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WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "Characterisation of the Offshore Precipitation Environment to Help Combat Leading Edge Erosion of Wind Turbine Blades" *by* Robbie Herring et al.

Anonymous Referee #1

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The paper contributed by Herring et al. from the title appear relevant for the journal. The offshore environment precipitation climate is a relevant research topic in perspective of leading edge erosion of turbine blades. The paper focuses on the meteorological side of this topic and in particular, compares the new drop size distribution data set versus the Best model published in 1950.

There are serious shortcomings in the paper. These can be categorized into

1) missing background information and discussion on precipitation meteorology.

2) insufficient presentation of the data processing, quality control and details on the instruments used.

3) statistical significance testing of the results.

4) relevance or implication of the new 'constants' and how these will influence assessment of precipitation in regard to leading edge erosion.

5) discussion of results is missing and the conclusion is a confusing mix of motivation and brief mention of some analysis results.

The title does not reflect the content of the article. There is no analysis of or description of how the observed rain data connects to combatting leading edge erosion.

These serious shortcomings are reason for rejection.

Below is given detailed review, in case the authors choose to improve the article and re-submit to a hydrological or meteorological journal.

Line 29. It says that Weather radars are widely used to predict the offshore precipitation environment. Please provide references to this and substantiate the entire paragraph on the background to your study. The text is short and unclear.

Line 34. It says that Kathiravelu et al. 2016 find Best to be outdated. This is not clear. The referenced work is a review on drop-size distribution measurement techniques during time. Along this line, it is noted that the sensor used in the current study is not in the list of Kathiravelu et al. The reference Agnew 2013 is mentioned and referenced as raindrops below 0.8 mm are slightly underestimated. Is that the only study available using this sensor? It is relevant to provide insight to the type of data collected versus other relevant recent data sets. The methods from 1950 are obviously not in use any more so the details on this appear out of scope for the current investigation.

The critical perspective on the Best function need a review of existing literature on this subject. This is missing from the article.

The drop size distribution is observed offshore in the North Sea. It would be relevant to cite and discuss other offshore drop size distribution data sets, e.g. from research

ships and other offshore sites (small islands), as well as coastal and land observations of drop size distribution in the UK. This information would be relevant as background information and introduction.

Does a weather radar cover the offshore site already with rain information available? In case, yes it would be interesting to have a brief background on this and the methodology in use (assuming it is Best model). Please add references.

Line 62. It says that two disdrometers are mounted, one at 25 m and the other at 55 m. Which of the two data series is presented in the current work? In the quality control section, it would be interesting to understand if both instruments observe similar precipitation and if quality check was done comparing the two time series. Looking at the photographs it appears that the flow field is different at the two heights. In case wind speed data are available it would be relevant to see if there is systematic influence as function of wind speed and wind direction to drop size and fall velocity between the two instrument's observations. One instrument is positioned vertical and the other horizontal.

Why? Table 1. Does this table include both liquid and solid precipitation, raw data before quality control?

Quality control is mentioned in subsequent section, so this is confusing. Are only data presented after this section quality controlled?

Line 81. The quality control appears too limited. It is recommended to ensure detailed quality control before subsequent analysis (Hasager et al. 2020 Renewable Energy). In particular, the hydrometeor-type frequency you present lines 114-137 was that included in the quality control? Did you use information on temperature to quality control hydrometeor type?

Figures 2 and 3. Do they include solid precipitation?

Lines 101-103. The seasonal breakdown of precipitation could be discussed later on

the discussion section, e.g. stratiform and convective events, and the influence to drop size distribution and rain intensity.

Line 108-110. The data is for one specific year. Annual variations are to be expected. So minutes and hours "a year" is misleading. Was this a wet year or a dry year?

Line 115. please refer to work on hail and leading edge erosion, e.g. Letson et al. 2020 WES, MacDonald et al. 2016 Wind Energy.

Line 115. In Bech et al., 2018 the rain intensity data were deduced from Jones and Sims, 1978, Maritime-temperate rain intensity frequency data. According to this data, a rain intensity of 10 mm/hr is exceeded approximately 0.06% of the time. This was rounded up to 0.1% in the presented model. 20 mm/hr was exceeded 0.02% of the time, and 50 mm/hr was exceeded 0.002% of the time. These numbers seem to be same size of order as what is reported in the present paper in review. Still the model and analysis presented in Bech et al. 2018 showed, that these few hours of heavy rain could cause the majority of damage observed on WT blades. However, the kinetic energy impact damage model probably over-estimates the effect of the droplet size, and thus the effect of rain intensity.

Line 118. A high amount of 'error' and 'unknown' occur (17.93%). It would be interesting to know if both instruments suffer 'equal' amounts of these and you could do 'gap-filling' from one instrument to the other, or find out what might be happening. In Table 1 it looks as July has most missing data. The total says 82.89% data that gives 17.11% missing data. Please clarify the numbers.

Figure 5. It is difficult to see the difference between snow grains and snowflakes with the colours chosen. Maybe use variation (not all open circles). It would be relevant to discuss the findings. How do you find your results are? As expected and reported elsewhere in literature? Give references in the discussion of results.

Figure 6. A suggestion is to use four different colours/symbols and put data into one

graphics. This would enable more clear reading of the data set and make it possible to see the lines be different. Furthermore, statistical test on significance of your results are necessary to draw conclusions. This also goes for Figs.7 and 8.

Table 4. You could include a row with the constants from Best 1950, i.e. merge table 3 and table 4.

Line 207. It says “Not appropriate to validate offshore weather radar data against Best DSD”.

It is unclear what you mean. Please clarify. It would be relevant to include reference to the work you have in mind stating this sentence, and explain the implication.

Line 215- 216. A section with discussion of the results versus state of the art research on the topic drop size distribution is necessary to include in the paper. It is also advisable to include perspectives on the drop size distribution and the impingement to turbine blades.

It is briefly mentioned (lines129-130) but not elaborated further. This would be a relevant perspective to discuss in the discussion section. The title of the paper says that you are studying leading edge erosion but you do not bring your data set into any perspective on this. So you will have to include that otherwise the title of the paper is misleading, and will need modification to reflect the content of the paper properly.

Line 227. It says “The offshore DSD aligned well with the data”. It is unclear what is meant. Please clarify.

The conclusion is a mixture of background, very brief sentences on the actual work, and very long part on future perspective. It would be beneficial to ensure the conclusion major part is related to the learnings from the current research.

List of references It is too short with lack of relevant meteorological literature.

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