# Manuscript ID: WES-2024-33 Drop-size-dependent effects in leading-edge rain erosion and their impact for erosion-safe mode operation

Nils Barfknecht, Dominic von Terzi

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**Dear reviewers,**

We would like to thank you for the time and effort you put into reviewing our manuscript. We believe that your comments have improved the quality of our work. Through this collaboration, we can move toward our common goal of mitigating leading-edge erosion.

Below, you can find your original comment on our work. Where appropriate, we have inserted our answers. We have spent a considerable amount of time carefully reviewing your comments. We hope that we have addressed them to your satisfaction.

Sincerely,

Nils Barfknecht and Dominic von Terzi

## Reviewer 1

**Comment** Use of DTU test data – 0.76 data is based on water spraying not pure droplet impacts and if the DNV RP 0171 calculation has been used to generate the VN curves then high speed video has shown the calculation doesn't give accurate values of number of impacts. The data is therefore not directly comparable with real wind turbine data.

Thank you for pointing this out. The 0.76 mm (G27 nozzle) droplets produced in the DTU rig should, indeed, more appropriately be described as a distribution of varying droplet sizes with a mean diameter of 0.76 mm, as shown in Figure 5 of Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127]. Based on your feedback, we have highlighted this peculiarity in our manuscript. In the original reference, the *impingement* (of the VN-diagram) is calculated with the following formula:

$$H(t) = \frac{P\omega t}{2\pi(r_o - r_i)d_{\text{drop, rp}}}. \tag{1}$$

This formula is simply a test-rig-specific formulation of the more general impingement formula:

$$H = WV_{\text{collection}}t. \tag{2}$$

Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127], did not use the DNV-RP-0171 guideline to calculate the impingement in their paper.

**Comment** High speed camera and sensor measurements also show that there is a mixture of direct impacts and glancing blows which have a dramatic difference in terms of the measured strains. You have assumed in your models that all droplets impinge head on at 90° which is at least not true in the test rig and likely not true on the turbine. Please include this assumption in the paper.

In our calculation of the impingement rate

$$\partial_t H_{I,V_{\text{wind}},\theta,\phi} = \frac{I f_{\phi,\text{plane}}}{V_\phi} V_{\text{collection}} f_I f_{V_{\text{wind}}} f_\theta, \tag{3}$$

and the impact velocity in particular

$$V_{\text{impact}} = \cos\alpha_\phi \left(\sqrt{V_{\text{inflow}}^2 + V_{\text{circumferential}}^2} - V_{\text{slowdown}}\right), \tag{4}$$

we did not assume that all droplets impact at a 0-degree angle (in our work, 0-degree is defined as head on) with the leading-edge. Rather, we calculate a drop impact angle $\alpha_\phi$ as shown in the following formula:

$$\cos\alpha_\phi = \frac{\vec{V}_{\text{res}}}{|\vec{V}_{\text{res}}|} \cdot \vec{n}_{\text{LE}} = \cos\left(\arctan\left(\frac{V_{\text{inflow}}}{V_{\text{circumferential}}}\right) - \varphi\right). \tag{5}$$

We do, however, assume that the coefficients of the *damage laws* are given for a head-on impact. That is

$$H_{\text{allowed}} = \frac{\alpha}{V_{\text{impact}}^{\beta}}. \tag{6}$$

Therefore, $H_{\text{allowed}}$ is the impingement to failure normal to the surface and $V_{\text{impact}}$ is the impact velocity normal to the surface. We added clarification about this in the text. From Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127] we understand that the test specimens were inspected at regular intervals for new damage and the associated location and the time in the erosion tester were recorded. From the passed time and known rotational speed of the test rig, the impingement and impact speed were calculated. As can be seen in Figure 4a. of Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127] most damage, indeed, seems to occur at the leading-edge and, therefore, the 0-degree assumption is valid.

**Comment** Impingement on the blade is affected by blade geometry and needs to account for impact location. Many droplets of different sizes under different wind and rotational velocities don't impact the leading edge according to two independent CFD model studies (OREC and Fraunhofer).

In general, the phenomenon of "many droplets [...] don't impact the leading edge" is expressed by the so-called collection efficiency, as commonly used in the context of aircraft icing. It is defined as the ratio of free-stream water flux to surface water flux and is expressed as

$$\beta = \frac{\partial y_0}{\partial s}, \tag{7}$$

see, e.g., Gent et al. [2000, DOI:10.1098/rsta.2000.0689]. The equation is visualized in the following figure.

