Response to Editor

We sincerely thank the editor for their guidance and support throughout the review process for taking the time to evaluate our manuscript entitled “Measurement report: Ice nucleation ability of perthite feldspar powder”. We greatly appreciate the constructive comments and suggestions provided. In the following, we address each of the editor’s comments, with our responses indicated in blue.

Response to Editor decision*: Publish subject to minor revisions (review by editor), 19 Mar 2026*
Public justification (visible to the public if the article is accepted and published): Based on the two new evaluations, the manuscript can be accepted subject to properly addressing the following comments:

Referee #2:

The revised manuscript entitled 'Measurement report: Ice nucleation ability of perthite feldspar powder' by Canet et al. has improved compared with the previous version. We acknowledge and appreciate the considerable effort the authors have made in revising the manuscript. However, we note that some of the reviewers' comments have not been fully addressed, even though some of them concern relatively minor issues. At the same time, we understand that it is not always possible to address every comment in full, as this may be limited by the scope of the study and available resources. Now, we provide our additional comments below and hope they will be useful in further improving the paper if it is to be published in ACP.

Additional comments:

1. Line 20: 'across all feldspar types' should be all tested types, please change the wording to be more specific.
We understand your point. In line 20, the wording has been revised to clearly specify that the feldspar types refer to those investigated in this study.
2. Harmonize the terminology of ice nucleation site (Vali et al., 2015). You have 'ice-nucleating sites' in Line 21 but you also have 'ice nucleation sites' in Line 24 and 365, etc.
The wording has been changed.
3. Line 26-27 contains a phrase of 'the suppression of certain subpopulations', which raises the question of what suppresses the ice nucleation ability of certain subgroups? We suggest saying the less efficient ice nucleation of subpopulations or decreased ice nucleation ability of subpopulations.
The wording has been revised following the editor's suggestion.
4. Line 392-393: 'Collectively, these trends indicate that the Na/K ratio exerts a primary control on IN activity, …'. Please check the wording. The statement now raises the question of what is secondary control? Was any comparison made in the manuscript?
The wording has been revised to avoid implying a hierarchy of controls, as the study does not explicitly quantify secondary factors.
5. Figure 3: Again, we must ask how many sample concentrations exactly did you measure? In Line 185, it was stated that 9 concentrations were tested, from 10 wt% to 0.001 wt%. However, Figure 3a showed 8 concentrations for LNa2 (from 5 wt% to 0.001 wt%) while Figure 3b and 3c showed 7 for LNa2 (from 5 wt% to 0.01 wt%). One reviewer has already patiently pointed this out. Please do a double check again and do it carefully!
The feldspar powders were examined across this overall concentration range; however, not every sample was tested at each specific concentration. Rather, the stated range reflects the full set of concentrations considered collectively. In some cases, no significant differences were observed between 10 and 5 wt%, and therefore the 10 wt% concentration was excluded. Likewise, very low concentrations that yielded results indistinguishable from pure water were also omitted in certain cases. Nevertheless, the concentration values reported in the figures are correct and represent the data shown (either Fig. 3 and Fig.S4 in the supplementary information).
Regarding Fig. 3b and Fig. 3c, it was already indicated in the main text (Section 3.3) and in the caption of Fig. 3, that since the freezing behavior at the lowest concentrations (≤ 0.001 wt%) was indistinguishable from

that of ultrapure water, concentrations ≤ 0.001 wt%, was therefore excluded from being plotted in Fig. 3b and Fig. 3c.

6. Thanks for your response to the General comment 8 and Minor comment 29 of Referee #2 and general comment bullet #3 of Referee #3. The comment from reviewer #2 did not mean the measurements of high concentration samples are not useful. One should be very careful to use the consistency of Ns (T) results of different concentrations to draw a conclusion that the results are of broad atmospheric applications. What about the Nm (T) results and results from other perspectives, e.g., size resolved samples of different concentrations (i.e., the discrepancy in your Figure 5a)? In addition, we are not convinced by the new statement in Line 374-375 in the revised manuscript: 'This demonstrates that the observed high-temperature IN activity is not an artifact arising from particle aggregation at high concentrations, but instead reflects intrinsic IN properties of the feldspar particles.' Again, using the Ns (T) data consistency of samples of different concentrations to claim the demonstration of super high active ice nucleation activity of high concentration samples (e.g., 10 wt%) carries risks of drawing improper conclusions, particularly if significant particle agglomeration occurs, such as sediment formation in the 10wt% samples. In this case, immersion freezing would no longer occur on atmospherically relevant mineral particles but rather on bulk mineral surface. Consequently, these results will not have meaningful atmospheric implications and cannot support the statement in the last sentence in the abstract. More convinced evidence to demonstrate such active cloud ice nucleation of these mineral particles (forming ice around −2 and −4 ℃) is to perform immersion freezing of single aerosol particles to mimic atmospheric scenarios, like using online freezing techniques. This does not mean we really insist the authors to do these experiments now, which can be suggested for future studies. Highlighting this limitation and giving caveats do not lessen the value of the research; we believe that scientific studies should consider both the merits and the drawbacks of their experiments.

We thank the editor for those comments. Figure 3c has been revised to highlight the concentration range that is more relevant for atmospheric aerosol conditions, thereby improving the clarity of the comparison across concentrations. At the same time, we acknowledge that our experiments were conducted under immersion freezing conditions, which are not directly representative of atmospheric aerosol particles. While effects such as particle aggregation at higher concentrations cannot be excluded, the consistency of the results across a range of concentrations suggests that the observed high-temperature ice nucleation activity is not solely driven by such effects and is likely linked, at least in part, to intrinsic properties of the feldspar particles.

