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Comment on wes-2022-59

Hamish Macdonald (Referee)

Referee comment on "Lifetime prediction of turbine blades using global precipitation products from satellites" by Merete Badger et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-59-RC2, 2022

Thank you for reviewing our manuscript and for the constructive comments. Our response is given here below (in italics).

Comments have been provided in the attached PDF. Several relate to grammatical and tense errors.

Response: Thank you for taking the time to provide detailed comments and corrections. We have responded to each comment in the .pdf file attachment.

The impetus for the paper is understandable, with satellite data potentially providing a useful alternative for the lack of rain measurement data offshore, However, the IMERG accumulations do not appear to be in good agreement with the in-situ data and the relative bias for rainfall intensities is a concern.

Response: We have visualized the differences between IMERG and in situ rainfall through two new figures (Fig. 3 and Fig. 7 in the revised manuscript). We agree that the rainfall differences and biases are large and we have given some reasons for this in the Discussion. We believe that the differences are due to the very different nature of the IMERG and in situ data sets and therefore, there is little we can do to improve the statistics. The values we find are similar to those published for the Netherlands where the rain and wind climate is not too different from that of Germany and Denmark (Borgerd et al., 2021).

It is a bit of a stretch to suggest this data is now suitable for lifetime predictions, as it could be rainfall is not as impactful a factor in the calculation. If there was a "ground truth" of actual physical blade condition to compare against this would provide a stronger argument but these are estimations.

Response: We agree that the model for blade lifetime predicition is more sensitive to wind speed than to rainfall intensity and we have added an explaination of this to the Discussion (line 402 in the revised manuscript): "In other words, the blade lifetime model applied in this study is less sensitive to rainfall rates than to wind speeds Bech et al. (2022)".

Unfortunately, we do not have access to ground truth information about blade conditions at present but an experiment is planned, which could lead to further insights in the future. In the Discussion, we have added the point that actual physical blade conditions would be valuable next step (line 462 in the revised manuscript): "Our focus has been on the precipitation data and despite differences between GPM and local observations the lifetime results compare well. Another focus could involve wind speed variation and impact speed variation on blades with erosion observed in the field as by (Prieto and

Karlsson, 2018; Visbech et al. in review). It would be valuable to assure the methodological reliability using GPM and different wind speed input and precipitation data near wind turbine sites with observed blade erosion for lifetime prediction".

The lifetime prediction calculation methodology needs be explained in more detail and the main statement of the paper justified further.

Response: We have expanded the description of the model used for blade lifetime predictions (line 213-234 in the revised manuscript). This manuscript represents a very first attempt to use precipitation data from satellites for blade lifetime estimations. There is a potential for improving the method and the results in the future through improvements of the IMERG data quality and/or updates of the empirical lifetime prediction model.

Please also note the supplement to this comment:
https://wes.copernicus.org/preprints/wes-2022-59/wes-2022-59-RC2-supplement.pdf