Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2020-120-RC1, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Interactive comment on "Offshore and onshore ground-generation airborne wind energy power curve characterization" *by* Markus Sommerfeld et al.

Mark Schelbergen (Referee)

m.schelbergen@tudelft.nl

Received and published: 22 December 2020

[wes, manuscript]copernicus

[1]MarkSchelbergen

[1]Faculty of Aerospace Engineering, Delft University of Technology, Kluyverweg 1, 2629 HS Delft, The Netherlands

Clustering wind profile shapes to estimate airborne wind energy production - Referee Comment

Mark Schelbergen

Printer-friendly version

Discussion paper

Mark Schelbergen (m.schelbergen@tudelft.nl)

11

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

December 22, 2020

1 General comments

A lot of the content reminded me of my own work (Schelbergen 2020), but with higher fidelity modelling and an alternative clustering approach. I feel that too little attention is put on the innovative part of your study, which is not so much the wind/performance modelling or clustering, but the way you calculate the AEP. The calculation is not described with enough detail and misses a strong foundation. There is no proof provided to draw conclusions about what AEP is realistic. Also the part on the AWES power coefficient is not very rigid and I expect the results to be specific to your methodology (initial guess optimizations). The c_p that we know from conventional wind energy technology is derived from physical laws, the presented coefficient is lacking this strong foundation (Buckingham's Pi theorem).

Writing: The paper still feels like a draft version. It could benefit a lot from more precise writing and better choice of words. A lot of suggestions are provided in the technical

CC BY

comments. There are quite a few bold statements in text, which either need more proof to back it up or could be formulated a bit more conservatively. E.g., at line 192 (page 10) it is mentioned that the cluster mean profiles that show decreasing wind speed above a certain height could be the result from choosing too many clusters. As these type of profiles are not covered by log/power law profiles, I would think that the added value of the clustering approach (and thus your methodology) is that you can identify such profiles from the data. Also there are a few statements that are not part of the scope of this paper. E.g., at line 372 (page 20) you briefly discuss the offshore WT tower design. This is a bit distracting and not adding a lot of value.

Figures, tables, and equations: The readability would improve a lot by including sublabels (a, b, c, etc.) and referring to them in the text instead of e.g. “(right, 3rd from top)“ for figure 10.

The legends of some figures are a bit confusing, e.g., the legend of figure 14 has different entries for line colors and styles and some combinations are missing (e.g. no round blue marker is included, while a square blue marker is and there is no separate general entry for a solid line). It would be good to be a bit more consistent.

The captions are often a bit long and sometimes include too much detail, e.g., the caption of figure 2 also states observations.

The size of the figures and tables are not very consistent, e.g., table 3 is huge for no clear reason.

Figures are placed in the appendix, while they are still covered in the text. In that case, in my opinion it’s good to have them in the body of the paper.

Place the equations at the related text and introduce all variables in the text.

CC BY

2 Specific comments

Introduction could be better structured, e.g., objective/contributions of the paper are not clear. The introduction is not very informative about closely related literature on AWE yield assessment. Also clustering is a separate section, but its roll in this study is not clear from the introduction.

I am missing a critical discussion about what data is suited for your methodology. In the conclusion you mention that the WRF data has a higher resolution than re-analysis data, but why would you need that? Also, doesn't the data assimilation used in the reanalysis lead to more trustworthy results? One of your conclusions is that evaluating the power output for 30 wind profiles (k=10 & p5/p50/p95 profiles) gives you the best estimate of AEP. So you discard most of the WRF simulated wind profiles for calculating the AEP. I would even expect that the data of a coarser model would be better suited for this context, as especially the p5 and p95 profiles are less prone to being eccentric, outlying wind profiles and thereby more representative.

Presenting the results with wind profiles could benefit from better structuring. Discuss choice for k at section 3. Based on my own work, I don't expect an elbow/kink in the inertia line. Be more specific about why you choose the number of clusters. Section 3 uses 10 representative profiles, then figure 4 only displays 4 profiles, figure 11 uses the p5, p50, p95 profiles for k=20, and figure 14 evaluates different k, but for only k=20 evaluates p5, p50, p95 (why not k=10?). Consider introducing your approach for these profiles at the start of each section. Also in the conclusions you state conclusions about the k=10 - p5, p50, p95 analysis which is not covered in the body of the paper.

