Response to reviewers of wes-2020-120

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Author response

Dear reviewers,

Thank you very much for your very detailed and helpful comments to our manuscript, “Offshore and onshore power curve characterization for ground-generation airborne wind energy systems”, wes-2020-120.

Before implementing the requested revisions, we would like to address some of the comments and clarify the purpose of the paper and the interpretation of our optimization results. We respond below to the main questions and propose changes to the manuscript. We would very much appreciate confirming if we have interpreted the comments correctly and that the proposed revisions will be acceptable before investing further effort.

The manuscript has already been through several rounds of major revisions by multiple reviewers and the editor since its initial draft more than 2 years ago. Most of the original code and result files were lost in a computer crash during my relocation to a new country. Much of the code had to be re-written and the computationally expensive and time consuming optimizations had to be re-run. As such, we would like to avoid running additional optimizations, both because of the significant afford that is associated with additional optimizations and post-processing now that the original project is complete and I have graduated, and because we feel that with proper re-framing and scoping the work your concerns can be addressed by the existing data sets. We therefore propose to focus on implementing changes to the manuscript text, the QSM model and figures, as detailed in the following specific suggested changes.

Sincerely, Markus Sommerfeld

1 Reviewer 1

Reviewer 1 rejects the paper after multiple rounds of major revisions by several reviewers and the editor. Most their critique focuses on the estimation of AEP from WRF-derived wind data and awebox power optimizations. They believe that the difference in estimated AEP shows that the methodology fails to adequately describe the wind profiles and power output with a single power curve. They demand more validation via numerical integration and ask for justification for using WRF data, as well as trade-offs between WRF and wind atlas data. They also state that simulating every wind speed profile in the entire data set has been done by Malz et al. and is therefore feasible and should be used as validation.

We argue that we need not justify one choice of wind data over the other in the context of this paper. Wind atlases cover a longer time period and larger geographical area, but the coarser temporal and spatial grid averages out the wind speed profiles relative to our WRF data set. The purpose of this paper is not founded on a comparison between wind data sets. In fact, wind atlas data or indeed physical measurements or any other wind data set can utilize the proposed method of clustering and simulation to derive AWES performance. The focus of this work is to investigate AWES performance subject to realistic, high resolution wind data, regardless of the data source and formulated to enable more accurate while still computationally tractable computations. Since long-term LiDAR measurements were not available, we (in cooperation with the co-author meteorologists) chose WRF because it gives us 10-min average wind speed profiles with sufficient vertical profile resolution. Our collaborating meteorologists and researchers at the University of Oldenburg confirmed that this is an appropriate tool to derive realistic, high resolution wind speed profiles in case measurements are not available. To be consistent, we then used the same wind data set to derive a wind speed probability distribution and estimated AEP from it. The purpose of WRF is not to derive a simple Weibull distribution for wind speeds from which to then to estimate AEP. The point is to use WRF directly as inputs to an analysis of realistic AWES performance. Indeed, the New European Wind Atlas (`https://map.neweuropeanwindatlas.eu/about`) is based on WRF simulations and therefore consistent with our approach.

We propose to fully clarify the main research question at the core of this manuscript, modify the paper title and justify the usage of WRF. We propose to rework the manuscript so that *Pattern Trajectory Height* (see `https://airbornewindeurope.org/resources/glossary-2/`) is the base case reference for all the other power and AEP estimates, based on different reference heights $z = 100m$ and $100 \leq z_{ref} \leq 400m$. We will modify the QSM model to include reel-in power losses in the QSM, by assuming reel-in speed and minimum tether tension (maybe $F_{tether} = 0$), to reduce the power and AEP difference between the QSM and awebox model.

We simply cannot numerically integrate simulated performance for wind speed profile in our data set to validate the results. This would entail computing 105,120 10-min wind speed profiles to get 1 year of performance data at both onshore and offshore location ($2 \times 6 \times 24 \times 365$). Our awebox simulations (including trajectory optimization) require 10 minutes computation time per profile, so it would take more than 2 years (for 1 design). We note that Malz was able to compute performance hourly as her model was significantly computationally cheaper, given the model assumptions and most importantly her focus on fly-gen systems which simply simple circular orbits, rather than the much more complex real-out and real-in pumping cycles we focused on. We chose to focus on the later given that the preponderance of commercial concepts are pumping mode, and to provide a differentiation to Malz's work. It should be noted that we did carry out a convergence study, wherein we found that increasing the number of profiles (by increasing the number of clusters), and/or taking more samples per cluster, does not significantly change the AEP. We will clarify this convergence analysis in the paper, as it provides statistical evidence of the method's efficacy.

