Referee report for:

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

Dalila Remaoun Bourega

Summary of the intended contribution

The manuscript studies a simplified model of a saturated moist atmosphere in a vertically oriented domain with circular horizontal cross-sectionional area whose radius may vary with altitude. The state variables are the gas density and temperature, together with a scalar intensity of the vertical flow and a position independent quantity representing the mean loading by suspended liquid or solid water. The dependence of horizontal variables is neglected. Condensation is modelled through the change, along an ascending trajectory, of the saturated vapour concentration. The weight of condensate enters the vertical momentum balance, while a memory kernel accounts for the finite residence time of droplets in the air.

In earlier works by Bourega, Aouaouda and Fujita Yashima the evolutionary model with a variable cross-section and reported oscillations of the vertical flow and water content. The main objective of the present manuscript is to place these models on a firmer mathematical ground by proving rigorously existence and uniqueness of a stationary solution of the reduced boundary-value problem (20)–(24). That is also what this referee report evaluate. The reduction assumes that the normalized vertical velocity w is constantly equal to one, and then reconstructs the cross-section $S(z)$ from the stationary continuity equation. Section 4 develops preliminary properties of a family of hydrostatic like initial-value problems, parametrized by $\vartheta \in [0, 1]$. Sections 5 and 6 introduce the additional parameter c_0 which substitute $g\Sigma$. Then a shooting problem is formulated for an additional upper boundary condition, and the author attempt to prove strict monotonicity with respect to c_0 by a comparison argument. This monotonicity is then used to infer uniqueness of the stationary state of the original system and to determine the flow intensity α from the condensate relation. Finally, it is shown that then $c_0 = g\Sigma$, where the latter is a function of the states T and ρ.

Section 7 and 8 give a numerical shooting procedure, compare dry and moist profiles, and revisit the damped oscillations of the evolutionary model. A Volterra integro-differential equation with a Gaussian memory kernel is proposed as a qualitative analogue of the feedback between flow intensity and droplet loading. The manuscript interprets the numerical convergence toward stationary values as evidence for the consistency of the model and for a memory-driven mechanism behind the observed oscillations.

General assessment

The manuscript clearly present the aforementioned programme. Not being an expert neither on moist atmosphere dynamics nor on numerics, I do not feel that I can give a final judgement on this part, but neither of them seems to be elaborated in sufficient details, but the final judgement should be left to an expert. My main objection is that the central mathematical result is not established due to a serious error in one of the lemmata, see below for more details.

My detailed assessment concerns mainly Sections 4–6. Overall, the mathematics is correct, presented in sufficient details and care. The techniques and estimates used are non-trivial and skillfully used. The level of details presented feels sometimes uneven, that is in some cases the details are excessive in another they are sparse.

The principal difficulty lies in Sections 5 and 6. The uniqueness proof depends on Lemma 5, specifically on the sign assertion (57). In equation display environment after line 260 the last equality is wrong. It should read $\frac{c_v+R_1}{R_1 T}$. The factor T is missing and leading to the wrong sign. The AI, see below for the declaration of usage, confirmed me that also numerically at c_0 the sign is in the wrong direction and that there is a sign change in the considered interval. This unfortunately destroys the entire proof of existence of the full system. I checked that this inequality seems essential. There are also independent algebraic errors in (59), that is in the denominator we have $\frac{T}{\rho}\frac{d\pi_{vs}(T)}{dT}$ instead what is there, however, this seems to have no serious consequences in the current presentation.

The physical modelling, the numerical simulations and the Volterra analogy are partly outside my expertise. I therefore do not assess their physical adequacy in detail. Some modelling choices require clearer qualification, and that the numerical figures and the AI noted that endpoint values appear internally inconsistent, see Appendix IV. Those sections should maybe not be presented as validation only, but also to cross check the analytical results by consistency consideration. For a more meteorological oriented venue, the author should give more care to the correctness and precision of the physical statement, the AI noted several inaccuracy and I personally could also sense a typical sloppiness mathematicians have with respect to physical interpretation, which may be acceptable in publication solely addressed to a mathematical audience, but insufficient if also meteorologist or physicists are supposed to read the paper.

Use of artificial intelligence in preparing this report

I used a generative AI system as an auxiliary tool during the preparation of this report. I used the AI to go through the text line by line with intermediate prompting and questioning from my side. It did algebraic checks, compare formulas appearing in different parts of the manuscript, and relate to literature. Symbolic identities and numerical result were checked independently with computer algebra and numerics package from phython, generated and executed by the AI (at least according to the AI). Section 4 was first evaluated independently of AI and was then used as a comparison against the AI-assisted analysis. The report was also drafted by the AI, but extensively rewritten. Purely AI generated parts, which I wanted to communicate to the author, but were not double checked by me a marked as such. Hence it should be thoroughly double checked by the author. For entertainment I attach as final Appendix V the unedited referee report. The accuracy was impressive, I did not caught any mistake by the AI, whenever I thought I found one, I did a miscalculation. I tried to use the AI as well to repair Lemma 5, however, that was less impressive. Maybe, I have to learn how to prompt the AI better for that. The automatically generated referee report, was quite ok, but had also the formulaic expression typical for AI and did not really followed my instructions well.