In general the p5, p50, and p95 need to be better introduced. They are first mentioned at the end of section 4.5. After reading more I inferred myself that these are percentiles. I think their introduction needs to be accompanied by a figure for clarity. Also p50 is the median: how much does it differ w.r.t. the cluster mean?

CC BY

I would not discuss this pre-optimization step for the tether sizing (section 4.3). Currently it is a bit confusing and does not add much value. It would be fine to consider the tether diameter as a given as this paper is about assessing performance metrics and not system design.

The non-linear grid on the x-axis of e.g. figure 12 needs to be justified. This part of your paper is where your work differs most from other studies: you should introduce it before the results section. part of your methodology I would expect the p5 and p95 causing a lot irregularities in the power curves and wind distributions, which vary depending on the metric which you put on the x-axis. As a result, the precision of the AEP which follows from integration over this axis will be low. In the figure differentiate between the p5, p50, and p95 data points.

My biggest concern about the AEP methodology is the mapping of the high dimensional space describing the wind profiles (60*2=120 dimensions?) to a 1D space (see figure 12). How would you justify integrating over only this 1D space? How accurate would this be? Do you also use p5, p50 and p95 wind profiles to the construct frequency distributions or only for the power curves?

How do you assess what AEP is realistic? For a very large k (i.e. k=number of WRF data points), the clustering output is the same as the input wind profiles. Applying your framework with a very large k is thus the same as doing the optimizations for every WRF wind profile, which would give you the best assessment of what is a realistic AEP. The trends in figure 14 don't converge to the diamond-marked value (k=20, p5,p50,p95-profiles) for very large k. This suggests that eccentric wind profiles are over-represented in the AEP calculation using the p5,p50,p95-profiles.

Mind that the c_p of a wind turbine is not constant, but is lowered after the rated wind speed is reached. You compare the AWES power curve with one of a conventional WT. However, you don't justify your choice for the rotor area of the WT. From what I understand, you conclude that $c_p = 0.3$ gives a good agreement with the AWES power

CC BY

curve, but it's not clear why you change the c_p and not the area. As a result, the comparison is not fair.

How would you use c_p/chord in practice for power estimation? I think it's still a rather challenging task as you still need to determine the relation between l_{path} and the wind speed. More importantly, for WTs c_p is often used to quantify the efficiency of the energy conversion. I don't think the given formulation is a good metric for AWES efficiency. Take for example the (hypothetical) situation where we have a uniform wind field and let's neglect the variation of the air density. As you implied earlier (line 356): the mean cycle power is not very sensitive to the flight path length. So for the same kite size, a shorter and longer pumping cycle with the same mean cycle power will give me completely different values for c_p while the AWES efficiency did not change.

Dividing cp by c is the same as having the area of the kite multiplied by the path length in the denominator of equation 5 - why not use the wing area instead of the swept area in the first place and leave out the path length?

3 Technical corrections

3.1 Abstract

page 1

- line 5: “A universal“ instead of “An ...“
- line 5: What is the problem with power curves for log profile/power law wind conditions?
- line 6: “complex tether and drag losses“ - why does this lead to a more problematic power curve description? Also WTs occasionally operate in wind

CC BY

conditions that are not covered by the assumptions made for determining their power curve e.g. low level jets.

line 7: The role of “rotor area normalization“ to the power curve description is not clear. One can compare the harvesting efficiency of a WTs with different scales by normalization, but normally the power curve just characterizes the (non-normalized) power.

line 7: Not clear where “Therefore“ refers to

line 14: put “with wind speed“ after “decreases“

3.2 Introduction

page 1

line 20: WTs reach above 100 m

line 22: Use acronym for wind turbines

line 24: The list items are a bit random, suggestion: “3-bladed HAWT with conical tower“ - as to my knowledge you don't find commercial HAWTs without nacelle and generator.

page 2

line 27: Replace “route“ by “concept“

line 29: Reel-in description is a bit simplistic: flexible kite is really pulled back in, whereas a rigid wing utilizes its gliding capabilities.

line 34: State that power curves are in general only used for a preliminary analysis.

line 35: Not clear why the reference to Malz is needed here, does it belong to the previous sentence?

