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Interactive comment on "Clustering wind profile shapes to estimate airborne wind energy production" *by* Mark Schelbergen et al.

Mark Schelbergen et al.

m.schelbergen@tudelft.nl

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Thank you for the comprehensive comments. We feel that they were very helpful for increasing the quality of the paper to the current level. Your comments, together with those of referee #1, led to a thorough revision of the paper. The most important changes to the paper include:

1. Including information on orography
2. Discarding the lidar discussion
3. Using one stability metric: the Obukhov length, and corresponding classification for identifying stability trends within the clusters

4. Fitting logarithmic profiles only to the lower part, i.e. <200 m, of the mean profile shapes

We respond to the referee comments by including our answers below the original comments. Our answers are preceded by one or both of the following labels:

AR = author's response

AC = author's changes in manuscript

1 General comments

Paper "Clustering wind profile shapes to estimate airborne wind energy production" describes statistical analysis of wind profiles in order to identify typical shapes of wind profiles that are later used to optimize Airborne Wind Energy systems. The paper is interesting, has scientific novelty and describes some interesting, meaningful and useful results. However, I would suggest that the paper would benefit from some careful editing. The overall quality of English is good, but the presentation of information could use some improvement. I had to apply some effort to follow the argument set out by the authors. Some important procedures (normalization of the wind profiles, fitting of the Obukhov length) are not explained sufficiently, while other aspects of the work that do not lead to any significant conclusions are explained at length.

[AR] Thank you for your comments. We have thoroughly revised the paper using your feedback and that of referee #1 (which had quite some comments). The most important changes include:

1. Including information on orography

2. Discarding the lidar discussion
3. Using one stability metric: the obukhov length, and using classes in terms of the Obukhov length
4. Doing the log fit only for the lower 200 m

2 Specific comments

- P2L6: “Computationally expensive, brute force calculations”. How expensive are the calculations? Can they be done in a few hours on a desktop computer, or is HPC required? An approximate estimation could be added here for the general reader.

  [AR] For the study of Malz, three months of three-hourly MERRA-2 reanalysis data was used which originally took ten days. We decided not to put any concrete numbers here as it is very much dependent on the machine.

- P3L10-L25: Here the word “reanalysis” is used to refer both to ERA5 and DOWA. Although formally DOWA fits the definition of reanalysis, in practice, at least in my experience, the word “reanalysis” is used exclusively for global reanalysis, such as ERA5, ERA-Interim, MERRA, etc. I would suggest using term “modelled datasets” or something similar, to refer to ERA5 and DOWA at the same time, to avoid confusing the reader.

  [AC] Changed "reanalyis" in "modelled" as suggested.

- P5L6: Hourly averages are calculated for the LiDAR data. Why? For the rest of the paper modelling results with hourly resolution are used, which are usually interpreted as the “instantaneous” wind speed, of course taking into account the fact that the mesoscale model cannot represent the turbulent fluctuations. Are the hourly averages comparable with model data?

[AR] In "Low-level jets over the North Sea based on ERA5 and observations: together they do better", P.C. Kalverla et al., various averaging methods are compared and it is shown that hourly averages align best with the instantaneous reanalysis data. However we discarded the lidar discussion completely since it was not adding much to the paper.

[AC] Discarded lidar discussion.

- P6L9: In the paper vertical wind speed profiles are fitted using logarithmic profile that has been corrected for stability. From the context I understand that Obukhov length L is the parameter that is being changed during the fitting procedure, but a more precise description of the methodology how the Obukhov length is acquired would be beneficial. If I understand correctly, the same procedure is applied for fitting the instantaneous and long-term mean profiles in Fig. 2. I would like to see some arguments why long-term means can be treated in the way authors do it here. I refer the authors to the paper “Kelly, Mark, and Sven-Erik Gryning. "Longterm mean wind profiles based on similarity theory." Boundary-layer meteorology 136.3 (2010): 377-390.” for a discussion about long-term stability correction of wind profiles.

  [AR] We are effectively fitting a mean value of the stability function (not using a mean value of L) which is the correct way to make the mean long term profile. The Kelly paper makes the point that it is not appropriate to calculate a mean L (based on flux measurements, etc) and then produce a mean profile.

  [AC] Added: ".. following the approach recommended by Kelly et al. From this, a mean value of the Obukhov length L can be inferred." to second paragraph of Sect 3.2.

- P5L18: Do I understand correctly that the parallel and perpendicular components are calculated for a different reference wind direction in each sample, namely, in each sample the wind velocity at 100 m will have only the parallel component? If so, please expand the description of this procedure in the text.

[AR] Correct

[AC] Added: "As a result, the perpendicular wind speed profiles are zero at 100 m."