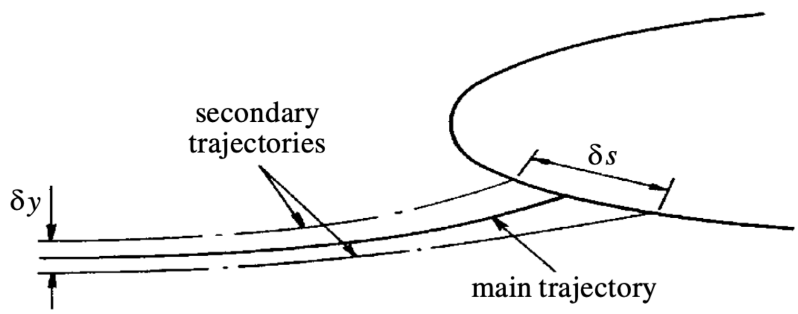


Figure 1: Catch efficiency according to Gent et al. [2000, DOI:10.1098/rsta.2000.0689].

$\beta$ is composed of two effects. (1) The increase of $s$ due to the slanting of the impact surface. (2) The widening of the droplet trajectories due to the stagnation pressure of the flow.

In our analysis, we took (1) into account, as already shown above, by multiplying with $\cos\alpha_\phi$. (2) was indeed not taken into account. It was excluded since it requires the computation of a 2D flow field (or even 3D) for every combination of angle of attack and free-stream velocity. Using these flow fields, Lagrangian trajectory computations need to be carried out for the full sweep of droplet sizes. This is a very costly endeavor and, hence, is out of the scope of this study. However, it constitutes an interesting research problem for future work.

The question remains whether (2) is significant and has the potential to change the conclusions of our study. An answer can be found by looking at the collection efficiency directly at the leading edge. Sor et al. [2021, DOI:10.2514/6.2021-2642], provide computations of collection efficiencies in the relevant speed and droplet size range. Collection efficiencies of 80 to 90 % close to the leading edge are reported, as shown in the following figure.

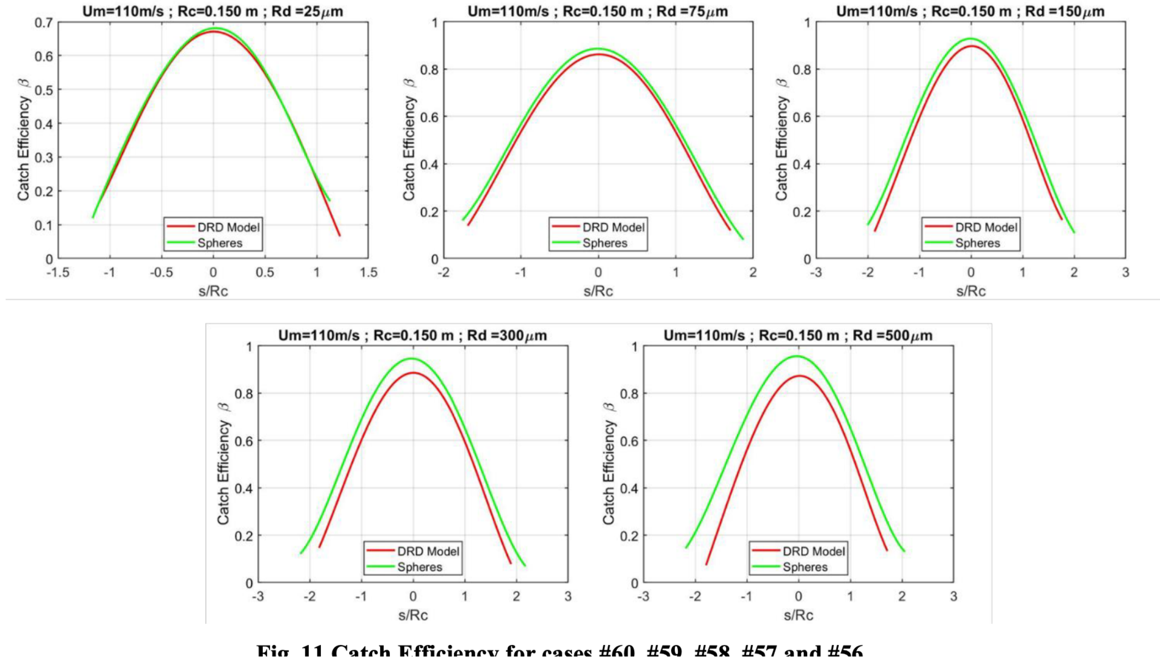


Figure 2: Computed catch efficiency of an airfoil that matches the parameters space of a wind turbine. The figure is from Sor et al. [2021, DOI:10.2514/6.2021-2642].

