Geoscientific Model Development Discussions

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Comment on gmd-2021-148

Anonymous Referee #2

Referee comment on "Evaluation of a quasi-steady-state approximation of the cloud droplet growth equation (QDGE) scheme for aerosol activation in global models using multiple aircraft data over both continental and marine environments" by Hengqi Wang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-148-RC2, 2021

This work develops a quasi-parcel model approximation to describe the activation of aerosol into cloud droplets near the cloud base of warm stratocumulus. The authors compile observations from several field campaigns around the world and use them to investigate the performance of their model. These closure studies reveal a good approximation of the parameterization to the observed cloud droplet number concentration at cloud base. This work adds to the existing pool of droplet activation parameterizations. The attempt of writing the parcel model equations on a dimensionless basis could help future development. The authors place less emphasis on trying to obtain a closed analytical solution and rather use a semi-analytical integration. However, current models may be able to handle the associated computational cost. On the other hand, the exposition of the theoretical basis and rationale behind the authors approach is flimsy and, in some cases, inaccurate. These should be clarified and corrected before the work could be published.

General Comments:

My main concern in this work is the lack of rationale behind the proposed approach. There is very little discussion regarding the approximations taken or the validity of the assumptions. Although an acceptable closure is achieved against observations, this does not guarantee that the approach is theoretically sound. Particularly as the evaluation of the scheme seems tightly constrained by observations.

The assumption of a constant saturation ratio, even over a short time step, is unfounded. S changes over a very short time scale and it is not likely that it would ever remain constant. Did the authors perform a timescale analysis to show under what conditions their approximation would be acceptable?

It is also not clear that this model can be called quasi-steady state since the environment and the droplet sizes are clearly changing, and none of their derivatives is negligible. What are the rigorous expressions from where the parameterization is derived?

The proposed model resembles a Euler integration of the regular parcel model where the differential equation describing the evolution of supersaturation was replaced by an iteration over an algebraic expression. The authors should explain the rationale behind such approach and compare it against a more rigorous model where the evolution of the supersaturation is computed explicitly using a differential equation.

Specific comments:

Line 27. “in affecting” does not sound correct. Better say “determining”

Lines 52-53. This is a confusing sentence. Please clarify.

Lines 58-63. This is misleading and inaccurate. Most theoretical parameterizations are approximate solutions to the parcel model equations. Hence they must be evaluated against the rigorous solution first. Then, they can be evaluated against observations. These are not “alternatives”. Both approaches aim to elucidate a different aspect of the parameterization accuracy.

Line 70. Is the closure experiment the same as the evaluation? Please rephrase.

Line 75. Remove “that are”

Line 80. Aerosol is plural already.

Lines 82-84. This is an awkward sentence. Please rephrase.

All equations. Please choose either supersaturation or saturation ratio, but not both. Changing between s and S makes things very confusing.

Line 89. Sp is the droplet equilibrium saturation ratio.

Line 92. Rephrase. “The parameters A, B and C account for … , given by,”

Line 104. Please clarify what water content means in this context.

Line 108. Different from what? Also why would this be important near water saturation, when the droplet activates?

Line 112. The system is missing equations describing the evolution of the saturation ratio, the temperature, and the droplet size distribution. So direct numerical solution would not be only expensive but impossible.

Line 114. I am not sure what the “non-linear behavior of the water vapor saturation ratio vertical profile” means.

Line 116. This is contradictory to the previous statement. If S can be assumed constant, how then is it that time steps much smaller than 1 s are needed? Supersaturation is relaxed quickly in cloudy parcels, so this would be wrong. The authors should add more explanation and justification to their assumptions. As it stands it seems very ad-hoc and possibly incorrect.

Line 156. What are the advantages of this calculation over writing a differential equation for S?

Line 164. Where exactly can you set the entrainment rate?

Line 166. Couldn't find any mention of this scheme in those papers.

Figure 2. Is the observed LWC used to drive the model?

Line 262. How does this compare against integrating over the full aerosol size distribution?

Line 276. Internally mixed aerosol is defined as a population where all particles with the same size have the same composition. Please correct.

Line 310. Please explain where this comes from. Wsub and W+ represent similar things. That is, each parcel moves with a given vertical velocity. A rigorous approach would integrate the parameterization over the distribution of W. In absence of that, a mean (in the sense of the mean value theorem) could be used. That would be either W+ or Wsub, but not both.

Line 322. This sounds akward. Maybe use, "using Eq.(21) into Eq. (20) we obtain"

Line 334. Awkward sentence. Maybe just say TKE is given by…

Line 382. Please explicitly define CDNC_M and CDNC_O

Line 392. Is R2 this the Pearson correlation coefficient?

Line 418. This agreement is somehow unexpected. Given the assumptions made, my suspicion is the observed LWC is used to drive the parameterization which along with the total aerosol number provides a strong constraint to CDNC. Please clarify whether this is the case.

Line 450. As written, Eq. (1), i.e., the droplet growth equation, does not imply this. The supersaturation balance is missing.

Line 476. How efficient? It would be appropriate to include some timing benchmarks (against rigorous solutions or other commonly used parameterizations) to assess the applicability of the scheme in large scale atmospheric models.