Response to Reviewer Comments for Manuscript "A Global Assessment of Warm-Phase Convective Invigoration from Aerosol-Cloud Interactions. Part 2: Observations"

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

Comments from Reviewer 1:

Combining observations from twelve aircraft campaigns is a major achievement, and the effort to harmonize and screen these measurements is impressive. Therefore, the observational compilation and supersaturation analysis are valuable and merit publication. However, the quantitative interpretation in terms of convective invigoration requires a major revision. The thresholds, susceptibility fractions, and cloud-regime conclusions should be restricted explicitly to the idealized analytical model, and the broader claims concerning shallow versus deep convective invigoration should be withdrawn or substantially qualified.

I have several groups of comments:

The suitability of the metrics

My principal concern is the quality and relevance of the metrics used to quantify invigoration. Your analytical metric is mathematically coherent for a narrowly defined quantity: the fractional change in terminal updraft speed predicted for an idealized thermal after an imposed fivefold increase in droplet number. However, I do not think it is a general metric of convective invigoration, due to the following reasons:

Fractional updraft increase versus energetic invigoration

The authors define substantial invigoration as a 25% increase in updraft speed. The required increase in kinetic energy per unit mass is:

$$\Delta KE/m = \tfrac{1}{2}(w_{polluted}^2 - w_{clean}^2)$$

$$= \tfrac{1}{2}[(1.25\, w_{clean})^2 - w_{clean}^2]$$

$$= 0.28125\, w_{clean}^2.$$

Consequently, a 15 m s^{-1} updraft must receive 25 times more additional kinetic energy per unit mass than a 3 m s^{-1} updraft to satisfy the same criterion. A weak updraft can therefore be classified as substantially invigorated after receiving relatively little additional energy, whereas a strong updraft can receive much greater aerosol-induced buoyancy work without achieving a 25% increase in speed.

Thus, the result that weak updrafts are more susceptible follows partly from the selected fractional-velocity metric. More precisely, weak updrafts are more likely to

experience a large fractional change in terminal speed. This does not mean that they necessarily experience the largest energetic invigoration.

Response: *We have chosen to limit the scope of the study to aerosol-driven changes in updraft speed (Δw). Changes in updraft speed have implications for convective mass flux, cloud microphysics, precipitation, and lightning, so there is motivation to better understand them. As far as we know, Δw is also the conventional definition of "convective invigoration". For example, see the introduction of recent review papers by Varble et al. (2023) and Fan et al. (2025).*

The reviewer makes a good point that changes in updraft kinetic energy (ΔKE) may be an interesting additional factor to consider. Figure R1 below shows ΔKE per unit mass predicted by the analytical model. For a given supersaturation in the clean thermal, ΔKE is approximately constant for all updraft speeds larger than the optimal updraft speed for warm-phase invigoration (e.g. for all $w \geq 3$ m s^{-1} for $S = 1\%$). This happens because $\Delta KE \propto \Delta w^2 \approx 2w\Delta w$, and $\Delta w \propto w^{-1}$ in the limit that w is much faster than the optimal updraft speed (Eq. 16 Part 1). The implications of this result for convective dynamics and cloud microphysics are not immediately clear to us. Given that this work is already a two-part manuscript with a lot of material, we prefer to limit the scope to Δw. A comment has been added to acknowledge the limitation that updraft kinetic energy is not considered in this work (line 423 Part 1). Perhaps ΔKE could be investigated in future work.

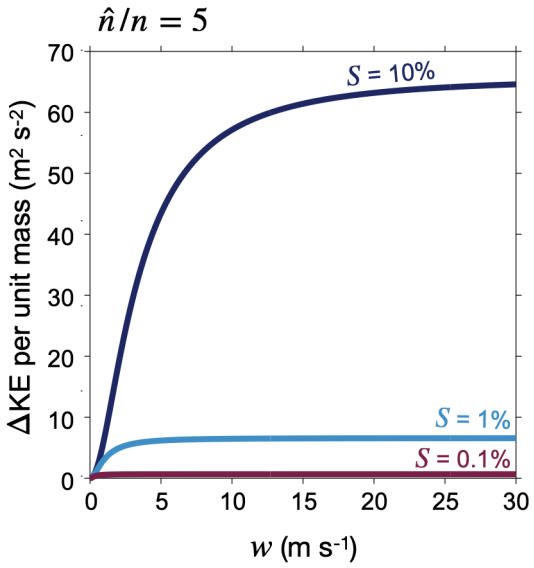


Figure R1. ΔKE per unit mass in response to a five-fold increase in cloud-droplet number concentration plotted as a function of w for three values of supersaturation in the clean thermal.