CC BY

line 36: “wind speed magnitude“ implies that the wind field in which an AWES operates can be described with one magnitude - needs some more explanation.

line 37: Ground-gen does not operate at a single altitude: “optimal trajectory“ includes information about altitude.

line 37: Suggestion: split sentence: “Simple ..“

line 41: “most ... studies“: it would be relevant to know which studies use an alternative approach.

line 47: Discuss directly using measurements/LiDAR data for assessing wind resource (no weather modelling).

line 48: Discuss your methodology (starting from “Results in ...“) in a separate paragraph. Also include here what exactly the contribution is of this paper. You already touch upon this in the paragraph starting at line 32, however it is not very concrete (probably you want to the content of this paragraph down here).

line 48: As I understand, you previously corrected WRF with LiDAR for the Pritzwalk location. Therefore none of the mentioned reasons for not using apply LiDAR apply here. So how do you justify using purely WRF data?

line 54: Suggestion: Section 2 introduces the WRF model set-up and compares the onshore and offshore wind resource that follow from the WRF simulations.

line 55: Would be good to mention clustering before when introducing methodology.

line 59: replace “derive“ by “produce“, replace “This includes“ by “These include“

page 3

line 61: As I read it: the coefficient **definition** directly follows from the results, however I don’t think this is what is meant.

3.3 Wind data

page 3

line 64: Representative for what?

line 67: AWE might be promising for other type of locations where it does not have to compete with WTs.

line 71: Why use different periods?

line 75: replace “in“ with “with“

line 79: replace “on“ with “in“

line 78: replace “The focus of this study is not on the detailed comparison between mesoscale models, but on AWES performance subject to representative onshore and offshore wind conditions determined based on clustered wind profiles (described on section 3). To that end“ by “For the assessment of AWES performance“

line 80: What is adequate?

line 80: remove “data“ in between sentences

line 82: Why use different data sources for boundary conditions.

line 88: replace “h“ by “hours“

line 88: Why use different approaches to simulate 1 year?

page 4

figure 1: It’s hard to assess the topography from this figure, leave out “topography“ in caption, or add information on color scale.

line 91: “High-Performance Computing“ without capitals?

line 94: Wind directions without capitals

CC BY

line 94: Dominant wind direction offshore is southwest for both 100 and 500 m.

line 94: Using “rotating“ is a bit strange here: the wind direction changes or the wind turns.

line 96: Start sentence with “The“

line 97: “Offshore conditions ..“ is a bit vague. How does the 10 degrees relate to the 5 degrees of the preceding sentence? If the wind would always veer 10 degrees than so would the average wind direction.

page 5

line 98: “the same westerly wind direction at high altitude“ - what do you mean? Figure 2 shows that offshore the wind is predominantly southwest.

line 99: Add point at end of sentence

line 99: Replace “The relative wind speed increase of“ by “The wind shear at“, and remove “and the already high wind speeds at lower heights“

line 102: replace “distribution“ by “distributions at each individual height level“

line 102: replace “These statistics give an insight into the overall wind conditions, but the actual profile shapes“ by “These distributions give insight into the wind speed statistics at the individual heights, but not onto the statistics of the wind profile shapes.“

line 105: replace “have a fairly narrow range“ by “are relatively low“

line 106: replace “up to high altitudes“ by “for the full height range“

line 106: replace “This leads to the development of“ by “The distributions show“

line 109: “Such multimodal distributions at higher altitudes are better described by the sum of two or more probability distributions“ - isn't this the definition of multimodal?

CC BY

line 113: “As mentioned above, the relative wind speed increase with height is less pronounced offshore than onshore.“ - why mention it twice?

line 114: suggestion “Conventional WTs benefit from low wind shear offshore ..“

line 115: replace “However, offshore AWES will also benefit from higher offshore winds and move offshore for other reasons such as safety or land use regulations“ by “Nevertheless, also AWES benefit from low wind shear. Among the reasons for placing AWES offshore are safety and land use regulations.“

page 6

figure 2: State that these results reflect single locations. Leave out “On average wind direction onshore rotates about 14 âŮę while offshore winds rotate about 5 âŮę between 100 and 500 m. Onshore shows a higher wind shear due to higher surface roughness and relatively high wind speeds offshore.“ - this belongs in the text.

line 116: “Another benefit of offshore AWES in comparison to conventional WT is the smaller and cheaper support structure.“ - Statement is a bit misplaced in the wind resource discussion and needs a reference. Also the arguments are one-sided, there are many reasons why not to put AWES offshore.

line 118: replace “categorized“ by “characterized“

page 7

figure 3: Clarify that this is not a 2D histogram, but independent histograms/distributions per height. State that these results reflect single locations.

line 126: Not clear if this is the classification as presented in the table.

line 129: Is the ocean always warmer? Remove “likely“

line 131: Not sure what the use is of this paragraph: only covers literature without touching upon the results.

