Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-524-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards parametrising atmospheric concentrations of ice nucleating particles active at moderate supercooling" *by* Claudia Mignani et al.

Anonymous Referee #2

Received and published: 31 July 2020

Review of "Towards parameterizing atmospheric concentrations of ice nucleating particles active at moderate supercooling" by Mignani et al.

General comments: This reviewer enjoyed reading this concise paper. The study presents several important aspects, such as precipitation INPs & their source appointment & implication of supermicorn INPs, that all could be an important addition to ACP. While the manuscript is overall well written, some sections deserve more articulations in this reviewer's opinion. See specific/technical comments given below. The reviewer hopes these help the authors and improve the manuscript.

The reviewer sees some speculative sentences (e.g., P3L71-75, P3L80-82). Concern-

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ing these, at the end, claims of novelty and priority (P3L85-P4L91) are questionable to this reviewer. The reviewer understands that the parameterization of cumulative INPs @ -15 dC for 1.5 months is a snapshot example of the authors' concept/proposal of IN parameterization for moderate spercooling. Regardless, an elaborated summary on how specifically beneficial the proposed parameterization would be to the community seem necessary in the conclusion section.

The authors may want to soften the tone regarding the comparison to D15 etc., and carefully re-formulate all the associated statements. The take-home message of the paper should not be 'a previous parameterization does not work' (this reviewer felt this to some extent). Instead, the authors should structure the discussion by blending/merging previous contributions in this work (rather than clipping) and emphasize how their new parametrization can complement the previously introduced one(s). Plus, the reviewer questions how meaningful INP measurements at single T (-15°C) would be concerning many speculations and absurd statements given in the manuscript. The reviewer is aware that the justification of this single T point is given in P1L15-17. Nonetheless, the authors need to soften their tones in some sections.

Specific and technical comments:

P1L2: –> . . .at -15 °C, at which a cloud-top. . .

P1L3-4: –> We found at a mountain top site in Swiss Alps that INPs active at -15 °C contained different fraction of coarse (>2 μm) aerosol particles, depending on . . .

P1L10-11: If the authors are discussing about INPs in general (not droplet freezing in particular), then the water saturation should be one of the most important factors/variables to complicate our understanding in [INP] and should be included in this particular sentence. Plus, there should be more comprehensive and appropriate references to cite and support this sentence other than what appears here.

P1L13-14: Although successful. . . –> Although the D15 parameterisation may be ap-

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plicable to temperatures below -20 °C, it remains. . .

P1L18: RE "it makes physically sense for various reasons" – then, the reviewer suggests the authors to briefly, at the least, discuss physical mechanisms of why it peaks specifically at -15 °C for the readers. Otherwise, -15 sounds like a magic number, making the manuscript and its concept sound speculative, in the reviewer's opinion.

P1L19-21: –> Recent findings from. . .; Creamean et al., 2015). However, a particle size is poorly represented in the empirical data-based parameterizations (Phillips et al., 2013). Perhaps, this part reads better this way?

P1L22: –> such high temperature active INPs

Section 2: How turbulent was it while sampling during snow precipitation? Some discussion of local meteorology and its impact on sampling/measurements beyond crude categorization based on observation seem necessary.

P2L26-27: –> While the site, surrounding mountains and nearby valleys were snow-covered, most of the foothill regions were not, and only rain persisted in those regions during our study period. Is this what the authors meant?

P2L31: The reviewer requests the authors to describe their inlet configuration in the manuscript. Was it a TSP isokinetic inlet? What was the height of the top of the inlet? Particle loss/transmission well characterized? How did the authors make sure there is no re-suspension of snow or soil got into the inlet as well as no influence of local gust/turbulence and other dynamic/thermodynamic effects?

P2L32: The reviewer requests the authors to elaborate their impinger particle caption efficiency in the manuscript. To the reviewer's knowledge, an impinger is good at capturing relatively large size particles, but not that efficient on trapping small particles. How did this kind of size-dependent trapping efficiency potentially affect the sampling activity and overall results should be addressed in the text.

P2L33: Perhaps some water were sucked up by a pump rather than being evaporated?

Then, replenishing pure water may have affected C_INP and n_INP estimations at the end? The authors presume only water evaporated and all aerosol particles remained in an impinger jar throughout individual samplings? Would it be really the case for the impinger, which was used in this study?

P2L36: Please clarify what "15 above, 1 below range" means.

P2L38: APS only appears once... No abbreviation seems needed.

P2L37-39: Number concentrations of particles should have been integrated rather than being averaged, correct?

P2L40: [n] and [INP] also scaled to standard T (273.15 K)???

P2L49: aerosol samples –> impinger samples

P3: General suggestion – the reviewer suggests the authors to discuss how their INP-15 generally compares to other, previous precipitation INP studies (e.g., Petters and Wright, 2015; https://doi.org/10.1002/2015GL065733) before jumping onto nX vs. INP-15. The reviewer understands that the authors intended to be straight on the point (and appreciate the concise, right on the point manuscript length to some extent), but the readers would appreciate this extra information to generalize/digest information at their end, in the reviewer's opinion.

P3: How SD-rich IN efficiency compares to Ullrich et al., 2017 or Niemand et al., 2012? The authors can estimate n_s and do comparisons?

P3L68-69: The reviewer is lost on the "It also reveals..." part. Please clarify what it means in an intuitive manner.

P3L71-75: Speculative sentences – Many questions came to the reviewer's mind - What was the influence of local thermodynamics & meteorology (esp. wind spd.)? Was a proper inlet used to eliminate the impact of local turbulence etc.? Chance of re-suspended particles getting into an impinger while high-volume sampling? Any hind-

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sight 20/20 situations?

P4L87-88: Cannot disagree – a wider spatiotempral coverage is indeed needed.

P7: Concentrations of ice nucleating particles active at -15°C or warmer [INP-15] –> Cumulative INP concentrations estimated at -15°C, [INP-15]

Fig. 1: Show correlation coefficients for fits. Add fits & Rs in Fig.1g, too. Discuss these in P7.

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