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Interactive comment on "Continuous online-monitoring of Ice Nucleating Particles: development of the automated Horizontal Ice Nucleation Chamber (HINC-Auto)" *by* Cyril Brunner and Zamin A. Kanji

Anonymous Referee #1

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Review of "Continuous online-monitoring of Ice Nucleating Particles: development of the automated Horizontal Ice Nucleation Chamber (HINC-Auto)" by Brunner and Kanji.

In this technical manuscript, the authors present a new instrument, HINC-Auto, allowing continuous measurements of INP concentrations at ïňĄxed temperature and humidity conditions. This instrument is based on the design of the HINC chamber with modiïňĄcations brought in order to remove the need of a human operator. It is the first paper to report such instrument capable to measure INP concentration online in an automatic way for period that can extend several months. The scientiïňĄc approach is well built

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Discussion paper

and does not raise any of my concerns. Furthermore, this manuscript is well written and easy to follow. This manuscript is a very valuable source of technicality for the ice community I am in favor of publication after the authors have answer the few questions and recommendations listed below:

Comments:

Line 18: "The interaction between aerosols and clouds contributes to the global energy budget by directly influencing the radiative forcing of the climate system." should read: "The interaction between aerosols and clouds contributes to the global energy budget by indirectly influencing the radiative forcing of the climate system."

line 66: Presently, no automated online INP counter is available (Cziczo et al., 2017; Lacher et al., 2017). A novel paper has just been submitted to AMTD, several weeks after the present one, showing another automatic online INP counter (expansion type chamber): https://amt.copernicus.org/preprints/amt-2020-307/

Line 92: "The surrounding sheath air is dried and filtered before entering the chamber. missing “dried”

Line 121: "A polyvinylidene fluoride (PVDF) spacer physically and thermally separates the two chamber walls."

Figure 3: Red and Green is a bad color combination as it is the most common form of colorblindness.

Figure 3a: How thick is the ice layer and is it included in the calculations (solid line)? The ice layer might decrease the actual volume of the chamber, increase the flow velocity and thus decrease the residence time of particles.

Figure 3: It is confusing. If I understood correctly, the message here is that “To smooth the flow field within the chamber and achieve a more consistent desired unidirectional flow field, a mesh was introduced 20 mm downstream of the sheath air injector holes.” For clarity, maybe the legend of Figure 3 could be changed to: “Figure 3. Calculated

and measured particle residence time in a) HINC (without mesh) for different injector positions at T = 253 K and b) HINC-Auto (using a mesh to make the flow more laminar) at T= 243 K. Box plots from pulse experiments: median with 25/75% quartiles, whiskers: 5/95% quantiles. Median of PIV experiment (circles,T = 288 K) and CFD simulation (crosses, T = 243 K)." or maybe comparing HINC auto with and without mesh would be more relevant?

Figure 4:- "residence time until outlet" is not very intuitive for this figure. I would suggest replacing it by "residence time after entering the chamber" and reverse the time scale. In that way, it is simpler to compare both chamber. The main message here being the effect of cooler temperature entering the chamber, not the total length of the chamber. That way it is easier for the reader to read, that with cooler air, it needs only 20 cm and xx second to reach equilibrium, compare to 30 cm in yy second at warmer sheath air temperature. Also for a and b legend: I suggest to put temperature first as it is the reason, and length second as it is the consequence. a) T sheath air=298K, (original length, maybe not needed) b)Tsheath air=248 K, (10 cm shorter, maybe not needed)

line 210: "The chamber is controlled via a newly developed guided user interface programmed using Python 3.7 and corresponding open source packages. The postprocessing of INP concentrations is done in real time. HINC-Auto can be accessed and controlled remotely if a internet connection is available on site." A suggestion to the authors is to add in the appendix, a screen shot of the software interface could be presented, together with basic parameter that can be set by the user (RH ramping, Temperature profile,. . .).

