**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Manchester Ice Nucleus Counter (MINC) measurements from the 2007 International workshop on Comparing Ice nucleation Measuring Systems (ICIS-2007)" *by* H. M. Jones et al.

Anonymous Referee #2

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Review

for the manuscript:

Manchester Ice Nucleus Counter (MINC) measurements from the 2007 International workshop on Comparing Ice nucleation Measuring Systems (ICIS-2007) by

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submitted to Atmospheric Chemistry and Physics - Discussion

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This paper describes a new instrument to measure ice nuclei based on the design of the Colorado State University continuous flow diffusion chamber CFDC. The new instrument, the Manchester ice nucleation chamber MINC is described in detail and selected measurements from the ICIS2007 workshop are presented and compared to data taken with the CFDC itself during the same workshop. I suggest to accept this manuscript for publication in ACP after major changes which are discussed here, first in general and then in more detail with page and line references:

In general, I have the feeling the paper assumes the reader to be already familiar with similar instruments down to very detailed aspects such as the data collection of the OPC. Without this knowledge, some parts of the paper are hard to understand. I would recommend to revise the document such that someone without detailed knowledge on these instrument details can read and understand it.

The authors are presenting many important and in most cases valid facts. However, they leave it to the reader to make connections between them. The paper would be a much better and easier read, if these connections were made by the authors. I will give a few examples later but suggest that the authors critically revise the document with this in mind.

Another more general remark: At some points the authors argue based upon facts or data not presented in this paper. Please, wherever possible, add the data (e.g. calibration data) needed for arguing so the reader can follow the arguments and come to his own conclusion. I will give some examples later.

Please be consistent in supplying information for used materials and components, (Name, Manufacturer).

P19278, lines 25/26: Please add a statement that it is clearer to the reader, that atmospheric IN mainly act as heterogeneous nuclei and the latter is the focus of the presented measurements. This connection is currently not made.

P19279, lines 1 and 8,9: Please make a direct connection between the mentioned mechanisms (line 1) and the conditions they are referring to (lines 7,8).

- , lines 9-13: Runaway sentence is a bit hard to read because many facts are listed. Would be better to split this information over 2-3 sentences.

- , lines 14... :again a very long sentence and three instead of the mentioned two options are listed.

P19280, line 26: . . . in Rogers (1988). The main.....

P19281, line 10: It would be very helpful to have a drawing (to scale) of the cross section of the chamber head to assess the design and compare it to other instruments. The design of this region is crucial for a faultless operation of the instrument without artifacts in the measurements. How are the sample and sheath layers merged? How are the flows distributed equally over the angular layers (e.g. by series of orifices or gaps)? - , line 27: How do you control/measure that the air is slightly supersaturated with respect to ice in the evaporation section?

P19282, line 20: Can you specify what material is used as hydrophobic plastic here (Type, Name).

Next lines: Can you specify how the residence time is split between nucleation/growth section and evaporation section? Please make clear that growth of ice crystals only takes place in the first section. Referring to the fact mentioned on p19283, line 14 it would be good to know how long the section with stable established supersaturation really is.

P19283, lines 1: Please replace (modern roof space insulation) with a brand name and supplier. Please specify thickness of the bubble wrap and the Armaflex layer).

P19283, line 7: What about the "funnel" section at the end of the evaporation section? Can you specify somewhere the cross section of this region (angle and width of gap)?

Next sentence: Can you explain in more detail where and how a transient supersaturation may form. The following description of how you are trying to overcome this problem is then easier to understand.

P19284, line 1: Is the second expansion valve of the same type as the first one?

- , line 25: replace experiment conditions with supersaturated conditions (because the temperature in the evaporation section will also change with changing experimental conditions).

P19285, lines 4,5: Can you explain this in more detail? As far as I know this detector normally operates at 30 LPM so this removal should not be necessary? It appears, that your detector has a standard flow rate below 10 lpm (p19287, lines 1-2). In this case: What is the effect of the removal of this orifice on the flow field inside the detector and therefore the detection efficiency? Turbulences within the detector due to the increased flow rate may influence the efficiency.

- , Aerosol losses: I have some major concerns regarding the loss measurements and the corrections which are made based upon these: First, at these high number concentrations mentioned, coagulation may become an efficient enough mechanism to reduce the number and shift the mean diameter of a given aerosol concentration. However, I am even more concerned with the corrections which are made for measured data. Please explain in detail step by step (with formulas if necessary) how this correction is being applied. The reason why this may be or become critical is the fact that the IN efficiency of a given aerosol is known to be a function of particle size (you even mention this in the discussion somewhere). I don't see how it is easily possible to deconvolve these two effects which are both size-dependent because for the ice crystals you detect you have no information about the initial particle size. This may have a significant impact on the quality of the derived data and may be the source of systematic errors. It gets even more complicated when measuring samples which may be externally mixed (which may be even true for some of the samples used in the ICIS workshop) where

the IN efficiency may be significantly different for individual modes within the sample.

P19286, line 14... : It would help if you presented the data and the fit for the reader either in form of a figure or table. Please provide the data for the conclusion you make in the next sentence (. . ..were found to be much higher....) before and not after the conclusion. Again, a detailed table would be very helpful instead of the data listed in the text later on. Please specify e.g. by formulas with parameters how the corrections are made. It is also important to know how the sample position is determined because you mention that the conditions for the sample are corrected.