1.1 Specific comment 1

Specific comments not addressed here will be implemented in the revision.
RC1: first comment by reviewer
AR1: first response by author
RC2: second comment by reviewer
AR2: (new) second response by author

RC1 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).

AR1 This comment is unclear, and could be directed at any wind energy converter, conventional or AWES. The same simplification is done for every application of the Weibull distribution or simple power curve derivation for conventional wind turbines. Wind shear and other input wind field non-uniformity, unsteady operations, controller actions and like all yield in reality a cloud of binned performance points over the wind speed range that are then described by a best-fit curve. They also simplify complex wind conditions to a simple distribution and ignore the details of the variation of wind speed along the rotor diameter. Our analysis is therefore consistent in approach to standard practice, but with the intent to embed the impacts of realistic wind profiles on performance.

RC2 Can you cite any work on conventional WTs that use mesoscale simulations to find a Weibull distribution to finally arrive at the AEP calculation?

AR2 Is your objection that we should not use mesoscale simulations to estimate a power curve or AEP at all? Is your objection against averaging out wind profiles to hub-height? Is your objection against using higher resolution wind data to derive wind speed probability distributions or use them to estimate wind power? Standard wind energy practise is to take an OEM power curve and integrated that with various sources of wind data include correlated wind speed histories, mesoscale reanalysis models, etc.

AR2 Here are some citations that use WRF [4, 8, 6, 2, 9, 5, 3]. These are using mesoscale (WRF) simulations to derive Weibull parameters; the extension to AEP prediction is available in any wind textbook, e.g. Wind Energy Handbook by [1].

AR2 To your original comment: We do not see why we need to justify mapping high-fidelity model output to wind statistics and power curve as single argument functions (height-range-averaged wind speed). Using a single wind speed (e.g. hub-height) is common for conventional WTs. Any wind data, measurements or simulation, can and are mapped to wind statistics and a single reference height for power curve description. In [7] you also simplify wind speed data from the Dutch Offshore Wind Atlas data, which is based on numerical weather model HARMONIE very similar to WRF, to a single reference height: 100m.

AR2 We do not believe that this comment needs to be addressed.

RC1 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.

AR1 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. The power curves for conventional WT use very expensive aeroelastic simulations, and later field test data, to produce 'simplified' binned power curves which are then used with Weibul curves to produce AEP estimates.

RC2 I understand your motivation (to simplify high fidelity and computational costly data). What I question is the justification/validation. I believe more proof is needed to increase confidence in the results.

AR2 Do we understand you correctly, that your objection is not against using WRF data to derive a wind speed distribution and power estimates, but rather against using a mesoscale simulation at all to perform this task, while other data , e.g. wind atlas is available? The New European Wind Atlas (`https://map.neweuropeanwindatlas.eu/about`) is based on WRF simulations.

AR2 Yes, information is lost in the process of deriving a power curve or AEP. But this high fidelity wind data was never the end goal of this investigation, but rather means to an end. The purpose of WRF is not to derive a simple Weibull distribution from it to estimate AEP. The point is to use it to generate realistic AWES performance which we would not get from temporally and spatially averaged data. We use WRF because we cooperated with meteorologists and researchers at the university of Oldenburg who confirmed that this is an appropriate tool to derive realistic, high resolution wind speed profiles in case measurements are not available. See also comments above on ability computationally to do a full year worth of 10 min simulations. It should also be noted that conventional wind aeroelastic analysis IEC standards requires only a limited number (6-10 per wind speed bin) worth of calculations for performance prediction. Granted they are unsteady turbulent ones, but more aimed at loads verification than power prediction. Future work in AWES will have to perform similar turbulent/unsteady calculations, which will be even more computationally hard, and our approach is a first step in that direction to use clustering to rationally down-select a subset of required simulation input conditions.

AR2 We propose to point out in the revised paper that the purpose of using WRF is to derive realistic wind conditions to investigate realistic performance of AWES. We could also rename AEP to something like ”estimated annual energy of this particular year” to differentiate it from long term AEP estimations.

RC1 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?

AR1 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.

RC2 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. This is definitely an interesting analysis, but the investigation of specific clusters/shapes on power output AEP is beyond the scope of this paper.

AR2 We propose to added this to future work.

RC1 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.

Author Already we have implemented a clarification in sub-section Wind profile model and agree that if it was not clear before, this is important that ready have the correct statistical understanding of the method.

RC2 ”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.