Limitations of the terminal-thermal response

Your calculation compares two terminal velocities after instantaneously increasing the droplet number by a factor of five while holding the liquid-water content, normalized spectral shape, thermal size, and environment fixed. It does not follow the vertical evolution of activation, condensation, condensate loading, collision–coalescence, precipitation removal, drag, or cloud survival.

These processes determine how aerosol-induced energy is expressed. Invigoration can appear as deeper or longer-lived ascent, increased work against drag, delayed precipitation, greater active updraft area, or enhanced convective mass flux without producing a large local increase in updraft speed.

Response: *The purpose of the paper is to isolate and understand one proposed mechanism of convective invigoration: more cloud-active aerosols → more cloud droplets → lower supersaturation → larger buoyancy → faster updraft. We agree that there are many additional factors that could influence the way in which aerosols change updraft speeds. A paragraph has been added to the assumptions and limitations section of Part 1 to describe the additional factors that are not considered by the analytical model (line 420 Part 1).*

The terminal response also assumes that sufficient vertical distance is available for the polluted thermal to approach its new equilibrium velocity. Your time-dependent calculation indicates that attaining most of this response may require ascent through approximately twice the thermal diameter. For the assumed radius range of 200–1000 m, this corresponds approximately to 0.8–4 km. This distance should be compared with the actual vertical depth available after the imposed droplet perturbation. Some shallow clouds may not have sufficient depth to realize the predicted terminal response, whereas deep warm-base clouds may have several kilometers over which the additional buoyancy can act. This issue is directly relevant to the conclusion that shallow clouds are the most susceptible.

Response: *We repeated the time-dependent calculation for the same settings as in Fig. 5 but with the updraft speed of the clean thermal set to $w = 3$ m s^{-1}. This calculation represents typical conditions for shallow cumulus. The polluted thermal attains 75% of its steady-state value for Δw after traveling a vertical distance equal to 1.2 times its diameter. This is now stated in the text (line 217 Part 1). Given that most thermals rise over greater vertical distances than their diameter, the steady-state approximation is justified for the idealized analytical model. Observations of shallow cumulus thermals from stereo photogrammetry also support the assumption of an approximate force balance between buoyancy and drag (Romps and Öktem, 2015).*

I therefore suggest distinguishing explicitly among:

- Fractional terminal-speed sensitivity, Δw/w, which your analytical model calculates.
- Added kinetic energy per unit mass, ½(w_polluted² − w_clean²), which permits comparison between initially weak and strong updrafts.
- Convective-system invigoration, which may involve cumulative buoyancy work, cloud depth, lifetime, active updraft area, or mass flux and cannot be inferred from local Δw/w alone. These are the most relevant metrics, but cannot be quantified by the idealized simulations of this paper.

Response: *As justified above, we prefer to limit the scope of the paper to $\Delta w/w$. However, a paragraph has been added to the assumptions and limitations section of Part 1 to emphasize that the factors mentioned in the final point are not considered in the analysis (line 420 Part 1).*

Interpretation of the susceptibility fraction

The fraction f is not the observed fraction of updrafts that became invigorated. It is the fraction of retained one-second samples for which the steady-terminal-thermal model predicts $\Delta w/w \geq 0.25$ under an imposed fivefold increase in droplet number.

I suggest calling f a “model-defined fractional-speed susceptibility index” and defining it as:

“The fraction of analyzed quasi-steady cloud-interior samples for which the steady-terminal-thermal model predicts at least a 25% increase in terminal updraft speed under an imposed fivefold increase in droplet number.”

Response: *We did not intend to claim that f is the observed fraction of updrafts that have been invigorated. The introductory paragraph in Sec. 3.2 “Susceptibility to Warm-Phase Convective Invigoration” states that the analysis “infers cloud susceptibility to warm-phase invigoration rather than showing direct observational evidence of the mechanism” (line 459 Part 2). We added “direct observational” in this sentence to emphasize that the analysis infers the results from the analytical model rather than showing direct observational evidence.*

The reported 2.5–97.5% range should not be interpreted as a complete confidence interval. It reflects only the assumed distributions of thermal radius and drag coefficient. It does not include measurement uncertainty, autocorrelation among consecutive samples, campaign-to-campaign variability, uncertainty in the fivefold droplet perturbation, or structural uncertainty in the terminal-thermal model.

