind data in particular in non-homogeneous terrain. For this purpose, as indicated in the introduction, linearized flow models, such as the WAsP model, are well known and have extensively been used within the wind energy sector for site assessment in the past 30 years. The underlying principle is to estimate the wind climate at a point by vertically extrapolating from a known nearby wind climate, either measured or modelled. The extrapolation is done by first "removing" topographic effects from the known wind climate, estimated by the linearized flow model from the best available topographical maps. Then, vertical extrapolation is done using drag-law relations. Finally, the topographic effects (estimated by the same model) at the target point are added to the wind climate. This concept only works for short distances where the geostrophic forcing is similar.

The WAsP linearized flow model has also been used successfully in combination with WRF for several regional and global wind atlases, such as the Global Wind Atlas (GWA) and the Wind Atlas for South Africa (WASA) [2, 3, 6]. The principle of e.g. WRF-WAsP downscaling is the same as above, but instead of removing and adding topographical effects estimated from the same maps, the WRF elevation and land-use is used for "removing" topographical effects and the best available maps are used for "adding" topographical effects.

3. Pg. 10. Do the surface roughness lengths vary depending on season? Won't the roughness lengths for cropland and deciduous forests be much different from summer to winter? If they are not seasonally varying, are they more representative of summer or winter?

   The minimum and maximum value of the surface roughness length in the vegetation table are the same; thus an annual cycle on the vegetation is not active. The constant value represent in most cases the geometric average. This is described in the companion paper [4]. Using a constant value of surface roughness facilitates the process of microscale downscaling.

4. Pg. 13, line 32. How were the data on opposing booms used to reduce flow

distortion effects? Presumably some sort of criteria were used to select one boom over the other. What were these criteria? Were the criteria dependent on wind direction, or only on speed differences? Was the selection based on hourly data, 1-min data, etc?

   In general, no correction was made to reduce flow distortion in the measurements beyond existing corrections from the data provider. Only in few instances where two cups or sonic anemometers were present at the same height, but at different boom angles, was selective sampling used to combine the measurements and minimise distortion. We expect flow distortion to have some influence on the results, which, based on the impact we saw of including the correction in [4] and indications by e.g. [7], could be up to a few percent difference in annual mean wind speed.

   Filtering was done on 10min averages.

5. Pg. 15, lines 4–14. "For evaluation of the downscaled wind climate at each mast site, some modifications to the WRF-WAsP methodology were made" and following sentences. If I read this correctly, a separate and different set of downscaling procedures has been applied only to those locations where the atlas is compared to the observations. Why do something different at the evaluation sites? Won't this be comparing winds that are different from the rest of the atlas? If there are advantages to this new methodology, why not apply it everywhere?

   From the onset of the NEWA atlas production, we choose to use the default WRF-WAsP methodology for long-term mean wind climates making up the atlas. This method is well documented, computationally performant, and in most cases the assumed Weibull distributions should capture the 30-year wind climates well.

   Our rationale for not assuming Weibull distributions for the validation, was to avoid parameterization biases that is expected to be exaggerated for these shorter one-year periods, compared to the long-term mean wind climate of final atlas. While the histogram-based approach means we cannot use the stability correction from [5] (since it works on Weibull-parameters), we assumed that this difference would be smaller than the parameterization biases caused by assuming Weibull distributions.

We agree that we should give a better indication of the quantitative difference between using the two approaches and propose to add this information to the revised manuscript.

1.2 Technical Corrections

1. Pg. 2, line 9: "It documents the meteorological basis for large parts of Europe". The meteorological basis of what? Of the atlas?

   The meteorological basis for locations across most of Europe. "Meteorological basis" should be understood as the mean wind climate.

2. Pg. 3, lines 2–8. If I interpret this correctly, the NEWA actually consists of two separate wind atlases. One is a mesoscale atlas based on WRF, while the second is a downscaled WRF-WaSP product. It this interpretation is correct, I suggest that the text be modified to clearly state this.

   We agree that it can be made more explicit in the text that the NEWA Wind Atlas consists of two individual "atlases" or "layers" (mesoscale and microscale).

3. Pg. 4, line 9. "while the nudging" should be "while nudging"

   Thank you, corrected.

4. Pg. 18, line 7. Does "mean wind speed" refer to the annual mean?

   Yes, it refers to the mean of the validation period, which is up to a year.

1.3 Additional Clarifications

We have received further non-documented feedback and spotted ourselves the following issues that we would like to clarify in the revised manuscript:

- Pg. 10 - line 22. Table B1 does not exist in Hahmann et al (2020) [4], it is B1 in the appendix of the second (this!) manuscript
- Pg. 24 - Figure 13. is not based on circular statistics in the submitted version of the manuscript, we have recomputed the metric and redrawn the figure without need for re-interpretation of the results
- We have added a discussion about optimising the wind atlas for the wind climate (distributions) instead of the accuracy of the time series.

1.4 References

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