Overall judgment

I cannot recommend publication of the manuscript so long the situation in Lemma 5 is not clarify, as the main part of the paper seems incorrect. If that can be corrected the paper should be published modulo repairing further minor inconsistencies. See Appendix II for the minor corrections.

Appendix I: The central error in Lemma 5

Warning this is purely AI generated explanation which I did only superficially checked. I confirmed by hand the insufficiency of the proof in Lemma 5.

Let $\pi = \pi_{vs}(T)$. Equation (58) defines

$$D_1(\rho, T, c_0) = -\frac{\left(1 + \dfrac{(R_1 T + L_{\mathrm{tr}})\pi}{R_1 \rho T}\right)(g\rho + c_0)}{\rho(c_v + R_1) + (R_1 T + L_{\mathrm{tr}})\left(\dfrac{\pi}{T} + \pi'(T)\right)}.$$

Lemma 5 claims that $\partial_\rho D_1 > 0$ under condition (45). It is enough to test the admissible case $c_0 = 0$. Direct differentiation gives

$$\left.\frac{\partial D_1}{\partial \rho}\right|_{c_0=0} = \frac{g(R_1 T + L_{\mathrm{tr}})}{\left[\rho(c_v + R_1) + (R_1 T + L_{\mathrm{tr}})\left(\dfrac{\pi}{T} + \pi'\right)\right]^2}\left(\frac{c_v}{R_1 T}\pi - \pi'\right).$$

The inequality used in the manuscript is

$$T\frac{\pi'(T)}{\pi(T)} > \frac{c_v}{R_1}.$$

It implies

$$\frac{c_v}{R_1 T}\pi(T) - \pi'(T) < 0,$$

and therefore

$$\partial_\rho D_1\big|_{c_0=0} < 0,$$

not > 0. Thus (57) fails already at $c_0 = 0$. The estimate in lines 279–281 also drops a factor $1/T$; with that factor restored, the resulting bracket has the opposite sign on the intended temperature interval.

This error breaks the first-crossing argument in lines 295–300. The proof there requires the asserted increase of D_1 with respect to ρ in order to exclude a first contact between two temperature profiles corresponding to different values of c_0. Since that monotonicity is unavailable, Lemma 4 is not proved. In particular, the manuscript has not established the strict ordering

$$\rho^{(ii)}(z)T^{(ii)}(z) < \rho^{(i)}(z)T^{(i)}(z)$$

for $c^{(i)} < c^{(ii)}$. Hence strict monotonicity and injectivity of the endpoint map

$$c_0 \longmapsto \rho_{c_0}(z_1)T_{c_0}(z_1)$$

remain unproved, and the claimed uniqueness of the shooting parameter, of Σ, and consequently of the stationary solution does not follow.

This does not by itself prove that the uniqueness statement is false. A different comparison quantity, a phase-plane argument, or a direct analysis of the endpoint map might still establish it. No such replacement argument is presently given. The theorem should therefore be regarded as unproved rather than merely in need of a local typographical correction. Existence may be recoverable from continuity and suitable endpoint estimates, but it must be separated from uniqueness and proved without relying on Lemma 4.

Appendix II: Minor corrections and suggestions

The following list is intended to help locate additional issues. Some entries are more than typographical, but they are secondary to the failure of Lemma 5 discussed above. Points marked with (AI) are maybe superficially counter checked by me or are pure (AI) creation. Usual caution should apply.