Response: *Measurement uncertainty is now included in the confidence interval for f (Appendix C, Part 2). The issue of autocorrelation is avoided in the sensitivity test where observations are averaged horizontally over individual penetrations of cloud updrafts, so each data point corresponds to a different cloud (Fig. B2 Part 2). To clarify the limitations of the uncertainty quantification, a sentence has been added that states that the reported uncertainty does not include structural uncertainty in the analytical model (line 688 Part 2).*

Sampled cloud types and their aerosol backgrounds

The suitability of the sampled clouds for developing high supersaturation needs to be evaluated more explicitly. High supersaturation relevant to aerosol invigoration is expected most readily when several conditions coincide: a very clean initial aerosol background, a warm cloud base, a sufficiently deep liquid-cloud layer, and enough collision–coalescence and precipitation removal to reduce the surface area of the pre-existing droplets.

According to Figure 1, the campaigns sampling congestus and deep convection were not located in the very clean remote tropical-ocean regime. The continental and coastal deep-cloud campaigns occurred in regions with appreciable climatological sulfate. The maritime congestus and deep-cloud campaigns near the Philippines, the Gulf of Mexico, and the Caribbean also do not capture the cleanest aerosol conditions that dominate the open oceans, as shown in the figure.

Conversely, the campaigns in the cleanest geographical environments primarily sampled shallow clouds. CSET and EUREC⁴A sampled shallow trade cumuli, RICO

sampled precipitating shallow cumuli, and SOCRATES sampled shallow clouds in cold-sector Southern Ocean conditions. These clouds do not provide the combination of a very clean aerosol background, a warm cloud base, and a deep warm layer required to test high supersaturation in deep tropical convection.

Therefore, the dataset appears to lack the cloud regime most relevant to the disputed mechanism: deep, warm-based convection developing in an extremely clean tropical ocean environment. The absence of high supersaturation in the sampled deep clouds cannot be extrapolated confidently to a regime that was not represented.

Response: *We agree that deep convection over very clean ocean conditions is not sampled in this study, so the analysis cannot determine how often such situations are susceptible to substantial warm-phase invigoration. This limitation is explicitly stated in the text (line 165, Part 2). Furthermore, the original draft stated that "precipitating shallow cumuli are uniquely susceptible to warm-phase invigoration", or some variation of this phrase. We removed "uniquely" from these statements in order to more accurately reflect the fact that this work does not determine if deep convective clouds in very clean ocean conditions are susceptible.*

Figure 1 also shows a long-term climatological surface sulfate concentration, not the aerosol population actually entering each sampled cloud. It cannot establish the CCN or UAP concentration during an aircraft penetration.

Moreover, the "ocean" composite combines shallow trade cumuli, precipitating shallow cumuli, congestus, deep convection, and cold-sector Southern Ocean clouds. These regimes have very different cloud-base temperatures, warm-cloud depths, aerosol populations, and precipitation histories. A susceptibility fraction calculated from their combined one-second samples does not directly demonstrate that "precipitating shallow cumuli over the ocean are uniquely susceptible." That conclusion requires separate results by cloud type and precipitation state.

Response: *We removed "uniquely" from these statements to better reflect the fact that the current analysis does not sample oceanic deep convection in very clean aerosol conditions, as the reviewer correctly points out.*

However, the analysis supports the claim that ocean shallow cumuli are most susceptible to substantial invigoration among the clouds that are sampled in this work. The analysis estimates a threshold supersaturation required for substantial warm-phase invigoration, and computes the fraction of updrafts with supersaturation that exceeds this threshold. The conditions with the highest fractions of susceptible updrafts that are sampled in this analysis occur over ocean with $1 < w < 3$ *m s*$^{-1}$*. Since these are typical conditions for oceanic shallow cumuli, it is justified to infer that they are more susceptible than other sampled cloud types and conditions. The inferred susceptibilities* $\Delta \ln w / \Delta \ln n$ *across the campaigns support this statement (Fig. 10c Part 2) (*n*: cloud-droplet number concentration). Note in particular that the highest susceptibilities are found in the Antigua campaign (RICO), which targeted precipitating shallow cumuli over the ocean.*

UAP and secondary activation

The role of ultrafine aerosol particles is essential to both the physical mechanism and the validity of the quasi-steady supersaturation estimate.

In an initially very clean cloud, collision–coalescence and precipitation can reduce the number and total surface area of the original droplets. Supersaturation may then rise sufficiently for UAP that did not activate at cloud base to undergo secondary activation aloft. The newly activated droplets increase the total condensational surface area, consume supersaturation, release additional latent heat, and can increase buoyancy. This is the central warm-phase pathway proposed for UAP-induced invigoration.

