Reviewer of:

Opinion: A Critical Evaluation of the Evidence for Aerosol Invigoration of Deep Convection

This paper evaluates the theoretical, modeling, and observational evidence for warm- and cold-phase invigoration pathways in deep convective clouds. It does a commendable job of reviewing previous work and highlighting that previous papers presenting evidence/explanations for convective invigoration are, at least partially, based on inaccurate assumptions, data sampling strategies, and statistical methods. Consequently, it claims that foundational observational studies supporting convective invigoration are highly questionable and provides supporting arguments. The authors also offer suggestions for a way forward.
I believe this opinion paper will make a significant contribution. Science should be driven by questioning previous results and raising doubts whenever alternative explanations for trends in observations or modeling data are possible. Moreover, I think this opinion paper is timely, considering the substantial body of literature published in recent years (mostly by the authors) that has questioned previous results. Additionally, this paper is well-written. While I think that the paper is in a publishable form as is, below I list a few minor suggestions that authors might want to consider.

Comments:
Line 54: I suggest listing the major relevant papers here instead of referencing Igel and van den Heever, 2021 to make it easier for the reader.

Line 55: I agree that a clear definition of "invigoration" is required, and focusing on vertical velocity makes the most sense. However, there are cases where an aerosol perturbation leads to larger mass flux to the upper troposphere, even without a change in vertical velocity, which subsequently affects cloud macro-physical and radiative properties (Dagan et al., 2020). It might be worth mentioning this in relation to the climate-relevant part in line 60.

Line 100: I suggest adding the paper by Marinescu et al., 2021, which also demonstrates diverging trends in multi-model comparison.

Line 121: This statement strongly depends on how one defines "notable." Wouldn't a 2-3% super-saturation under clean conditions be enough to make an impact? Romps et al., 2023 puts the limit at >1%. Additionally, while I appreciate the intention to present a similar x-axis range in Fig.1 left and right panels, I find the range presented in the right panel somewhat misleading. A 1K difference is already quite large, and the current presentation of this figure gives the impression that it is very small.

Line 134: I agree that warm-phase *invigoration* cannot be negative, but the aerosol effect on warm-phase convection can be negative (Jiang et al., 2006; Small et al., 2009; Dagan et al., 2015).

Line 412: The sentence starting with "Indeed" is a bit long and complicated to follow. I suggest splitting it into two parts.

Around line 480: I would add another point here: "try to avoid making strong conclusions based on a single model simulation" or "focus more on model intercomparisons." These MIPs have proven to be very informative in different sub-disciplines (e.g., RCEMIP for convective self-aggregation and cloud feedback), and I think they are not utilized enough in the ACI community.

References:

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Dagan, G., Stier, P., Christensen, M., Cioni, G., Klocke, D., and Seifert, A.: Atmospheric energy budget response to idealized aerosol perturbation in tropical cloud systems, Atmospheric Chemistry and Physics, 20, 4523-4544, 2020.

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Romps, D. M., Latimer, K., Zhu, Q., Jurkat-Witschas, T., Mahnke, C., Prabhakaran, T., Weigel, R., and Wendisch, M.: Air pollution unable to intensify storms via warm-phase invigoration, Geophysical Research Letters, e2022GL100409, 2023.
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