- P5L19-20 “each sample is normalized by the 90th percentile of its wind speeds at each height”. This sentence is highly confusing, because one interpretation could be that at each height the distribution of wind speeds is constructed, and each height has its own normalization speed calculated as the 90th percentile of all wind speeds in given height. I suggest expanding the description of normalization to remove ambiguity.

  [AC] Rephrased to: ".. the 90th percentile of the sample’s wind velocity magnitudes is used to normalise the wind speed components."

- I think that Figure 2 is not successful in conveying the important information in an effective manner. The horizontal axis for PC1 and PC2 are not the same. The hodograph is small and the markings (dotted line vs uninterrupted line) are not explained. However, my biggest problem is the fact that the x-axis is chosen in such a way that the plots for parallel component and magnitude of the wind, which arguable are the most important features of the profile, are very small. In order to understand the results and conclusions I had to look at profiles that had the width of less than a centimeter (when printed out). I would like the most important features of profile to be plotted large and easy to understand. I would recommend rearranging Figure 2, dividing it into separate figures. To keep paper at a reasonable length, I suggest editing and shortening further sections of the manuscript. These issues continue in further Figures, such as Fig. 8. etc. I recommend focusing on the important parts of the graphs, making them large and omitting less important information for brevity.

[AR] We have experimented a great deal with how to present these figures. We agree that they are not the best for depicting details in the profiles. However, they allow our results to be shown in a consistent and compact manner, while still showing the overall trends of the profiles. Our conclusions are mostly about the latter and therefore we feel that it is justified to keep the current lay-out of the figures.

[AC] Changed the horizontal axes for PC1 and PC2 to be the same. Hodograph dotted line explained in caption.

- Section 3.3. requires some editing. Many complicated metrics are introduced, but only briefly, which does not allow the reader to follow the discussion easily. For instance, the fact that higher values of silhouette score mean better similarity within cluster, should be mentioned when the metric is first introduced (P8L10), or at the start of the discussion paragraph (e.g. P10L12)

  [AC] Added: "The dimensionless score ranges from -1 to 1: a negative value suggests that the sample is assigned to the wrong cluster, a value around zero indicates that the sample lies between two clusters, and a high value indicates that the sample is assigned to a distinct cluster". Also added expressions for the fit error metrics.

- P8L1: Why is “itk” used as a parameter name for the number of clusters? It is used twice and then never used again, for instance, in Fig. 5.a. Maybe it is better not to introduce such variable at all.

  [AR] Sorry for the typo. It should have been an italic k, where k refers to the number of clusters (k-means).

  [AC] Corrected

- Fig.5.(a). If the “cluster mag” and “cluster 2d” lines are identical, then maybe one of them can be omitted?

  [AR] They are not identical, but similar. We think it is helpful to show how the two error metrics for the same representation compare, as both metrics are used for drawing conclusions: ’mag’ is used for comparing to the log fit and ’2c’ is equivalent to WCSS.

- Figure 9 and Figure 12: (a) panel could be omitted without loss of important information – all years are the same. Please make the rest of the plots larger – again I was forced to concentrate on very small portions of plots to arrive at conclusions. Maybe Figures 9 and 12 can be combined to save space for legends. I would also suggest using the same Richardson number bins for both figures to make interpretation easier.

  [AR] In contrast to the other panels, the absolute frequency is on the y-axis and serves to show which part of the total dataset is represented by each of the clusters. This is deemed necessary since the table listing the cluster frequencies is discarded (see next comment item).

  [AC] To make interpretation of the stability easier, the samples have been binned using stability classes in terms of the Obukhov length.

- The discussion of filtered vs full dataset, e.g. Table 2, could be omitted to shorten the paper.

  [AR] Agreed

  [AC] Table discarded

- Two different measures of stability are used in the paper – L (Obukhov length) and bulk Richardson number. For clusters (2) and (3) in Fig.8. L values that correspond to neutral stability conditions are reported (P11L13). From Fig.9. and the discussion (P16L11-L17) they are associated with stable conditions. How do authors explain this discrepancy? Additionally, I would like to point out that clusters (2) and (3) are associated with higher windspeeds, and higher windspeeds typically are associated with higher frequency of neutral conditions, see for instance, Holtslag et al. 2014.

[AR] Due to these discrepancies between the fitted L to cluster-mean profiles and the recorded samples stabilities, we decided to change the fitting procedure such that the log profiles are fitted only to the wind speeds in the lower 200 m. This led to both approaches yielding more consistent results. Also the log profile is used in a more valid manner, i.e., the layer < 200 m approximates the surface layer.

