Dear reviewers and editor,

Thank you again for taking the time to review our work and provide detailed comments to reinforce our document disseminating our findings. Please see below specific answers to the reviewer's comments in blue. Our team really appreciate your efforts.

Sincerely,

Prof. Palacios

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The paper describes the design process of an electro-thermal de-icing system for HAWTs. The procedure is based on experiments of ice accretion and ice shedding due to centrifugal force on a rotating blade section. Once data is obtained at different rotational speeds, corresponding to different radial positions, a control sequence is proposed for de-icing a full blade in different icing conditions.

The design procedure proposed is interesting, and the topic is of sure interest to the reader of Wind Energy Science.

The English language is not fully satisfactory. Although its usage is globally correct, some typos and grammatical errors should be corrected (e.g., in ll. 19-20, 39, 45, 55, 57, 68, 122, 153, 159, 185, etc.). Providing some examples from Section I, I advise as well:

1. to check for consistency in the flow of information for easier reading (e.g.: ll. 55-65 regard wind energy in general; ll. 68-73 regard issues related with icing on wind turbines; ll. 56-58 regard wind energy in cold regions, which might be moved right before l. 68);

Thank you for your recommendations and for pointing out the typos in the paper. We have read over the document and we have addressed mentioned typos.

2. to avoid wordy repetitions (e.g., in ll. 82-87, the subject «de-icing» is repeated in every sentence);

We believe that making clear the type of system being discussed (de-icing vs anti-icing) is critical to avoid confusion, even if it requires labeling the system in every sentence. We have reduced the number of mentions while hopefully avoiding confusion.

3. to omit unnecessary information, or better contextualize it (e.g., ll. 91-96, which include issues (1) and (2) as well).

The abstract should be rewritten to be more generic and include fewer details: it is difficult to understand all the information provided before reading the whole paper.

Information related to the specific icing conditions tested was removed from the abstract.

An overview of the structure of the paper and its contents is missing. I suggest including it at the end of Section I to facilitate reading.

The objectives section was re-written to enumerate the tasks which are followed in structure of the proposed paper and that describes the design process

I have one major concern about the methodology applied.

In particular, it is not clear why the cross-sectional area of accreted ice on the scaled model should match the one of the full wind turbine, as per equations (1) and (2) (ll. 171-172). This assumption, which was not justified and cannot be taken for granted a priori, is critical within the design process since it assigns a centrifugal force that may not be linked to the full-scale model. In fact, the ice accretion rate depends on the shape and the size of the object as well [1, 2, 3].

Moreover, the experiments are carried out on a «1/2 scale model of the 80% span region of a generic 1.5 MW wind turbine blade». It is stated that «representative ice shapes were first accreted as per ice scaling laws provided by Bond et al., 2004» (ll. 156-157); it is not clear if (and which) scaling law is applied, and if this is related to the assumption above. Later, it is stated that the impact velocity only was matched during ice accretion experiments (ll. 232-234). The authors should clarify the approach chosen.

Since the design process is intended for a full-scale model, I invite the authors to discuss the

legitimacy or the consequences of their assumptions, either numerically or by providing results from existing literature.
A "Testing Methodology" section was added to the document to clarify the need for representative types of ice and accretion areas. It is critical to match the type of ice accreted on the ½ scale airfoil to that of a full-scale, since the cohesion strength of ice is dependent on the type of ice. Cohesion between heated zones must be overcome during the shedding process.

My second concern regards unclear, confusing, or contradictory information provided within the paper. In particular:
1. Linked to what already written above, confusing information is provided regarding the influence of different parameters on ice accretion. In ll. 74-75, authors should mention that the shape and size of the object affect the ice accretion rate on the object itself [1, 2, 3]. In ll. 79-81, it is implicitly stated that impact velocity does not affect the thickness, which is certainly wrong and contradicts the results of the experiments. More attention should be paid while defining the effect of the different parameters on ice accretion.
Impact velocity does affect the ice accretion rate. It has been made clear that in the effort the ice accretion rate recorded is used in the heater controller design procedure, but ultimately, the ice accretion rate must be measured real time in the field by ice thickness sensors.

2. It is not clear if experiments for blade span r/R = 0.8 were run. Line 239 states that «80% span was not tested in the AERTS facility due to the potential rotor imbalance on the test stand at such RPM» (Table 1 still reports «3, 5 and 7» mins of accretion time for that blade span); accordingly, results for that blade span are obtained through extrapolation (please note that the order of results reported in Table 2 may be wrong). In the following Section (V.B), it seems that experiments at r/R = 0.8 will be performed; however, results in Figure 20 do not include r/R = 0.8, so it is not clear how results in Figure 23 are obtained for that blade span. Once more, in Section V.C, r/R = 0.8 is not tested and, accordingly, results are not presented. Was the case of r/R = 0.8 ever tested?
80% span was not tested due to rotor imbalance concerns upon shedding. It has been made clear on the text that tests did not include 80% span testing conditions.

Minor concerns regard occasional scarce attention to details. Some of them are listed below.
- Data described in line 62 does not match what is shown in Figure 2. See modification on text
- «currently», in line 63, refers to 2011 data. In 2020, it produced 8.42% of electricity in the US [4].
Thank you very much for the additional information. We have added it to the text.
- Units of measure should be written consistently: e.g., 10_GW (line 62), g/m^3 (ll. 226-227).
- Some acronyms have never been introduced (IRT: Icing Research Tunnel; FAR: Federal Aviation Regulations). The acronyms have been fully written.
- In Figure 11, some lines are overlapping text boxes. Fixed
- Resolution of Figure 30 should be improved. Redone
- I suggest changing the title of Section VII to "Conclusions and Future Work".
- The author of NASA/CR—2004-212875 is Anderson only. Correct.
- The Bibliography should be ordered alphabetically. Done

Given the comments above, I suggest that the paper undergoes a major revision, before being reconsidered for publication on Wind Energy Science.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)
Dear Authors,
thank you for addressing most concerns on the submitted paper, which can now be accepted for publication. Congratulations!

I only have a short suggestion: please comment that Appendix C conditions are taken from FAA regulations and their applicability to wind turbine icing is not straightforward. I expect that in-flight icing conditions differ from ground level ones, where I am expecting operating parameters to be different as well as a strong influence of the terrestrial boundary layer.

Thank you. We have written on the document that the types of ice selected are representative of aircraft icing and not wind turbines, since a comprehensive icing envelope for wind turbines are not available to our knowledge. Ultimately it is not the goal of the paper to match ice shapes for a given wind turbine but showcase the proposed design process for a wind turbine ice protection system, with emphasis on the need to overcome cohesive forces between adjacent heater zones.