1. Section 4 contains useful structural observations, but several arguments would benefit from being reorganized around the explicit two-dimensional vector field obtained by solving (26)–(27) for (T', ρ'). The determinant of this system is positive in the stated domain, so local solvability follows directly from standard ODE theory. The subsequent continuation and monotonicity arguments need more careful hypotheses, notation and sign checks. The analytical thresholds are not handled consistently: there are numerical rounding errors, the lower threshold is a singular boundary of the constitutive law rather than an ordinary admissible value, and the function used to define Θ_1 appears dimensionally inconsistent unless an unstated nondimensionalization is intended.
2. **Lines 21–24.** (AI) The claim that cumulonimbus clouds, tropical cyclones and thunderstorms are primarily caused by upward flow and condensation is too broad. A narrower statement tied to the model would be preferable.
3. **Lines 29–35.** For nonconstant $S(z)$ the domain is not a cylinder in the strict geometric sense. "Axisymmetric vertical tube with variable cross-section" would be more accurate. Positivity and regularity assumptions on S should be stated.
4. **Line 40.** (AI) "Gravitational waves" should almost certainly be "atmospheric gravity waves."
5. **Lines 42–48.** (AI) "Self-sustaining oscillatory cycle" is stronger than the later observation of damped oscillations. The physical chain should be presented as a proposed delayed feedback mechanism, not as an established causal explanation.
6. **Lines 75–79.** (AI) Define the normalization of w. The decomposition $v = \alpha w$ is nonunique without a condition such as $w(0) = 1$ or an averaged normalization. The assumptions $\partial_t w \approx \partial_t \rho \approx \partial_t T \approx 0$ should be identified either as exact closures or as asymptotic approximations.
7. **Equations (1), (2) and (25).** Clarify whether ρ is dry-air density or total gas-phase density. Converting saturation vapour pressure to vapour density ordinarily requires the specific gas constant of water vapour, not that of dry air. (AI) claims that in (2) that R_1 should be place by R_v.
8. **Lines 91–95 and equation (3).** (AI) H_{tr} has the units of mass per unit volume per unit time; it is a signed phase-change rate, not an "amount." The label "turbulent condensation rate" is unsupported by the displayed formula.
9. **Equation (7).** Σ is a height-averaged suspended-condensate density, not a total amount. If it is meant as a volume average in a tube with variable area, the integral should include $S(z)$ and be divided by $\int_0^{z_1} S(z)\,dz$. Furthermore, it is unclear to me, why it does not depend on z, as it gives the density of liquid or solid water.
10. **Equation (10).** Substitution of $v = \alpha w$ leaves factors of α in the transport and condensation terms. The printed evolutionary equation is inconsistent unless $\alpha = 1$ or stationarity is imposed before division.
11. **Equations (13), (17) and (22).** With the memory integral beginning at $t = 0$, a constant positive Σ is not an exact stationary solution. Formula (17), and hence (22), is the limit as

$t \to \infty$ or the result of assuming an infinite stationary prehistory. This interpretation should be stated.

12. **Line 105:** "external environment" what is meant with that.
13. **Equation (18).** The time-dependent equation again lacks the factor α and is dimensionally inconsistent. Its stationary reduction is valid after assuming $\alpha \neq 0$ and dividing by α.
14. **Line 127 and equation (19).** The two intermediate formulas have the wrong sign before $(\pi_{vs}/\rho)'$. The final formula (19) has the correct positive sign in the exponential.
15. **Lines 129–136.** Setting $w \equiv 1$ is not merely a normalization; (AI) it imposes altitude-independent cross-sectionally averaged velocity. The construction of $S(z)$ should be described as an inverse-geometry choice supporting that restriction.
16. **Equation (24).** (AI) Since $P = R_1\rho T$, the quantity q_0 is pressure divided by R_1, not pressure itself. Its units and relation to the external top pressure should be stated. How this extra boundary condition appears.
17. **Equation (26)** Explain the role of Θ. A direct physical interpretation should be taken with a lot of care.
18. **Equation (29).** (AI) Recalculation gives approximately $\Theta_+ = 1275.9845\,\mathrm{K}$, which rounds to $1276.0\,\mathrm{K}$ at one decimal place, not $1275.9\,\mathrm{K}$. It should be $\ln(10)\,1845$.
19. **Line 155:** The relation between $T\frac{d}{dt}\log(\pi_{vs}(T)) > \frac{c_v}{R_1}$ and (29) is unclear. The relations $\frac{c_v}{R_1} = \frac{5}{2}$ appears out of nowhere.
20. **Lines 157–165.** (AI) $\Theta_- = 31.25\,\mathrm{K}$ is the singular boundary of the empirical formula, not an ordinary physical threshold.
21. **(31).** the inequality is strict for $\theta > 0$.
22. **line after (40)** " by virtue of (36)" That is what has been used in the sentence before.
23. The definition of z^* is not so clear.
24. **(43).** "$\rho_0^{R_1}$" should be ρ_0.
25. **Definition of F_{ms} and condition (45).** The terms in (45) do not have matching physical dimensions. The condition seems to derive from the error in Lemma 5. (AI) The reported root is approximately $66.0961\,\mathrm{K}$, which rounds to $66.10\,\mathrm{K}$.
26. **Proposition 5.1 and equation (47).** The displayed infimum is misleading. The proof indicates that the intended variation is over q_0, or equivalently over c_0; the statement should be rewritten.
27. **Equation (48).** State explicitly that c_0 is the substitute for $g\Sigma$.
28. **Line 245.** $\nu(c_0)$ was not defined yet D_1 as well. The definition of $\overline{\mathbf{C}_0}$ could be more explicit. (51) it is true that the limit exist, but why should it be Θ_1. I think a more careful definition of $\overline{\mathbf{C}}_0$ is needed.
29. **Line 254.** "Become more singular" this is at Θ_- maybe but not at Θ_1.
30. **Equation (56).** (AI) "In the neighborhood of z" is imprecise. State that $(\rho, T)d$ is a local solution of the initial-value problem and specify its interval of existence.
31. **Equation (59).** The denominator obtained by eliminating T' contains

$$(R_1 T + L_{\mathrm{tr}})\left(\frac{\pi_{vs}(T)}{\rho} + \frac{T}{\rho}\pi_{vs}'(T)\right),$$

not the printed term involving $(c_v/R_1)\pi'_{vs}(T)$.