CC BY

3.4 Clustering of wind conditions

page 8

table 2: State that these results reflect single locations.

line 136: Statement is not very informative, rephrase to emphasize that it's not just time- and space-averaged wind velocities that effect the power output of wind energy systems, but also the variation in time and space.

line 138: Elaborate on excessive averaging.

line 141: replace “proxy“ by “metric“, replace “a metric that“ by “, which“

line 143: Only one study is listed

line 143: Not clear what diverge with height means: large wind speed spread at high altitudes?

line 144: Do you mean to say that there is no correlation between the wind speed profile over a high elevated layer and the surface-based stability?

line 146: Also grouping based on stability is “based on data similarity“: be more precise.

line 146: What does the reader need to see in the appendix? - Don't refer to results before the explanation of your methodology is finished.

line 148: Mathematical clustering is a vague term: explain. You use clustering, classifying, categorizing, and binning interchangeably: be more consistent.

line 154: The u-component is per definition along the x-direction. Explanation is not clear: also u_{main} and $u_{deviation}$ are not throughout this paper anymore - so why introduce them here?

line 158: Explain what a data point consists of. Also not clear that each data point is assigned to the closest centroid. Replace “defined“ by “represented“

CC BY

line 159: Suggestion: “The clustering finds the centroid positions that minimize ..“

line 160: Distances between?

line 161: Rephrase: the centroid will at best coincide with a data point by chance

page 9

line 162: I misunderstood at first: I thought you were talking about the labels assigned to each data point. Leave out mentioning the initialization as it is confusing. Just mention that the label number that each cluster gets is rather random and does not have any mathematical meaning.

line 164: replace “Later evaluation uses clusters sorted by average wind speed up to 500 m.“ by “As presented, the resulting clusters sorted by average wind speed up to 500 m.“

figure 4: Use the same color scheme for the clusters as for figure 5. It’s unclear from the right column of figure 4 what is plotted on the y-axis as there is no grid/ticks.

line 168: "inertia reduction becomes marginally small with increasing number of clusters" - this is not completely true: above all the elbow method says that kinks in the inertia trend indicate sensible choices for k. It’s hard to observe them with evaluating so little values of k. Best to just use a step size of 1.

line 169: that doesn’t make inertia meaningless

line 170: add space after hyphen

page 10

line 171: table 1 doesn’t show any difference between the vertical levels (grid) of the two analyses. How different are they? I would not expect the vertical grid to have a large effect. More probable is a larger spread of the centroids.

Printer-friendly version

Discussion paper

line 172: replace membership by a more precise explanation

line 176: also if you would use a fixed (not random) initialization the cluster would not be arranged in a logical order, suggested: "Note that the order of the clusters is random and does not follow any logic."

line 176: It's still on the y-axis.

line 178: Too many I don't find very likely. At least the mean silhouette score always decreases with k. Do you have a reference?

line 179: Not clear what you mean with "the continuous nature of wind which results in a high cluster proximity" and how this effects the silhouette scores - please expand.

line 180: For readability I would suggest not to jump to the conclusions of the next section.

line 181: Replace "intersect .." by "are grouped together with monotonic wind profiles"

line 182: closing parenthesis missing

line 184: Is there a reason for using first wind speed and after velocity? If not, rephrase.

line 185: replace "comprising .." by "WRF-simulated wind speed profiles that are input for the clustering"

line 185: suggestion: "Within a cluster, the wind speed profiles span.."

line 189: False statement: this explains the higher inertia, not the silhouette score. Also the onshore clusters are distinct.

line 191: I don't see why "directional differences" result in a decrease of wind speed with height - explain.

line 192: What about small-scale weather phenomena?