The quasi-steady approximation assumes that droplet number and total droplet diameter concentration remain approximately constant over the supersaturation-adjustment timescale. It is therefore not valid during secondary activation. In a high-S cloud, the method is valid only after activation ceases, either because all UAP capable of activating at that supersaturation have been exhausted or because supersaturation has fallen below their activation thresholds.

Requiring the aircraft to be more than 250 m above the estimated lifting condensation level does not demonstrate that this condition has been satisfied. UAP can activate kilometers above cloud base. The study provides no measurements of interstitial UAP or CCN and therefore cannot establish that the relevant aerosol reservoir had been exhausted in the retained high-S samples.

This creates an important interpretive problem. If UAP are still activating, the quasi-steady method is not valid. Excluding observations within 250 m of cloud base is a reasonable and necessary step because primary droplet activation there violates the quasi-steady assumption. However, this filter addresses cloud-base activation only. It does not exclude secondary activation of UAP higher in the cloud, where supersaturation can increase after collision–coalescence and precipitation have reduced the surface area of the pre-existing droplets.

Ongoing secondary activation violates the quasi-steady supersaturation assumption because droplet number and total droplet diameter concentration are changing. It also violates the terminal-velocity assumption because the associated condensation and buoyancy perturbation initiate acceleration. Even after activation has ceased and supersaturation has returned to quasi-steady conditions, the thermal may still be adjusting dynamically toward a new terminal velocity. Thus, locally weak acceleration does not necessarily demonstrate that the effects of a recent activation event have fully disappeared.

Both assumptions may again become approximately valid only after the relevant UAP have been exhausted or supersaturation has fallen below their activation thresholds, and after the subsequent dynamical adjustment has been completed. In that case, however, the observed cloud element may already represent an aerosol-modified state. Applying a further fivefold increase in droplet number estimates its susceptibility to an additional hypothetical perturbation, not necessarily the response from the original clean-cloud state. Because the study does not measure the interstitial UAP population or document where secondary activation begins and ends, excluding the cloud-base activation layer alone is insufficient to establish these conditions.

Response: *The reviewer correctly points out that the quasi-steady approximation of supersaturation is invalid in air parcels with ongoing secondary activation of cloud-condensation nuclei above the cloud base, and the filtering method developed in this work does not exclude samples in which this process occurs. A sentence has been added to acknowledge this limitation (line 255, Part 2).*

The undesirable consequences of assuming a fixed LWC

The equal-LWC assumption does not invalidate the observed supersaturation distributions, but it does invalidate the use of the calculated Δw as a quantitative estimate of the net aerosol effect on updraft speed in a developing precipitating cloud. By forcing condensate loading to be identical in the clean and polluted thermals, the model removes the aerosol-induced differences in collision–coalescence, rain formation, condensate retention, and precipitation unloading that contribute directly to buoyancy and acceleration. This is not simply an omitted uncertainty; it suppresses a central component of the dynamical response by construction.

The consequence is that the calculated Scrit is neither a necessary nor a sufficient physical threshold for substantial invigoration in real clouds. A sample with S > Scrit may fail to achieve the predicted acceleration if the polluted thermal retains additional condensate and experiences greater loading. Conversely, a sample with S < Scrit may still undergo a dynamically important aerosol response through altered precipitation, unloading, cloud survival, or cumulative buoyancy work. The direction of the error cannot be corrected with a single factor because it depends on the evolving precipitation and loading history.

This directly invalidates three principal interpretations in the study:

1. The calculated Δw/w cannot be interpreted as the expected net updraft enhancement in a natural cloud. It is only the response in a hypothetical system in which aerosol changes supersaturation but is prohibited from changing LWC and condensate loading.
2. The fraction f cannot be interpreted as the fraction of cloud updrafts susceptible to substantial aerosol invigoration. It is only the fraction of retained samples satisfying a threshold in that restricted equal-LWC model.
3. The comparison between shallow and deep clouds cannot establish that shallow maritime clouds are uniquely susceptible or that deep convection is generally unsusceptible. Aerosol-induced precipitation and unloading differ strongly among cloud regimes and may alter or even reverse the ranking obtained from the terminal-speed metric.