The collection efficiencies only drop slightly when inspecting the locations slightly above or below the leading edge. Therefore, in the area of interest, high collection efficiencies are observed for all relevant droplet sizes. The collection efficiencies close to the leading edge *should not be interpreted* as: 10 to 20 % of droplets approaching the leading edge miss the blade. Rather, droplets are spread and impact on a larger area $s$ in comparison to the free-stream reference area $y$.

Droplets that do not hit the airfoil because they are *flicked away* can only be observed toward the upper and lower edge of the airfoil. See, e.g., Figure 1 of Sor et al. [2021, DOI:10.2514/6.2021-2642].

The collection efficiency decreases as droplet size decreases. I.e., smaller droplets follow the air flow more easily. This effect is small, but it means that the water surface flux is reduced for smaller droplets. Therefore, the impingement is reduced for smaller droplets and, in turn, the damage attributed to them reduces. This is in line with the other effects described in this study. Hence, neglecting (2) is a conservative assumption. If included, it would make the conclusions of this paper even stronger.

**Comment** 90 & 95 Averaged law and drop-size dependent law – erosion performance is heavily related to material properties so will be unique for each LEP and the test parameters. This should be made clear in the paper that you are only considering one case. We expect and have observed that some LEPs will be drop-size independent whilst others are dependent – as observed by Verma and Amirzadeh. The droplet size damage model is based on the response of a limited set of materials and discludes the individual stress strain response of those LEPs.

Correct, the damage laws are indeed only applicable for one set of materials. We added this as a disclaimer in the text. It should be noted that according to Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127], the tested material is a commercial PU coating of a large OEM. Hence, the conclusions drawn from this study should provide at least some generality with respect to the current state-of-the-art.

**Comment** Vimpact – LEP materials can be tuned to erode differently at different velocities although in general Vimpact is the key driver.

Thank you for pointing this out.

**Comment** 220 according to CFD the effect of Vimpact leading to more impingement is correct but it isn't linear as it changes impact location which can then be away from the LE. Please remove the statement that it is linear.

We added a disclaimer that, with the assumptions made in this paper, it is linear.

**Comment** 335 the normalized damage model only works for purely elastic LEPs

Thank you for pointing this out. We added a disclaimer. It might be interesting to note here that, e.g., Hoksbergen et al. [2023, DOI: 10.1016/j.renene.2023.119328] writes: "[... at high strain rates, polyurethane and polyurea materials behave in a glassy way with more linear elastic behavior and less viscoelasticity.]" and "It is expected that most materials considered for LEP coatings behave glassy in the relevant strain rate regime and that the linear elastic model is sufficient for prediction of the elastic stress field.". We are aware that the role of viscoelastic effects in leading-edge erosion is studied as well.

## Reviewer 2

The paper investigates the effects of drop-size on leading edge erosion by developing a simplified model for computing the damage per rotor blade section. It heavily relies on assumptions from another paper that is still under review.

Overall the approach is interesting and research questions are of great interest, however the execution needs improvement.

**Comment** The paper is written as if a general answer is provided to the research questions posed, instead it is a case study of a particular turbine in a particular location. This should be made clear throughout the paper. The authors would need to model several turbines and locations if they would like to arrive at some more general conclusions, this would also raise the significance/impact of the paper.

See the answer to your more detailed comment of L.62.

**Comment** The model assumptions need to be discussed and their sensitivity needs to be assessed. A sensitivity analyses needs to be performed.

In our study, we have conducted a detailed sensitivity analysis focusing on the drop-size effects, which are a critical component of our model. These analyses are presented in Sections 2.2.1 to 2.2.4 and Section 3, where we independently identified and quantified the relevance of various drop-size effects on the erosion damage model, erosion lifetime, and the erosion-safe mode.

