Response to Reviewer Comments for Manuscript "A Global Assessment of Warm-Phase Convective Invigoration from Aerosol-Cloud Interactions. Part 1: Theory"

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I thank the reviewers for their helpful comments, which have improved the manuscript. I have considered all of the comments and made changes accordingly. Reviewer comments are written in black standard text below, and my responses are written in *blue italic text*.

Comments from Reviewer 1:

I find this to be an interesting and valuable study that develops a theoretical framework for understanding warm-phase convective invigoration due to aerosol–cloud interactions. Given the complexity of the problem and the substantial remaining knowledge gaps in our understanding of cloud dynamics, entrainment, and the coupling between cloud microphysics and dynamics, an analytical treatment of this kind necessarily requires simplifying assumptions. I therefore do not regard the use of such assumptions as a weakness in itself. However, some of the assumptions required to make the problem analytically tractable also enter directly into the mechanisms controlling the predicted magnitude of invigoration. I am therefore concerned that the consequences of some of these assumptions are not yet sufficiently quantified. In particular, the assumptions concerning entrainment and the representation of thermal drag appear potentially important for the main conclusions. I think the manuscript would be considerably strengthened if the authors clarified their physical basis and quantified the sensitivity of the results to relaxing them.

1- The assumption that clean and polluted thermals have identical fractional entrainment rates is an imposed closure and is important because it leads directly to equal MSE at a given height and hence Eq. (1). Established entrainment theory allows fractional entrainment to depend on cloud radius, updraft velocity, and exchange across the cloud boundary; organized entrainment can also depend on $\frac{1}{w}\frac{\partial w}{\partial z}$. Thus, although an increase in w does not necessarily imply a particular change in ϵ, the aerosol-induced changes in buoyancy, velocity, and acceleration could themselves modify entrainment.

This is potentially important because dynamically induced entrainment provides a negative feedback on updraft acceleration, i.e. increased buoyancy and acceleration may enhance environmental inflow and dilution, thereby opposing the increase in w. Prescribing $\epsilon = \hat{\epsilon}$ removes this possible feedback from the theory. I therefore suggest that the authors not only justify this assumption, but propagate uncertainty in $\hat{\epsilon}/\epsilon$ through the derivation to the final predicted Δw. This would establish whether the main invigoration results remain robust when the entrainment response is allowed to differ between clean and polluted thermals.

Response: *Thank you for this suggestion. I added a paragraph to the assumptions/limitations section that describes the neglected feedback from changing organized/dynamical entrainment when the updraft accelerates (line 357 Part 1). In this paragraph I use the time-dependent solutions from the analytical model to estimate that organized entrainment could increase the overall fractional entrainment rate by ~10% when the polluted thermal adjusts following a sudden change in buoyancy (assuming typical values for parameters). This feedback damps* Δw*, so excluding it leads to an overestimation of warm-phase invigoration. The magnitude of this bias will depend on environmental relative humidity.*

2- The treatment of drag also requires further justification. The manuscript varies prescribed values of c_d, but assumes the same c_d for clean and polluted thermals despite predicting $\widehat{w} \neq w$. Romps and Öktem (2015) show that the wave-drag component of the effective drag coefficient depends on the Froude number F=w/(Nr), and therefore on w, r, and stratification. They also show regimes in which wave-drag magnitude decreases with increasing w, whereas constant c_d produces drag proportional to w^2.

Since the negative drag feedback at large w is central to the conclusions, could the authors justify treating c_d as invariant or test the sensitivity using a velocity-dependent $c_d(F)$? In addition, the time-dependent momentum equation does not appear to include the added-mass effect considered by Romps and Öktem (2015), i.e. it is missing the effective-inertia factor of 3/2 for a spherical thermal. This term does not affect the terminal steady-state solution, but it would modify the transient adjustment timescale and therefore the adjustment length used to justify the steady-state approximation. Could the authors clarify the omission of this term?

