Referee Report

Manuscript: *Stochastic Collection Equation: Invariant Evolution of the Cloud Droplet–Size Distribution*

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Recommendation: Major revision

1 Summary of the manuscript

The manuscript applies scaling (similarity) arguments to the stochastic collection equation (SCE, Smoluchowski coagulation equation) in order to clarify when a separate "rain" mode can emerge from an initially unimodal cloud–droplet spectrum. After a nondimensionalisation (Sect. 2) and an unsuccessful attempt at a separable solution (Sect. 3), a family of scaling transformations is identified that leaves the SCE form-invariant provided the kernel is homogeneous of degree γ (Sect. 4); similarity forms are inferred for $\gamma = 0, 1, 2$. Section 5 re-derives similarity solutions as a generalised separation of variables in $(t, \xi = \alpha(t)x)$ and recovers the classical constant-kernel (Schumann) solution and, for $\gamma = 1$, two candidate asymptotic solutions. Section 6 presents numerical integrations with Bott's (1998) flux scheme for $K = 1$, $K = x + y$ and $K = x^2 + y^2$. The central claim is that for a strictly scale-invariant kernel the spectrum merely translates towards larger sizes while preserving its shape, so that a rain mode requires a scale-*breaking* ingredient (a scale-dependent kernel, or sedimentation).

2 General assessment

The underlying idea has merit. The observation that the cloud/rain dichotomy of bulk microphysics cannot follow from the SCE alone, but must originate in a breaking of the scaling symmetry of the kernel (or in a process outside the SCE), is a potentially useful statement for the cloud-physics community, and combining a scaling argument with direct numerical integration is an appropriate strategy. Whether the paper delivers on this idea is, at present, difficult to judge, because the execution has a large number of problems — some presentational, several substantive. My main concerns are:

- **Novelty is not delineated.** The manuscript itself points to a substantial existing literature (Grigoriev and Meleshko 1995; Grigoriev et al. 2010; Lin et al. 2024; Xie 2024) but dismisses it in one sentence as "too involved". It is never stated which results are new; as far as I can judge, most of the similarity structure derived here is classical.
- **The symmetry analysis is not a symmetry analysis.** What is performed is a scaling ansatz on the *parameters* of the equation. Transformation (4.1) acts on the kernel K as well, i.e. it is most likely an equivalence transformation of the family of SCEs and not a symmetry of a given SCE. No infinitesimal generators, no commutators, no group dimension and no invariant surface conditions are given. These points do not necessarily have to be mentioned, but in that case this paper should not be called "symmetry analysis", but rather something along the lines of "a scaling approach was taken".
- **Notation impedes reading.** Symbols are re-used for different objects ($\tilde{}$, ν, ξ, m, λ, t_0), and quantities introduced as dimensional reference scales later reappear as group parameters.

- **Carelessness.** There are many algebraic slips, unnumbered equations, missing derivation steps, mutually contradictory statements, and a reference list with errors in almost every second entry. At least three errors invalidate the displayed algebra as printed (M12a, M13c, M14d).
- **Missing physics: gelation.** For homogeneous kernels with $\gamma > 1$ the SCE loses mass in finite time. This is precisely the "collapse at t_0" found for $\gamma = 2$, yet gelation is never mentioned, the relevant literature is not cited, and mass conservation is asserted without qualification (l. 70).
- **The central claim is never tested.** All three kernels of Sect. 6 are homogeneous, so only the negative half of the argument is illustrated.

I recommend **major revision** and would be willing to review a revised version. I have checked a number of the equations. Please check all of this again carefully, as I may, of course, have made a mistake in my calculations.

3 Major comments

M1. Novelty relative to existing symmetry analyses (Sects. 1, 4, 5).

(a) The example given as evidence for excessive complexity, Eq. (5.16), is the classical Golovin (1963)/Scott (1968) additive-kernel solution: a closed form in one modified Bessel function, neither involved nor obscure. Justify the characterisation or delete it.

