

Interactive comment on “Comparative measurements of ambient atmospheric concentrations of ice nucleating particles using multiple immersion freezing methods and a continuous flow diffusion chamber” by Paul J. DeMott et al.

Anonymous Referee #2

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In the submitted work DeMott et al. present measurement data for a number of INP collection and immersion freezing methods and compare these to (nearly) contiguous measurements from the CSU CFDC, an instrument that has been running for many years and represents probably the most well understood and calibrated instrument currently in the field. This data is unique in that it attempts to represent sampling done in close temporal and spatial proximity from a number of field locations in the western USA. Furthermore, the work focuses on field data, for which there exists relatively few

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representative comparisons for different sampling instruments. Thus the work fits well into the scope of ACP and is an important contribution to the community.

The manuscript is well-written and I believe is publishable “as is” subject to one general comment and a few minor comments that if addressed I believe would add value to the analysis and discussion.

General Comment:

One difficult aspect of digesting the submitted work is that it is at times unclear what instrument specific discussion points can be found in the cited papers (generally dedicated to individual instrument systems) and what is more specific to what is presented in this manuscript. Although, I expect many interested readers have also read the cited literature it is difficult to keep it all at the forefront of ones thoughts. Thus, I would suggest that in revision the authors attempt to more clearly enumerate where instrument specific information can be found in referenced literature and where they are making new statements. For example, the issue of sample storage is raised multiple times but addressed in different ways – it is a challenge to repeatedly return to the literature to see how different instrumental systems have responded to (or not responded to) sample storage and what if any error this introduces.

For some such issues tables, for example including the instrument specific sampling and temperature uncertainties or tolerances, in the text or supplementary material may be beneficial.

Minor Comments:

• lines 215-220 f_{nu} and f_{ne} must be explicitly defined. In the cited literature f_{nu} exists for 2 size ranges and it is unclear what is referred to here. Possibly a combination of the two? Furthermore, at least a sentence or two should be dedicated to an explanation of the origin of these correction factors. This is where the link to the cited material should be provided. Also, please make clear if the parameters used are identical to those

C2

previously published, or are they specific to the particular sample analysis? A short reading of the two papers does not make this evident.

•lines 267-268 See my above example regarding the storage issue. '(not shown)' is a very unsatisfactory parenthetical. Perhaps a better description could be made. e.g., X randomized samples were tested for storage effects by freezing before and after Y days/weeks/months of storage and showed no statistically significant.....

•line 464 'holding for hours at one temperature' The wording is strange here.

•lines 475-490 The discussion of the factor of 3 added as a line in Figure 3 should at a minimum be introduced earlier. Preferably when the figure is introduced. Furthermore, it seems a somewhat deeper discussion of the meaning of this line is missing – that could remain in the discussion. It is clear that the DeMott 2015 et al., paper suggests that this correction factor is used for field measurements of immersion freezing of natural mineral dust for the CFDC – when comparing to a parameterized model of INP. How this relates to the results from other instruments etc. is less clear (e.g., Each of these instruments may have their own c.f. with regard to the DeMott parameterization.). My best understanding is that the 'true' aerosol concentration of (mineral dust) INP as measured by the CFDC should lie somewhere between (inclusive) the 1:1 and factor 3 lines. However, this estimate is also subject to the size limitations of the instrument and parameterization (0.5-2.4 microns). Given the other instruments also operate outside of this range a deeper discussion that ties these links seems warranted. Thus, I also suggest least-squares trendlines be added to the Figure 3 panels or their exclusion defended (For example these trends are essentially explored in Figure 4, but the link is not explicit). Fitting the Figure 3 data by eye, it appears that any trendline would be steeper than the 1:1 line. Is this truly systematic? Are there potentially different explanations for the different instruments? Including at least representative error bars in panels a-c may also assist the discussion.

•Figure 1. Please be explicit (throughout text) with regard to the confidence intervals.

C3

Poisson error, Gaussian?

•Figure S1. Using n_{inp} as the y-axis label maybe confusing. The upper points are actually INP per concentrated liter of sampled air if I understand correctly.

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C4