

Response to Reviewer #1

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“Stationary Solutions and Oscillatory Dynamics in a Mathematical Model of a Saturated Moist Atmosphere”

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Dear Editor and Anonymous Referee #1,

I sincerely thank the referee for a thorough, critical, and highly constructive review. Every point raised has been addressed, either by a direct revision of the manuscript or, where a genuinely new numerical experiment was requested, by a clearly identified addition that completes the requested analysis. The manuscript has been substantially restructured: the existence/uniqueness result is now presented as a single proposition in a dedicated section, with the full technical proofs moved to two appendices and, in unabridged form, to the Supplementary Material; the description of the numerical code has been reduced to a few sentences with a discussion of grid convergence added; and the Volterra analogy has been condensed from a full section to a single paragraph with no dedicated figure. Below is a point-by-point response. Manuscript locations refer to the revised main text unless noted otherwise.

General comments

Perhaps, the biggest weakness of the work is that it does not connect to the convection parameterization problem [...] Perhaps the most comprehensive paper to start with is Arakawa and Schubert (1974) but there is also Zhang and McFarlane (1990), Emanuel (1991), Plant and Craig (2010).

Response: We are grateful for this observation and for the suggested references. Section 1 (Introduction, third and fourth paragraphs) now explicitly connects the present idealized approach to bulk mass-flux convective parameterization, citing Arakawa and Schubert (1974), Zhang and McFarlane (1995), Emanuel (1991), and Plant and Craig (2010). We explain that realistic mass-flux schemes represent environmental mixing through entraining plumes, causing a premature loss of buoyancy, whereas our simplified model neglects lateral entrainment and microphysical detail entirely in order to isolate the purely vertical thermodynamic feedback and preserve mathematical tractability. We explicitly acknowledge, in the same paragraph, that this simplification comes at the cost of neglecting layer stability (neutral buoyancy level, free-convection level), energetics (CAPE, CIN), and mixing – all central to realistic parameterization schemes. Regarding the Zhang and McFarlane reference: the correct year is 1995, not 1990 (Zhang, G. J. and McFarlane, N. A.: Sensitivity of climate simulations to the parameterization of cumulus convection in the Canadian Climate Centre general circulation model, *Atmosphere-Ocean*, 33, 407–446, 1995); we have added it to the bibliography accordingly.

The paper is too long and poorly organized. The analysis part (existence and uniqueness of stationary solution) could be written as a single section, with the main result highlighted and presented first as a theorem or one proposition or two. [...] Technical details (such as Section 6 which was all about proving a lemma) could be included in an appendix or a series of appendices.

Response: The manuscript has been fully restructured along these lines. The existence and uniqueness result is now stated as a single Proposition 4.1 in Section 4 (“Existence and Uniqueness of the Stationary Solution”), with only a short paragraph sketching the proof strategy in the main text. The two supporting lemmas (the hydrostatic comparison lemma and the monotonicity lemma, formerly spread across the old Sections 4–6) are now stated, with brief proof sketches, in Appendix A and Appendix B respectively. The complete, unabridged derivations – including all intermediate algebraic steps – have been moved to the Supplementary Material, Sections S1 and S2, with explicit cross-references from each appendix.

The section on numerical solution should be written better. There is no need for a lengthy description of the code used to solve the steady-state system. A few lines will suffice. However, a description of the scheme used to solve the unsteady system is needed.

Response: The former code-documentation-style Section 7 has been reduced to a short paragraph (Section 5, first paragraph): the stationary parameter $c_0 = g\Sigma$ is found by a standard root finder applied to the top-boundary pressure mismatch, with the spatial profiles obtained by explicit Euler integration. We have also added, in the same section, an explicit description of the scheme used for the unsteady (evolutionary) system: an explicit Euler scheme in time combined with upwind finite differences in space for the spatial-structure equations, coupled at each time step to the ODE for $\alpha(t)$ and to the discretized memory convolution for $\Sigma(t)$. The full, code-documentation-level description (module by module) has been moved to Supplementary Material, Section S3, together with the code archive.

The section on the Volterra model is not necessary. The analogy and the properties of this equation could be summarized in a paragraph or so. Readers interested can look it up in the literature.