The inconsistency is amplified by the different timescales in the calculation. Equal LWC is imposed at the instant droplet number is increased, whereas the predicted terminal response may require 0.8–4 km of subsequent ascent. Over this distance, the clean and polluted droplet spectra, rainwater production, sedimentation, and retained condensate cannot reasonably be assumed to remain equal. The study therefore combines an instantaneous equal-LWC perturbation with a long-distance terminal response while excluding the microphysical evolution that occurs between them.

Accordingly, the supersaturation observations remain valuable, and the analytical calculation may still be presented as an idealized condensational-heating-only sensitivity test. However, the derived thresholds, susceptibility fractions, and cloud-regime conclusions must be explicitly restricted to that hypothetical equal-LWC system and cannot be presented as quantitative constraints on aerosol convective invigoration in natural clouds.

Response: *The main purpose of the analytical model is to illustrate the key processes involved in warm-phase invigoration to guide how we should think about the mechanism. For this reason, the model is designed to show the direct link between changes in supersaturation, buoyancy, and updraft speed. A new insight from this line of reasoning is that as updrafts rise faster, the drag force gets stronger, creating a negative feedback process that resists invigoration. The analytical model is designed to illustrate this feedback process with minimal complexity.*

The issues raised by the reviewer, including the assumption of constant LWC, are discussed in detail in the Sec. 4 "Key Assumptions and Limitations" in Part 1. The errors of the individual assumptions are estimated to cause errors on the order of 10% for Δw for a given value of S. In contrast, the feedback from the drag force varies from being negligible when $W \ll 1$ to damping Δw by two orders of magnitude when $W \approx 4$, where W is the non-dimensional updraft speed (Fig. 6 Part 1). This dependence is much stronger than O(10%) errors due to factors raised by the reviewer.

To illustrate this point, let us imagine that the analytical model represents the relation between changes in aerosols, supersaturation, and drag, but it predicts errors of ~50% for Δw due to the missing processes described by the reviewer. The errors in Δw can be propagated to estimate the errors in S_{min} (Fig. R2 below). Even considering the edges of the error envelope, S_{min} for $w > 10$ m s^{-1} is approximately an order of magnitude larger or more than S_{min} for $w < 3$ m s^{-1}, where S_{min} is the threshold supersaturation required for substantial invigoration. This supports one of the main claims in the paper: S_{min} is much lower for typical mean values of shallow convective updraft speeds than typical mean values of deep convective updraft speeds, so S exceeds S_{min} more often in shallow clouds.

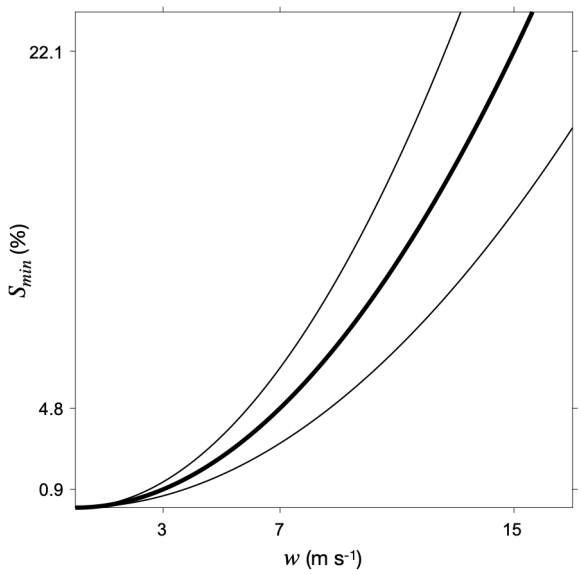


Figure R2. Threshold supersaturation in the clean thermal that is required for substantial warm-phase invigoration (S_{min}) plotted as a function of the clean-thermal updraft speed (w). The thick black line shows the results from the main paper, and the thin black lines show hypothetical values that would occur if the analytical model predicts an error of Δw of $\pm 50\%$. (The figure is similar to Fig. 8 Part 1).

Recommendations

Response: *Thank you again for these recommendations. The changes that were made to address the recommendations are summarized below, and justifications are given in the responses above.*

1. Preserve the observational supersaturation analysis as the principal robust contribution of the paper and separate it clearly from the more model-dependent inference of invigoration.

   The introductory paragraph to Section 3.2 Susceptibility to Warm-Phase Invigoration has been modified to emphasize that the results in that section are derived from the analytical model and that they infer susceptibility to invigoration rather than showing direct observational evidence of the mechanism (line 459 Part 2).

2. Rename f as a “model-defined fractional terminal-speed susceptibility index” and define it explicitly as the fraction of retained quasi-steady samples for which the analytical model predicts Δw/w ≥ 0.25 under an imposed fivefold increase in droplet number.

