Offshore and onshore ground-generation airborne wind energy power curve characterization - Referee Comment

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The manuscript attempts to capture the detailed WRF results with a single curve describing the probability of the wind conditions at variable operational height and the associated power curve of an airborne wind energy system. The difference in estimated AEP (10%) depending on the choice of the curve's variable suggests that the methodology fails to adequately describe the WRF profiles and power output with a single curve. The work lacks validation of the calculated AEP with a detailed numerical integration. The presented power curve expressions are ambiguous, rely on site-specific training, and in my opinion conflict with the author's 'easily understandable' requirement. Lastly, the work would benefit from more focus to present the research output.

Please find my response to the author's response below with the bullets in blue indicating the more urgent topics.

[RR] - Referee report

[AR] - Author response

[R2A] - Response to author by referee

[RR] I find it unsatisfying that there is no justification/discussion on the degree of simplifications needed to map the high-fidelity model output to wind statistics and power curve as single argument functions (height-range-averaged wind speed).

[AR] The same simplification is done for every application of the Weibull distribution or simple power curve derivation for conventional wind turbines. They also simplify complex wind conditions to a simple distribution and ignore the variation of wind speed along the rotor diameter.

[R2A] Can you cite any work on conventional WTs that use mesoscale simulations to find a Weibull distribution to finally arrive at the AEP calculation?

[RR] I would expect that the accuracy benefits of the relatively high fidelity (and computational costly) models in the first computational steps, cancel out when the author simplifies them before calculating AEP.

[AR] See previous answer. These simplifications are done every day when applying wind statistics. They have been made to generate easily understandable, comparable to conventional power curves and AEP estimates.

[R2A] I understand your motivation. What I question is the justification/validation. I believe more proof is needed to increase confidence in the results.

[RR] I understand that the goal is to get a simple characterization of the power output/AEP similar to that of a WT. However, how much is this worth when you loose precious details in the process given that you went through all the effort of setting up the suggested high-fidelity tool chain?

[AR] – This is a different application of the data. I do not think that I lost these details. We can still investigate the time series data or statistics if we want to, what we did in Sub-section 5.1 and 5.2. Other analysis are possible, but beyond the scope of this paper. – The description of power curve and AEP are supposed to be simple to be similar to that of conventional WT.

[R2A] - I meant details on the connection between the wind profiles and AEP. E.g.: how much of the AEP is attributed to certain wind conditions? This information is lost when casting the WRF results in the probability distributions. - That would be convenient. However, I would argue that more importantly they should provide a reasonable AEP prediction.

[RR] Section 2 misses a discussion on why WRF data is used. This study only considers 1 year of simulation data, which is far too little for giving a good representation of the wind climate and thus for AEP calculations. Typically 30 years of data is needed for this purpose

[AR] – Added comment in introduction and AEP section. – The investigation only make statements regarding the investigated years.

[R2A] You mention only 1-year of data is used "to simply the analyses" - what is more simple about it? Would you also recommend others to use the same approach? Wouldn't using wind atlas data simplify the analysis more and at the same time allow you to use more years of data?

[RR] There are a few wind atlases available that cover a wide area (including the investigated sites) for a much longer period, which might be a better source for the wind data?

[AR] As mentioned in the paper, ideally we could use long-term measurements, but this is not possible. Therefore, WRF provides a compromise between computational cost, spatial and temporal resolution and accuracy of the wind data. WRF and wind atlases do have their justification and application!

[R2A] So what is the trade-off between WRF and wind atlases? Why did you opt for WRF?

[RR] What would the added value be compared to alternative approaches such as that of Malz and Schelbergen?

[AR] - I don't understand the question. We are using different approaches, models and model fidelity, locations and analyses. I believe there is enough justification for both our research. - I am using higher resolution wind data and a dynamic optimization model to estimate AWES performance.

[R2A] - I'm not saying that there isn't. But make it explicit in the Introduction. With this you can make your key contribution mentioned in the introduction more precise. - I don't find in the text why these are need to answer your main research question/hypothesis. What is you main research question?

[RR] The QSS model is a very simplistic. I would suggest also including the reel-in in the model, similar to what is done by Luchsinger. The current model does not give a good estimation for a pumping mode system.

[AR] – Investigating the performance using a detailed QSS model is beyond the scope of this paper. The focus is the investigation of AWES performance using the awebox dynamic optimization model. The QSS model merely contextualizes the results. – Which specific model are you referring to? I am only aware of Pumping Cycle Kite Power from Luchsinger and this publication does not include a reel-in model for pumping mode systems

[R2A] - The quasi-steady state calculation you're referring to is not made for estimating the mean power of a cycle, solely instantaneous power. So the results can not be fairly compared with cycle results and thus do not help contextualizing the results. - It does, see sec 3.2.