Our model assumptions were explicitly stated and chosen to focus on the primary factors influencing erosion without overcomplicating the model. Given that we have already addressed the key sensitivities related to drop-size effects, we believe that additional sensitivity analyses on all model assumptions would provide limited additional value. The current sensitivity analyses have already demonstrated significant impacts and effectively guided our conclusions.

**Comment** Why is all damage assumed to happen around the LE? From an aerodynamic point of view this is not realistic.

See the answer to Comment 3 to Reviewer 1 for background information. The collection efficiency of rain droplets is usually slightly higher just off the leading edge in the direction of the stagnation point. This is, however, a fairly minor effect. Therefore, for simplicity, in erosion research, damage prediction is usually performed on the leading edge itself. The erosion framework that we described provides the flexibility to also interrogate other locations by choosing another $\vec{n}_{\mathrm{LE}}$. Choosing another location close to the leading edge will neither meaningfully change the results nor the conclusions.

**Comment** Generally the entire derivation seems convoluted and excessively detailed, whilst important limitations are left out.

We improved the structure of the description of the damage model. See also answer to Comment L.108.

- 3.1.1 We present the formula used to calculate the damage (Palmgren-Miner damage rule). The formula contains a term in the nominator and a term in the denominator.
- 3.1.2 We describe the term in the denominator (allowed impingement).
- 3.1.4 We describe the term in the nominator (collected impingement).
- 3.1.3 We describe the calculation of the impact velocity/collection velocity, which is contained in the denominator and nominator.

The damage model is described such that the results can be fully replicated. Some of the derivations are placed in the appendix.

**Comment** By using such a simplified model would it not make sense to reduce its complexity even further? After all some big assumptions are underlying its derivation so one could drop details without impacting its accuracy.

We do not believe that the model represents *such a simplified model*. It has the necessary detail and complexity to answer the research questions of this study. To highlight this point in more detail, we will look at different aspects of the model:

- The probabilistic framework: The model is built on a probabilistic framework. Four probability density functions (PDF) are used to describe the rotational position, the droplet size, the wind speed and the rain intensity. All four PDFs are required to describe the various drop-size effects. Similar probabilistic frameworks have been used in other well-known research, such as Verma et al. [2021, DOI: 10.1002/we.2634]. Using an approach based on discrete data-series is possible but not desirable due to its, in comparison, high computational cost.
- The damage metric: As the metric, we use impingement. It is possible to also use other metrics such as the droplet's kinetic energy, see Bech et al. [2018, DOI: 10.5194/wes-3-729-2018]. Recently, the impingement metric has gained a lot of traction, especially in research stemming from DTU. We, therefore, believe that impingement is a state-of-the-art damage metric that should not be seen as an over-simplification.
- The accumulated impingement calculation: The methodology to calculate the accumulated impingement is, to our knowledge, the most elaborate that has been described in the literature so far. In contrast to other research, we are considering induction factors and the novel slowdown effect. Additionally, we model the droplet impact angle with the blade's leading edge by considering the airfoil's LE normal vector, the angle of attack, and twist and pitch angles. The pitch angle varies with the wind speed according to IEA 15MW's reference control. The control is described in the turbine's reference report. To conclude, here we are cutting-edge. Recent literature that uses a similar but less-detailed modeling approach in impingement than ours can be found in Bech et al. [2022, DOI: 10.1016/j.renene.2022.06.127], Visbech et al. [2023, DOI: 10.5194/wes-8-173-2023], Lopez et al. [2023, DOI: 10.1016/j.renene.2022.12.050].
- The damage law: We use a power law to describe the erosion resistance of the material. This is well within the current best practices. For example, the Springer model follows a power law, too. The erosion-test-rig measurements of Bech et al. [2022, DOI: 10.1016/ j.renene.2022.06.127] that were fitted to a power law are, in our opinion, the best measurements that are currently available in the public domain.

To conclude, the model represents a state-of-the-art damage model as it is used in other current research. With respect to the drop-size effects, it is so far the most elaborate model that has been published in the literature. It does not directly model the droplet-substrate impact using a multi-physics solver but instead uses very good erosion test measurements.

The model is described by Equations 1 to 29. In our opinion, one could potentially simplify the model by dropping the induction factors as described in Equations 12 and 15. However, we do not see how this would improve the quality of our work.

**Comment** Why is the model not verified with some high-fidelity simulations? This should be added.

This is clearly out of this work's scope. The work of performing high-fidelity simulations of certain aspects of our model would constitute an independent paper. We also believe that in our study, we do not do anything peculiar that would warrant verifying and validating our results with a high-fidelity simulation so that our results become publishable. The model to describe the slowdown effect was validated at length in Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169].

