Review of manuscript "Condensation/immersion mode ice nucleating particles in a boreal environment" by Mikhail Paramonov and coauthors

Paramonov et al. studied the ice nucleating particles (INPs) in the condensation/immersion mode in the boreal environment of southern Finland during winter-spring of 2018. The number concentrations of INPs were measured using a continuous flow diffusion chamber PINC, along with the measurements of total aerosol particles (DMPS, CPC, APS), aerosol chemical composition (L-ToF-AMS), biological fluorescent particles (WIBS), and meteorological conditions (RH, T, WS, etc.). The measurements were used to investigate the number concentrations, sources, and possible compositions of INPs at this location during the studied time period. A few case studies were also presented to show the variability of physical and chemical properties of INPs over a short time period. This study is important as it is a nice addition to the rare INP measurements conducted in the boreal forest environment, and will help improve our understanding of INPs in the atmosphere. However, some of the conclusions/hypothesises reached in the manuscript were not well supported by the data, along with some other issues that the authors may consider to address in the revision.

Authors response: The authors would like to thank the reviewer for a detailed and insightful review. The line numbers below refer to the version without track changes.

Major comments:

1. **P1 L18 and P9 L268: the conclusion "there are no local sources of INPs" cannot be drawn based solely on the lognormal distribution of [INP] frequency. Welti et al. (2018) only suggested "the absence of a strong local source". Also, this conclusion is contradictory to the fact that biological particles released by the surrounding forest were considered as a source of INPs in Sect. 3.2.2. Please revise the statement to make it clear.**

   Authors' response: We have reworded this conclusion in lines 18 and 286 to say that "there are no single dominant local sources of INPs".

2. **P1 L25: The conclusion "ambient INPs are most likely in the size range of 0.1-0.5 μm in diameter" was not well supported by the data. Fig.4 shows that overall INPs didn't correlate with $N_{0.1\text{-}0.5\mu m}$ at all. Also, the design of the setup removed all the particles >2.5 μm, which may contribute to a large fraction of INPs (Mason et al., 2016). This should be discussed in the manuscript.**
   - The statement that ambient INPs are most likely in the size range of 0.1-0.5 μm in diameter does not stem from the overall correlations shown in Fig.4 The reviewer is correct – we did not find an overall correlation of [INP] with this size channel. Reasons for this are discussed in the last paragraph of the section 3.1.4, i.e. not every 300 nm particle would act as an INP, so a correlation would be unlikely given the low AF values. The conclusion about INPs being 0.1-0.5 μm in size was drawn from the comparison of the INP enrichment factors and total aerosol enrichment factors as measured by the PFPC. This is described in lines 349-361 (page 12).

- The reviewer is also correct that we did not probe INPs over 2.5 μm in diameter. Reasons for excluding particles over 2.5 μm in diameter are discussed in lines 123-125 (page 4).

3. **Introduction: most of the result discussion focused on the composition and size information of INPs. Corresponding background information about compositions and sizes of INPs should be expanded in the introduction.**
   Authors' response: The discussion about the size and chemistry of potential INP species has been expanded; see lines 63-71.

4. **Sect. 2.2: it's hard to navigate through this section. Subsections of each instrument or instrument type are recommended. Also it's confusing what instruments are in operation at different time of the campaign (e.g. PFPC, L-ToF-AMS, WIBS), a table listing the operation time period of each instrument might be helpful. When using "the first half and the second half of the campaign", please specify what period is considered as first half and what period is considered as the second half.**
   Authors' response:
   - Section 2.2 now includes subsections.
   - The authors believe that a table describing operation of instruments would be of limited use only. Operating times of the instruments are mentioned in the Methodology section. Figure 1 was updated to include additional instrumentation deployed in the campaign.
   - What is meant by the first/second periods of the campaign has now been clarified. See lines 125-126 and 250-253.

5. **The last paragraph on P9, a few comments regarding the back trajectories:**
   (1) L276: arrival height of 100m above ground level or sea level? The site is 181m a.m.s.l and the inlet is 2m tall. Why doesn't the arrival height match the height of the inlet? Are the trajectories sensitive to the height?
   Authors' response:
   - Arrival height of trajectories was clarified by modifying the sentence in lines 205-206 to read: "Trajectories were calculated for the arrival height of 100 m above ground level.".
   - We selected the arrival height of 100 m agl because we wanted to investigate the effect of surface emissions, and this height is the lowest arrival height for which HYSPLIT can calculate backward trajectories. We also looked at the results when the trajectory arrival height was set to 200 m and 500 m. The trajectories were not sensitive to the arrival height.

   (2) L280: For people who don't know the geography of Europe very well, it's hard to tell which area you're referring to by saying "north-east towards the Kola Peninsula and north-west above the Norwegian Sea". Please add labels on the map, or include a separate map panel.
   Authors' response: Using the text in lines 290-294 together with the Fig. 3A should make it rather clear to the reader where the mentioned areas are. We prefer not to clutter the already busy figure with the labels, but we included clarifications in lines 290-294 to help guide the reader.

