

Review of Braga et al. “Energetic Derivation of Water Vapor Supersaturation at Cloud Base”

Braga et al. propose a new method for quantifying supersaturation near the cloud base in developing cumuli with aircraft observations. The method is based on energy-budget arguments, allowing it to avoid some parameterizations in conventional methods. Because cloud-base supersaturation is an important quantity for the activation of cloud-condensation nuclei aerosols, the paper and method have the potential to make a meaningful contribution to the field. However, the paper in its current form is unclear in some important ways and lacks a comparison to other studies and methods, which makes it harder to accept the argument that the current method is superior to existing methods. If these issues can be addressed, then the paper may be suitable for publication.

Major comments:

- My first major comment is that several important aspects of the theory and method are not adequately explained.

First, I find the difference between the terms “liquid supersaturation (S_l)”, “water vapor supersaturation (S_v)” and “cloud supersaturation (S_c)” quite confusing. In the cloud physics textbooks I have read, supersaturation is computed relative to an infinite flat surface of pure water. I suspect that much of the audience of this paper will have that concept in mind. It would be really helpful if the authors could explain how S_l , S_v , and S_c are related to the common textbook definition of supersaturation, and how they should be physically interpreted. It is especially confusing that S_l , S_v , and S_c are used several times before they are defined near line 260.

Second, the paper would benefit from an explanation about what microphysical assumptions are being made. Cloud microphysics, in particular the integral radius of the cloud droplets, affects the condensation term “ C_d ” in the supersaturation development equation in Eq. 1. However, the parcel model on which the authors’ method is based does not explicitly discuss or consider microphysics at all. There must be some implicit assumption about the cloud microphysical properties baked into the theory, right? It would help if the authors can explain this.

Third, Eq. 14 seems to assume that the process is isothermal. Does this conflict with other assumptions in the theory? A justification for this step is needed.

Fourth, why is the analysis limited to cumuli at the initial stage of development? The method seems agnostic to the lifecycle stage of the cumuli. It would help to justify and explain this choice.

Fifth, it would help if the authors can be more explicit about where in the cloud their method is applicable. The current draft just says that the supersaturation is estimated “at cloud base”. Does this mean the lowest layer of the cloud where CCN activate, supersaturation changes rapidly with height, and supersaturation reaches a local maximum (approximately the lowest 10 m of the cloud)? Or is it valid above this layer? It seems like the method can’t be valid in the activation layer since microphysical details are very important to the supersaturation development there but are not explicitly treated in the derivation. If the method is only valid above the

activation layer, then the proposed method provides the same information as the standard quasi-steady-state approximation for estimating supersaturation (Squires, 1952). A comparison with that method, including explanations of advantages and disadvantages of each method, would be really helpful.

- My second major comment is that the paper would be much stronger if it compared the method and results to other studies. In particular, Romps et al. (2023) estimated supersaturation from the same aircraft campaign but using a different method. How do the results of their work compare with the present study, and if there are differences, then why? In particular, I find it hard to reconcile the results of Fig. 4, which shows “cloud supersaturations” up to 8-9%, with Romps et al. (2023), who found supersaturations that are almost always less than 1%. Why are the results so different? If the “cloud supersaturation” is not the correct quantity to compare with Romps et al. (2023), then what is, and why?

More generally, there are approximately a dozen studies that have used in situ aircraft observations and the quasi-steady approximation to estimate supersaturation in liquid parts of cumulus clouds (a list is cited in Wall et al. (2026)). Comparing the current results with that literature would be helpful.

- My last major comment is that I find it hard to believe that supersaturation strongly increases with cloud-droplet number concentration (N_d), as shown in the power-law fits in Fig. 5, because the condensation rate scales with supersaturation times N_d . With all else equal, a larger N_d will increase condensation. This, in turn, would lower supersaturation. The only place that the proposed power-law relationship might make sense (to me) is at the narrow layer (order 10 m) at cloud base where CCN activate, so N_d and supersaturation both change rapidly with height. It would help if the authors can explain and justify the physical interpretation of their results, rather than just fitting a power law to the data.

Minor Comments

- As far as I know, equivalent temperature is usually denoted with an “e” subscript, but in this paper it has a subscript “Q”. Consider changing to the conventional subscript.
- Line 336: I find it confusing to report uncertainties in supersaturation as a percent because supersaturation itself is often reported in units of percent. The way it is written now, I can’t tell if the authors are reporting absolute or relative uncertainty. For example, does 10% uncertainty for S_c mean that if one calculates $S_c=1\%$, then the true S_c is $1 \pm 10\%$, or $1 \pm 0.1\%$? The former is so large that it is hard to imagine that it is useful, and the latter seems too good to be true. Because it is so challenging to accurately quantify supersaturation in clouds, it is important that the meaning of the quoted uncertainties is clear. It is also important to compare the uncertainty to uncertainties of other methods

References

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