AR2 The profiles within each cluster are similar, by definition of the clustering algorithm. Similar in the sense of euclidean distance between profile velocities across height. To represent the variation within each cluster we chose high (P95), low(P5) and median(P50) wind speed profiles. This also reduces the computational cost. They are assumed to be representative of the wind conditions of this cluster, and including additional profiles in each cluster for analysis was not found to affect the results. We did not choose extreme cases because they will not represent the cluster as well as they are very rare and would only minimally contribute to overall integrated energy production. We also choose not to use the cluster centroid, because it is an averaged profile across all profiles in the cluster and not an actual profile seen in the wind speed data; the centroid is therefore potentially non-physical. The 3 profiles per cluster are directly implemented into the optimization as boundary conditions to derive power estimates. The entire point of clustering is to reduce the number of required profiles to get meaningfulness performance estimates.

AR2 We propose to clarify this in the text in Section “Wind conditions”.

RC1 Which of the data points represent p5/50/95 profiles in fig 12?

AR1 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.

RC2 You could distinguish with different markers. Same for the power points.

AR2 To get any information out of this you would need to indicate not only the P-value, but also the corresponding cluster. We feel this would make everything more confusing to the reader. The point of this figure is not to investigate the individual cluster, but the overall performance relative to reference wind speed, which is an easier to understand metric than clustered percentile profile. Again, the point is not to investigate the clustering. The point is to use clustering to get representative profiles to get meaningful results with reduced computational cost.

AR2 We will clarify in the text the intent of presenting the clusters together in the figures vs identifying individual profiles. We will also include the actual profiles in online appendix data sets, should future researchers care to investigate further.

RC2 Regarding Fig 13: 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?

AR2 Yes, the area underneath is 1. You can easily assess this by looking at the bar plot.

AR2 We propose to clarify in the text. The wind speed distribution of $100 \leq z \leq 400m$ is the distribution of average wind speed within this range (1st calc average of every profile within height range, 2nd derive distribution). The wind speed distribution at operating height is derived from the wind speeds along the flight trajectory of the simulated profiles.

2 Reviewer 2

Reviewer 2 asks for major revisions such as using a wind speed multiplier for each shape and normalizing the wind speed profiles. They would like discussion section amended and miss a discussion of the power curve and its definition in the conclusion. Most of the comments can be addressed by changing the wording and clarifying the text.

We do agree that normalizing the wind speed profiles and determining wind speed multipliers is a very useful contribution to apply the approach to a generalized site. Unfortunately, this is beyond the scope of this already quite lengthy paper. We propose to add this to the future works section as part of a general extension of the work for the purposes of long-term AEP assessment. We will add the requested reference and address the comments made directly in the PDF.

2.1 Specific comment 1

- Why are only calculated profiles used? As far as I know, the sites and times are correlating with LIDAR measurement campaigns.
  - Any kind of wind data can be used with this methodology. LiDAR measurements are available for the onshore location, but high quality data was only 6 months. WRF has the benefit that there is no missing data (e.g. power outage, low data availability / quality). We would have preferred using LiDAR data, but it was not possible to do an investigation of annual AWES performance. In any case, this type of re-analysis data is commonly used in practice. Also for a real site, only a specific location of data would be available, whereas an AWES wind farm would be operating over a fairly broad area and require the use of such reanalysis data in layout planning.
- Is it true that the QSM is neglecting force constraints and the retraction phase? Because they can be easily incorporated
  - Force constraints are used for the QSM. Could you please point out why you think they would not be, i.e What line? So that we can clarify.
  - We will include reel-in power losses in the QSM, by assuming reel-in speed and minimum tether tension ($F_{tether} \approx 0$), to reduce the power and AEP difference between the QSM and awebox model
- For the logarithmic optimization runs: Maybe 2m/s steps at 10m reference height is a bit rough
  - We see your point, but I want to avoid re-running optimization and post-processing the data. This choice was made, because we are investigating many different configurations and running one optimization takes more than 10 minutes, so practical computational limitations were taken in account in the study design.
- The implementation of Lagrange polynomials into the OCP and the difference between the grey and colored lines in Fig 11 and FigA3
  - The Lagrange polynomials actually go through all the implemented data points and deviate in between these points. Not every data point could be implemented, because a high number of data points leads to over-fitting (see Figure 2:

`https://en.wikipedia.org/wiki/Overfitting` and oscillation between data points. Therefore, the number of data points was reduced, which is why there is a small difference between the red and grey line. We propose to add this explanation to the text.

References

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