CC BY

line 192: Why too many clusters (in which context)? I think it is very insightful to find these type of cluster mean wind profiles. Them having a lower probability makes it even more interesting to evaluate why the clustering gives these clusters as a result as normally k-means clustering tends to produce equally sized clusters.

line 195: Is the wind speed inversed or the wind shear?

line 197: replace “determining“ by “dominant“ and replace “stacked“ by “ordered“

line 200: Suggestion: add reference to Schelbergen

page 11

line 204: For which application/analysis are long term averaged wind speed profiles considered? Using a averaged profile **shape** is commonly done for AEP calculations, but that's a somewhat different approach.

figure 5: The wrf-simulated wind profiles only show as a filled area, so not very informative - but I suppose it doesn't hurt either.

page 13

line 220: The name of the “other“ category suggest to me that these conditions are exceptional/not often occurring, however your analysis shows the opposite. How should I physically interpret this category?

line 220: What do you mean with wind power assessment? Why is the impact of low wind conditions low? I don't understand the comparison.

line 221: Basically what you're saying is that how often wind conditions occur (the wind speed distribution) has a large effect on the AEP? - I would say that the reader is familiar with this.

WESD

Interactive comment

Printer-friendly version

Discussion paper

CC BY

3.5 AWES trajectory optimization

page 13

line 231: Very technical start of the section - readability would benefit from further introduction.

line 232: Which unstable dynamics are you referring to?

line 233: Which multiple inputs, multiple outputs?

page 14

line 238: Which simulated profiles? Aren't you using the cluster means?

line 239: replaced "during" by "of"

line 247: What is the main wind direction?

line 252: Suggestion: "We use the aerodynamic model of the Ampyx AP2 (Malz et al., 2019; Ampyx)"

line 253: The footnote is confusing as you mention that no other data is available while you are referring to a source.

line 259: Introduce kappa and bring equation 2 forward.

line 260: "This results in an overestimation of output power and lower cut-in speed in comparison to a heavier aircraft." - unclear how you come to this conclusion.

line 263: Mentioning focus of paper is a bit misplaced, better save that for the outlook.

page 15

figure 9: Why mentioning Loyd in caption?

CC BY

line 264: rephrase

line 265: Leave out or mention how you would estimate it.

equation 2: Position at related text and introduce properly.

line 270: mention conditions for which this is valid.

line 270: Last part of sentence is confusing. Suggestion: ".. , thereby the reel-out speed is expected to remain below 10 m/s as the wind speed hardly exceeds 20 m/s.

line 271: Implies that the ratio is fixed, however I don't think that's what's meant. - example values 10/15 is superfluous.

line 272: I would not discuss this pre-optimization step. As you mentioned yourself: this paper is about assessing performance metrics and not system design. Consider the tether diameter as a given.

page 16

line 280: No need to state subsection number, table number suffices.

line 281: why "or"?

line 282: add "coefficient" - why do you use a linear relation?

line 285: replace "prevailing" by "can be adapted to", replace "dynamically" by "continuously"

line 286: replace "AWES" by "AWESs"

line 290: Isn't 19 m/s a bit low? - this is equivalent to a mild storm

line 293: Explain what a p-value is: add a figure to explain.

line 294: Why average wind speed up to 500 m?

line 294: On line 176 you mention that a representative k is 10. Better to choose k already at section 3, instead of quickly mentioning it here.

WESD

Interactive comment

Printer-friendly version

Discussion paper

CC BY

page 17

line 296: Rewrite “cluster centroids.. implemented“

line 297: Not clear when you use the p5,50,95 and when you don't and how you use them if you do. Please rewrite paragraph. So you only use these p-values for determining the power curve? I would say this is the most important part of this study so it should get more explanation.

equation 3: Move to the related text.

line 304: Would the analysis benefit from running the optimization using multiple starting points?

line 306: remove hyphen: “inequality“

line 306: Which equality constraints? You only mentioned inequality constraints.

line 307: As far as I know, an optimal control problem always uses discretized control intervals, irrespective of if direct collocation is used or not.

line 307: missing space after dot

line 307: Is this the input you give to awebox for generating an initial guess? Explain estimated aircraft speed - this will vary between reel-in and reel-out and also within the reel-out phase over one loop.

3.6 Results

page 17

line 317: Shouldn't it be “average wind speeds for different height ranges“?

line 320: “Rayleigh distributed log-profiles“ is not very precise

line 323: “representative“ does not fit the context, list which profiles you have chosen.

