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.

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.

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.

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.

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.

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? (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.

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.

Minor comments:

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

8. P4 L119: typo "dryer"

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?

10. P9 L274: typo "HYSPLIT"

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.

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 $N_{tot\ 0.5\text{-}2.5\mu m}$?

13. P11, L352: a recent paper (Si et al., 2018) correlated the activation fraction with the INP size, which supports your observation here.

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.

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.

References:

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