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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Enhanced ice nucleation efficiency of microcline immersed in dilute NH_3 and NH_4^+-containing solutions" *by* Anand Kumar et al.

Anonymous Referee #2

Received and published: 25 February 2018

General comments: I support publication of this manuscript in ACP. The observation of solutes' effects on the IN efficiency of microcline is novel and highly relevant to ACP as well as IN research community. The experimental approach employed in this study is elegant, and the authors conducted very careful and dedicated experimental works. Further elaborating/tightening the conclusion section with their observed results and detailed evidence would benefit the overall quality of the manuscript. Other than that, I have some minor comments that should be adequately addressed prior to the final publication.

Minor specific and technical comments: P2 L38: DeMott 2013a (a needs to come first)

P2 L67-68: This statement seems misleading - Harrison et al. (2016) points out that the fresh sanidine can be as active as microcline.

P2 L70-72: I suggest the authors to provide some additional information regarding the active site "assumption" (there is no direct observation of it in immersion freezing) and prescribe how it may be responsible for heterogeneous freezing in more detail here. Such information may benefit some statements in this manuscript (e.g., P9 L296-299; P14 L489) to sound less ambiguous.

P3 L74: ". . .included in the current IN parameterizations."?

P4 L115: The authors may want to introduce more of theoretical description of the water activity based immersion freezing approach (see Knopf et al., 2018 and references therein; doi: 10.1021/acsearthspacechem.7b00120) here first for the readers, who are not familiar with it.

P4 L130-132: The authors may briefly discuss the reproductively of onset temperatures, such as Thet, Thom and Tmelt, by DSC for the samples used in this study. This additional information may be beneficial to the readers, who are not familiar with the DSC technique.

P4 L144-P5 L150: Please clarify if the samples were re-sonicated or re-homogenized prior to each time trial DSC analysis. The presence/absence of any additional physical sample modifications should be stated.

P5 L164-167: How does this BET specific surface value compare to the other values from previous microcline IN studies? Did the authors observe any deviation between BET and the geometric surface-to-mass ratio (i.e., the ratio of the total surface area concentration to the total mass concentration estimated by SMPS/APS)? If so, what is its atmospheric implication?

P5 L172-174: I like this statement showing negligible concertation dependency of the onset freezing T in DSC. Great job.

P6 L205: Clarify/elaborate what “an initial increase” meant here – e.g., an increase in Thet near aw $\sim$1.0.

P8 L269-271 or P9 L320-321: The authors may extend this simulation-based discussion of ice-like ordering on the second bilayer, which seems to hold true for even non-mineral dust particles, by citing some more papers, such as Lupi et al. (2014, J. Am. Chem. Soc.) and Lupi and Molinero (2014, J. Phys. Chem. A).

P9 L314: “. . . when compared with . . .”.

P10 L328-332: I suggest the authors to separate this sentence into at least two sentences – e.g., “. . .K+ in the surface water. For instance, the comparison of. . .”.

P10 L334-337: I suggest the authors to separate this sentence into two sentences – e.g., “. . .small structural fragments. This cations release presumably leads to. . . (Add proper references for this part if they are any)”.

P10 L353-359: Please include the discussion of the recent study done by Abdelmonem et al. (2017, ACP, 7827-7837) regarding the effect of pH on IN.

P11 L369-372: This observation regarding non-reversible IN ability is great. Good job.

P12 L411: “An exception includes. . .”.

Fig. 2: What does the minor heat flow peaks at T > 255 K indicate? It seems not apparent in Fig. 4. What caused the observed difference?

Fig. 3: Is there anyway to overlay the results of previous ABIFM results for some reference mineral dusts in comparison to the authors’ results? If yes, please do. Adding some reference points may benefit the paper.

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