Response to the reviewer's comments:

The manuscript describes a new approach to retrieve in-cloud water vapor supersaturations based on radar and lidar measurements. The approach is applied to data of the ACE-ENA field campaign, including a comparison with in-situ measurements and an assessment of uncertainties. The manuscript is overall interesting, well-written, and I support its publication in Atmospheric Measurement Techniques once my minor concerns are addressed.

We would like to thank the reviewer for careful reading of the manuscript and helpful comments.

P. 1, l. 13: The supersaturation in clouds does not only depend on the updraft velocity of a cloud (to which "calm" and "energetic" most likely refer to) but also on the cloud microphysical composition (see, e.g., Grabowski and Wang (2013, doi: 10.1146/annurev-fluid-011212-140750)).

We modify the text based on the reviewer's comment:

"~~Supersaturation (s), varying from less than 0.1\% in calm stratiform clouds to larger than 1% in energetic convective clouds, plays a crucial role in cloud droplet formation and growth (Lamb and Verlinde, 2011).~~ **Supersaturation, which plays a crucial role in cloud droplet formation and growth, depends on air vertical velocity and can also be adjusted by cloud microphysical composition (Lamb and Verlinde, 2011; Grabowski and Wang, 2013).**"

P. 2, ll. 1 – 2: The adverb "simultaneously" describes a process happening at the same time. It is, however, also important that the measurements are co-located in physical space.

Agreed. We modify the text as,

"If we can simultaneously measure water vapor pressure (e) and temperature (T) **at the same location**, …"

P. 2, ll. 11 – 12: It is misleading to write about a specific location of the effective s determined by the CDNC closure method ("s in the CCN counter"). To my understanding, the s describes the maximum s at cloud base where it is able to activate the number of CCN that are measured in the CCN counter.

Agreed. To make the sentence clear, we modify it as,

"For the CDNC closure method, s in the CCN counter is ~~taken to be the effective s in clouds~~ **used to describe the maximum s at cloud base** if ~~the number concentrations of CCN and cloud droplets are similar~~ **the number of activated CCN in the counter is similar to that of cloud droplets**."

P. 2, ll. 19 – 21: Since Eq. 2 is not an integral, changes in the microphysical composition in time and space are not important. However, I agree that once Eq. 2 is applied to a larger volume (i.e., it is integrated), changes in the microphysical composition will matter, especially if they are fast as it is the case for small Nd and large w as stated correctly by the authors. Therefore, the sentence should read as: "[…] for which the time scale for the change of cloud microphysical properties is shorter than the time scale for the change of environmental conditions."

We still think that the quasi-steady state assumption does not hold when the time scale for the change of cloud microphysical properties (which is the phase relaxation time) is **longer (not shorter)** than the time scale for the change of environmental conditions (e.g., the mixing time). The phase relaxation time describes how quickly a cloud can response to the change of environmental conditions. A longer phase relaxation time means the cloud responds slower to the environmental change, in other words, cloud needs a longer time to return to the quasi-steady state. In order to eliminate the potential confusion, we modify the text as,

"However, this assumption might not be valid in clean (low N_d) and/or vigorous (large w) clouds, ~~for which the time scale for the change of cloud microphysical properties is longer than the time scale for the change of environmental conditions~~ **for which clouds need a longer time to return to the quasi-steady state for the change of environmental conditions**."

P. 3, Eq. 4: It might be helpful to give the reader a hint on why one can neglect the time dependency of f(r) during integration (although it becomes apparent after some thinking).

The reviewer raises a subtle point! Equation 4 follows from the continuity equation for particle number, which is conserved for condensation/evaporation, for which an integration by parts uses the fact that f(r) vanishes at the limits of integration. We add more description to the manuscript:

"**It should be mentioned that equation 4 follows from the continuity equation for particle number, which is conserved for condensation/evaporation, for which an integration by parts uses the fact that f(r) vanishes at the limits of integration (McGraw and Wright, 2003)**."