32. **Equation before (64).** Integration of (48) gives a factor $1/g$; the estimate should contain $(c^{(ii)} - c^{(i)})/g$.

33. **Line 298.** "By virtue of (57)" the reference seems to be wrong

34. **Section 7.** (AI) The nonlinear solver should respect $c_0 \geq 0$. The reported residual is a residual of the discredited shooting map, not automatically of the continuous problem. A mesh-refinement study and ODE tolerance data are required, especially because uniqueness has not been established.

35. Why Θ has been fixed. Θ looks more than an interpolation parameter than a real physical quantity. At least it is not physically derived.

36. why L is not T independent as in the mathematical consideration. I am not so convinced that the proof can be extended to non constant $L(T)$.

37. **Figures 1 and 2.** (AI) The endpoint products visually inferred from Figure 1 do not agree with the values of q_0 reported in the text and Figure 2, see also Appendix IV. The code should report $\rho(z_1)$, $T(z_1)$ and the boundary residual directly for each run.

38. **Figures 3 and 4.** (AI) The apparent limiting values of α and Σ do not coincide with the tabulated stationary values, and the profiles do not visibly match those in Figure 1. The meaning of "convergence to the stationary state" should be reassessed after the data are reconciled.

39. **Lines 469–473.** (AI) The Gaussian kernel is integrable on $[0, \infty)$; the statement that the formulation may fail an integrability condition is therefore incorrect as written.

Appendix III: Simpler proof for the monotonicity of ρ and T

Warning this is a purely AI generated note, I did not check in details, but seems correct.

Fix a constant parameter

$$\vartheta \in [0,1].$$

Consider

$$\rho c_v T' - R_1 T\rho' = \vartheta(R_1 T + L_{\mathrm{tr}})\left(\pi_{vs}(T)\frac{\rho'}{\rho} - \pi_{vs}'(T)T'\right), \tag{26}$$

$$R_1(\rho T)' = -g\rho, \tag{27}$$

$$\rho(0) = \rho_0, \qquad T(0) = T_0. \tag{28}$$

Here $T' = dT/dz$ and $\rho' = d\rho/dz$.

1. Linear system for (T', ρ')

Equation (26) can be rewritten as

$$A(\rho, T, \vartheta)T' - B(\rho, T, \vartheta)\rho' = 0,$$

where

$$A(\rho, T, \vartheta) = \rho c_v + \vartheta(R_1 T + L_{\mathrm{tr}})\pi_{vs}'(T),$$
$$B(\rho, T, \vartheta) = R_1 T + \vartheta(R_1 T + L_{\mathrm{tr}})\frac{\pi_{vs}(T)}{\rho}.$$

Equation (27) is equivalent to

$$R_1\rho T' + R_1 T\rho' = -g\rho.$$

Thus

$$\begin{pmatrix} A & -B \\ R_1\rho & R_1 T \end{pmatrix}\begin{pmatrix} T' \\ \rho' \end{pmatrix} = \begin{pmatrix} 0 \\ -g\rho \end{pmatrix}.$$

2. Determinant

The determinant of the coefficient matrix is

$$\begin{aligned}\Delta(\rho, T, \vartheta) &= R_1(AT + B\rho) \\ &= R_1\left[\rho T(c_v + R_1) + \vartheta(R_1 T + L_{\mathrm{tr}})(T\pi_{vs}'(T) + \pi_{vs}(T))\right].\end{aligned}$$

For the constitutive function

$$\pi_{vs}(T) = \frac{E_0}{R_1 T}10^{\frac{7.63(T-273.15)}{T-31.25}},$$

one has

$$\frac{\pi_{vs}'(T)}{\pi_{vs}(T)} = -\frac{1}{T} + \frac{7.63\ln(10)(273.15-31.25)}{(T-31.25)^2}.$$

Consequently,

$$\begin{aligned}T\pi_{vs}'(T) + \pi_{vs}(T) &= \pi_{vs}(T)\left(T\frac{\pi_{vs}'(T)}{\pi_{vs}(T)} + 1\right) \\ &= \pi_{vs}(T)\frac{7.63\ln(10)(273.15-31.25)\,T}{(T-31.25)^2} > 0\end{aligned}$$

for every $T > 31.25$.