6. **Sect 3.2.2: it has been mentioned that the surrounding ground has been covered by snow, how about the area where the air masses come from? Was it covered by snow as well during the campaign? Would mineral dust be a possible source? I agree with the author that the correlation with fluorescent particles made the biological particles a likely source. But the mineral dust particles can't be fully ruled out.**
   Authors' response:
   - It is not possible to quantitatively say whether all the areas where the 48-hour trajectories travelled from were covered by snow. However, looking at this area in Figure 3 and remembering that the campaign took place in February and March, it is quite likely that there was snow on the ground.
   - We do not suspect dust to be a possible source of INPs at the measurement site. We did not find a correlation with larger size bins across the entire campaign, and we did not see trajectories originating from the dust source areas (Figure 3). Mineral dust and its effect on shorter time scales cannot be completely ruled out; however, its presence is unlikely.

Minor comments:

7. **P2 L55 and P3 L75: the discussions of [INPs] in the atmosphere are redundant.**

   The first instance highlights the maximum observed [INP] and compares that value to the typical CCN concentrations. The second instance highlights the range of observed [INP] values. Therefore, while similar, these discussions are not necessarily redundant. No changes made.

8. **P4 L119: typo "dryer"**
   corrected

9. **P8 L245: was it 5% confidence interval or 95%? If 5%, is it reasonable to compare two data sets at a 5% confidence interval?**
   "5% confidence interval" changed to "5% significance level" in both cases, in lines 262 and 266.

10. **P9 L274: typo "HYSPLIT"**
    corrected

11. **Section 3.1.3: the authors should be careful when comparing INP measurements. The size range of INPs, the techniques could be different. For example, Mason et al. (2015) measured INPs using a different technique than PINC and measured INPs up to 10µm.**
    That is correct. In practice, it becomes very difficult to compare [INP] among studies not only due to the size range that was sampled, but also due to other instrumental differences. However, given that the average natural variability over the course of a measurement campaign is so large, the instrumental differences owing to sampled aerosol size cut-offs often contribute negligible error to such comparisons. To acknowledge this, we have added a caveat on lines 317-318, page 11.

12. **Fig.4: how are the size ranges determined? It seems a little bit random. There are some overlapping. Also, does $N_{tot\ >0.5\mu m}$ mean $N_{tot\ 0.5\text{-}2.5\mu m}$?**
    We tried to examine as many size channels as possible in an attempt to decipher the effect of particle size on [INP]. The annotations are correct. I.e. $N_{tot\ >0.5\mu m}$ means everything above 0.5 µm. $N_{tot\ 0.5\text{-}2.5\mu m}$ means only what is between 0.5 and 2.5µm.

However, $N_{tot >0.5\mu m}$ and $N_{tot\ 0.5\text{-}2.5\mu m}$ are practically identical quantities. No changes made.

13. **P11, L352: a recent paper (Si et al., 2018) correlated the activation fraction with the INP size, which supports your observation here.**
The authors are unsure what is meant by the comment. The discussion in lines 362-375 (page 12) is about the rarity of ambient INP as is highlighted by the presented AF values. We do not discuss the INP size here, only the (very small) number.

14. **Fig. 5 and 6: the capital letters A, B, C are used in the figures, while lower cases a, b, c are used in the text.**
corrected to include capital letters in the text.

15. **Fig. 5A and 6A: how are the back trajectories generated? Does the arrival time still correspond to the mid-point of the INP measurement time? It seems like a new trajectory was generated every 6h.**
In Figs. 5 and 6 the trajectories are generated every six hours during the scenario days in question. This was clarified by modifying the sentence in both sections 3.2.1 and 3.2.2 to read: "The 48-hour trajectory analysis showed that during this time period air masses were arriving from…".

References:

Mason, R. H., Si, M., Li, J., Chou, C., Dickie, R., Toom-Sauntry, D., Pöhlker, C., Yakobi-Hancock, J. D., Ladino, L. A., Jones, K., Leaitch, W. R., Schiller, C. L., Abbatt, J. P. D., Huffman, J. A. and Bertram, A. K.: Ice nucleating particles at a coastal marine boundary layer site: correlations with aerosol type and meteorological conditions, Atmos. Chem. Phys., 15(21), 12547–12566, doi:10.5194/acp-15-12547-2015, 2015.

Mason, R. H., Si, M., Chou, C., Irish, V. E., Dickie, R., Elizondo, P., Wong, R., Brintnell, M., Elsasser, M., Lassar, W. M., Pierce, K. M., Leaitch, W. R., MacDonald, A. M., Platt, A., Toom-Sauntry, D., Sarda-Estève, R., Schiller, C. L., Suski, K. J., Hill, T. C. J., Abbatt, J. P. D., Huffman, J. A., DeMott, P. J. and Bertram, A. K.: Size-resolved measurements of ice-nucleating particles at six locations in North America and one in Europe, Atmos. Chem. Phys., 16(3), 1637–1651, doi:10.5194/acp-16-1637-2016, 2016.

Si, M., Irish, V. E., Mason, R. H., Vergara-Temprado, J., Hanna, S. J., Ladino, L. A., Yakobi-Hancock, J. D., Schiller, C. L., Wentzell, J. J. B., Abbatt, J. P. D., Carslaw, K. S., Murray, B. J. and Bertram, A. K.: Ice-nucleating ability of aerosol particles and possible sources at three coastal marine sites, Atmos. Chem. Phys., 18(21), 15669–15685, doi:10.5194/acp-18-15669-2018, 2018.

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