Response to the reviewer's comments:

Supersaturation (s) in cloud is hard to measure even with an in-situ instrument that aims at measuring it directly in cloud, mainly due to the difficulty of measuring humidity in cloudy environment. Estimating it utilizing ground remote sensing instruments would be even more challenging. The authors in this manuscript propose a new approach to estimate s in such a way. The formulation of the equation that derives s seems to be done solidly and the key variables in the equations seem to be obtainable from radar and lidar output with some assumptions. If s can be estimated reasonably well by the proposed method, it would benefit a lot on the effort to estimate cloud supersaturation profile in a continuous manner, which would be impossible from aircraft measurements that are inherently episodic and expensive. Despite such enormous benefits, however, the uncertainty of estimated s with ground remote sensing data seems insurmountably high, according to the results described in this manuscript. Not only the PDF of the estimated s is much narrower than those obtained from in-situ cloud microphysics measurement but also the mode values of s do not seem to match well among each other, about which the authors did not mention anything. Moreover, there is no guarantee that the estimated s from the in-situ cloud microphysics measurement represent true s in cloud because such estimation itself is based on the big "quasi-steady state assumption," rather than from direct and correct humidity measurement. So even if they do match well, that does not mean that the estimated s from ground remote sensing data represent the true s. Therefore, I am not sure if realistic in-cloud supersaturation values are obtainable from this approach. Perhaps this approach of estimating s can still be very useful as a way to get a relative measure of in-cloud supersaturation. In that sense, I urge the authors to do s estimations in some other clouds using the same approach and see how much they are different from the one presented in this manuscript. Matching in situ cloud microphysics measurement may not be available for these other clouds but that is ok. Here the purpose is to demonstrate the capability of this approach to estimate different s distribution for different clouds.

We would like to thank the reviewer's constructive comments. We agree with the reviewer that, even when supersaturation fluctuations estimated from our method agree with that estimated from in-situ measurements, we still don't know the truth, and we believe that nobody knows the truth at present. As the reviewer states "this approach of estimating s can still be very useful as a way to get a relative measure of in-cloud supersaturation"—this is also the idea we want to express in the manuscript. For example, Figure 4b shows the s profiles in stratocumulus clouds. The most important and useful information is **not** to say whether s fluctuation is 0.3% or 0.4% (which depends on those retrieval variables), **but** that there are relatively narrower or broader regions of s fluctuation. It is interesting and useful to investigate the s distribution for different clouds in the future, but it is beyond the scope of the scope of this manuscript for the following reasons. There are two main points to be made.

First, we think it is important to convey to people that there is a new method and that it works by evaluating the retrieved variables and estimated supersaturations with in-situ measurements before we extend this method to estimate supersaturation under other cloud conditions. To do so, we went through all cases during the ACE-ENA field campaign to find that the case we present in this study is the best one and the only one suitable for evaluation due to the stable cloud base and long flight leg. "Matching in situ cloud microphysics measurement may not be available for these other clouds but that is ok" is meaningful after we show some evidence that our method compares reasonably well with in-situ measurements, which is the scope of this study.

Second, we agree with the reviewer that it will be very interesting to "look at other clouds using the same approach and see how much they are different from the one presented in this manuscript." However, one should be very careful to estimate supersaturation for different cloud conditions. The reason is that supersaturation estimated based on our method strongly depends on the retrieved variables, and the retrieved variables might have different uncertainties under different cloud conditions. The advantage of the case we choose in this study is that the environmental conditions are very stable (Figure 1), which is the best case for retrieval and evaluation compared with other days. Further, as mentioned by the other reviewer, use of a stratocumulus cloud minimizes the effects of lateral entrainment and mixing, which may not be true for other cloud types (except for their more-spatially limited cores).

In summary, we agree with the reviewer that supersaturation estimated based on our method might not "represent the true s", because nobody knows what truth is. But it is a method to get "a relative measure of in-cloud supersaturation". We believe that there could be rich scientific implications of this study, as the reviewer mentioned, to look at the "supersaturation in some other clouds". It is interesting, but more time and effort is needed. In this study, we choose one aspect of the scientific application: the supersaturation fluctuation profile in the stratocumulus cloud (Figure 4), which is not achievable from in-situ measurements. The relative broader regions of s fluctuation suggest the important roles of dynamics and thermodynamics.

