

Second review, Covert et al., acp-2021-656

The authors' response to my and the other reviewers' comments have greatly improved the manuscript and I recommend it for publication pending a couple extremely minor technical corrections (see final paragraph).

I am still of the opinion that the shape of the enhancement factor profile is only "general" under conditions of "idealized single layer stratocumulus with a well-defined inversion," as this is what gives rise to the adiabaticity constraint discussed by the authors (e.g., line 397 of revised manuscript), but I appreciate the effort to include Fig. 11. It lends great confidence to the idea that there is a consistent (if perhaps not "universal") structure to the profile of E_q .

I also very much appreciate the addition of the autoconversion (Au) rate curve to Fig. 8 and the discussion in lines 310-313 and 439-445 of the implications of lower E_q coinciding with maximum Au rate near cloud top. I think this is one of the most impactful findings in terms of GCM microphysics development, and I look forward to the authors' future work with a two-moment scheme to evaluate whether variability of N in the entrainment zone augments or dampens E_q . As the authors state, this is likely to be highly dependent on mixing assumptions.

Lastly, I must admit that I made a mistake in suggesting a reference in my first review. I intended to suggest the authors cite Lebo et al. 2015 (<https://doi.org/10.1175/JAMC-D-15-0066.1>) instead of Lebo and Feingold 2014. One other minor recommendation is to explicitly state the type of microphysics scheme used to produce the results shown in Fig. 11 – the author response states that these simulations are all with bin microphysics, but this is not reflected in the manuscript.

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