**Comment** The authors should consider splitting the paper, as currently it really is made-up of two parts, a model and discussion of drop size and the ESM. Each one could stand on its own, however currently the ESM section is extremely short and hard to follow. If the authors add to this section, which is necessary for the reader to follow the author's arguments, the paper will get even longer, especially if they address the comments made regarding the first section.

There is ample supporting material regarding the ESM in the appendix.

**Comment** More detailed comments are given in the attached pdf using the comment feature of a pdf viewer. The authors can directly answer the comments in the pdf or address them in a sperate [sic] document.

The answers are given below with the line numbers corresponding to the original preprint.

**Comment** L.20: Try to be a little more specific here, not every turbine with erosion needs to be repaired more frequently it depends on how quickly the erosion grows.

We agree, we changed the sentence to make it more nuanced.

**Comment** L.22: add reference

Added a reference that treats this point.

**Comment** L.23: mention range to give a realistic idea, it is below 5%

We agree that the realistic AEP loss associated with erosion is somewhere in the low single-digit percentage range. However, depending on the study the reported values differ and the exact quantification is still an open research question. We, therefore, think that mentioning a specific threshold, i.e., below 5% is not appropriate here. Since we gave four references the reader should have enough literature to form his own conclusions.

**Comment** L.52: This is a little vague. Try to be more specific. It is already well known that the ESM does depend on droplet size, it is not really clear what "... droplet-size-related effects ..." means without having read the paper.

We agree that we should be more precise here. We have given a definition of the the term *drop-size-related effects* after the research questions. This should hopefully give enough clarification about what we want to explore within our study.

**Comment** L.62: The research questions above are very general, so choosing a single site and turbine will not be sufficient to answer those questions. The paper is more of a case study, which should be made clear in the abstract, introduction and conclusion. The results cannot simply be extrapolated to make some general claims.

We think this matter is a bit more nuanced. For this study, a typical (future off-shore) turbine, a typical (coastal) site, and a typical commercial leading-edge material were chosen. So, we believe that we have chosen a parameter space that is fairly representative.

We believe that our answers to the research questions are also fairly robust with respect to the choice of our parameter space. We will look at the answers we have given in the conclusions.

- 1.1) *Four drop-size effects were identified [...]*: The effects of Sections 2.2.1, 2.2.2 and 2.2.4 are aerodynamic effects that occur on every turbine. The magnitude of the slowdown effect is dependent on the chord and thickness of the blade (section). With the trend to larger and larger turbines, this effect becomes more relevant. The relevance of the slowdown effect was also previously established in Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169] for the NREL 5MW turbine. The effect of 2.2.3 is indeed dependent on the material. Here, a different material could, indeed, lead to a different behavior.
- 1.2) *The higher erosivity of large droplets can be attributed to their higher impact-velocity.*: This stays true as long as the slowdown effect is sufficiently large.
- 1.3) *The parameter space of leading-edge erosion is affected by drop-size effects.*: While the specific numbers might depend on the turbine and site, the parameter space is still affected.
- 2.1) *Drop-size effects push the damage production to higher rain-intensities.*: Again, the numbers might change, but the effect is still there.
- 2.2) *The VI-ESM strategy is highly sensitive to drop-size effects.*: This conclusion still holds.

Nevertheless, we decided to add a disclaimer in our manuscript to highlight that we answered our research questions with a typical selection of a turbine, site and material and that the reader should be aware of it.

**Comment** L.84: The description and argument for using this law is insufficient. There are other approaches that also include material properties. Your paper from 2023 is still under review and the reader does not necessarily know what the Bech measurements are, for what material, under which conditions etc?

Is this assumption not too simplistic, should not depend on the material, if so what material are these constants valid for?

We, indeed, acknowledge that Section 2.1.2 provides too little background information. We have, therefore, expanded this section.

**Comment** L.95: Is this a fully general law? Please discuss.

We added more clarification about this in the text. See comment above.