Hence, under

$$\rho > 0, \qquad T > 31.25, \qquad R_1 > 0, \qquad c_v > 0, \qquad g > 0, \qquad R_1 T + L_{\rm tr} > 0,$$

we have

$$\Delta(\rho, T, \vartheta) > 0.$$

3. Explicit vector field

Cramer's rule gives

$$T' = -\frac{g\rho B}{\Delta}, \qquad \rho' = -\frac{g\rho A}{\Delta}.$$

Equivalently,

$$T' = -\frac{g\left[R_1\rho T + \vartheta(R_1 T + L_{\rm tr})\pi_{vs}(T)\right]}{R_1\left[\rho T(c_v + R_1) + \vartheta(R_1 T + L_{\rm tr})\big(T\pi'_{vs}(T) + \pi_{vs}(T)\big)\right]},$$

and

$$\rho' = -\frac{g\rho\left[\rho c_v + \vartheta(R_1 T + L_{\rm tr})\pi'_{vs}(T)\right]}{R_1\left[\rho T(c_v + R_1) + \vartheta(R_1 T + L_{\rm tr})\big(T\pi'_{vs}(T) + \pi_{vs}(T)\big)\right]}.$$

Therefore the system has the explicit form

$$(T', \rho') = F_\vartheta(T, \rho).$$

4. Local existence and uniqueness

On every open set contained in

$$\mathcal{D} = \Big\{(\rho, T) \in \mathbb{R}^2 : \rho > 0,\ T > 31.25\Big\},$$

the function π_{vs} is smooth. If $L_{\rm tr}$ is constant, then F_ϑ is smooth on $\mathcal{D}$. More generally, the same conclusion holds if $L_{\rm tr} = L_{\rm tr}(T)$ is sufficiently regular and $R_1 T + L_{\rm tr}(T) > 0$.

Since $\Delta > 0$ on $\mathcal{D}$, the vector field has no algebraic singularity there. Standard Picard–Lindelöf theory therefore yields:

For every fixed $\vartheta \in [0, 1]$ and every initial value $(\rho_0, T_0) \in \mathcal{D}$, there exists a unique local C^1 solution of (26)–(28).

This is a local statement only. Extension to a prescribed interval requires a priori bounds that keep the solution in a compact subset of $\mathcal{D}$.

5. Sign information

The denominator in both explicit equations is positive. Moreover,

$$R_1\rho T + \vartheta(R_1 T + L_{\rm tr})\pi_{vs}(T) > 0,$$

so

$$T' < 0$$

throughout the admissible region.

For ρ', one additionally needs

$$A(\rho, T, \vartheta) = \rho c_v + \vartheta(R_1 T + L_{\rm tr})\pi'_{vs}(T) > 0.$$

This follows, for example, on any temperature interval on which $\pi'_{vs}(T) \geq 0$. For the displayed constitutive law, $\pi'_{vs}(T) > 0$ on the interval later used in the manuscript, namely $31.25 < T < \Theta_+$.

6. Interpretation of ϑ

Nothing in the local ODE argument requires a physical interpretation of ϑ. It may be treated simply as a fixed homotopy parameter interpolating between the systems at $\vartheta = 0$ and $\vartheta = 1$.

Appendix IV: Inconsitency of the graphs

Warning this is pure AI and should be thoroughly checked.

Figure 1 and Figure 2 appear not to describe the same stationary solutions. The top boundary condition for a stationary profile is

$$\rho(z_1)T(z_1) = q_0.$$

According to the text and Figure 2, the two examples correspond to

$$q_0^{\text{humid}} = 94.08, \qquad q_0^{\text{dry}} = 85.51.$$

However, approximate visual readings from Figure 1 give

$$\begin{aligned} \rho(z_1) &\approx 0.235, \\ T_{\text{humid}}(z_1) &\approx 246 \text{ K}, \\ T_{\text{dry}}(z_1) &\approx 226 \text{ K}. \end{aligned}$$

Consequently, the endpoint products inferred from Figure 1 are approximately

$$\begin{aligned} \rho(z_1)T_{\text{humid}}(z_1) &\approx 0.235 \times 246 \approx 57.8, \\ \rho(z_1)T_{\text{dry}}(z_1) &\approx 0.235 \times 226 \approx 53.1. \end{aligned}$$

The comparison is therefore

	Figure 1, inferred	Text/Figure 2
humid case	≈ 57.8	94.08
dry case	≈ 53.1	85.51

Although the values read from Figure 1 are only approximate, the differences are too large to be explained by ordinary graphical reading error. Thus, at least one of the following must be incorrect:

1. the density profile displayed in Figure 1;
2. the temperature profiles displayed in Figure 1;
3. the scaling or labelling of one of the axes in Figure 1;
4. the reported values of q_0;
5. the assertion that Figures 1 and 2 correspond to the same computed stationary solutions.

The inconsistency is particularly important because Figure 2 assigns the values

$$(\Sigma, q_0) \approx (0.0153, 94.08) \quad \text{and} \quad (\Sigma, q_0) \approx (0.0562, 85.51)$$

to the two numerical solutions, whereas the endpoint products visible in Figure 1 are substantially smaller.