We add some discussion in the manuscript to highlight the merit and limitation of this study,

"…**It should be mentioned that our approach of estimating s cannot obtain the true supersaturation at the current stage due to (1) the difficulty of directly measuring s for evaluation and (2) the uncertainty in retrieved variables used in our method, but it still can be very useful to get a relative measure of in-cloud supersaturation. For example, for the profiles in Figure 4b, the most important and useful information is to say where there are relatively narrower or broader regions of s fluctuation, not whether the s fluctuation is 0.3% vs. 0.4%, as the latter would require exact accuracy in the retrieved variables. It is interesting and useful to investigate the s distribution for different clouds in the future.**…"

Some specific comments:

P2, L12: Why might this method (Yum et al., 1998) overestimate s due to kinetic limitations? Due to counting unactivated but large haze droplets as activated cloud droplets in cloud probes? Explain more clearly.

"~~However, this method might overestimate s due to kinetic limitations~~ **s estimated based on this method indicates the maximum supersaturation at cloud base for droplet activation and does not represent the real-time in-cloud supersaturation fluctuation**…"

P4, L26: No explanation is given on ground remote sensing instruments. Add brief explanation.

We add more discussion about the remote sensing instruments in the manuscript.

"**The site's zenith-pointing Ka - band cloud radar (KAZR), ceilometer, micropulse lidar (MPL), and microwave radiometer were used to derive the information on vertical air velocity, liquid water content, and cloud droplet number concentration. The vertical resolution of the KAZR is 30 m (one range gate), and for the ceilometer and MPL is 15 m. The temporal resolutions of the KAZR, ceilometer, and MPL are 2 s, 16 s, and 10 s respectively. At the distance of the observed targets, the radar and lidar beams are, respectively, about 3 m and 2 mm wide. The microwave radiometer employs three receiver channels operating at 23.84, 31.4, and 90 GHz, providing liquid water path estimates at a temporal resolution of 3 s. Specifically**,…"

P5, L8: Nd is estimated using a lognormal distribution assumption. This distribution is similar but different from the Weibull distribution that was originally used for formulating s equation (Eq. 11). Explain the effect of such change.

The method we use to retrieve cloud droplet number concentration is based on Snider et al. (2017), in which a lognormal size distribution is assumed. If we use a Weibull distribution, the retrieved cloud droplet number concentration will be 25% larger. Such difference is smaller than using different retrieval methods, as shown in Figure 5d. Using the exact same method and size distribution used in Snider et al. (2017) is helpful to compare the retrieved cloud droplet number concentration with other studies, and it will be easier to extend the application of equation 11 for people who only use retrieval products. To make the text clear and consistent in the manuscript, we add more discussion,

"**It should be mentioned that the method we use to retrieve cloud droplet number concentration is based on Snider et al. (2017), in which a lognormal size distribution is assumed. If we use a Weibull distribution, the retrieved cloud droplet number concentration will be 25% larger (detailed in the Appendix B). Such difference is smaller than using different retrieval methods, as shown in Figure 5d. Using the exact same method and size distribution used in Snider et al. (2017) is helpful to compare the retrieved cloud droplet number concentration with other studies, and it will be easier to extend the application of equation 11 for people who only use retrieval products.**"

"**Appendix B: Retrieving cloud droplet number concentration**

Based on Snider et al. (2017), the cloud droplet number concentration can be retrieved from the lidar backscatter coefficient (σ) and liquid water content (q_l),

$$N_d = \frac{2e^{3\sigma_x^2}\rho^2}{9\pi}\frac{\sigma^3}{q_l^3},$$

where the cloud droplet size distribution is assumed to be lognormal and have a standard deviation of $\sigma_x = \ln 1.4$. If we assume the cloud droplet sizes to follow a Weibull distribution (Equation 10), the cloud droplet number concentration has a similar relationship between σ and q_l but a different prefactor,

$$\boldsymbol{N_d = \frac{\rho^2}{8}\frac{\sigma^3}{q_l^3},}$$

Specifically, the retrieved cloud droplet number concentration using a Weibull distribution is 25% larger than if a lognormal distribution was used."

P9, L11-12: 20% overestimation is not exactly meaning that the true value is 0.8 times the retrieved value. Just use one metric to avoid confusion.

To make the text clearer, we modify it as,

"Specifically, we will assume that the "true" values of those variables, respectively, are **0.5x, 0.8x, 1.2x, and 2.0x** our original retrieved values."

P10, L4-5: Similarly confusing. We do not usually say "overestimate 0.5 times." Just say 0.5 times the true value (underestimation) or 1.2 times the true value (overestimation).

To make the text clearer, we modify it as,

"The "true" value of each retrieved variable is assumed to be systematically smaller (**0.5x or 0.8x**) or larger (**1.2x or 2.0x**) than the original retrieval."