**Comment** L.100: Why not use SI units? Move comments to footnotes.

In the original reference, the coefficients of $a$, $b$, and $\phi_0$ were determined such that $\phi$ must be substituted in millimeters. It is, of course, possible to adjust these coefficients so that the formulas become consistent with SI units. However, this would also require an explanation in the text so that the reader does not get confused about the apparent inconsistency in the numerical value of the coefficients among references. We, therefore, think that mentioning the original coefficients is more appropriate. The comment alerts the reader about this fact should he decide to implement the methodology described in this study. Moving the comment into the footnote merely moves it out of the reader's sight and does not contribute to the goal of alerting the reader.

**Comment** L.106: Hard to follow this argument when the derivation has not been done yet. Consider how to adapt this

We changed the structure of the derivation of the damage model. See answer to Comment 4 of Reviewer 2.

**Comment** L.108: not sure you need to list them like this. It would be more useful to embed them in the text and discuss each of them with regard to the study you are presenting

See answer to Comment 2.

**Comment** L.109: what would be "imperfect advection" ? A simpler statement would be, "acting as passive tracer, ie advection by local wind vector", which includes the induction, or similar

Changed to: Rain droplets are advected with the local wind vector, which is comprised of the wind speed and the wind turbine's induction factors.

**Comment** L.112: A big claim.... Just say "rigid blade" or equivalent

Changed to: The blade is rigid.

**Comment** L.114: ? The tangent in 3D is ill defined. What do you mean?

After some considerations we decided to remove this bullet point.

**Comment** L.122: remove, the drawing should be sufficient.

We removed the sentence.

**Comment** L.122: It would be good to include the vectors in some form in the figures. Where is Vres, Vrain etc as it is a little hard to follow.

We added Vrel to the figure and provided a definition of Vrain.

**Comment** L.122: So Vslowdown is acting along the ballistic trajectory, but why do we need to project it by the LE normal vector? The underlying assumption is that all rain impacts at the LE? This is a big simplification. Why is this done? The impact is around the stagnation point which depends on the AoA at which the aerofoil is operating at, which depends on the turbine pitch operation/control design.

We need the normal component of $V_{\text{slowdown}}$ because $V_{\text{impact}}$ is defined as:

$$V_{\text{impact}} = (\vec{V}_{\text{sec}} - \vec{V}_{\text{rain}}) \cdot \vec{n}_{\text{LE}}, \tag{8}$$

The assumption is made that any differential velocity between the blade and the rain tangent to the leading edge does not cause damage. Additionally, in the appendix, it is shown that the accumulated impingement merely depends on the normal component.

We do not assume that all rain impacts at the leading-edge. We merely perform the analysis for the leading edge. That is, we calculate the impingement and damage that is accumulated by the leading-edge. See also the next comment.

**Comment** L.122: Also why use a single Vslowdown, not all rain drops will experience this only the ones exactly hitting the stagnation point. A little next to the stagnation point and there is almost no slowdown left.

$V_{\text{slowdown}}$ is a result of solving

$$m\frac{d^2x}{dt^2} = F_{\text{drag}}, \tag{9}$$

$$\frac{3}{16}m\frac{d^2a}{dt^2} = F_\sigma + F_p. \tag{10}$$

Hence, the slowdown is a function of many parameters. Most importantly, the droplet size $\phi$, the vector $V_{res}$, and the background velocity field $V_{air}$. The following plot shows $V_{air}$ for a ballistic trajectory of an FFA-W3-211 airfoil at a 5-degree angle of attack. One line shows $V_{air}$ for the leading edge, whereas the other one shows it for the stagnation point. The figure was obtained using data generated by Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169]. 2D-RANS results were loaded into Paraview. The air velocity was extracted on a ballistic trajectory originating from the leading edge and the stagnation point.

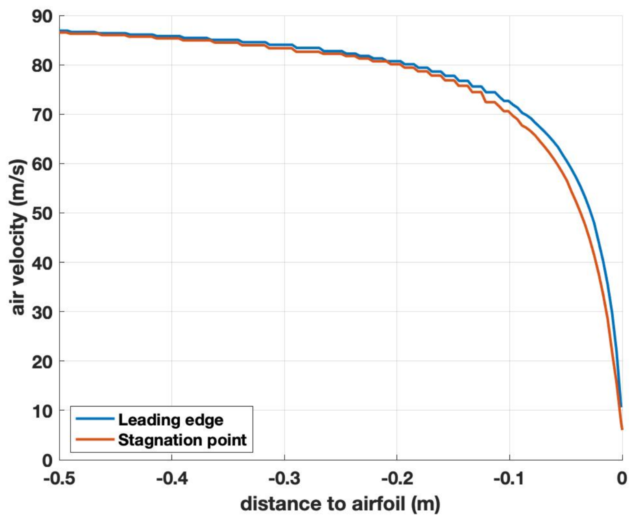


Figure 3: $V_{air}$ as a function of distance to the leading edge

It can be seen that $V_{air}$ is slightly lower for the trajectory to the stagnation point. However, this has only a minor effect on the slowdown computation. Hence, the slowdown effect is also significant for the leading edge. See also the experimental results referred to in Comment L.287.

