

Responses to Anonymous Referee #1

I thank the reviewer for his/her constructive comments that I address below point by point (responses are in italic).

This is a good paper and correctly categorized as a technical note as it does not contain considerable new scientific findings. It follows on several papers, including several by Marcolli et al., and the advance here is that it provides a more comprehensive treatment of the theory of pore condensation freezing than in previous works. As a technical note it represents an important resource and should be highly referenced by those working on this ice nucleation mechanism. It should be published with minor changes.

Some suggestions for the author to consider:

1. The introduction only discusses cirrus clouds as the application of pore condensation freezing. The author may consider if other systems might be impacted, e.g. those beyond the atmosphere.

Indeed, PCF is applicable also in other fields. Yet, since this technical note will be published in an atmospheric journal, I would like to keep the focus on the atmosphere.

2. Also in the introduction, can it be stated that homogenous freezing via pore condensation does not exclude heterogeneous mechanisms? The term 'prevailing mechanism' in line 28/29 makes it unclear if the author is suggesting this is the dominant mechanism (which I do not believe has been shown) or one of several depending on specific conditions (which I believe is the consensus). A sentence or two to clarify would be helpful.

This sentence refers to Marcolli (2014), where it is indeed claimed that PCF is the prevailing nucleation mechanism at low ice supersaturation (i. e. below water saturation in the mixed-phase cloud regime, and below homogeneous freezing of solution droplets at cirrus conditions). Further down in the introduction, evidence supporting this statement is given. Conversely, there is indeed no experimental evidence that substantiates a depositional ice nucleation mechanism, i.e. is able to prove the absence of liquid water in deposition nucleation.

3. Given the density of equations in Section 2 and after, it would be helpful to have a table with variable definitions in the paper, e.g., Appendix A and then use B for the derivations / parameterizations.

I implemented a table with variable definitions as Appendix C to the paper.

4. The use of Figures 1 and 11 is appreciated for clarify on the specifics of pore modeling, this will be of help to the broader readership of this Technical Note. The author may consider moving Figure 11 earlier in the paper as a description of how this mechanism is impactful on the atmosphere.

Thank you for this suggestion. This figure is now Fig. 1 of the revised manuscript in the new Sect. 2 (Atmospheric scenario of PCF).

5. I concur with the point made by Reviewer 2 regarding significant figures, e.g. Section 3.1.1 in the equations and perhaps this could be incorporated into a table or another Appendix.

I added Appendix D to the revised manuscript with Table D1 listing values of the density parameterization given in Appendix A and Table D2 giving values of the pressure dependent CNT parameterizations described in Appendix B.