We agree that further studies using techniques that better mimic atmospheric conditions would be valuable to further constrain the atmospheric relevance of these findings.

7. Minor comment 13 of Referee #2: We understand that it is not possible to test the same power sample for both freezing and BET. The response does not answer the point we were interested in in our comment. Our concern was about whether the degassing process will change the sample property and thus affect the BET results for the degassed sample. And the BET surface area was used for calculating the Ns (T) of powder samples that did not undergo such a degassing process. One could ask why the authors don't do degassing at a lower temperature which does not influence the sample property or shows less influence.

We thank the reviewer for this important comment. We agree that the concern is not whether the BET degassing conditions (393 K, which is 119.85 ºC, during 24 h) are sufficient to induce a bulk structural transformation of feldspar, but whether the dry surface area obtained after degassing is fully representative of the effective surface involved in immersion freezing experiments.

In our view, the degassing conditions used for BET analysis are not expected to modify the bulk crystal structure of feldspar, since they are far below temperatures required for polymorphic transformation or Al-Si reordering in K-feldspars. However, we acknowledge that degassing may alter the surface state by removing adsorbed water and possibly changing the accessibility of pores, cracks, or other nanoscale surface features to the probe gas. If lower degassing temperatures are applied, incomplete removal of physisorbed water and

other volatile species may occur, which could lead to an underestimation of the accessible surface area and reduced pore accessibility during BET measurements.

8. Minor comment 16 of Referee #2: The authors should do a double check if their background freezing of pure water in their system really occurred at ~−21 °C. In Figure 3a, it clearly shows that pure water started to freeze at >−20 °C and shows influences on samples of the lowest two concentrations.

We agree that freezing onset of pure water in our system occurs slightly above −20 °C, as shown in Fig. 3a. We have revised the manuscript to clarify this distinction. We also note that at the lowest feldspar concentrations, the number of active sites per droplet is reduced, and the freezing behavior increasingly approaches that of pure water.

9. Minor comment 17 of Referee #2: We don't think the SEM images in Figure 2 show the sample porosity. The texture could be more attributable to bulk mineral surface defects, mineral phase and/or composition heterogeneity.

We have revised this section to improve clarity, as our intention was to emphasize that the aligned porosity can be observed through optical microscopy. While SEM allows the identification of fluid inclusions, observations under an optical microscope (particularly once the mineral has been prepared as a slab) enable the visualization of both features. Optical microscopy shows that the black spots correspond to alignments of fluid inclusions originally present within the mineral, which became exposed and disrupted during the cutting of the sample slab.

Referee #3:

The manuscript was improved significantly, it is clearer, provides a better discrimination of different sample properties and their impact on IN and the discussion is more structured. My critique in the first review regarding the sample classification and characterization is not fully resolved and the work would still benefit from more petrographic micrographs, SEM images and in particular EPMA analyses, but in the context of the work's scope, the portrayed main characteristics (Al-Si ordering, microcline/orthoclase, Na-K-composition, perthites) and their impacts on IN are well discriminated and well discussed.

A few sentences need to be fixed:

197-199: "The absence of mica phases in the Imerys samples implies that Al is incorporated into the feldspar structure via Si-Al substitution, with charge compensation provided by elevated K^+ content (Ribbe, 2018)." => There is space for 4 K+ per unit cell yielding the composition KAlSi3O8. If the Si-Al ratio is less than 3, i.e. the composition is (K,Na,Ca,…)Al1+x Si3-xO8, the Si-Al substitution goes together with the incorporation of 2+ ions (Ca, Ba, Sr) not with "elevated" K+. The Si/Al-ratio of 2.9 shown in table 1 should be explained with higher Ca content of IK1 and minor muscovite (or sericite) inclusions. The higher K of IK1 compared to IK2 is only the different K-Na composition.

We thank the editor for this comment. This section has been removed. Calcium (Ca^{2+}) is present only in trace amounts. The Si/Al ratio in the Imerys sample is close to 3, in agreement with typical feldspar compositions. In contrast, the Llançà sample exhibits a higher Si/Al ratio of around 4, which exceeds the expected range for feldspar. This difference suggests that the increased Si content is more plausibly explained by the presence of additional SiO_2, consistent with a higher quartz content in this sample.

511-517: It should be clarified that samples FS08-64o and FS08-64c were shifted to intermediate compositions (KK=0.43) at 850°C before they were annealed at 550°C. During this shift, cracks parallel to the Murchison plane are formed (Kiselev et al. 2021, section 2.1.1 and Fig 1). During annealing at 550°C for 64d, more cracks are formed in sample FS08-64o and in sample FS08-64c no additional cracks but 30nm wide exsolution lamellae are formed (Kiselev et al. 2021, Fig. 5).

The manuscript has been revised to include this additional detail on the composition and treatment history of the samples from Kiselev et al. (2021).

526: "…crack walls generated during vacuum annealing" => should be adjusted accordingly

This has now been clarified in the revised manuscript.

533-534: “We hypothesize that these additional sites may be associated with increased surface disorder” => in the context of this work, I read “disorder” as Al-Si disorder. The starting material for Kiselev et al. (2021) is highly disordered sanidine equilibrated at around 1000°C, annealing at 550°C could only increase Al-Si ordering. Please clarify.

Thank you for the comment. The text has been clarified.

Luis Ladino