[AC] Changed the fitting procedure. Converted the bulk Richardson number to L, allowing direct comparison.

- P11L13: Fitted Obukhov lengths are only reported for clusters (1)- (3). I understand why they should not be reported for cases where the fit cannot reproduce the shape of the profile, e.g. cluster (7). But what are the fitted Obukhov lengths for other clusters, such as (5) and (8)?

  [AC] Added figures 9 and 11, showing how the Obukhov lengths found compare. We think stating the exact values of L is not necessary, as the fits serve most importantly to show to what extent the cluster-mean wind profile shapes deviate from non-adiabatic logarithmic profiles.

- The authors claim that clusters 4-7 indicate potential low-level jets. I do not disagree, but in my opinion, the evidence presented is not strong enough and is somewhat circumstantial. Mostly, because the amplitude of wind speed maxima in Figures 8 and 11 is quite small (except for cluster (7) where I agree that the jet-like profile is quite distinct). Sometimes in literature it is required that wind speed in jets is at least 3 m/s higher than the surrounding flow. Due to the normalization procedure it is hard to estimate how pronounced are these wind speed maxima. The authors' position could be strengthened if more robust evidence to associate the clusters with jets could be provided – a case study or some wind speed profiles that are not normalized and show how distinct the jet actually is.

[AR] Our conclusion was stated somewhat boldly. The "low-level jets" description only served to describe the shape of the resulting profiles. We rather refrain from discussing what defines a low-level jet and therefore phrased our conclusions more mildly.

[AC] Rephrased "low-level jets" to "jet-like shapes".

- If the conditions for cluster (5) MMIJ DOWA are analyzed (Fig. 9), than one could conclude that they are very similar to cluster (1) for MMIJ DOWA, with the exception of wind direction, so all the interpretation for conditions for cluster (1) MMIJ DOWA could apply to cluster (5) as well, with the exception that the upwind flow is advected over the water for shorter time, as the prevailing directions for cluster (5) are from South. In fact, in PC1-PC2 axis (Fig. 7) the cluster (5) is next to cluster (1). The problem is that for MMC such conditions are impossible and therefore cluster (5) for MMC is not the same as cluster (5) for MMIJ, nor in the shape of the profile and nor in the location of cluster in the PC1-PC2 axis. Therefore, I cannot agree with the statement that cluster (5) for MMC is similar to MMIJ (P17L20).

  [AR] Our statement was not phrased carefully and therefore misinterpreted.

  [AC] Rephrased to: "The profile shapes for MMC-5–7 are (slightly) jet-shaped, as is the case for the offshore clusters MMIJ-4–7." Also, the parallels drawn by the reviewer for the offshore clusters MMIJ-1,5 are now explicitly described in Sect. 4.2.

- Table 3, column "Most frequent area". What is this classification based on? Are the orography and land use data used in DOWA available? If so, then adding this information to Figure 15 as a plot would strengthen the authors' argument, because currently, although I agree that features seen in Figure 15 are probably related to orography, again I would argue that more evidence is needed to support the authors' claims. I would not expect the general reader to be familiar with the topography of the Netherlands and Germany

[AR] DOWA does not include such data, at least it is not published. The classification was based on 'quick and dirty' observations, so not precise and well supported by data. We agree that the topography was missing.

[AC] We have discarded the "Most frequent area" column in the table, as they were blunt conclusions. We have included information on the surface elevation to Figure 1 and 16, such that the claims in the text are now better supported.

3 Technical comments

- Paper referenced as "Sommerfeld et al.": "Improving mid-altitude mesoscale wind speed forecasts using LiDAR based observation nudging for Airborne Wind Energy Systems" does not seem to contain anything related to k-means clustering. Maybe a different paper with the same first author is meant instead: Sommerfeld, Markus, et al. "LiDAR based characterization of mid altitude wind conditions for airborne wind energy systems." Wind Energy 22.8 (2019): 1101-1120.

  [AR] Correct

  [AC] Changed reference

- Order of figures. Figure 8 is referenced in text before Figure 7. Figure 8 is referenced in P11L10. Figure 7 is referenced in P11L21.

  [AC] Corrected

- P1L19: "Deviating profiles are likely to occur". I suggest "deviations from the expected profile shape are likely to occur".

[AC] Replaced with: "Moreover, within this layer, not all wind profiles can be described well with these relationships."

- P4L11: “Better representation of the coastal morphology”. “Coastal morphology” is the study of natural processes that change the shape of coastline, e.g., erosion or sediment transport. I suggest using “coastline” or “better resolution of coastline” instead.

  [AC] Replaced with “coastline”

- P16L9: “the wind profile is typically will mixed”. Probably, “well mixed”.

  [AC] Corrected