Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-108-RC2, 2020

WIND ENERGY SCIENCE DISCUSSIONS

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

Anonymous Referee #2

Received and published: 17 February 2020

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.

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.
- 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.
- 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?
- 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. "Long-term 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.

- 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.
- 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.
- 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.

- 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)
- 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.
- Fig.5.(a). If the “cluster mag” and “cluster 2d” lines are identical, then maybe one of them can be omitted?
- 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.
- The discussion of filtered vs full dataset, e.g. Table 2, could be omitted to shorten the paper.
- 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.

- 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)?

- 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.

- 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).

- 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.

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.

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

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

- 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.

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

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-108, 2020.