Response: Agreed. The former standalone Section 7 and its dedicated figure have been removed. The analogy is now a single paragraph (“A qualitative Volterra analogy”) at the end of Section 6, immediately after the discussion of the damped oscillations it is meant to illuminate. We have also added an explicit caveat, requested implicitly by several of the referee’s specific comments below, that this is a qualitative illustration only, not a rigorous reduction of the PDE system to a scalar ODE.

Specific comments

Abstract (line 16): Not sure if this is the case without comparison to observations or comprehensive model simulations such as LES, etc.

Response: The claim has been softened throughout. The abstract no longer states that convergence provides a “robust validation”; it now states that it “provides an internal mathematical consistency check between the two parts of the analysis,” and closes with an explicit statement that the results “are not intended as a substitute for observational or high-resolution (e.g. LES-based) evidence.” The same softened language is used in Section 6.2 where the numerical convergence is discussed.

Lines 22–23: Simply say that these atmospheric phenomena are driven by deep-moist convection or accurately explain what moist convection pertains to. Saying that thunderstorms are primarily caused by upward air flow and water vapor condensation may

be interpreted as if those two processes are separable while in reality they are not; one cannot be sustained without the other!

Response: Corrected exactly as suggested. Section 1 now opens: “[...] are driven by deep-moist convection, in which upward air flow and water-vapor condensation are fundamentally inseparable processes: neither can be sustained without the other.”

Lines 29–31: A statement like this would make the reader expect that the present work is going to suggest an alternative that leads to realistic results. However, the author hasn’t provided any physical evidence to back up such claim. The damped oscillations and Volterra dynamics are still theoretical views based on mathematical models.

Response: We agree and have reframed this passage. Section 1 now explicitly introduces the mechanism as “a different, complementary candidate mechanism, which we explore here purely as a mathematical hypothesis rather than as an established physical result,” and closes the paragraph with: “we do not have direct observational or large-eddy-simulation evidence that this specific feedback loop is realized in nature, and we do not claim it supersedes existing explanations of rain-rate oscillations.”

Lines 34–35: Is there any physical evidence to justify that a constant vertical velocity profile is more realistic.

Response: We do not claim physical realism for this choice; it is presented purely as a mathematical normalization. Section 3 now states explicitly that setting $w \equiv 1$ “is possible because the evolutionary model only constrains the product $\alpha(t)w(t, z)$,” and simply assigns all temporal variation of the flow to $\alpha(t)$; no claim of physical superiority over alternative profiles is made.

Line 93+1: [...] Plus v is not the vertical motion but the velocity of the vertical motion. Doesn’t the model invoked simply express conservation of water substance for the rising parcel of air under the assumptions that (1) the parcel remains saturated and (2) lateral entrainment and time variations are neglected.

Response: We have adopted this precise phrasing. Section 2 now describes H_{tr} as “the rate at which water leaves the vapor phase for a parcel rising with velocity v that remains saturated (neglecting lateral entrainment and explicit time variations).” The distinction between vertical motion and vertical velocity has been corrected throughout.

Line 93: Why “turbulent condensation”? Where is the turbulence in the model?

Response: The word “turbulent” has been removed; the quantity is now referred to simply as the condensation rate H_{tr} .

Eqn 8: [...] I don’t see how the two systems communicate. [...] I’d expect ϑ to depend on $S(z)$ but this doesn’t seem to be the case. Also, using the same variable name for environment and plume variables doesn’t help!

Response: We have clarified this point directly. Section 3 now states explicitly: “this reference profile is a static comparison state used only to fix the boundary target q_0 ; it does not represent an interacting external environment, and it is not coupled dynamically to the ascending column.” The hydrostatic reference problem is used solely to compute the top boundary target q_0 in (??); it is not a second dynamically coupled system, which is why ϑ does not depend on $S(z)$.

Eqn 10: why the time derivative term is kept in ρ but neglected in T ?

