Comment on “Future Rime Ice Conditions for Energy Infrastructure over Fennoscandia Resolved with a High-Resolution Regional Climate Model” from Oskari Rockas, Pia Isolähteenmäki, Marko Laine, Anders V. Lindfors, Karoliina Hämäläinen and Anton Laakso

General Comment

With this paper, the authors present an important contribution to the highly relevant question on how atmospheric icing can change regionally in the future under the conditions of climate change. So far, there are still only a few international studies on this problem. The paper shows how high-resolution regional results for Fennoscandia were obtained by the use of an ice accretion model that utilizes the outputs from the high-resolution regional climate model HCLIM driven by results of two global climate models for the RCP 8.5 emission scenario.

The results for rime ice are presented and discussed for two time periods in the future. They show “a general decrease for in-cloud icing conditions” over Fennoscandia for two time periods in the future, compared to the historical period with exceptions in the northern parts of Fennoscandia and locally over higher altitudes, where increasing trends are found. Authors suggest that “the main driver for the decrease of in-cloud icing conditions over Fennoscandia is the warming trend in temperatures. Although the warmer atmosphere allows for a higher moisture content, icing does not occur when temperatures are above zero degrees Celsius”, i.e. for most regions of Fennoscandia. They explain exceptions by the “…increasing trend [of in cloud icing] over some of the northern regions in mid-century could be explained by the increase of LWC over regions where freezing temperatures remain, but, on the other hand, the temperatures are not too cold, allowing water to stay in liquid form.” This interpretation is consistent with and supports the findings of other authors.

If one evaluates these results, it is clearly evident that more research is needed to analyze future ice accretion conditions in order to provide society and infrastructure operators with robust data on ice accretion on structures in the future. The paper is in accordance with the scope of the journal.

The main concerns relate to certain inconsistencies between the title, abstract and conclusion of the paper and its content, which also reports regional results for 7 area and for specific application fields (power lines and wind turbines). Title says “Future Rime Ice Conditions for Energy Infrastructure over Fennoscandia …”, the abstract states “Thus, since climate change is expected to impact winter weather conditions in northern Europe, its effects on atmospheric icing occurrence over the Fennoscandian region …” and the conclusions summarize “The objective of this study was to assess the simulated changes in rime ice formation during in-cloud icing episodes across the Fennoscandian region …” The only “hint” to more specific results is made in the title by “for Energy Infrastructure”. The first time that specific evaluations are mentioned is in “2.3 Processing of the ice model outputs” with “The icing model output was further processed to allow a comprehensive evaluation of the changes in icing climate from multiple perspectives. …”

The paper must be revised to consider, whether it needs to take two regional (region-specific) perspectives into account. If so, it needs to be addressed (in more detail) at least in the abstract, in the introduction (Was there any project related information and/or information from power lines or wind parks?) and in the conclusions.

Furthermore, the paper has to be (significantly) improved in two important aspects.

I. There is no plausibility check (validation is not even required because the availability of icing data is sparse) of the results of the icing model or (almost all) other parameters used in, not even with references to other papers. The absolute values for ice masses in Table 2 and (at least) tables A.1 and A.2 as well as the changes in absolute ice masses in Figure A1 seem to be to small compared to results from other studies (for instance Iversen et al., 2023; even when taking in mind the different measures for extreme values) if one takes in mind that the results are for heights >= 50 m above terrain.

II. There is no information on whether or not a climate change signal is detected. As one can expect a significant natural climate variability even for icing data, it needs to be checked (tested), if the changes in the results of modelled icing data are significant, at least due to the very small absolute values and absolute changes in ice masses (see Figures 4 and A1 for instance).

Keeping the comments and requirements mentioned before into consideration, the paper needs to be re-structured (at least amended). The main topics are mostly clear and understandable. The methods need to be improved in order to get plausible results and reliable conclusions.

Specific comments

Structure of comments: Line: “Citation (if necessary)”, Comment

1) 33: “… rime ice … loads …” are used by reference to Iversen et al. (2023). This is right.
   55: “ … mean and annual maximum ice loads …”. This needs to be checked for the whole paper as the term is used alternately with “ice mass”. (In order to be precise, “ice load“ should refer to a force with unit [N/m], or similar whereas “ice mass” refers to a (unit length related) mass with [kg/m]. Please check the units throughout the paper, too, because sometimes only [kg] is used for (unit length related) mass.
2) 61: Most of the statements regarding the rime ice model refer to the paper by Hämäläinen and Niemelä (2017). One of the Conclusions in this paper is “The model is not able to forecast the accumulated ice mass precisely. However, in an icing atlas type of product, the frequency of ON–OFF behaviour is a more critical parameter than is absolute ice mass.” The “Results of the observation comparison” state “The purpose of this case study was to show that the model simulates icing events in a realistic manner. The ice mass can grow and melt by responding to atmospheric forcing correctly. On the other hand, the absolute values are not precise. However, the main goal here is to estimate the length of the icing periods, and therefore, absolute ice mass values have less importance.” Please show, how the model has been improved in order to produce more realistic results for ice mass or discuss the topic with respect to the results of this paper later on (this is a key point related to I.). Are ther new results from Luosto? Are there data that can be used from wind parks and/or power lines in areas 1-7?
3) 78: “various parameters related to icing are calculated; most notably the ice mass (gm^{-1}), but also the ice density (gm^{-3}), the total diameter of the cylinder and ice”
   62: “Rime ice accretion is modeled over a vertical (length = 1 m), freely rotating standard cylinder with icing rate (gs^{-1})”
   The diameter of the “standard cylinder” is not specified. Does “standard cylinder” mean “reference collector” as defined in ISO 12494 (2017), i.e. a diameter of 0.03 m?
4) 79: “Makkonen Lasse, 1984 … and Stallbrass J.R”, unify the form of references here and in the references section. Delete first names or initials here (and in references) or add it elsewhere.
5) 115: “Figure 1 displays the specific areas referenced in Table 1” There is a lack of key information regarding the specified areas: Total area of each, number of grid points, relief

information (mean, maximum and minimum altitude, for instance). These are supplementary information for understanding the results of the ice accretion.