The authors should report the numerical endpoint values

$$\rho(z_1), \qquad T(z_1), \qquad \rho(z_1)T(z_1),$$

for both runs directly from the code, together with the final boundary residual

$$\rho(z_1)T(z_1) - q_0.$$

They should then regenerate the figures or explain why the apparent endpoint values in Figure 1 do not coincide with the values represented in Figure 2.

Appendix V: The unedited referee report

Summary of the intended contribution

The manuscript studies a simplified model of a saturated moist atmosphere in a vertically oriented domain whose cross-sectional area may vary with altitude. The state variables are the gas density and temperature, together with a scalar intensity of the vertical flow and a scalar quantity representing the mean loading by suspended liquid or solid water. Condensation is modelled through the change, along an ascending trajectory, of the saturated vapour concentration. The weight of condensate enters the vertical momentum balance, while a memory kernel accounts for the finite residence time of droplets in the air.

The paper is presented as a continuation of Bourega, Aouaouda and Fujita Yashima, *Rain oscillation in a mathematical model of a thunderstorm* (2018). That earlier work introduced the evolutionary model with a variable cross-section and reported oscillations of the vertical flow and water content. The present manuscript seeks to place those computations on a firmer mathematical basis. Its principal analytical objective is to prove existence and uniqueness of a stationary solution of the reduced boundary-value problem (20)–(24). The reduction assumes that the normalized vertical profile is constant, $w \equiv 1$, and then reconstructs the cross-section $S(z)$ from the stationary continuity equation. Section 4 develops preliminary properties of a family of hydrostatic initial-value problems, parametrized by $\vartheta \in [0, 1]$. Sections 5 and 6 introduce the additional parameter $c_0 = g\Sigma$, formulate a shooting problem for the upper boundary condition, and attempt to prove strict monotonicity with respect to c_0 by a comparison argument. This monotonicity is then used to infer uniqueness of the stationary state and to determine the flow intensity α from the condensate relation.

The later sections give a numerical shooting procedure, compare dry and moist profiles, and revisit the damped oscillations of the evolutionary model. A Volterra integro-differential equation with a Gaussian memory kernel is proposed as a qualitative analogue of the feedback between flow intensity and droplet loading. The manuscript interprets the numerical convergence toward stationary values as evidence for the consistency of the model and for a memory-driven mechanism behind the observed oscillations.

General assessment

The manuscript has a clearly identifiable mathematical programme: reduce the atmospheric model to a tractable stationary system, establish a well-posed shooting problem, and relate the stationary state to the previously observed oscillations. This is a reasonable direction of investigation. In its present form, however, the central mathematical result is not established.

My detailed assessment concerns mainly Sections 4–6. Section 4 contains useful structural observations, but several arguments would benefit from being reorganized around the explicit two-dimensional vector field obtained by solving (26)–(27) for (T', ρ'). The determinant of this system is positive in the stated domain, so local solvability follows directly from standard ODE theory. The subsequent continuation and monotonicity arguments need more careful hypotheses, notation and sign checks. The analytical thresholds are not handled consistently: there are numerical rounding errors, the lower threshold is a singular boundary of the constitutive law rather than an ordinary admissible value, and the function used to define Θ_1 appears dimensionally inconsistent unless an unstated nondimensionalization is intended.

The principal difficulty lies in Sections 5 and 6. The uniqueness proof depends on Lemma 5, specifically on the sign assertion (57). That assertion is false under the very inequality used elsewhere in the paper. Consequently, the comparison proof of Lemma 4 and the claimed strict monotonicity of the shooting map do not follow. There are also independent algebraic errors in

(59) and (64). These are not merely presentational defects; they affect the logical chain leading to the main theorem.

The exposition is generally understandable at the level of the intended ideas, but it is not yet sufficiently precise for a mathematical paper. Definitions, physical interpretations and proof steps are frequently interwoven, and several statements move from an approximation to an exact identity without explanation. Quantifiers and parameter dependences are sometimes unclear, while intermediate formulas contain sign or factor errors that are corrected, implicitly, in later formulas. The manuscript would profit from a shorter and more disciplined proof structure: state the domain and assumptions once, derive the vector field explicitly, isolate each comparison result, and indicate exactly which result is used at each later step.

The physical modelling, the numerical simulations and the Volterra analogy are partly outside my expertise. I therefore do not assess their physical adequacy in detail. I note only that some modelling choices require clearer qualification, and that the numerical figures and reported endpoint values appear internally inconsistent. Those sections should not be presented as validation of the theorem until the analytical and numerical discrepancies have been resolved.