**Comment** L.134: so the height above ground? Rain always has the same speed when hitting the ground?

Added clarification that $h$ is the height above ground. Under ideal conditions (stationary air, etc.), the terminal falling velocity of a droplet is a function of its diameter and the air density. The latter is affected by $h$.

**Comment** L.134: footnote or SI units

Please see the comment of L.100.

**Comment** L.138: This is computed for which height?

Good catch. This was computed for $h = 0$ km. So, at ground level. We added clarification to the figure's caption.

**Comment** L.148: how well does this fit the real behaviour of droplet deformation that is highly non-linear and 3D?

This question is answered in great detail by Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169]. The Lagrangian approach presented here can, of course, not model the intricacies of the various break-up modes that a droplet encounters depending on its Weber number. However, in Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169] it is shown that the approach is sufficient to accurately determine the impact speed of the deforming droplets.

**Comment** L.153: why not use a 2D flowfield instead? Is this relation valid for lifting bodies, ie when there is circulation or only when there is not lift (symmetric aerofoil at AoA=0).

We would like to refer to Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169]. The approach is valid for lifting bodies and non-lifting bodies. It is also valid for a range of different angles of attack, including 0-degree. For the latter, $R_c$ can be modified depending on the angle of attack. In Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169], it was shown that the angle of attack correction is fairly small for a ballistic trajectory. Hence, it was neglected for this work. It is important to note that with respect to drop-size effects, a ballistic trajectory is a conservative assumption, i.e., it underpredicts the influence of drop-size effects.

Performing Lagrangian simulations in a 2D flowfield is costly, presently too costly for a damage model that is based on four independent parameters. Much work was done by Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169] to develop a formulation that reduces the problem from a 2D to a simpler 1D problem while maintaining accuracy.

**Comment** L.167: Nice and simple formulation

Thank you.

**Comment** L.171: footnote, why do you not normalise?

Most results in this study are normalized. See, e.g., Table 1 or Figure 16. For example, in Figure 16, the lifetime of a blade operating under an ESM is normalized with the lifetime of a blade that does not utilize the ESM.

**Comment** L.201: This has a huge impact, why is it "deemed acceptable" ? Also how do you then include the effect of blade pitching ?

Assuming that $f_I$ and $f_{\text{wind}}$ are not statistically correlated does not have an effect on the calculation of the blade pitch. The method that is used to calculate the blade pitch is given in Appendix C.

You are right, it would, indeed, be better to connect $f_I$ and $f_{\text{wind}}$. For example, by using a copula. Colleagues at DTU disclosed that they are currently working on such an approach. We feel that we should not *poach in their territory* prior to their publication. Assuming that $f_I$ and $f_{\text{wind}}$ are not correlated is definitely within the state-of-the-art in probabilistic erosion frameworks, see e.g., Verma et al. [2021, DOI: 10.1002/we.2634].

In our follow-up publication for Torque 2024:

> N Barfknecht, R Imhoff and D von Terzi. Mitigating blade erosion damage through nowcast-driven erosion-safe mode control. Journal of Physics: Conference Series, 2767 032001, June 2024.

We use actual time series data to drive the ESM. These discrete data include the connection between rain intensity and wind speed. It was found that the ESM performs comparably.

**Comment** L.206: unnecessary information

We removed the sentence.

**Comment** L.208: Why are you not including the effect of small droplets deflecting? This certainly happens and is usually included.

See the answer to third comment of Reviewer 1.

**Comment** L.218: Is this a formatting issue? There seem to be a lot of ! in your paper.

There are nine exclamation marks in this work. We use exclamation marks to highlight points that, in our opinion, should receive special attention from the reader. Italics can be used to highlight specific words. Underlining entire sentences seems, from a stylistic perspective, questionable. Hence the exclamation marks. We will consult with the editor whether this is allowed within the house style of WES.