P. 3, Eq. 8: By restricting the temporal change of LWC to changes in height and then vertical velocity (Eq. 8), other important processes affecting the supersaturation (foremost entrainment and mixing processes) are neglected. I believe that this simplification is valid in stratocumulus, in which entrainment and mixing are less important than in cumulus clouds. And in fact, the authors have chosen a relatively low turbulent stratocumulus cloud (p. 4, ll. 19 – 20) in which the inherent assumptions of Eq. 8 are probably fulfilled. However, I strongly recommend the authors to comment more on the implications of Eq. 8, especially the neglected cloud processes, to account for potential other applications of this approach.

Very good point. We add more text on the limitations of Eq. 8.

"~~However,~~ **If the lateral entrainment and mixing are not the main processes affecting supersaturation, which is likely to be true for stratocumulus clouds,** the change in LWC with time ~~is~~ **can** also **be** linked to the change of LWC in altitude,"

P. 5, l. 8: It probably will not change the results significantly, but why do the authors not assume a Weibull distribution here (as done in Sec. 2)?

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),

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

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."

P. 8, l. 7: s fluctuations at the cloud base could also arise from changes in cloud base height, and therefore differences in thermodynamics and not turbulence.

The reviewer makes a very good point. But we think the turbulent effect is stronger than the thermodynamic effect under most of the conditions in our case, because: (1) the cloud-base height is roughly constant around that time as shown in Figure 1; (2) when we calculate supersaturation, we only consider the case when both layers (upper layer and lower layer) have the retrieved LWC values—if the cloud-base height is higher such that only one layer (upper layer) has a retrieved LWC, we do not use the profile; (3) Figure 4 shows that the gradient of LWC is small at cloud base, compared with that at cloud top, suggesting that the broader distribution of s fluctuation at cloud base is unlikely mainly due to the narrower distribution in LWC gradient (thermodynamic effect). But it is still possible that the thermodynamic effect is stronger than the turbulent effect at the cloud base in some local regions. We add some discussion to the manuscript,

"…**It should be mentioned that a larger fluctuation in s at cloud base could also arise from changes in cloud-base height. However, this thermodynamic effect is unlikely the main cause in our case because the gradient of LWC close to cloud base is relatively small, as shown in Figure 4d.**"

P. 9, ll. 4 – 5, "[…] large drizzle and raindrops which are abundant in marine stratocumulus clouds […]": This means that all marine stratocumulus clouds are drizzling or raining which is not true. In fact, the authors state that the analyzed stratocumulus cloud does not precipitate (p. 4, ll. 18 – 20).

Agreed. To make the manuscript accurate and consistent, we modify it as,

"…large drizzle or raindrops which are ~~abundant~~ **frequently observed** in marine stratocumulus clouds…"

P. 12, Eqs. A2, A3, A4: There are some typos in the equations. Check Korolev and Mazin (2003, doi:10.1175/1520-0469(2003)060<2957:SOWVIC>2.0.CO;2) for details. Please also check if these errors affected the results of the manuscript. I state the corrected equations below (changes are highlighted in red):

Thank the reviewer for checking the equation carefully. The equation is correct because we follow the equations used in Lamb and Verlinde (2011). The difference is that Lamb and Verlinde use a mole-based unit, while Korolev and Mazin use a mass-based unit. For example, the unit of l_v is J/mol in Lamb and Verlinde (2011), but it is J/kg in Korolev and Mazin (2003). To make it clear, we added a statement to the manuscript:

"**Note that the unit here is mole-based, i.e., the units for l_v is J/mol (e.g., not J/kg).**"

Technical Comments

P. 3, l. 1: It should read “k-th” or “k^{th}”, but not “k_{th}”.

Corrected in the manuscript.

P. 3, Eq. 8: The equation should read: $\frac{dlnLWC}{dt} = \frac{\partial lnLWC}{\partial z}\frac{dz}{dt} = w\frac{\partial lnLWC}{\partial z}$, with a total derivative of z.

Corrected in the manuscript.

P. 7, l. 8: The flight was probably at 1.471 ± 0.004 km above sea level and not 1.471 ± 0.004 m.

Corrected in the manuscript.

P. 11, l. 7: Add parentheses to all squared percentage values, e.g., $(20\ \%)^2$.

Corrected in the manuscript.