6) 118: “a maximum of all grid points was calculated for each time step” Do you mean “the maximum value from all grid points of the area was determined for each time step”?
7) 120: “For areas 1–5, extreme values of ice mass were calculated by taking the 99th quantile of the 20-year periods” What values of ice mass were used how? All ice mass values from all grid points of the area from each time step for the whole period? In Table 2 and A1-to A4 you refer to “IM (Max Avg Min) is for the regional maximum, average and minimum of the 99th quantile of ice mass” That means, you determine the “99th quantile of ice mass for all grid points of the area from each time step for the whole period and determine from that maximum, average and minimum values for the area”? Please clarify.
8) 123: “The icing hours and episode durations were calculated from the average of the grid points in the area.” Does it mean “The annual icing hours were calculated from the average of all grid points values in the area, where the respective grid point values fulfill specific icing conditions (defined thereafter) during a year are counted (and adjusted due to the time resolution of 3 hours?) to the annual total sum of icing hours” How do you calculate the annual episode duration of icing? Please specify in more detail.
9) 138: “Changes in ice load (mean and annual maximum)” means “Changes in ice load (annual mean and annual maximum per grid point”?)
10) 148 and the whole description thereafter: “The relative trend is pronounced, with some regions experiencing close to a 100 % decrease, indicating conditions where icing would no longer occur. However, the mean loads in these regions are relatively low to begin with, resulting in a small absolute change”. See II.
11) 180 and thereafter: “Both the annual mean and the annual maximum are calculated for the regional maximum of the grid points.” See 5) That means, that each box in the plots in Fig. 4 for each time period consist of 20 values (annual mean or annual max for each of the 20 years of the time period)? With reference to Fig. 4 only the trends of median values are discussed (even though the median is mentioned only one time). What about the variability of results, represented by boxes and whiskers. The variability seems to change for different time periods (in different ways). Do we really see “evident” trends for the annual means and a less clear “signal” for annual maxima? See II.
12) Figure 4: “Ice load [kg]” is twice over, see 1).
13) 206: “note that the model output was calculated for a cylinder of diameter of 3 cm” Finally found the diameter of the cylinder! Needs to be defined in 2.1 already, see 3).
14) 245: “(International Organization for Standardization, 2017)”, unify the form of references here and in the references section. “ISO 12494” is used before
15) 260: “Some areas in … southernmost Sweden, … may experience close to zero months of freezing conditions at the end-of-century”. Does it coincide with results for icing?
16) 263 and thereafter (chapter 3.2.2): Whereas in the chapter for temperature before, the discussion for LWC is more a general one (regarding changes) and does not point to icing conditions. This is done later on but could be here already (as for temperature before).
17) 274 and thereafter (chapter 3.2.3): see 16) but valid here for wind. For instance, it could be discussed, why some regions with increasing wind speed show a decrease in icing and vice versa.
18) 291: “near the surface” What does it mean? >=50 m above ground? Is that near to the surface?
19) 293: “. The mid-century increase is approximately 30–50%, while the strongest negative changes approach 100% toward the end-of-century.” This is a misleading formulation. It seems, that “The mid-century increase is approximately 30–50%” refers to “there is a

temporary increasing trend during mid-century … over parts of northern Fennoscandia and at higher altitudes" from the sentence before. But "while the strongest negative changes approach 100% toward the end-of-century" seems to refer to "decreasing trend in mean rime ice conditions near the surface over most of Fennoscandia by the end-of-century." (see Fig 2 a). Consider rephrasing.

20) 312: "is expected"? Do you mean it in the sense of "expectation" or do you mean it in the sense of "has been analyzed" (or similar)?
21) 318: "Correspondingly, this can explain the increase in the annual maxima for the end-of-century; even with an enhanced temperature increase, the likewise increased LWC allows for temporarily large ice loads." Does it mean higher variability, too? How does this match with the results for areas 6 and 7, where variability seems to decrease in area 7 (and increases in area 6).
22) 322: "In our study,the absolute ice loads are smaller compared to the ensemble means in the study of Lutz et al. (2019) and thus may represent the lower end of the estimated ice distribution." This is the conclusion chapter. It should be discussed in 3.1 and can be one contribution to aspect I.
23) 324: "This was supported by our rough comparisons with LWC obtained from the 325 CERRA reanalysis data (stands for Copernicus European Regional Reanalysis, (Ridal et al., 2024)). For 50 and 400 meters, about two times smaller amounts of mean LWC were observed in Finland in HCLIM data (not shown); however, for monthly maxima, HCLIM data showed larger amounts." See (put it to) 16) as a contribution to aspect I.
24) 328: "Moreover, atmospheric icing encompasses various subtypes, each with distinct formation processes, which rely differently on influencing parameters such as ice density. The impact and severity of different icing types vary significantly depending on the affected target: aviation and in-cloud icing, forests and wet-snow-damages, passive-icing episodes and wind turbines, and ice loads over power lines or electric towers." These statements are right but what do the contribute to the conclusions with respect to all the analysis before? Are the results only being useful for wind turbines and powerlines and not for aviation and forests? Why this is needed to be emphasized because the area specific analyses point to the wind power and power lines.