Response: *The assumptions/limitations section has been updated to discuss the fact that the analytical model ignores variations in* c_d *with Froude number F (line 352 Part 1). To demonstrate the implications of this approximation, Fig. R1 below plots* c_d *and a function of* w *for typical values of* $N = 0.01$ *s*$^{-1}$ *and* $r = 600$ *m based on relations derived by Romps and Öktem (2015).* c_d *monotonically decreases as updrafts rise faster for* $1 \leq w \leq 6$ *m s*$^{-1}$*, but it approaches a constant when* $w > 6$ *m s*$^{-1}$*. Unfortunately the relation between* c_d *and F has a very complicated functional form, and I have not been able to incorporate it into the theory developed here and still obtain analytic solutions. However, one can consider qualitatively how the solutions would change if the relation between* c_d *and F were included in the theory. Including this relation would not change the solutions of* Δw *very much for* $w > 6$ *m s*$^{-1}$ *because* c_d *is approximately constant there, but it would increase* Δw *for* $1 \leq w \leq 6$ *m s*$^{-1}$ *(where* w *is the updraft speed of the clean thermal). This would make invigoration for typical shallow convective updraft speeds (*$w \approx 3$ *m s*$^{-1}$*) even greater than invigoration for typical deep convective updraft speeds (*$w \approx 10$ *m s*$^{-1}$*) for a given value of supersaturation in the clean thermal. This would reinforce one of the main arguments of the paper, which is that typical shallow convective clouds are more susceptible to warm-phase invigoration than typical deep convective clouds.*

Finally, the section on time-dependent solutions has been updated based on the reviewer's suggestion. The adjustment length scale is stated more precisely than it was in the original draft, and a comment has been added to state that including the effective inertia factor from Romps and Öktem (2015) increases the adjustment

length scale by 50% (line 219 Part 1). (I verified this by numerically integrating solutions).

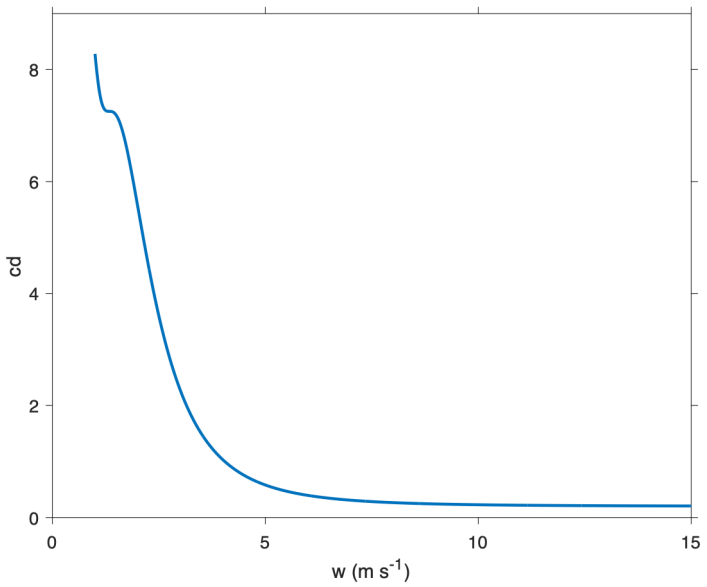


Figure R1: *Drag coefficient (c_d) plotted as a function of updraft speed (w) from relations derived by Romps and Öktem (2015). Typical values of $N = 0.01$ s^{-1} and $r = 600$ m are used, where N is Brunt–Väisälä frequency and r is the radius of the thermal.*

Comments from Reviewer 2

The study presents an analytical model of an ascending air parcel under different aerosol concentrations. After introducing several simplifications and assumptions, the author derives a simple expression that allows one to evaluate the increase in vertical velocity (warm-phase convective invigoration) with the increase in aerosol (drop) concentration. This increase is found to be quite significant. The paper is well written, and I recommend its acceptance subject to minor revisions.

1. I recommend that the author read and cite the following study:

Fan, J., and A. Khain, 2021: "Comments on 'Do ultrafine cloud condensation nuclei invigorate deep convection?'" *Journal of the Atmospheric Sciences*, 78, 329–339. https://doi.org/10.1175/JAS-D-20-0218.1

In particular, I recommend *considering the Appendix* of Fan and Khain (2021), where convective enhancement is simply derived from the analysis of the supersaturation equation. The Appendix shows that convective invigoration follows directly from the equation governing supersaturation. It also discusses the validity of assumptions employed in the manuscript under review. I believe that referring to this analysis would help place the theoretical results of the present study in the context of previous work and clarify their physical interpretation.

Response: *Thank you for this suggestion. I revisited Fan and Khain (2021) and added a caveat to the text of the current manuscript that states that the quasi-steady supersaturation may overestimate the true supersaturation when the cloud-droplet*

number concentration is especially small ($n < \sim 10$ cm^{-3}). Fan and Khain (2021) is cited to support this caveat (line 115 Part 1).

References

Romps, D. M., and Öktem, R.: Stereo photogrammetry reveals substantial drag on cloud thermals. Geophys. Res. Lett., 42(12), 5051–5057. https://doi.org/10.1002/2015GL064009, 2015.