P19287, whole section OPC: The setup used here for detecting and counting particles by using an OPC as a source for analogue pulses which are then converted into binned digital counts (if I understand this correctly) is very hard to understand. I am not sure if the counting efficiency reported by CLIMET for the OPC for the two size classes (high gain, low gain) can be used for this setup as the detection efficiency depends on the setting of threshold values in the custom made electronics described later on line 11 (comparator, Schmidt trigger). For someone unfamiliar with the latter electronic terms, could you explain what they do to the signal? How many size bins do you have in your setup and what are the sizes of these bins? How do these correlate with the threshold of 3 micron you describe later? I recommend to re-arrange this section so that reader can easier follow the details of the setup (e.g. by first describing the whole setup by following the signals during processing). Please make a new paragraph where you only describe the calibration of the OPC with the APS, Grimm detector etc. If applicable, one could compare the PSL calibration then with the efficiency reported by the manufacturer but be careful here because the detection efficiency may be modified by the non-standard flow rate anyhow. A figure depicting the signal and data flow and a figure/graph with the calibration system and data would help so that this section is easier to read and understand. In the end you are discussing calibration data and interpreting it which is not accessible to the reader. If you feel it is important for the reader, please present this data, otherwise don't discuss and comment on it.

Section numbering and manuscript structure: Beginning with section 4 (Results) the structure of the remaining document is unclear to mean. What is the purpose of having Section 5 (Detection of IN) with subsections which describe the results of the experiments in details when there is already a very brief (summary) section named results? I recommend a more logical re-structuring of these sections.

Figure 3 : please use a log-scale for the activated fraction as in Figures 4 and 5.

P19290, line 3: Since the dates of operation were already reported, the first sentence may be removed as it does not fit in this section.

- , line 9: ...here AS THE . . ... Also: please clarify that this is the way you define the ICE activated fraction assuming that all particles above three microns are ice crystals.

- , line 13: As already mention before: Can you please plot the initial and final size distributions before and after the transmission correction? It is then easier to discuss possible effects of size on the detection efficiency as you mention later in the discussion or conclusion that this may be a problem. I am wondering how you plan to operate the system in the field then. Do you plan to always measure size distributions in parallel to be able to apply this correction?

P19291, lines 6-15: It would be better and help the structure of the document to discuss differences between the instruments in the appropriate paragraphs in section 2 and only repeat those here briefly which are discussed in connection with the results presented here (e.g. diffusion losses and Nafion drier).

- , lines 27/28 ..and referring to Figure 5: I cannot follow he conclusion that the corrected CFDC data (for 3 micron in red) fits better to the MINC data than the uncorrected data in black. Visually I would tend to say the opposite. However, it would be better to use mathematical (statistical) methods to proof this. The scatter of both datasets for the two individual measurements is larger than the visual misfit between the datasets. For this reason it would be interesting to look at the reasons for this (changes in the

aerosol). What do the experiment numbers in Figure 5 mean? Ar these different days or experiments and times? As you discuss some of this in the following sentences I would remove this one sentence. It does not fit so well if you read it after reading down to P19292, line 6.

P19293, line 4: Does this data in Figure 7 correspond to some of the measurements presented in the previous Figures?

- , line 17-19: I think you want to make the valid point here that the experiment-to-experiment variation in the aerosol (size distribution but maybe even composition?) was significant and may contribute to the uncertainties between individual measurements with your instrument and the differences between MINC and CFDC (and others). For this conclusion it would be good if you could specify or explain which data was taken with the same aerosol (same experiment, even when the time was different) and where you are comparing data from different experiments (different aerosol preparations). Were the conditions under which the aerosols were produced always identical? Please re-word this so that these points become clearer.

- , paragraph starting on line 23: May this be the result of two different aerosol modes (e.g. size or maybe chemistry or both) to which the two instruments are differently sensitive (due to the losses you describe for your inlet system). See also one of my earlier comment on that (correction for losses).

P 19294, lines 5-8. What kind of trend analysis did you perform? Linear regression? If yes, did you expect a linear trend in these values with temperature? Why?

- , line 24: Particle_ concentrationS.

P19295, line 1: due to A difference....

- , lines 3-5, as mentioned earlier: How did you analyze the data to come to this conclusion?

- , lines 10/11: To my knowledge: As long as the evaporation section is isothermal and

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has ice covered walls (= meaning it is ice-saturated), ice crystals should not evaporate in any case. How do you come to the conclusion that it may be sub-satured, the condition that ice crystals may evaporate? If there is a slight temperature difference between the walls you would even have a supersaturation. In the description of the instrument (p.19281,line 27) you state that you keep the section slight supersaturated with respect to ice.

P19296, lines 1-2: How do you know that you have transient conditions which are problematic? I am not sure that your instrument has these problems...

In general I would move the suggested improvements into the section: Conclusions, since the focus of this paper is an instrument description. But I leave it to the taste of the authors if they agree with that or leave it like it is.

- , line 20: I guess you rather wanted to refer to Sect. 6.1 here.

- , line 25...: The requirement to grow ice crystals is still given for most phase discriminating detectors for two reasons: 1. Sensitivity: They might not be able to detect ice crystals AND determine the phase accurately if the particles are too small. More important: How do the unactivated (dust) particles interfere with the phase discrimination for comparable sizes?

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