Response: We have re-examined this point and clarified the manuscript accordingly. The quasi-stationary assumption $\partial_t \approx 0$ is applied uniformly to w , ρ , and T specifically in the reduction to the *stationary* system (Section 3): none of Eqs. (7)–(9) in the revised manuscript retains any time derivative, in either ρ or T . The full, unapproximated *evolutionary* conservation laws of Section 2 (mass, momentum, energy) legitimately retain genuine physical time derivatives in both ρ and T on an equal footing, since they are exact conservation laws prior to any approximation; this is now stated explicitly where the quasi-stationary assumption is first introduced.

Out of curiosity, what is the motivation behind the form of the “memory kernel” in Eqn 15?

Response: A sentence has been added in Section 2 explaining that the Gaussian form of $\varphi(\tau)$ is chosen as “a simple, smooth statistical closure consistent with fallout being governed by a single characteristic timescale b rather than a sharp cutoff,” and noting that more detailed microphysical droplet-size-distribution models could in principle replace it without altering the qualitative structure of the analysis.

Eqn 18: why is the subscript “cond” suddenly added to H_{tr} ? What changed?

Response: The inconsistent subscript has been removed; the quantity is denoted H_{tr} consistently throughout the revised manuscript.

Line 129+3: Eqn (18) is also a conservation law. In fact none of these equations are conservation laws; they are conservation laws with source terms!

Response: Corrected; Section 2 now refers to Eqs. (3)–(5) as “conservation laws with source terms.”

Eqn 127+3: “(derived in Section 2, but ...)”. No need to say this.

Response: This parenthetical remark has been deleted.

The elementary calculations leading from Eqn 18 to the steady solution in (19) can be deleted. The reader of the journal can work out these details on their own if needed. Otherwise they are just distracting.

Response: These intermediate algebraic steps have been removed from Section 3; the manuscript now states the boundary-value substitution and gives the resulting expression for $S(z)$ directly.

Line 131: use math mode for $w(z)$.

Response: Corrected; $w(z)$ (and $w \equiv 1$) is now consistently set in math mode throughout.

Line 135–136: [...] by assuming constant vertical velocity, the vertical variations in updraft mass flux is all carried by the increase in convection area fraction. Also $S(z)$ is not a volume but a surface area. However, environmental mixing and entrainment in particular is neglected.

Response: We have adopted this exact framing. Section 1 and Section 3 now both state that the vertical variations of the updraft mass flux are carried entirely by the growth of the cross-sectional area $S(z)$ under the constraint $w \equiv 1$, that $S(z)$ denotes a cross-sectional *area* (corrected throughout, not a volume), and that this growth is unrelated to, and does not represent, environmental entrainment, which remains entirely absent from the model.

Eqn 24: how q_0 and z_1 are being determined. In realistic mass flux schemes the plume is integrated till it reaches its level of neutral buoyancy.

Response: Section 3 now states explicitly that $\bar{z}_1$ is prescribed a priori (we use $\bar{z}_1 = 10\,000$ m throughout the numerical examples) rather than determined self-consistently, and that q_0 is the value of ρT at $z = \bar{z}_1$ predicted by the reference hydrostatic profile of Appendix A. We explicitly acknowledge the referee’s point: “in realistic mass-flux schemes the plume top is instead determined self-consistently as the level of neutral buoyancy. Adopting such a self-consistent closure would be a natural extension of the present framework but is outside the scope of the existence/uniqueness analysis below.” This is also listed as a direction for future work in the Conclusion.

Why Section 4 is called preliminaries for the stationary solution?

Response: The section has been renamed. The corresponding appendix is now titled “Hydrostatic Reference State” (Appendix A).

Eqn 25: isn’t this the same as Eqn 2? Why the need for writing it again here?

Response: The duplicate restatement of $\bar{\pi}_{vs}(T)$ has been removed; the expression is now defined once, in Section 2, and referred to by equation number thereafter.

Line 15: There is no parameter ϑ on the right hand side of Equation 20. This sentence is poorly written. [...] The transition between moist and dry regimes is non-smooth and best represented by a Heaviside function.

Response: We have clarified this throughout Section 4. ϑ is now explicitly described as “not a physical mixing fraction interpolating between a dry and a moist air mass – such a transition is in reality discontinuous and would be more realistically represented by a Heaviside-type switch between regimes – but a purely mathematical homotopy parameter” used to connect the dry and fully moist limits within the existence proof and the boundary-value construction. We also clarify that $\vartheta = 2/3$, used only for the reference profile in the numerical examples, is “simply a representative intermediate case,” not a claim about a physically partially-mixed atmospheric state.