Printer-friendly version

Discussion paper

CC BY

page 18

line 325: missing closing parenthesis, replace “figures“ by “figure“

figure 10: In caption list which profiles are plotted. Also describe what is highlighted using the colors in the left column. Remove “The deviation of the colored lines is caused by the approximation of discrete data points with Lagrange polynomials.“ - too much detail for caption.

line 328: replace “optimization“ by “modelling“

line 328: Wind speed vs wind velocity profiles is a bit confusing. Just mention that only the wind speed is plotted and that changes in wind direction with height can be seen in the lower left plot.

line 330: Didn’t you introduce the “rotated u and v component“ as u_{main} and $u_{deviation}$? - then also use them here

line 330: replace “wind velocity components as experienced by the AWES in color“ by “with the part of the profiles corresponding to the height range swept by the kite in color“

line 332: not clear which are the onshore and which the offshore profiles.

line 333: first should be x-z plane

page 18

line 335: perpendicular to x is both y and z-direction

line 335: What is unrealistic about them?

line 338: Can you identify whether the system is de-powering or not? If so than you can differentiate between the two.

line 338: What about de-powering by increasing the reelout speed?

Printer-friendly version

Discussion paper

line 342: So for an elevated path, you don't want to fly at maximum cl^3/cd^2? This is not what we observe in figure 10: the most elevated path has the lowest angles of attack during reelout.

line 347: So you're relating this observation in the reel-in phase (reaching max reel-in speed) to an observation in the reel-out phase (de-powered loop)? Isn't it more likely that the de-powering is triggered by reaching max tether force during reel-out?

line 355: Which previous analyses?

line 356: Not completely: zero would probably not give you the best performance.

page 19

line 363: Clarify that you're not just using the 4 profiles that were treated in the previous subsection.

line 363: What is $z_{operation}$? I would expect a height range, not a single value.

line 364: Replace "emerge"

line 365: add comma after onshore, correct for this in the rest of the paper

line 365: remove "and typically higher winds offshore" from end of sentence and mention in a different sentence that generally tether length increases with wind speed

page 20

line 369: "on the other hand" - comparison is not clear

line 372: "This also has implications for tower-based.." - Leave the WT discussion out. It's well known that offshore turbines have lower towers than onshore.

Printer-friendly version

Discussion paper

line 376: The distributions for 25 and 50m2 are not that much different and is worthwhile mentioning.

line 378: Figure 11 shows only onshore 1% exceeding 600 m for 20m2 and offshore 0%

figure 11: Differentiate between p5, p50 and p95 using for example different markers. Put occurrences on the horizontal axis. If I understand correctly your only plotting the results of 60 optimizations: this would be more clear by plotting occurrences.

page 21

line 384: replace “unanimously accepted“ by “consensus“

line 385: replace “wind speed probability distribution“ by “wind resource model“

line 387: I would just mention that AWESs generally operate in a larger height range

line 389: So this leaves you with a non-linear grid on the x-axis? Also the order is different for every reference height range? - If so, mention this. I would expect the p5 and p95 causing a lot irregularities in the power curves and wind distributions. Would it be possible to show data points represent p5, p50 and p95 powers? About using the power curves for integrating: basically you're trying to map this high dimensional space describing the wind profiles (60*2=120 dimensions?) to a 1D space. By doing so you loose a lot of information on the wind profiles. How would you justify integrating over only this 1D space? How accurate would this be? Do you also use p5, p50 and p95 wind profiles to the frequency distributions? Is the area underneath your frequency distributions 100% in that case?

line 393: “respective AWES operating altitude“ - why using 60 - 629 m and 59 -551 m? Not all trajectories fly over this full range.

Printer-friendly version

Discussion paper

line 394: “black“ instead of “red“

line 395: The power curve shows a varying c_p^{WT}

line 397: what is the value of the rotor disc area?

equation 4: U^3

line 400: What do you mean with limit specific designs?

line 405: Isn’t this just due to your choice for the rotor disc area?

line 406: Explain more precisely: if wind speed is monotonically increasing with height $u_{100m} < u_{100-400m}$. So the data points move to the when plotted against $u_{100-400m}$ with respect to when they are plotted against u_{100m}.

line 409: replace “overlap“ by “lie on top“

line 409: Not clear why 200m ref would be better, since you mention in the previous sentence that every reference height gives roughly the same power curve.

line 410: with divergence you mean large difference?

line 411: Also c_p^{WT} is lowered after the rated wind speed.