   The sentence defining f has been modified to state that it is an estimate of the fraction of updrafts that are susceptible to substantial invigoration, rather than a true observed quantity (line 495 Part 2).

3. Replace “fraction of updrafts” with “fraction of retained one-second samples.” Coincident measurements from two instruments should not be counted as independent physical cloud samples.

We prefer to keep the phrase "fraction of updrafts" but acknowledge more explicitly that f is an estimate of this quantity (see previous comment). This is justified based on the fact that similar results are found whether one performs the analysis with 1-Hz data or with data averaged over cloud updrafts (Fig. B2 Part 2).

4. Report the predicted added kinetic energy per unit mass, ½(w_polluted² − w_clean²), alongside Δw/w. This would reveal whether the apparent preference for weak updrafts persists when the response is expressed energetically rather than as a fractional velocity change.

   We prefer to keep the scope of the paper focused on Δw because it is the focus of recent debates on invigoration in the published literature and because the current manuscripts already have a lot of material. A comment has been added to acknowledge the limitation that updraft kinetic energy is not investigated in this study (line 423 Part 1).

5. State explicitly that the analytical calculation represents a condensational-heating-only, equal-LWC sensitivity experiment. It does not predict the net response after aerosol-induced changes in condensate loading, rain formation, precipitation unloading, cloud lifetime, or cloud depth.

   A paragraph has been added to acknowledge that the analysis does not consider these factors (line 420 Part 1).

6. Do not interpret the calculated critical supersaturation as a universal necessary or sufficient threshold for aerosol convective invigoration. Its validity is restricted to the assumptions of the steady-terminal, equal-LWC model.

   The text has been modified to state that the analysis "infers cloud susceptibility to warm-phase invigoration rather than showing direct observational evidence of the mechanism" (line 459 Part 2). We added "direct observational" in this sentence to emphasize that the analysis infers the results from the minimal-complexity analytical model rather than showing direct observational evidence.

7. Compare the model's dynamical adjustment distance with the available cloud depth. A predicted terminal response requiring up to 0.8–4 km of ascent may not be realizable in many shallow clouds.

   The dynamical adjustment length scale is now quantified for typical shallow cumulus parameters and explicitly stated in the text (line 211 Part 1).

8. Present results separately for shallow, congestus, and deep clouds and, where possible, distinguish precipitating from nonprecipitating clouds. The heterogeneous ocean composite does not directly support cloud-type-specific conclusions.

   We agree that such a separation would be insightful, but we are not aware of an objective method that could be applied with the available data to separate shallow/congestus/deep clouds and precipitating/non-precipitating clouds

across all campaigns. For this reason, we kept the original separation by land/ocean/coast and updraft speed.

9. Discuss explicitly that the deep-cloud campaigns did not sample the very clean tropical-ocean aerosol regime. Figure 1 is a climatological surface sulfate field and cannot substitute for contemporaneous measurements of the CCN and UAP populations entering the sampled clouds.

   This sampling limitation is explicitly stated (line 165 Part 2).

10. Acknowledge that excluding observations near cloud base removes primary activation but not secondary UAP activation aloft. The absence of interstitial UAP measurements prevents confirmation that activation had ceased and that both quasi-steady supersaturation and terminal velocity had been attained.

    This limitation is now explicitly stated (line 255 Part 2).

11. Revise the principal conclusion. The results support the statement that slowly rising oceanic samples are preferentially classified as susceptible by the chosen fractional terminal-speed metric. They do not establish that precipitating shallow maritime cumuli are uniquely susceptible to convective invigoration or that deep convection is generally unsusceptible.

    The word "uniquely" has been removed from these statements to more accurately reflect the fact that the analysis does not determine whether pristine oceanic deep convective clouds are susceptible to substantial invigoration.

Comments from Reviewer 2:

The manuscript investigates aerosol-cloud interactions, particularly focussing on warm-phase convective invigoration. I was able to smoothly follow the assumptions underlying the analysis and assess how they propagate through the methodology to the main conclusions of the study.

Overall, I appreciate the manuscript's careful writing, clear structure, and scientific rigour. Notably, the authors demonstrated meticulous attention to detail in harmonising observations from multiple field campaigns. They adopted a conservative approach to data selection, excluding flight segments and conditions that could invalidate the assumptions underlying supersaturation calculations. They invest a lot of effort in identifying potential uncertainties, clearly state the methodology's assumptions, and discuss their associated limitations.

