

Thank you to the Reviewer for providing constructive and thoughtful feedback, which have helped us to improve the manuscript. Our point-by-point responses are provided below in blue text following the Reviewer's comments, reproduced in black.

Review 3 Comments (RC3)

This is a review of Cox et al., "The De-Icing Comparison Experiment (D-ICE): A study of broadband radiometric measurements under icing conditions in the Arctic." The authors present a study of the effects of the accumulation of various types of ice (snow, frost or rime) on radiometer domes and of the performance of distinct mitigation systems designed to remove or prevent ice accumulation via ventilation and/or heating. The topic is appropriate AMT.

The study is well-organized and the presentation is clear and well-written. The results are likely to be of significant value to the radiometric measurement community. They will be of use in shaping the selection and design of instruments for future field projects and in quantifying uncertainties in measurements from instruments currently in use. I am suggesting only a few minor revisions to help with clarity.

Lines 26-27: It's not clear what "mitigating 77%" and "90+% effective" mean, given the limited details available in the abstract. Could these be expressed more concretely here?

We have clarified the statement, which now reads as follows: "Ventilator and ventilator/heater performance overall was skilful with the average of the systems mitigating ice formation 77% (many > 90%) of the time during which icing conditions were present."

Lines 34-36: Could you clarify here whether this is for both shortwave and longwave fluxes?

Yes, we added "in both the shortwave and longwave."

Line 100: Is there a need here to explain briefly what is meant by "global" downwelling shortwave flux?

Yes, we have clarified the jargon.

Line 170-171: What is meant by "rime or frost was observed to be present in the vicinity of the D-ICE systems"? This and the associated paragraph are a bit unclear. In particular, what is meant by "sublimation period" and by "duration of the presence of ice"? Does this mean ice on the radiometer domes, or ice evident elsewhere?

In Section 2.2 we are characterizing the natural icing events that were observed and so we are referring to ice appearing on surfaces other than the domes. We have added an introductory sentence at the beginning of the section to clarify. We have also clarified this again in the first sentence of the section's second paragraph. When ice is present, it may be developing (e.g., deposition) or disappearing (sublimation since the temperatures were almost always below freezing). We referred to the latter condition as a "sublimation period", but there really is no reason to define such a term since it is only used once. Therefore, we have replaced the term with a descriptive statement, "portion of time the ice was present and sublimating". We have also added clarity to the statement of duration as "... duration of presence of ice on surfaces surrounding the experiment."

Lines 285-286: Note that in ARM parlance, "NSA" encompasses both BRW and OLI. Are the included NSA radiometers at BRW?

You are correct that "north slope Alaska" is a definition that encompasses the broader area. Unfortunately, the term "NSA" as used by the ARM community has become ambiguous during the development of the ARM observatories at Barrow, Atqasuk, and Oliktok. Atqasuk and Barrow for example are designated the C1 and C2 facilities of NSA (with E* subsites), but the addition of Oliktok in 2013 did not create a C3 NSA facility. Rather, ARM considers Oliktok to be a separate deployment, in part because of the fact that it is a mobile facility (AMF-3) and not a permanent installation. We have spoken with the site scientist for Oliktok, who has confirmed that while NSA refers to Barrow and surrounding, OLI is separate. Our choice of NSA and OLI as reference identifiers is consistent with the file naming conventions used in ARM's data streams that we analyze (Atqasuk = atq and Barrow = nsa), which we believe is convenient for readers or those who want to have a look at the data themselves. We agree that the conventions can be confusing but we were explicit in the geographical descriptions at the top of Section 2.1 where we define our use of initialisms. We would prefer to keep these conventions.

Line 300: No relationship was observed between "the number of radiometers included in estimated uncertainty" and what?

A limitation of the uncertainty calculation is that removal of data during quality control screening means that at some times fewer measurements are available for the calculation than at other times. We were concerned that this could affect the robustness of the metric. We found that the calculated uncertainty is uncorrelated with the number of values that were used to calculate it, which is evidence that our concern was not significantly impactful. We have rewritten the statement in a way that we hope is clearer.

Line 352: This goes back to my question about "sublimation period" for Lines 170-171. As far as I can determine, this is the first use of the term "deposition period." It would be helpful to clearly define this and "sublimation period" earlier in the paper.

As before, there is no reason for us to define such a term since it is only used once so we have replaced it with a descriptive statement, "...time during which the frost was observed to be growing through deposition."

Line 422 and elsewhere: Ditto my earlier comment re. "NSA" vs. "BRW" vs. "OLI". Please refer to our response to the earlier comment.

Line 425: So is t_icing the same as the length of the "deposition period"? Maybe try to standardize a bit (e.g., "icing period" instead of "deposition period").

No, t_icing is the "amount of time icing conditions occurred", which would be the total length of time of deposition followed by sublimation. The distinction is not important for t_icing and since the confusing terms "sublimation/deposition period" have already been removed from the text, we have not made any additional changes with respect to this comment.

Line 476: "When outside of the ventilator", this means when the *fan* is operated outside of a ventilator?

Correct. We have clarified the statement.

Lines 499-508: It would be interesting to see the results of a similar experiment but with the flow direction of the fan reversed. There would be no fan waste heat warming the air flowing over the dome and no adiabatic compression effects near the dome.

This is an excellent suggestion! We would like to try this right now if it weren't for the fact that COVID-19 protocols prevent us from accessing the lab. In thinking about it, we suspect there might actually be adiabatic processes occurring within the ventilator chamber because the air pressure within the chamber would likely be lowered by the action of the fan. This would cause some cooling that could affect the case. It is less clear what would happen at the dome because the air flow around the dome with air being drawn in instead of forced out of the vent would probably follow a different path.

Line 529: Should this be "specific volume *at* the total pressure"?

Yes, something is amiss. It should be "...specific volume *of the air at* total pressure...". We have made this change.

Line 560-561: Maybe be more specific than "amount of ice", since this could be confused with mass of deposited ice.

Yes, this was ambiguous. We have added clarity to the text.