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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Equilibrium droplet size distributions in a turbulent cloud chamber with uniform supersaturation" *by* Steven K. Krueger

Anonymous Referee #2

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General Comments

The author derived analytical equilibrium solutions from the equations which govern the evolution of the droplet size distributions [Eqs. (2) and (6) in the manuscript]. The motivation of the author's study is to understand the experimental results of the so-called Pi-chamber, a laboratory cloud chamber at Michigan Technological University, which recently obtained the equilibrium droplet size distributions under the turbulent cloud conditions. To model the condition in the Pi-chamber, the author assumed (i) there is uniform supersaturation [terms with the factor ξ in Eqs. (2) and (6)], (ii) cloud droplets are activated continuously from externally injected CCNs [$A(r)$ and $B(s)$ in

Eqs. (2) and (6), respectively], and (iii) cloud droplets are removed from the system with the rate proportional to the droplet squared radius [terms with the factor h in Eqs. (2) and (6)]. (iii) is explained to be a simple model for the loss of cloud droplets due to sedimentation. From these assumptions, the author derived analytical solutions of cloud droplet size distributions at equilibrium state [Eqs. (12) and (17)], and also derived various analytical expressions associated with those solutions, such as various moments ($\overline{r^n}$, $n = 1 - 5$), precipitation flux, condensation rate, etc.The author then used these results to infer the condition in the Pi-chamber (Sec. 6), inferring the actual supersaturation in the Pi-chamber (from 0.008 to 0.6 % which seems to be reasonable) and also demonstrating the importance of the truncation radius of the size distributions measured in the Pi-chamber.

The form of particle loss rate which is proportional to $-k_1 r^2$ is originally proposed by the author in the present study. Based on the present study, Chandrakar et al. (2019, QJRMS, doi: 10.1002/qj.3692) has recently confirmed the validity of this form of loss rate using the experimental data. It should also be noted that the applicability of the author's analysis, such as inferring the supersaturation in the Pi-chamber and checking the importance of the truncation radius, are not necessarily limited to the case considered in the author's present study. In principle, these ideas of analysis can be applied to other cases such as under the condition of fluctuating supersaturation.

I think the author has made an original contribution and the manuscript is appropriate for the Atmospheric Chemistry and Physics. I only suggest minor revisions before acceptance as below.

Specific Comments

Size distribution at $r = r_a$

From Eq. (9), it can be written as below

$$v(r_a) = \frac{r_a}{\xi} \int_{r_0}^{r_a} A(r) dr.$$

On the other hand, from the general solution Eq. (12),

$$v(r) = Dr \text{exp}(-Cr^4/4).$$

Does this mean that Eq. (9) can be related to Eq. (12) by substituting $r = r_a$ in Eq. (12)? I think it might be informative for readers if the author adds an explanation on how Eq. (9) is connected to the general solution Eq. (12).

Nominal supersaturation in Fig. 6

In page 14, line 4, the author cites Rogers and Yau (1989) and explains that the critical radius for injected NaCl particles is about $r_* \sim 0.6\mu$m. I think the same textbook also gives an estimation of the critical supersaturation for those particles and I expect it to be about $S_* \sim 0.1$%. On the other hand, according to Figure 6, the inferred nominal mean supersaturations for the Pi-chamber experiments with two largest number densities of cloud droplets are smaller than 0.01% ($\overline{S}_{\text{nominal}} < 0.01$%). This seems somehow strange, because aerosol particles cannot be activated to cloud droplets if the supersaturation $\overline{S}_{\text{nominal}}$ is much smaller than the critical supersaturation S_*. Does the author have possible explanations for this apparent discrepancy? If so, providing those explanations in the manuscript might be helpful for readers.

Technical Corrections

1.	p. 3, Sec 2.3, line 1	:	$u/h\Delta t = k_1 r^2/h\Delta t$	$\longrightarrow$	$(u/h)\Delta t = (k_1 r^2/h)\Delta t$
2.	p. 4, line 6	:	$k_1 r^2/h\Delta t$	$\longrightarrow$	$(k_1 r^2/h)\Delta t$
3.	p. 9, Figs. 3 & 4, y-axis	:	pdf$(\mu$m$)^{-1})$	$\longrightarrow$	pdf$((\mu$m$)^{-1})$ or pdf$(\mu$m$^{-1})$

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