The central error in Lemma 5

Let $\pi = \pi_{vs}(T)$. Equation (58) defines

$$D_1(\rho, T, c_0) = -\frac{\left(1 + \dfrac{(R_1 T + L_{\mathrm{tr}})\pi}{R_1 \rho T}\right)(g\rho + c_0)}{\rho(c_v + R_1) + (R_1 T + L_{\mathrm{tr}})\left(\dfrac{\pi}{T} + \pi'(T)\right)}.$$

Lemma 5 claims that $\partial_\rho D_1 > 0$ under condition (45). It is enough to test the admissible case $c_0 = 0$. Direct differentiation gives

$$\left.\frac{\partial D_1}{\partial \rho}\right|_{c_0=0} = \frac{g(R_1 T + L_{\mathrm{tr}})}{\left[\rho(c_v + R_1) + (R_1 T + L_{\mathrm{tr}})\left(\dfrac{\pi}{T} + \pi'\right)\right]^2}\left(\frac{c_v}{R_1 T}\pi - \pi'\right).$$

The inequality used in the manuscript is

$$T\frac{\pi'(T)}{\pi(T)} > \frac{c_v}{R_1}.$$

It implies

$$\frac{c_v}{R_1 T}\pi(T) - \pi'(T) < 0,$$

and therefore

$$\left.\partial_\rho D_1\right|_{c_0=0} < 0,$$

not > 0. Thus (57) fails already at $c_0 = 0$. The estimate in lines 279–281 also drops a factor $1/T$; with that factor restored, the resulting bracket has the opposite sign on the intended temperature interval.

This error breaks the first-crossing argument in lines 295–300. The proof there requires the asserted increase of D_1 with respect to ρ in order to exclude a first contact between two temperature profiles corresponding to different values of c_0. Since that monotonicity is unavailable, Lemma 4 is not proved. In particular, the manuscript has not established the strict ordering

$$\rho^{(ii)}(z)T^{(ii)}(z) < \rho^{(i)}(z)T^{(i)}(z)$$

for $c^{(i)} < c^{(ii)}$. Hence strict monotonicity and injectivity of the endpoint map

$$c_0 \longmapsto \rho_{c_0}(z_1) T_{c_0}(z_1)$$

remain unproved, and the claimed uniqueness of the shooting parameter, of Σ, and consequently of the stationary solution does not follow.

This does not by itself prove that the uniqueness statement is false. A different comparison quantity, a phase-plane argument, or a direct analysis of the endpoint map might still establish it. No such replacement argument is presently given. The theorem should therefore be regarded as unproved rather than merely in need of a local typographical correction. Existence may be recoverable from continuity and suitable endpoint estimates, but it must be separated from uniqueness and proved without relying on Lemma 4.

Use of artificial intelligence in preparing this report

I used a generative AI system as an auxiliary tool during the preparation of this report. It helped to organize a line-by-line reading, propose algebraic checks, compare formulas appearing in different parts of the manuscript, and draft preliminary formulations. Substantive symbolic identities were checked independently with computer algebra where feasible. Section 4 was first evaluated independently of AI and was then used as a comparison against the AI-assisted analysis. The AI output was treated as a source of suggestions rather than as mathematical authority; the scientific responsibility for the report remains with the referee. In particular, every AI-suggested correction should be checked again before it is incorporated into a final report or communicated as a definitive claim.

Overall judgment

I cannot recommend publication of the manuscript in its present form. The main existence-and-uniqueness claim is the central mathematical contribution, and its uniqueness part relies on a false derivative sign. The proof must be replaced, not simply edited. A substantial revision should also correct the auxiliary formulas, distinguish exact stationary statements from long-time limits of the memory equation, and reconcile the numerical outputs with the boundary conditions.

The underlying problem remains potentially worthwhile. A revised manuscript that states a narrower theorem, gives a complete proof under transparent assumptions, and presents reproducible numerical checks could merit reconsideration. At present, however, the mathematical conclusions in the abstract and conclusion are stronger than the analysis supports.

Minor corrections and suggestions

The following list is intended to help locate additional issues. Some entries are more than typographical, but they are secondary to the failure of Lemma 5 discussed above.

1. **Lines 21–24.** The claim that cumulonimbus clouds, tropical cyclones and thunderstorms are primarily caused by upward flow and condensation is too broad. A narrower statement tied to the model would be preferable.

2. **Lines 29–35.** For nonconstant $S(z)$ the domain is not a cylinder in the strict geometric sense. "Axisymmetric vertical tube with variable cross-section" would be more accurate. Positivity and regularity assumptions on S should be stated.

3. **Line 40.** "Gravitational waves" should almost certainly be "atmospheric gravity waves."

4. **Lines 42–48.** "Self-sustaining oscillatory cycle" is stronger than the later observation of damped oscillations. The physical chain should be presented as a proposed delayed feedback mechanism, not as an established causal explanation.

5. **Lines 75–79.** Define the normalization of w. The decomposition $v = \alpha w$ is nonunique without a condition such as $w(0) = 1$ or an averaged normalization. The assumptions $\partial_t w \approx \partial_t \rho \approx \partial_t T \approx 0$ should be identified either as exact closures or as asymptotic approximations.

6. **Equations (1), (2) and (25).** Clarify whether ρ is dry-air density or total gas-phase density. Converting saturation vapour pressure to vapour density ordinarily requires the specific gas constant of water vapour, not that of dry air. The unit conversion of E_0 should also be explicit.

