

Atmos. Chem. Phys. Discuss., referee comment RC2  
<https://doi.org/10.5194/acp-2021-744-RC2>, 2021  
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## **Comment on acp-2021-744**

Anonymous Referee #2

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Referee comment on "Measurement report: Introduction to the HyICE-2018 campaign for measurements of ice-nucleating particles and instrument inter-comparison in the Hyytiälä boreal forest" by Zoé Brasseur et al., Atmos. Chem. Phys. Discuss.,  
<https://doi.org/10.5194/acp-2021-744-RC2>, 2021

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### **Review of the manuscript "Measurement report: Introduction to the HyICE-2018 campaign for measurements of ice nucleating particles in the Hyytiälä boreal forest"**

Brasseur et al. presented the HyICE-2018 campaign, taking place at one site in the Finnish boreal forest from late February to early June 2018. This is an extensive field campaign involving several institutions and many co-authors. First results from this campaign have been published in Paramonov et al. (2020) and Schneider et al. (2021). Brasseur et al. are now presenting all the instruments that have been installed for the campaign, intercomparing INP concentrations using online and offline measurement techniques, and lay the groundwork for further studies conducted as part of the campaign, which are currently manuscripts in preparation. The manuscript reports measurements within the scope of the journal.

### **Major comments**

In general, the INP concentration inter-comparison is based on only a few data points and hours of measurements. The online comparison measurements take place during the day on four different days, and the offline measurements are made within 24 hours. It would be interesting to learn more about how representative these time spans were in terms of the observed variables (such as INP concentration, aerosol loading, new particle formation events, meteorology, etc.) throughout the entire HyICE-18 campaign. Further questions that are relevant for the manuscripts in preparation might be addressed: Can these results be generalized for the entire campaign? Do you expect similar/different results on other randomly selected days during the campaign?

It would be helpful to add some information about the findings of the four manuscripts in preparation conducted during the campaign (i.e. P5L20, P9L16, P12L3, P15L31) as a mere listing of the topics discussed in the manuscripts is not useful from the reader's perspective without more information.

One conclusion of the manuscript is that "the DeMott et al. (2010) parameterization tends to underestimate the observed INP concentration while the Schneider et al. (2021) parameterization tends to overestimate the INP concentration and does not capture the daily variability [...]". I have some reservations about drawing this conclusion. First, this statement should be elaborated to certain temperature regimes. Also, these parameterisations are not being used here exactly for what they were intended. For example, the Schneider et al. (2021) parameterization is intended to be predicting daily INP concentrations based on daily air temperature, not for capturing the daily variability. In addition, the DeMott et al. (2010) parameterization, for example, has meanwhile been updated (i.e. DeMott et al., 2015), which is constrained above -20 °C only weekly. I would suggest using the parameterizations as they were intended by their authors before drawing a conclusion.

In line with reviewer 1, I think that the paper uses too many vague descriptions (e.g. P1L4-5: "lower", "higher", P3L27: "any", P8L33 "larger", "smaller", Fig. 4: "see text for details" to state only a few) and the figures often contain repeating results (like in: Figs. 2 & 7, Figs. 7 & 8, Figs. 7 & 9, Figs. 11 & 12), which should be avoided by merging them.

### **Minor comments**

Please note, I have tried to avoid minor comments that have already been made by reviewer 1.

P2L16 What about pore condensation and freezing (e.g. David et al., 2019)?

P2L21-23 Please add references and state what is known concerning sources and properties in the boreal forest environment, even if only little is known.

P5 Table 1. The abbreviations TSP, PM<sub>2.5</sub>, PM<sub>10</sub>, PM... are not defined in the manuscript. Also, I would maybe tell the reader in the caption as a side remark that the information about the APS and the SMPS can be found in Table A1.

P5L19 What do you mean by "INP signals"?

P6L16 Maybe you could leave "with parallel plate design" away as this is described in the next line in more details.

P6 Fig.3 Some items in the schematic are not clearly defined (e.g. in (a) ice crystal, liquid droplet, in (c) frozen droplet). In (b) "Pressure / Temperature" is a little confusion as you use "INP / L" elsewhere. (d) Maybe you could show the actual operating conditions during your campaign instead of the feasible operating conditions.

P7L9 Please state the inlet size cutoff and refer to the table.

P8L29 Maybe refer to Table A1 too?

P8L33 Can you please be more precise regarding the size-dependent enrichment factor here?

P9 Fig.4 What do you mean by "its complete setup"? Also, could you please describe in the caption what the dashed lines represent?

P11L4 Can you please elaborate shortly why there was no correction method for the SPIN measurements?

Section 2.2.3 Did you do background measurements (e.g. Polen et al., 2018) with INSEKT and uL-NIPI during the campaign? If so, please shortly describe the results.

P16L14 Please shortly elaborate how boundary layer depth was estimated. What was the mean boundary layer depth during the flights?

P20 Fig. 10 "right side panel" -> "lower panel"

P22L7 Can you think of another reason that could maybe explain the rest?

P23L12 What temporal resolution was used for the parameterization of Schneider et al.

(2021)?

P24L9 Please rephrase "strong temporal agreement". Be more specific. Do you mean that the concentrations decrease from morning to night samples? I would also be a little careful with the tone of this statement as it is made based on 7 samples collected over 24 hours only. Authors should point this out. Maybe also mention the inconsistent temporal resolution and discuss whether and/or how this might have affected the results.

P25L5 Does this mean that you did not consider the dilution factor in the subsequent calculation of INP concentrations? I would suggest to do so.

P26 Fig. 14 Please add the appropriate colours for the error bars. Maybe show open symbols for diluted samples.

P28 Fig. 16 The parameterizations often have a specific temperature regime for which they are valid. If you extend this temperature range, please make this clearly visible in the figure.

P32-34 Table A1-3 What do you mean by "Hitu-hut"? Please use consistent wording.

## References

David et al.: Pore condensation and freezing is responsible for ice formation below water saturation for porous particles, PNAS, 2019.

DeMott et al.: Predicting global atmospheric ice nuclei distributions and their impacts on climate, PNAS, 2010

DeMott et al.: Integrating laboratory and field data to quantify the immersion freezing ice nucleation activity of mineral dust particles, Atmos. Chem. Phys., 2015.

Paramonov et al.: Condensation/immersion mode ice-nucleating particles in a boreal

environment, Atmos. Chem. Phys., 2020.

Polen et al.: Cleaning up our water: reducing interferences from nonhomogeneous freezing of "pure" water in droplet freezing assays of ice-nucleating particles, Atmos. Meas. Tech., 2018.

Schneider et al.: The seasonal cycle of ice-nucleating particles linked to the abundance of biogenic aerosol in boreal forests, Atmos. Chem. Phys., 2021.