Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-208

Anonymous Referee #2

Referee comment on "The sensitivity of the ice-nucleating ability of minerals to heat and the implications for the heat test for biological ice nucleators" by Martin Ian Daily et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-208-RC2, 2021

The authors investigated the responses of heat treatments (namely dry- and wet-) on different types of atmospheric ice-nucleating particle (INP) proxies using their offline cold stage instrument. Based on their findings, they made some technical recommendations regarding the offline heat treatment study of INPs (L695-725). The study objective and hypotheses are valid. The reviewer generally agrees that different ice-nucleating materials respond to heating in various ways (e.g., L576 etc.). The authors' messages are clear (L639-640; L643-645) while some explanations sound speculative. The reviewer has some major and minor comments. Some re-organizations of sections seem necessary to improve the readability.

Major comments

Proteinaceous structures can be destroyed below boiling temperature (Steinke et al., 2016). For example, Szyrmer and Zawadzki (1997) found some known cell-free IN-active microbes (e.g., *Fusarium* nuclei) are stable only up to 60 °C. Other studies of IN active bacteria, fungi, and lichens have shown heat sensitivity at lower than 100 ^{o}C. The reviewer is missing the detailed discussion of what protein is (and what is not) denatured in different temperature ranges. It is somewhat temperature-dependent and perhaps employing ~ 80 °C for 10-30 min (i.e., Fig. A2) may be comparable to using truly boiling temperature for heat treatment? What is the minimum time for proteins to be denatured?

Fig. 1 and all associated discussions fit better in the results & discussion section rather than the materials & method section.

L338-358 & L466-467 & L478-479: Hydrolysis and dry-heating likely alter SSA and other physical properties of materials, which impact their ice nucleation abilities. Reporting SSAs of a subset of materials after wet- and dry-heating would clarify the authors' hypothesis given in these parts and strengthen the paper. Unless the authors can directly quantify the loss of active sites and/or the number of denatured proteins by a set of heat treatments, some arguments seemingly remain speculative.

Sect. 2.3.2: The choice of 250 dC for dry-heating seems appropriate, but if the authors wish to do the apples-to-apples comparison of wet-heating vs. dry-heating, wouldn't it make more sense to use the same heating temperature and period for both heating

methods? The authors state that "The dry heat test is a harsher treatment than wet heating…" in L369-370. Do the authors think the measurements with multi-temperatures could be a better procedure in dry heat tests (e.g., 100 dC vs. 250 dC etc.)? Perhaps, Amazonite microcline may have a different response to dry heat at 100 dC? SSA may be changing depending on the employed heat temperature?

The snapshot example of quartz in Fig. A1 is very nice. The reviewer wishes to see a similar dataset for wet-heat stable compounds (e.g., kaolinite and MCC). Does a similar trend hold for non-quartz samples?

Sect. 2.2.: The used suspension concentration of 1% w/v for MCC etc. seems to exceed what is recommended in previous literature (e.g., Sect. 3.1. in Hiranuma et al., 2019). What is the rationale behind such a high concentration? Wouldn't such a high concentration cause some issues (e.g., flocculation of suspended particles)? How do these high concentration n_s spectra compare to previous studies? The reviewer sees the K-feldspar reference spectrum in Fig. 2 but not for other materials the authors examined for this study.

L648-670: Indistinguishable by the heat reaction itself but complementary mineralogy and composition analyses can distinguish these two populations. Do the authors intend to argue the applicability of heating on environmental samples (i.e., the mixture of different compositions)?

L755-756: The reviewer thinks that online heating (i.e., INP measurement with a heating inlet etc.) could be a good alternative approach for the quantitative test. Perhaps, the offline heating tests can be done with a set of different temperatures? Include these points in P25 (a), (b), and (d)?

Minor comments

What is the minimum detection limit of evaluation ice nucleation ability and/or efficiency of the cold stage for this study?

L25-28: This sentence makes sense without the last few words (", so long as … K-feldspar"). This part sounds speculative. The reviewer suggests removing this part from the abstract. The importance of K-feldspar as an INP seems not the main focus of this study.

L57-59: Adding a discussion of the emission rates and atmospheric abundances of mineral dust and biogenic INPs might make this paragraph even more meaningful. Please consider providing some information to the reader.

L67-70: Tobo et al. (2019) shows soil dust has some contributions to it, too. This can be briefly discussed here?

L287-293: Repetitive, and this part does not fit in the results and discussion section.

L295-303: Better fits in the materials & method section.

L307-307 & L316-318: The authors can introduce a brief statement to guide the reader where the explanation of the observed results is given later in this section (L349-).

L326-336: Fits better in the intro section.

L399-403: Fits better in the materials and methods section.

L405: Quartz presented before feldspar in Fig. 3a. Fig. 3b is not discussed until Page 17. The arrangement of Fig. and sub-sections seems a bit odd.

L430-431: Sounds speculative.

L433-455: Fits better in the materials and methods.

L504-521: Fits better in the materials and method section or SI.

L549-558: Fits better in the materials and method section or SI.

L596-598: Fits better in the materials and method section or SI.

L684-691: Sounds speculative

L739-741: Showing the altered specific surface after wet heating can be more direct evidence. Accounting a different SSA may explain the alternation in IN ability of the material?

Fig. S1: The axis text/numbers and legends are too small to see.

Fig. S3: Is this ns plot generated using the FF spectra data in Fig. S1(u)? It seems that Fig. S3 is missing the point above -12 dC for a non-treated sample. Or the authors did another set of measurements for generating Fig. S3? Please clarify.

Technical comments

L3: Murray1[1]

L30: à the absence of ice nucleation active sites,

L39 our models à atmospheric models

L56: 2015b comes before 2015a

L63: ice active à ice nucleation active

L63-66: How small & how great? The reviewer suggests that the authors provide some quantitative information to the reader in this paragraph.

L82: à characterize the ice-nucleating activity of

L84: à known bacterial, fungal, and archaeal

L250-274: "A" should be defined in L251, or the authors can introduce Eqn. 2 in L273.

L306: after a closing-parenthesis change "," to ".".

L409:))

L770: 1 mL or 1.5 mL? One figure says 1.5. What is the vendor and model number of the tube?

References

Steinke, I. et al.: J. Geophys. Res.-Atmos., 121, 13559–13576, 2016.

Szyrmer, W. and Zawadzki, I.: A review. Bull. Am. Meteorol. Soc. 78, 209–228, 1997.

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Tobo, Y. et al.: Nature Geoscience, 12, 253–258, 2019.