Response to Anonymous Referee #1

First of all, we thank the referee for submitting helpful and productive comments and annotations, which have led to improvements and clarifications within the revised manuscript, which we submit with this review response.

We have prepared a revised manuscript that addresses the questions and comments of all referees. Furthermore, below we explicitly respond to each of the items raised in the comments of anonymous referee #1. These comments are indicated in *italics,* whereas the author's response is presented in blue. Changes in the manuscript are given in green. The differences are also highlighted in separate PDFs with track changes enabled. All line and page numbers refer to the AMTD manuscript, and not the revised manuscript.

The submitted manuscript, "A view on recent ice-nucleating particle intercomparison studies: Why the uncertainty of the activation conditions matters," from Schrod and Bingemer, is a detailed investigation into the potential consequences of temperature uncertainties associated with various measurement techniques, targeted at determining ice nucleating particle (INP) concentrations. I find the manuscript well written, fairly easy to digest, and also note that it raises an important point of discussion for the community participating in these measurements. Although the title is somewhat general, the submitted manuscript really only deals in detail with temperature uncertainty. As the authors point out, even though many investigators discuss and consider temperature uncertainty, the common practice is that results like activation curves are not reported with ΔT error bars. I believe that the author's main point is that, especially when activation curves are steep, small temperature changes mean pronounced changes in INP activity parameters. Although there are several technical corrections needed throughout the manuscript, I would suggest that with those corrections this is a suitable manuscript for publication and a valuable contribution to the field. I suggest points for technical correction in (page number, line number) form below.

> We thank the reviewer for their assessment of our manuscript.
>
> In accordance to the comments of reviewer 1 we rephrased the title to "A view on recent ice-nucleating particle intercomparison studies: Why the uncertainty of the activation temperature matters" to more fittingly represent the focus of the manuscript.
>
> As reviewer 1 suggested, we will more prominently state that most investigators do consider temperature uncertainty, although we feel that we have done so already on several instances throughout the text.
>
> We will go through the suggestions of technical corrections one by one.

Itemized technical corrections:

Abstract -- The authors should consider (for the atmospheric community) if they would like to make a more clear distinction between their choice of EF to mean "error function", given this is also a common notation for "emission factor".

Yes, the reviewer is absolutely correct. To avoid any confusion, we now say *temperature error factor (TEF)* throughout the text.

(page 2, line 36) -- strike "out", should simply read, " ....ice particles precipitate earlier..."

Corrected as suggested.

(3,77) "Per" is strange to begin a sentence with, I would suggest, "For every 5..."

Corrected as suggested.

(3,80) replace "highest" with "most" and "aerosol" with "particles"

Corrected as suggested.

(3,85) strike "according to Eq. (1) and Eq. (2):" and replace with "as"

Corrected as suggested.

(3,88) should be "methods sections"

Corrected as suggested.

(4,95) I suggest replacing "of" with "corresponding to the"

Corrected as suggested.

(4,103) Rephrase: We considered studies for our investigation when the following criteria are all met:

Corrected as suggested.

(4,104) specify "published since 2015" as in the last 10 years will not age well.

Corrected as suggested.

(4, 109) rephrase, "We identified..."

Corrected as suggested.

(4, 111) strike "a few"

Corrected as suggested.

(4,124) The sentence beginning, "A number of uncertainty estimates...." needs to be clarified and/or expanded. What kind of interpretation?

What we mean is that some statements were not in the style of "The temperature uncertainty is ± X.X °C". Instead, we have a long list of footnotes (a to l) in Table. S1 in the supplement (to which we know refer in the listed sentence), highlighting the cases

where estimates were somewhat ambiguous. Of those, the most commonly observed non-straight-forward estimate was when the text was a little unclear whether accuracy or precision is indicated. For another example, sometimes the authors give multiple error sources and the reader needs to decide if those can simply be added together. Or sometimes only a minimal temperature error or an uncertainty range is stated. Also there was one instance where one measurement was described at a temperature of -15 °C ± 1.5 °C. The text does not explain however if this reflects temperature variation during a specific experiment or uncertainty.

As we further explain in lines 128 and following, even if clear statements were made, often no description was given detailing how this estimate came about. Further, when the estimate was not given in the study itself, it was sometimes not easy to decide which of several other estimates to choose from of those other studies using the same specific instrument. Finally, there sometimes exist different versions of the same instrument at different institutes and they give different temperature uncertainty estimates.