page 22

figure 12: On y-axis is the energy contribution: not E average

line 414: That the flight trajectory influence mean cycle power should be clear from the literature study.

line 415: An overkill to use a separate plot when only adjusting the cp. What if you change the rotor disc area?

line 416: Leave out “A better .. future study.“. Mention in the outlook at the end of paper.

line 418: Rephrase “integral multiplication“

line 419: replace “Its total“ by “The area underneath the distribution“

page 23

line 420: Rewrite “shifts towards higher wind speeds due to .. higher wind speeds“

line 421: How is that similar?

line 426: Which effect scales with size? - Isn’t this specific to your methodology, i.e. the integration of the equivalent 1D wind profile space.

line 428: Leave out “This indicates that onshore wind conditions favor higher operating altitude due to higher wind shear.“

line 429: What reduction? The AEP goes up.

line 432: “This main difference“ - which difference is explained exactly?

line 433: The cp is very much dependent on the area you chose, which you haven’t justified. Also, the onshore 20m2 curve for reference height 100m lies on top of that of the wind turbine.

line 434: “wind speed along the actual AWES trajectory“: this is a varying property - how would you use it as a reference?

line 436: Figure 11 shows that the operational height range of 100 m are common. Why would a 500m height range be a good reference in that case?

page 25

line 439: How would you use this power coefficient in practice for power estimation? I think it’s still a rather challenging task as you still need to determine the relation between l_{path} and the wind speed. More importantly, for WTs cp is often used to quantify the efficiency of the energy conversion. I don’t think the given formulation is a good metric for AWES efficiency. Take for

Printer-friendly version

Discussion paper

example the (hypothetical) situation where we have a uniform wind field and let's neglect the variation of the air density. As you implied earlier (line 356): the mean cycle power is not very sensitive to the flight path length. So for the same kite size, a shorter and longer pumping cycle with the same mean cycle power will give me completely different values for cp while the AWES efficiency did not change.

line 441: You mean average wind speed?

line 442: Air density at which height?

line 444: $z_{operation}$ suggests a single height, but probably needs to be a height range? Change throughout the paper.

line 451: cp is not truly "velocity profile independent": especially at low wind speeds you see a vertical spread for fixed wind speed. How sensitive is power output to the wind profile shape?

line 452: sentence does not explain any difference

line 452: rewrite "The difference .. remains almost constant (see sub-section 5.1 and 5.2)" - unclear what you're saying.

line 457: Dividing cp by c is the same as having the area of the kite multiplied by the path length in the denominator of equation 5 - why not use the wing area instead of the swept area in the first place and leave out the path length?

line 459: Fit looks poor for larger wind speeds. Why do you only plot the line up to 18 m/s?

line 462: "contrasts" replace by "compares"

page 26

figure 13: The markers are not connected by the lines. Why is this?

page 27

Printer-friendly version

Discussion paper

CC BY

equation 7: leave out h/year

line 472: remove “static“

line 473: use “probability distributions“

line 475: replace “justified“ by “justify“

line 480: I think it is a bit bold to say that they are averaged out. You could reason that they are underrepresented. Also show with a figure similar to 12 how both computations are different.

3.7 Conclusions and outlook

page 27

line 490: replace “characterized“ by “evaluated“

line 492: replace “deduced“ by “uses“

line 493: replace “Representative wind velocity profiles based on k-means clustering were chosen to reduce computational cost.“ by “A representative wind resource model is deduced using the results of clustering wind profiles“ - and place after discussing WRF

line 494: Do you need this high resolution if you drastically simplify the wind model in the end?

line 497: “acceleration“?

line 511: replace “implemented“ by “given as input“

line 514: replace “fast“ by “high“

line 517: See earlier comments on WT power coefficient.

line 519: remove part about social acceptance: not touched upon in body of paper

CC BY

line 522: replace “collapse ..“ by “yields a location and size independent metric“

line 526: You did not evaluate the AEP for 10 clusters and the p5, p50, p95 profiles if I’m not mistaken. So how do you get to this conclusion? If the AEP is higher than for all other calculations I wouldn’t expect it to be the most realistic.

line 529: I don’t see this back in Figure 14, it shows quite a large bias between the log and cluster AEP.

line 536: Why do you expect this?

line 538: Be more general in this paragraph.

Printer-friendly version

Discussion paper