Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2022-216-RC1, 2022

Comment on amt-2022-216

Anonymous Referee #1

Referee comment on "A High-Resolution Record of Ice Nuclei Concentrations Between −20 to −30 °C for Fall and Winter at Storm Peak Laboratory with the autonomous Continuous Flow Diffusion Chamber Ice Activation Spectrometer" by Anna L. Hodshire et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-216-RC1, 2022

The paper reports and discusses field observations of ambient INP concentrations obtained with an improved automated continuously running CFDC instrument. The paper is worthwhile to be published.

I have several remarks and somel questions:

P1, L31: Schmale et al. (2018) is not in the references.

P5, L61-65: Later on the INP parameterization of De Mott et al. (2015, D15) is used. Please explain here how you got the particle number concentration n500 (particles with diameter > 500nm). n500 is the aerosol input parameter in the D15 retrieval.

P6, L66: How large is the uncertainty in the derived surface area concentration? Should be discussed already here! Big particles (with large surface area), but not measured, may introduce a severe bias. Please discuss this point! What is the inlet size-dependent cutoff characteristics?

P7, L106: The 'lack of temperature dependence on INP concentration' must be discussed in much more detail. The argumentation is simply not satisfying, not convincing. 5-K temperature change usually causes one of order of magnitude INP number concentration change (in the favorable -25 to -30C range), disregarding the aerosol type. So, what went wrong with the observations? … was my question when I was reading the first paragraph of page 7 and when studying Figure 4! All possible reasons (and potential uncertainty sources) must be discussed here to clarify this temperature-independent effect.

Figures 4 and 5: The Ullrich 2017 (U17, dust) and the McCluskey 2018 (McC18, clean marine) differ by three orders of magnitude as shown in Figure 4. That seems to be ok! But in several publications it was found that D15 numbers (used in Figure 5) are typically lower by a factor of 100 than the U17 INP numbers for immersion freezing at temperatures of -25 to -30C. You can check that for a given bi-modal size distribution for which surface area (used in U17) and n500 (used in D15) is given. But that means that D15 is only a factor of 10 larger than the pure marine INP values (McC18), and that is wrong, to my opinion. D15 INP numbers are usually a factor of 1000 larger than McC18, so that U17 should be a factor of 100000 larger than McC18 INP numbers. Meanwhile it is known that D15 is much more reliable than U17 (for dust immersion freezing at -25 to -30C).

Figure 2: the plots (c) and (d) are almost useless…. especially (c). Most parts in 2(c) are just dark blue.