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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intercomparing different devices for the investigation of ice nucleating particles using Snomax® as test substance" *by* H. Wex et al.

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The experiments described in this paper make valuable contributions. It is a good exploration both of the specific material tested (Snowmax) and of the degree of agreement that can be achieved among different measuring systems. The authors are to be congratulated on the organization of the cooperative effort and on the cohesive analyses of the results. Some points are raised in the following for the authors' consideration with the aim of clarifying some aspects of the data presentation and interpretation.

22327/2 In a round robin tournament all teams play all other teams, so the use of

this term is not strictly valid here, but it is suggestive of the intentions of the organizers.

22330/20-22 The logic of these two sentences is elusive. Minimal time-dependence is not related to the shape of the spectrum (frequency) of INM of different activities.

22331/3 It would be helpful to point out that in this study ice nucleating ability is expressed per unit mass of material, not per unit surface area as is done in some other studies with solid materials. This is due to the two complicating factors discussed later: changes in particle size due to dispersion as dry particles, and the unknown shapes of the particles. Are there other reasons?

Eq.(1) is written with the implied assumption that all drops have identical values of V_d and C_m. Stating this would help the later discussion.

Eq. (2) and (3) are written for size-selected samples, i.e. same d_p

22332/7-> "INM" is used in places where it really refers to Snowmax particles as they exist and are distributed into droplets before the freezing experiment. This is confusing. Perhaps the author would consider using SP to refer to the dry particles.

22332/14 Eq. (4) is just a repetition of Eq. (1) with different symbols. The value of λ is given by the product $n_\mathrm{m} \cdot C_\mathrm{m} \cdot V_\mathrm{d}$. The validity of these equations is constrained to $\lambda < 1$ (cf. Vali, 1971); the restriction of use of these equations to fractions less than unity (below the plateau region) follows from that. This makes much of the discussion regarding Fig. 9 and most of section 5.1 somewhat superfluous. Not incorrect, but not necessary if the f 1 region is excluded by definition. As pointed out in 5.1, the Poisson distribution is only confirmed for $\lambda < 1$.

22332/16 fraction of particles, not number

22332/19-20 The significance of this sentence is worth spelling out. I understand it to mean that the plateau is not due to having more than one INM per droplet but because all SP become active (INM) near -10 or -12 °C. Of course this fact is collaborated by the plateau observed in the suspension measurements (Fig. 10.).

22332/24 It should perhaps be made a little clearer to the reader that the uncertainty derived here is an error range estimate, quite different than probability of allocation of particles considered in the preceding paragraph. The caption in Fig. 2 refers to 'theoretical considerations' - while this is true, it is simpler to say that the standard deviations reflect predicted uncertainties due to limited sample sizes.

22344/1 The emphasis in this section is well placed. The two temperatures at which sudden rises in concentration are observed indicate the frequent occurrence of two types of sites with different characteristic temperatures. This type of sudden rise in not common, most materials exhibit gradual rises in the frequency of sites of different characteristic temperatures. The value of omega defined by Vali (2014) here observed is about 4.5, well above the values shown in Table 1 of Vali (2014) for heterogeneous nucleation. If the results were displayed in a differential rather than cumulative spectral form (Vali, 1971 and 2014) the presence of two peaks in activity would be visually apparent.

22347/26 True, as long as the manufacturer doesn't change the manner of production of Snowmax. Also, if the material currently available is as tolerant to handling and generation, etc., perhaps there is no need for the caution expressed in the last sentence of the Abstract.

22348/7-8 "... plateau value below 1..." is not well expressed

22348/10-22 Doesn't the presence of the plateau in the particle-method measure-

ments argue against it being an artifact in the suspension-method measurements? If so, the argument here presented is misplaced and needless. While the mentioned limitations do in fact exist they were not a factor in this experiment.

22352/18 -> It is surprising that the authors use a time-dependent parameterization which is tailored to LACIS's short exposure time and the SBM with a relatively arbitrary time. The analyses in this paper are based on the singular approximation (22330/15) and the time-dependence found by Budke and Koop (2014) is acknowledged to be small and is neglected in the analysis. A function for n_{m} versus T alone (as is shown in the right-hand side of Eq. (6) is an adequate representation of the results.

References:

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