I generally support publication, but I believe two additional points should be addressed to further demonstrate the robustness of the main findings against potential measurement and analysis biases.

1- It's very important to quantify and propagate measurement uncertainties into the reported invigoration statistics. This will provide a more comprehensive understanding of the uncertainties associated with the findings.

The manuscript presents a transparent and reasonably rigorous discussion of the sources of uncertainty in the parameters used to calculate supersaturation and their

associated error propagation. However, the resulting uncertainty of approximately 20–60% in supersaturation is substantial and could potentially impact some of the reported conclusions. While the authors have partially addressed this issue by adopting relatively broad supersaturation categories, it would be beneficial to quantify directly how measurement uncertainties propagate into the final statistical results rather than solely relying on the width of the supersaturation bins.

Therefore, I strongly recommend performing a Monte Carlo-type uncertainty analysis. For instance, the relevant input parameters for each field campaign could be repeatedly jittered according to their estimated measurement uncertainties, leading to recalculations of supersaturation and the derived statistics for each realisation. The perturbation distributions should ideally reflect the known uncertainty characteristics of each measurement; if only bounded uncertainty estimates are available, an appropriately justified uniform distribution could be used.

Repeating this analysis over a sufficiently large ensemble would enable the authors to quantify the robustness of the reported relationships to measurement uncertainty. Instead of presenting a single curve for quantities such as f, the manuscript could, for example, display the ensemble median alongside a shaded confidence/uncertainty interval. Moreover, this approach would provide uncertainty estimates for higher-level conclusions, including the fraction of clouds classified as susceptible to substantial invigoration for each cloud type, as well as other aggregate statistics reported throughout the manuscript.

Response: *We added the suggested Monte Carlo calculations to the uncertainty quantification for the estimated fraction of updrafts that have supersaturation exceeding the threshold for substantial invigoration (f). We included a systematic error term and a random error term whose magnitudes are estimated from pairs of coincident supersaturation estimates (Appendix C: Uncertainty, Line 668 Part 2). This makes the error bars wider, but it does not affect the main interpretation of the results.*

2. Potential contamination by cloud-edge or cloud-free samples, particularly for shallow cumulus.

The analysis in Fig. 8 illustrates the importance of high spatiotemporal-resolution measurements for this type of study. Even within a cumulus congestus penetration, a cloud-free or cloud-edge parcel can enter the analysed time series despite the application of multiple filtering criteria. This highlights the difficulty of completely excluding such samples and raises the question of whether the problem could be more pronounced for shallow cumulus, where individual cloud penetrations are generally more limited in spatial and temporal extent.

Compared to deeper cumulus or congestus clouds, shallow cumuli generally have shorter in-cloud sampling periods and potentially a larger relative contribution from cloud-edge regions. Consequently, contamination by cloud-edge or cloud-free samples may become particularly important for this cloud category. This issue deserves careful consideration, as one of the notable conclusions of the manuscript is that precipitating shallow cumuli over the ocean are usually susceptible to substantial warm-phase invigoration.

I am unsure which cloud microphysical measurements are consistently available across all the field campaigns included in the study. However, many aircraft datasets include measurements from instruments such as the CDP and/or FCDP, and some configurations provide particle-by-particle (PbP) data at very high temporal resolution. Where available, these measurements could provide a powerful and largely independent criterion for identifying genuine in-cloud sampling and removing samples affected by cloud edges or intermittent cloud-free air.

I recommend that the authors investigate whether high-resolution cloud-droplet measurements can be used as an additional cloud-presence/cloud-edge filter, particularly for the shallow-cumulus subset. At the very least, the authors should quantify the sensitivity of the shallow-cumulus results to a more restrictive definition of in-cloud sampling. If sufficiently high-resolution measurements are not available consistently across all flights or campaigns, this sensitivity analysis could be performed for a representative subset of shallow-cumulus flights for which such measurements (e.g. PbP) are available. Even a limited analysis of this kind would provide a useful demonstration of the extent to which potential cloud-edge or cloud-free contamination may influence the reported results and would help establish the robustness of the conclusions drawn for shallow cumulus clouds.