7. **Lines 91–95 and equation (3).** H_{tr} has the units of mass per unit volume per unit time; it is a signed phase-change rate, not an "amount." The label "turbulent condensation rate" is unsupported by the displayed formula.

8. **Equation (7).** Σ is a height-averaged suspended-condensate density, not a total amount. If it is meant as a volume average in a tube with variable area, the integral should include $S(z)$ and be divided by $\int_0^{z_1} S(z)\, dz$.

9. **Equation (10).** Substitution of $v = \alpha w$ leaves factors of α in the transport and condensation terms. The printed evolutionary equation is inconsistent unless $\alpha = 1$ or stationarity is imposed before division.

10. **Equations (13), (17) and (22).** With the memory integral beginning at $t = 0$, a constant positive Σ is not an exact stationary solution. Formula (17), and hence (22), is the limit as $t \to \infty$ or the result of assuming an infinite stationary prehistory. This interpretation should be stated.

11. **Equation (18).** The time-dependent equation again lacks the factor α and is dimensionally inconsistent. Its stationary reduction is valid after assuming $\alpha \neq 0$ and dividing by α.

12. **Line 127 and equation (19).** The two intermediate formulas have the wrong sign before $(\pi_{vs}/\rho)'$. The final formula (19) has the correct positive sign in the exponential.

13. **Lines 129–136.** Setting $w \equiv 1$ is not merely a normalization; it imposes altitude-independent cross-sectionally averaged velocity. The construction of $S(z)$ should be described as an inverse-geometry choice supporting that restriction.

14. **Equations (20)–(22).** The unknowns are $\rho(z)$, $T(z)$, Σ and α, not only (ρ, T, α). Equation (20) was obtained after cancelling the velocity and therefore is not automatically justified at $\alpha = 0$.

15. **Equation (24).** Since $P = R_1 \rho T$, the quantity q_0 is pressure divided by R_1, not pressure itself. Its units and relation to the external top pressure should be stated.

16. **Equation (29).** Recalculation gives approximately $\Theta_+ = 1275.9845\,\mathrm{K}$, which rounds to $1276.0\,\mathrm{K}$ at one decimal place, not $1275.9\,\mathrm{K}$.

17. **Lines 157–165.** $\Theta_- = 31.25\,\mathrm{K}$ is the singular boundary of the empirical formula, not an ordinary physical threshold. The assertion that all analytical conditions are automatically satisfied should be revisited once the later threshold Θ_1 is corrected.

18. **Definition of F_{ms} and condition (45).** The terms in F_{ms} do not have matching physical dimensions unless temperature and the constitutive quantities have been nondimensionalized. The manuscript should supply the nondimensionalization or reformulate the condition. The reported root is approximately $66.0961\,\mathrm{K}$, which rounds to $66.10\,\mathrm{K}$.

19. **Proposition 5.1 and equation (47).** The displayed infimum is malformed when q_0 is fixed, since it appears to be taken over a singleton. The proof indicates that the intended variation is over q_0, or equivalently over c_0; the statement should be rewritten.

20. **Equation (48).** State explicitly that $c_0 = g\Sigma$, including the units and the condition $c_0 \geq 0$.

21. **Equation (56).** “In the neighborhood of z” is imprecise. State that (ρ, T) is a local solution of the initial-value problem and specify its interval of existence.

22. **Equation (59).** The denominator obtained by eliminating T' contains

$$(R_1 T + L_{\mathrm{tr}}) \left(\frac{\pi_{vs}(T)}{\rho} + \frac{T}{\rho} \pi'_{vs}(T) \right),$$

    not the printed term involving $(c_v/R_1)\pi'_{vs}(T)$.

23. **Equation (64).** Integration of (48) gives a factor $1/g$; the estimate should contain $(c^{(ii)} - c^{(i)})/g$.

24. **Section 7.** The nonlinear solver should respect $c_0 \geq 0$. The reported residual is a residual of the discretized shooting map, not automatically of the continuous problem. A mesh-refinement study and ODE tolerance data are required, especially because uniqueness has not been established.

25. **Figures 1 and 2.** The endpoint products visually inferred from Figure 1 do not agree with the values of q_0 reported in the text and Figure 2. The code should report $\rho(z_1)$, $T(z_1)$ and the boundary residual directly for each run.

26. **Figures 3 and 4.** The apparent limiting values of α and Σ do not coincide with the tabulated stationary values, and the profiles do not visibly match those in Figure 1. The meaning of “convergence to the stationary state” should be reassessed after the data are reconciled.

27. **Lines 469–473.** The Gaussian kernel is integrable on $[0, \infty)$; the statement that the formulation may fail an integrability condition is therefore incorrect as written.

28. **Conclusion, lines 487–500.** Claims of rigorous existence and uniqueness, exact convergence to stationary values, and robust validation should be weakened until the proof and numerical inconsistencies are resolved.