

## ***Interactive comment on “The seasonal cycle of ice-nucleating particles linked to the abundance of biogenic aerosol in boreal forests” by Julia Schneider et al.***

### **Anonymous Referee #3**

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In the manuscript titled “The seasonal cycle of ice-nucleating particles linked to the abundance of biogenic aerosol in boreal forests”, Schneider et al. describe results from a year-long measurement campaign of ice nucleating particles (INPs) over a forested site in Finland. The data are unique and provide an additional constraint for INPs present over a boreal environment, which are of value to the aerosol-cloud interaction community. I have several comments, most of which I think can be addressed by the authors and I hope to support the publication of this manuscript once these comments have been adequately addressed.

SECTION 1 INTRO The introduction is concise and has room to address two current

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missing items: 1) a description of the goal of the manuscript or study; and 2) because one of the main deliverables of this paper is parameterized INP concentrations at this location, it is important to describe the physical mechanisms that INPs may be generated locally or other regional aerosol sources that may impact the INP populations - this is important in creating a physically-based parameterization that may one day be expanded to modeling studies.

SECTION 2 METHODS The methods describing measurements of INPs is quite thorough. I would like the authors to expand on the uncertainty description for INPs - it is not clear where the systematic error percentages were derived. Were blanks collected?

The inlet for the “additional instrumentation” was heated and “RH remains below 40%” – how does this compare to the inlet used for the INP filter collections? Without knowing the exact RH at this site, I suspect RH can get quite high and may increase collection efficiencies of large particles.

For the other measurements, there seems to be a few missing details: - What exactly was measured by the L-ToF-AMS? What sizes are detectable by this measurement? - Was the WIBS connected to a similar inlet as the other instruments? Where large super micron particles measurable? Could the authors include a comparison between the scattering particle size distribution measured from the WIBS and the APS data?

SECTION 3 RESULTS AND DISCUSSION Figure 1 is nearly impossible to read, consider expanding this figure to take up the full page and also increase font sizes for the axes.

L200-204 – are the increases in INPs, organic aerosol, and fluorescent particles statistically significant? I’m not sure I would consider the organic and fluorescent particle concentrations to demonstrate “clear increases”.

Figure 3 – It looks as though the INP data are smoothed significantly in Figure 3a. This is especially clear when one compared the subfigure (Figure 3b) with the rest of the

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timeline. what kind of curve is fit to the INP data in Figure 3a? If there is no curve, why is it so smoothed? The daily data or monthly averages as points with standard deviations would be a better, more clear, way of visualizing the data.

L208 – This section is titled “Comparison to meteorology and aerosol properties”, yet only temperature and snow cover are compared. Are there other ambient variables that are correlated, for example relative humidity & winds? It is possible that relative humidity or winds may impact emissions?

L258 – While heating does remove a significant portion of the ice nucleation activity, I think it is important for the authors to also acknowledge that the heat-resistant residual is still associated with significant INP numbers. That is, the INP population is not just heat-labile and may not be entirely biological. I think the way the manuscript is currently written indicates that all INPs are biological, but that is not supported by Figure 5, where there are still significant INPs remaining after heating the samples.

L272 – “. . . as they do not include seasonal dependencies.” – this is not necessarily the case. Seasonal variability in INP abundances is accounted for by being linked to aerosol amount (in these cases n500 or nFBAP), which have seasonal variability.

L267 - I understand the WIBS was used to determine the fluorescent biological aerosol particles (FBAPs); How does this method compare to the UV-APS used in the Tobo et al. (2013) study? Given the number of uncertainties associated with measuring fluorescent measurements, can the authors describe the possible differences between the UV-APS and WIBS and how that would impact the performance of the Tobo et al. (2013) parameterization? Also, how does the Boreal forest in this study differ from that measured in Tobo et al. (2013)?

Figure 6 – I think it would be helpful to mention that only data from the HyICE-2018 period were possible to use in the Tobo et al. (2013) (2) parameterization and therefore that panel has fewer points.

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L295 – Should clearly state that this parameterization is based on ground-level ambient temperature, not “ambient temperature”. If T is described as ambient temperature, a model will implement this as the temperature predicted at any level, which I do not think is the intention. Additionally, while I understand the impressive correlation between INP and ambient temperature is inviting for a parameterization, I caution the authors in publishing this as an INP parameterization given that it is highly specific to this location and has not been tested for other years, or is not really supported by a physical mechanism. For example, aerosol-based INP parameterizations have a physical mechanism – an aerosol particle that is seasonally variable with an assumed ice nucleation density. The second parameterization presented in this paper has more physical meaning, linking INP abundance to the physical process of snow melting and exposed surface emissions. Without a clear physical basis for a “ground-level temperature-based” parameterization, I would recommend removing this.

L311 – What is T in this equation?

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