Response: *Ideally one would identify 1-Hz data points that intersect cloud edges using measurements with temporal resolution much higher than 1 Hz from a type of cloud probe that is available in all of the campaigns. The data that matches this description most closely is the cloud mixing ratio q_l from the light-scattering probes. 8 of the 12 campaigns provide q_l at 10- or 25-Hz, which we call "high-rate data" (~4-10 m horizontal resolution). When high-rate data are available, we require that $q_l > 10^{-5}$ kg kg^{-1} in every instance within a 1-sec interval in order for the 1-sec interval to be retained for analysis. This is a stringent filter that is expected to exclude 1-s observations that intersect cloud edges. The remaining 4 out of 12 campaigns provide q_l only at 1 Hz (~100 m horizontal resolution). In these cases, we require that $q_l > 10^{-5}$ kg kg^{-1} at time $t-1$, t, and $t+1$ in order for the observation at time t to be retained for analysis (line 239 Part 2). This criterion is less stringent than the one applied to the campaigns with high-rate data.*

Among the four campaigns that do not have high-rate data, two targeted congestus and deep convection (ESCAPE, SEAC^{4}RS), one targeted trade cumulus (EUREC^{4}A), and one target shallow cumulus and congestus (CAMP^{2}Ex). We examined particle-by-particle images for special cases with unusually high estimated supersaturation in these campaigns ($S > 5\%$), as suggested by the reviewer (Fig. 8 Part 2). For this reason, the reviewer's main critique regarding insufficient filtering of cloud-edge points in trade cumulus applies mostly to EUREC^{4}A data and the subset of CAMP^{2}Ex data that represent shallow cumuli.

As a sensitivity test, we repeated the calculations while excluding the four campaigns that do not have high-rate data (ESCAPE, SEAC^{4}RS, EUREC^{4}A, CAMP^{2}Ex). The results are very similar (Fig. R3).

The reviewer makes a good point that some of these details were unclear in the first draft. To clarify them, we added information about which campaigns provide high-rate q_l data in Table 3 of Part 2, and we changed the text to state the number of such campaigns (line 241 Part 2).

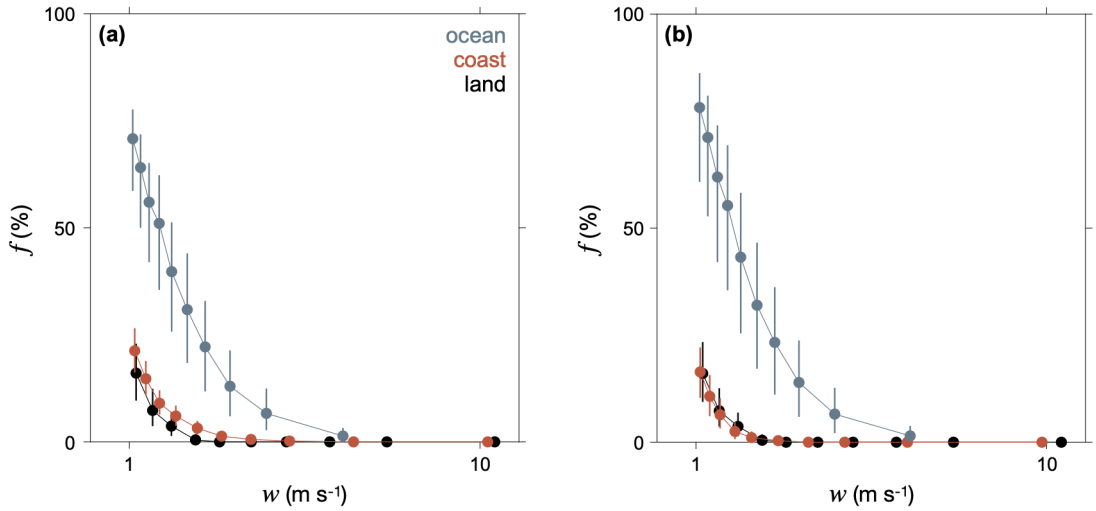


Figure R3. Fraction of cloud updrafts in which S exceeds the theoretical threshold for substantial invigoration (f) plotted as a function of w. (a) Values computed from all campaigns (identical to Part 2 Fig. 11). (b) Similar to (a) but excluding the four campaigns that do not have high-rate data.

Overall assessment

This is a carefully conducted study that effectively utilises a challenging multi-campaign observational dataset. The conservative data-selection strategy, transparent treatment of assumptions, and detailed discussion of limitations are important strengths of the manuscript. My two comments above are primarily concerned with establishing the robustness of the conclusions against (1) propagated measurement uncertainty and (2) possible cloud-edge/cloud-free contamination, particularly for shallow cumulus. Addressing these points would, in my view, provide stronger quantitative support for the manuscript's central conclusions and further increase confidence in the reported evidence for warm-phase convective invigoration.

Thank you again for these suggestions. We made changes to address them, as described above.

References

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