Line 162: “lemmata” which lemmata? [...] use lemmas instead of lemmata.

Response: Corrected to “lemmas” throughout, and the lemma structure has been simplified: there are now exactly two supporting lemmas (Lemma A.1 and Lemma B.1), each stated once and referenced unambiguously.

Line 207+2: This paragraph is not needed [...] It is just an elementary technique for the integration of (38), which is a separable equation.

Response: This paragraph, along with the explicit introduction of the auxiliary variable y in the main text, has been removed from Appendix A; the appendix now only states that the transformed equation “yields an explicit, separable equation for $T(z)$,” without walking through the substitution. The full derivation, including the auxiliary-variable substitution, remains available in Supplementary Material, Section S1, for readers who wish to verify it in detail.

Line 211: Why by virtue of (36)? [...] Don't you need the quantity $T_0 - g/(c_v + R_1)z$ to be non-negative also to make the statement?

Response: This passage has been removed from the main text as part of the general condensation of the appendices (see previous response); the complete argument, including the non-negativity bound referred to by the referee, is preserved and can be checked in Supplementary Material, Section S1.

Isn't it better to call Lemma 1 a proposition or theorem instead of having intertwined lemmas that makes the paper hard to read?

Response: The overall structure has been reorganized precisely to avoid this issue: the main mathematical result is now a single, clearly labeled Proposition 4.1 in the main text, with two supporting lemmas confined to the appendices, rather than a chain of intertwined lemmas in the body of the paper.

Section 7: [...] presented more like the description of the numerical code in a typical code-documentation literature [...] completely uncalled for in a scientific article.

Response: As noted above, this section has been reduced to a short paragraph in Section 5; the full code documentation is now provided separately, in Supplementary Material Section S3, together with the code archive itself.

Line 374: What does this mean? Wasn't the author able to solve these equations using simple ode integration combined with basic root finding techniques?

Response: Yes, exactly, and Section 5 now says so plainly: the stationary parameter is obtained via explicit Euler integration combined with a standard root finder (`scipy.optimize.fsolve`).

Line 400: The author needs to clarify and describe the numerical method(s) used to solve the evolution equation and which equations they have exactly solved.

Response: Section 5 now states explicitly that the evolutionary system (the spatial-structure equations together with the ODE for $\alpha(t)$ and the discretized memory convolution for $\Sigma(t)$) is integrated using an explicit Euler scheme in time and upwind finite differences in space, following the scheme of Bourega et al. (2018), with the specific discretization parameters ($h_t = 10$ s, $h_z = 10$ m, total duration 7400 s) given in Section 6.

Line 403 and elsewhere: what are the units of b ? [...] Can the author confirm whether a 7.5 minute residence time is realistic or not? [...] Also what happens when it is varied (smaller or larger than 7.5 min)? Does the oscillatory behaviour break down?

Response: The units of b are confirmed as seconds ($b = 450 \text{ s} = 7.5 \text{ min}$); this is now stated explicitly in Section 2. Rather than the unsupported qualitative claim in the previous version, Section 2 now cites two concrete literature benchmarks for the closely related warm-rain coalescence timescale: satellite-based estimates ranging from about 26 minutes to a few hours (Stephens and Haynes, 2007), and classical adiabatic-parcel-model simulations of order 60 minutes (Jonas, 1996). We state candidly that $b = 450 \text{ s}$ lies *below* this broader observational range, that we treat b as a tunable memory parameter of the idealized model rather than a precisely calibrated microphysical quantity, and that calibrating it against these observed timescales – which would favor larger values, of order 15 minutes or more – is left as an explicit direction for future work; both references have been added to the bibliography.