line 233: "AF is the ratio of all particles, that are detected in the indicated size bin, to all sampled particles, measured with a CPC within the sample flow." A comment about the smaller particle: The authors discuss about the bigger size of aerosol that can enter HINC auto without being lost due to gravitational loss after activation within HINC-auto. However, what about the other extreme of the particle size distribution, the smaller particle? What is the (needed) detection limit of the CPC, and why this detection limit was chosen? in other words, is there a minimum size for the particle to enter HINC that could have a chance to activate but not to grow enough to be detected as ice crystal? There is no restriction for small size particle to enter the chamber, would a CPC with very low cutoff size recommended? Or is there enough small particle loses due to diffusion prior to enter the chamber, in that case what is the cut of size of HINC? or in other word, how can we be sure that all particle that enter HINC and are counted in CPC, can have the possibility to grow big enough to be counted as ice crystal? Can HINC-auto able to measure INP properties of new particle formation that are of size of 5 nm?

Fig6a) Why the activated fraction is not 0 at the beginning? is it a real AF or a background of big non activated particle?

Fig6a) Why in opc channel >0.3 um there is a small but steady increase of the AF between 0.85 to 0.98 Sw? Is it due to the hygroscopic growth of particle just smaller than 300nm, which with higher humidity grow bigger than 300 nm?

line 261: “In either case, the temperature increases in the direction of the air flow, because of the parallel-flow setup of the cooling liquid (see Figure 2b). Therefore, the resulting total temperature variation in the center between the two cooling walls is T $\pm$ 0.24 K.” I don’t understand how the authors get this value of +/-0.24K.

line 270: “The CPC used for validation experiments has a counting uncertainty of $\pm$ 10% which yields in a relative uncertainty in the reported AF of $\pm$ 14%” same comment as earlier. how the detection threshold of the CPC affect the AF?

paragraphe 3.2.1: -So here the improvement is due to the flow which is more laminar in HINC-auto, correct? There is no mention of this in the paragraph. -Is this done at particle of size 200 nm? It is not mention in the text. And if it is monodisperse at 200 nm, why is there an increase of AF bellow 0.975 Sw?

Line 297: "The sheath to sample flow ratio was 12:1 for both chambers." It was stated

Line 267 that the sheath to sample flow ratio should be 9:1. Why is the ratio changed?

3.2.1 Improvement in precision: Why has −40C been chosen to show the homogeneous freezing onset? At this temperature, the water saturation is very close to the Koop line and it might be hard to distinguish between both. Going lower in temperature would show two distinct activation points.

Line 356: "The for the initial scan (solid line) the sheath flow set to 5 L min−1, and for the second scan to 2 L min−1 (dashed line)." please correct. Also, authors could state that at lower sheath flow, SMPS covers bigger particle size while losing smaller particle size.

line 359: "This is contradicting our assumption and the lower sheath flow rate of the DMA for the second compared to the ïňĄrst measurement could be the reason." Could it be that with different sheath flow rates, the author can measure at different size range? And also during the time in the stainless steel chamber, coagulation of particle could have made the particles to grow bigger.

line 364: "For atmospheric relevant conditions at the JFJ with 400 cm−3 $\leq$ N $\leq$ 1000 cm−3 the drop occurs after 8.5 hours, for 95 cm−3 $\leq$ N $\leq$ 200 cm−3 after 13 hours." - how the authors define the drop? (drop of 20%?) -while the data agree with authors statement (data started at 25/12 and at almost 26/12), later in the experiment, after time 2, the data seems to suggest a need of more frequent rewetting. especially the experiment just after "time 2" and the next one.

line 369: "Based on the above experiment, we chose a rewetting time of 10 hours for field applications (Section 3.3)." As the rewetting is automatic and fast, why not choosing a more frequent rewetting to ensure good data, (i.e. to be on the safe side)? or did the author choose a 10 hours rewetting interval because at higher altitude, residence time is shorter, and in result the water depletion is longer?

line 378: "Design changes implemented in a second field campaign started in February

2020 resulted in a median LOD of 1.37 std L−1" Could the author specify which design change has been made, which allows a decrease by factor of almost 3 of the LOD? This would be very valuable for the INP community.

typo: caption fig 8: "with identical particle residence times. of" remove "."

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