We did change "A number of" to "Some" however.

(5, 140) -- When discussing the Castarède et al., 2023, paper I would suggest the authors also highlight that in this manuscript the authors make some effort to argue that in CFDCs (in particular, PINCii there) it might be that the important activation condition is in fact the strongest thermodynamic forcing condition present within the chamber at a given time. The details appear to be discussed more in depth in the first author's PhD thesis. But this raises an important issue with online type instruments. At times activation conditions are the important reported parameter, not simply INP counting. The two types of measurements will not be impacted in the same way by temperature uncertainty of such chambers.

We added a reference of Castarède and Brasseur et al. (2023) to the sentence in lines 137-138. Further, we agree with the reasoning that activation conditions are often the most important parameter influencing the INP concentration estimate and often outweigh simply INP counting uncertainties. We have said so in lines 225-227 and hint at in lines 331-334 for example.

(6, 175) The range of temperatures is strangely presented. Mixing digits and text, and "tenth" should at a minimum be "tenths" I believe.

Corrected as suggested.

(8, 198) suggest: ....different instruments usually agree to within 1 order....

Corrected as suggested.

Table 2: n_m is introduced in table without first being defined in text. Also in the first bullet related to the DeMott et al., 2017 paper, it is not clear weather differences get smaller or larger as concentrations go above or below 1 INP/L. Please rephrase so intent is clear.

Actually, n_m is first introduced in page 3 line 91, thus prior to Table 2. Differences in DeMott et al. (2017) increase at lower INP concentrations. We rephrased the text to make it clear.

(10, 247) perhaps: ...density per unit surface...

Corrected as suggested.

(11, 260) "upper grey line" and in fact all of the "grey isolines" referred to in the caption are extremely difficult to distinguish.

We prepared a new version of Fig. 2, with more easily distinguishable isolines, changing their color to a bright yellow with more contrast.

(11, 262) This claim that "a hypothetical instrument mistakenly assuming to measure at -25 would actually report nINP of the true temperature of -23.5 is difficult to visualize with the presented figures. Can the reader be coached through how to understand this?

We understand that it is difficult to intuitively understand Fig. 2. Therefore, we did add the hypothetical scenario, which, it seems, still does not fully deliver in helping to understand what is depicted. We try again in other words here: Figure 2 plots the actual temperature error (likely unknown to the researcher) versus the measured instrument temperature. The resulting over- or underestimation due to a false temperature measurement is presented in the color code (and isolines). The hypothetical scenario means to illustrate this. Here, at the reported temperature of -25°C a concentration of 25 L^{-1} is measured (cannot be inferred from Fig. 2, but can be calculated from Eq. 4). However, in this scenario the actual temperature of the instrument was -23.5°C (i.e., +1.5°C warmer). At the intersect of the incorrectly measured temperature (-25°C) and a +1.5°C temperature error, Fig. 2d gives a TEF of 0.5. This means that the instrument reports a value that is 50% lower than what the real concentration at -25°C would have been (50 L^{-1} in this example). Or in other words, when you assume that your temperature measurement is absolutely correct, but the actual activation temperature was 1.5°C warmer than you assumed, your reported INP concentration is 50% too low. We have added a circle to Fig. 2d to mark the intersect of T_m = -25°C and δT = +1.5°C. We hope that with this clarification and the adjustment to Fig. 2 it is now understandable.

(12, 282) suggest: δT is larger

Corrected as suggested.

(12, 293) suggest: ...is acceptable, increasing only to an EF...

Corrected as suggested.

Conclusions: I think for the offline droplet/assay freezing methods the fact that time dependence is largely ignored needs to be mentioned again (as I believe it is in the introduction) in the paragraph spanning pages 13 and 14.

We added a sentence to line 325: “Furthermore, it should be mentioned that the time dependency of nucleation events is often disregarded in DFCS measurements.”

(15, 369) suggest "possibly" should be replaced by "possible"

Corrected as suggested.

(15, 380) This is a great question, and I applaud the authors trying to take one step to solving this underlying problem.

We thank the reviewer for their praise. We hope that our raised questions did not come off as too negative. In fact, we feel that the community is moving forward in great strides, considering the development of new instruments, a larger coverage of observational data in space and time, and more consistent intercomparisons, which we also note in lines 210-211.

Appendix B: suggest: The following figures provide further details for (or perhaps from) the analysis presented in....

Corrected as suggested.