Regarding the sensitivity of the oscillatory regime to b : we have now run both a smaller and a larger value. At $b \approx 150 \text{ s}$ (about one third of the base value), $\alpha(t)$ rises and settles onto a plateau with no visible oscillation within the tested window, confirming that short memory drives the system toward a near-monotone approach to equilibrium. At $b \approx 900 \text{ s}$ (twice the base value), the oscillation amplitude grows much larger than at $b = 450 \text{ s}$, and $\alpha(t)$ transiently becomes negative, reaching values below -25 within the tested window; this was verified at both a coarser spatial resolution and at the paper’s reference resolution $h_z = 10 \text{ m}$, with closely matching trajectories, ruling out a discretization artifact. Importantly, we do not present this negative- α behaviour as a physical prediction: as now stated explicitly in the revised Section 6, the derivation of the condensation rate H_{tr} (Section 2) assumes an ascending, saturated parcel with $v = \alpha w > 0$, so $\alpha(t) < 0$ signals that the numerical solution has been extrapolated outside the regime for which the model’s physics was derived, rather than that the oscillatory regime itself becomes unstable. We are grateful that this exchange prompted us to check this carefully, as it identifies a genuine, and previously unstated, boundary of the model’s applicability. By contrast, $\alpha(t)$ remains strictly positive throughout the $b = 450 \text{ s}$ simulation used for Figure 3, so the headline comparison with the stationary prediction stays entirely within the model’s region of physical validity – which is now given, alongside the numerical-tractability argument, as the reason this value was retained, and not because it was tuned to force agreement (the stationary target itself is recomputed independently for each value of b : the stationary Σ does not depend on b , while α_{stat} scales as $1/b$, as made explicit in Section 6). We also verified that this validity boundary does not affect Proposition 4.1: its construction rests on $c_0 \geq 0$, which yields $\alpha_{\text{stat}} \geq 0$ by construction independently of b , so the stationary result is unaffected. Determining the true upper bound on b , and extending the model to properly describe the descending/downdraft regime, are left as explicit directions for future work.

Figure 4d: This is rather a peculiar plume behaviour. The author needs to explain/justify what’s the meaning of these damped oscillations. [...] it is not [explained] why the amplitude of these oscillations would decay as the plume matures!

Response: A dedicated paragraph, “Physical interpretation of the amplitude decay,” has been added in Section 6 immediately after the oscillation figure. It explains that droplet fallout is an irreversible loss of condensed mass (not restored to the system), which acts as a dissipative term analogous to friction in a classical damped oscillator: at each cycle, part of the mechanical energy sustaining the acceleration/deceleration contrast is removed together with the falling water, so the oscillation amplitude decreases progressively until production and loss of condensate balance exactly at the stationary state.

Figure 4: why is there a $w(z)$ and $w(t)$? Isn’t w a function of both t and z ?

Response: Yes, $w = w(t, z)$ in general; the two panels show two different slices of this function – $w(z)$ at a fixed (final) time, and $w(t)$ at a fixed (mid-height) location – and the revised figure

captions state this explicitly (“Density profile $\rho(z)$ at final time” and “Evolution of the normalized velocity $w(t)$ at mid-height,” Section 6, Figure 4) to avoid the ambiguity noted by the referee.

The section on the Volterra model is unnecessary. [...] I assume the same methods were used for the convection problem, however this is not clear. [...] Beside the analogy is poorly drawn. Readers will wonder what the variable $y(t)$ represents and [...] the storm system has multiple variables and it is a PDE while the Volterra problem is a scalar ODE.

Response: As described above, this material is now a single paragraph within Section 6, not a separate section, and the dedicated figure has been removed. The paragraph now states explicitly which variable plays which role (y mirrors α , Ψ mirrors Σ) and, directly addressing the referee’s concern, adds: “We do not present this as a rigorous reduction of the full storm PDE system to a scalar ODE – the two problems differ in structure, and the analogy should be read purely as an illustration that delayed, memory-based restoring feedback of this Gaussian-kernel type generically produces damped oscillatory relaxation toward equilibrium.”

Line 510: The code availability “upon reasonable request” policy is rather odd. Shouldn’t the code be made all publicly available for all EGU publications.

Response: The Data Availability statement has been updated: the complete Python scripts are now stated to be “publicly available as Supplementary Material accompanying this article,” and are attached as a code archive with this submission.

I am grateful to the referee once again for a review that has substantially strengthened the manuscript, and I hope the revised version, together with the Supplementary Material, adequately addresses all the points raised.