**Comment** L.220: One could derive it here instead. by substituting.

Equation 31 is the result of combining Equations 3, 23 and 30. Since Equations 3 and 23 are both very short, coming to the conclusion that $\partial D \propto V_{\text{impact}}^{\beta+1}$ is trivial.

**Comment** L.223: So estimating beta correctly is detrimental

We unfortunately do not understand this comment.

**Comment** L.224: Could this be generalized more by normalising appropriately?

We believe that we normalize the results appropriately. We are, of course, open to suggestions.

**Comment** L.240: By taking the LE as the point of erosion, I am not sure this is really a needed discussion.

We believe the reviewer thinks that we assume that droplets always impact perpendicular to the tangent of the leading edge (assuming a 2D airfoil). However, this is not the case, as shown in Figure 2a. The droplet's impact angle $\alpha_\phi$ varies depending on its size. This aspect is investigated in this section.

**Comment** L.258: A lot depends on the work by Bech so you need to describe it and also discuss its impact on your results. Are these results for an aerofoil, does it include the effect of deflection?

With the previous changes in the text and a more elaborate explanation of the Bech measurements, this should be clearer.

**Comment** L.280: Was this not already discussed?

We reorganized this section and moved the (duplicate) content to Section 2.1.4.

**Comment** L.285: This was already mentioned

See previous answer.

**Comment** L.287: Is this for an aerofoil at an angle of attack?

The published images of Sor et al. [2019, DOI: 10.2514/6.2019-3307] were taken in an area above the leading edge. These measurements were obtained in a rotating-arm rig. The angle of attack is zero. The behavior is expected to be similar for other angles of attack. The break-up mode of the droplets is governed by the (temporal evolution of the) Weber number. The main driver for the Weber number is the tip-speed of the turbine. The angle of attack influences the Weber number too, however, to a much smaller degree than the tip-speed, see Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169].

About the area: The same research group has also published high-speed images directly in front of the leading edge with identical droplet behavior. In Barfknecht and von Terzi [2023, DOI: 10.5194/wes-2023-169], a collection of these images is reproduced.

**Comment** L.300: slowdown effects and size dependent damage more specifically or what is meant by that term?

The four effects described in Sections 2.2.1, 2.2.2, 2.2.3, 2.2.4. We have added an explicit definition in Line 53.

**Comment** L.312: Another "!"

See answer before.

**Comment** L.319: a strong statement, for me drop-size effects would also include the deflection of smaller droplet for example or where they actually impact the aerofoil

The question of the deflection and resulting impact location has been discussed earlier in this rebuttal. It might be worth recalling that the collection efficiency increases for larger droplets. I.e., the ratio of free-stream to surface water flux approaches unity as the droplet diameter increases. This increases the erosivity of larger droplets in comparison to their smaller peers. Hence, modeling merely part of the collection efficiency (see previous comments) is a conservative assumption regarding the conclusion of this work.

**Comment** L.365: This is not a novel conclusion, one could discuss how this study differs from existing findings. Is there something new or surprising, what is the most important contribution which would allow simplifying your model?

We believe that this is a novel conclusion. Since the reviewer believes otherwise, we would like to ask him to point us to the relevant literature that supports his claim.

**Comment** L.372: This is not surprising.

We believe that the sentence helps in discussing the figures and supports the points we are making in the paper.

**Comment** L.376: It is fine to give details of the ESM implementation but some details should be given. How was the turbine modeled, its control etc. This is very important in this context.

In line 383, an explicit reference to Appendix B is made that describes, at length, the ESM implementation used in this work. The calculation of the AEP and pitch angle is described in Appendix C. References to Appendix C can be found in lines 61, 161 and 745. We took great care in ensuring that the methodology of Chapter 2 and Appendix B/C were described such that a complete replication of our results is possible.

**Comment** L.407: setting?

We changed the wording.

**Comment** L.415: It is hard to follow this discussion

As indicated in the first paragraph of this section, the appendix provides the necessary supporting material for the definition of the ESM. We have revised parts of this section to improve the readability.

**Comment** L.430: Was some of this evaluated using an aeroelastic solver?

No, as mentioned in the assumptions, aeroelasticity was not considered. The entire method to determine the ESMs is discussed in Appendix B.

We also reworked Appendix A2, so that $V_{\mathrm{slowdown}}$ is excluded from $V_{\mathrm{collection}}$.