[RR] After reading this section, I did not have a good understanding of how the p5/p50/p95 cluster profiles are used. I would argue that this is the most important part of the paper and therefore the approach taken there should be presented unambiguously.

[AR] Implemented a clarification in sub-section Wind profile model

[R2A] "From these sorted wind profiles, the 5th, 50th and 95th percentile profile are chosen and assumed to be representative of the spectrum of wind conditions within this cluster" - The clarification is still missing important details: Why 3 profiles? Representative how? Provide more arguments.

[RR] I'm puzzled by the last sentences - do you sum up 10 min energy production of every WRF data point, or do you integrate the product of power curve and probability 20 function? Also you ”interpolate within each cluster linearly between p5,p50 and p95” - do you do this to get the power output of every 10 min data point? Why do you use 3 points? Isn't 3 points very little?

[AR] – rewrote sub-section and added additional explanation to AEP sub-section – It's 3 points per cluster and therefore 20x3 = 60 data points which are used to derive the power curve and estimate AEP!

[R2A] - Still unclear in Sec. 6.3. Why use this approach against e.g. using 60 clusters and only use the cluster means? Why using linear interpolation?

[RR] which profiles are presented in fig 11?

[AR] These profiles were solely chosen to visualize the range of wind conditions and the resulting trajectories. Giving the specific cluster number and p-value is meaningless.

[R2A] It would give a better understanding of how it fits in the methodology. Illustrative examples help understanding the methodology.

[RR] Which of the data points represent p5/50/95 profiles in fig 12?

[AR] Figure 12 shows operating heights for 3(3 profiles within each cluster)x20(number of clusters)=60 profiles . Therefore, it is not possible to indicate the percentile of each profile. p5 of a high wind speed cluster might be close to p95 of a low wind speed cluster.

[R2A] You could distinguish with different markers. Same for the power points.

[RR] how do you get to the dashed curve in fig 13a/b? a curve fit of some kind? - describe how

[AR] Yes, least square fit, interpolated data points. Added clarification.

[R2A] The clarification is still incomplete. What fitting function do you use?

[RR] By doing the fitting, you loose precious information about how the power output relates to the wind profile that you acquired with the computational costly optimizations.

[AR] I guess that is true to some degree, but this is done to be comparable to conventional WT power curve / AEP derivation.

[R2A] It depends on the size of the error that is introduced if this argument justifies the error, i.e., it is important to quantify what the penalty is.

[RR] Also by expressing the probability function of the wind resource as a single argument function (solely a function of height-range-averaged wind speed) you loose much information that was acquired with a lot of computation effort by the WRF and clustering. I think a more detailed numerical integration would be in place here instead of going through all this effort of detailed system/wind modelling to end up with simplistic models for power output and wind statistics. However, I might have misinterpreted the methodology used here (see comments section 4)

[AR] I understand your point, but we decided to use an already established, easily understood method to describe the wind resource as an annual wind speed probability distributions.

[R2A] It's common practice in preliminary AEP calculations for WTs though it relies on quite big assumptions. Also I would say your method is quite different; in particular how you derive the probability functions. How do you come to the probability distributions? Do you use curve fitting again? Is the area underneath even 1?

[RR] Also, since the relation between Uref(z operating) and U(z W=100 m) is non-linear, you'll get a differently shaped power curve/probability distribution and thus AEP, depending on which of the two properties you use to express these functions. This tells me that this approach is mathematically unsound. I would say more proof is needed to justify your methodology.

[AR] – Yes, this is correct. Using different reference heights does lead to differently shaped power curves with different probability distributions and AEP estimates (Figure 15 shows AEP for different reference heights). The point of this investigation is to show that the choice of reference height is important and needs to be investigated – Added clarification in the introduction section – In the conclusion it is mentioned that the choice of reference height is significant, particularly onshore

[R2A] Why would the AEP be different when all come from the same WRF/cluster results? I can understand that the curves appear to be different depending on the quantity on the x-axis. However, the discrepancies in AEP suggest that the methodology is faulty.

[RR] Does the approach suggested in eq 10 act as a benchmark for the other AEP calculations? W.r.t. which property do you integrate? If I understand correctly, it comes down to sampling 3 points per cluster (p5/p50/p95) to quantify the power output within this cluster. Why just 3? I would suggest you use a sampling technique that picks a higher number of samples. Only 3 does not give a trustworthy benchmark.

[AR] – I assume you mean interpolate?! ([R2A] yes) The profiles are sorted by wind speed up to 500 m, which is used as an a priori proxy for operating height. – Added a sentence to AEP section. – Using more profiles per cluster would probably improve the accuracy and trustworthiness of this approach. Ideally one would perform a trajectory optimization for every profile, but this is not feasible and comes at a very high computational cost.

[R2A] Malz did such a computational costly analysis, which shows that evaluating more profiles per cluster should be feasible.