(b) State explicitly — ideally in a table — which transformations and similarity solutions are *new* and which reproduce Grigoriev and Meleshko (1995), Grigoriev et al. (2010), Lin et al. (2024), Xie (2024) or the classical self-preserving-distribution literature. The manuscript already concedes priority for Eq. (4.3c) (Srivastava 1988), Eqs. (4.4a–c) (Xie 2024) and Eq. (5.6a) (Xie 2024).

(c) The mathematical and statistical-physics literature on scaling solutions of Smoluchowski's equation is not cited at all. Essential context: van Dongen and Ernst (1988) for the classification of scaling solutions by (γ, μ) and the distinction between mass-conserving and fat-tailed profiles; Leyvraz (2003, Phys. Rep.) for a review including gelation; Menon and Pego (2004, CPAM) for the solvable kernels $K = 1$, $x + y$, xy; Escobedo, Mischler and Perthame (2002) and Fournier and Laurençot (2005/2006) for existence, uniqueness and gelation. Several questions left open here (e.g. the failure of the $\gamma = 1$ ansatz, M14e) are answered there.

M2. Physical motivation of Eq. (2.1) (Sect. 2). For a paper aiming at physical insight, a pointer to Pruppacher and Klett (1997) is too little. Add a short derivation (half a page) covering: the mean-field assumptions behind the product form $n(x - y)n(y)$; the origin of the factor $\frac{1}{2}$ in the gain term (each pair is counted twice); the origin of the loss term; and the interpretation of $K(x, y)\,\mathrm{d}t$ as a coalescence probability per unit time and volume, together with the requirements $K \geq 0$ and $K(x, y) = K(y, x)$. The symmetry requirement is used implicitly throughout and is violated by the kernel on l. 242 (M13c).

M3. Double role of the scaling constants (Sects. 2, 4). In Eq. (2.2), x_0, t_0, K_0, N_0, n_0 are *dimensional* reference scales; in Eq. (4.1) the same symbols are *dimensionless group parameters*. Use two distinct symbol sets, e.g. $x_{\mathrm{ref}}, t_{\mathrm{ref}}, \ldots$ in Sect. 2 and a_x, a_t, a_K, a_n (or better e^{ε_x}, see M9) in Sect. 4, and state why the two operations are formally similar but logically distinct. Relatedly, the whole discussion from Sect. 4 onwards is conducted in terms of the scaling *factors* rather than the transformation itself, which is unusual and hard to follow; please reformulate in terms of the transformation (equivalently, its generators).

M4. “the latter is conserved with time” (l. 70).

(a) $\mathrm{d}m/\mathrm{d}t = 0$ follows from Eq. (2.1) only after interchanging the order of integration and symmetrising, and only if the integrals converge. Show this (two lines): Eq. (2.4b), Eqs. (4.4b,c) and Eqs. (5.4,5.5) all depend on it.

(b) For $\gamma > 1$ mass is *not* conserved beyond a finite gelation time (Leyvraz 2003; Escobedo et al. 2002; Fournier and Laurençot 2006 — already cited for another purpose). Since $\gamma = 2$ is one of the three test cases, and since the “collapse at $t = t_0$” in Eq. (4.10c) and Sect. 6.3 *is* gelation, this must be stated and connected to the numerics. The finite-time blow-up of Eq. (4.10c) then becomes a physically meaningful backward self-similar approach to t_{gel}, which strengthens the paper. Note also that the kernel truncation at 10^6 (l. 370) regularises exactly this singularity (M15b).

M5. l. 80: nondimensionalisation does not imply scale invariance. Eq. (2.5) shows that the SCE is *form-invariant* under the rescaling Eq. (2.2) provided $\tilde{K}$ has the same functional form as K — an assumption on K (homogeneity), not a property of Eq. (2.1). Form-invariance is moreover a statement about the equation, not about “the solution”. Please reformulate and add a forward reference to the scaling group of Sect. 4. The qualifying remark at ll. 82–83 arrives a paragraph too late and should be merged into the statement.

M6. Notation must be made unambiguous. This is my most important presentational request, and in places a matter of correctness. Collisions:

- **Tildes:** nondimensional variables in Sect. 2, then the shape function $\tilde{n}$ and time factor $\tilde{N}$ from Sect. 3 on. The warning at ll. 90–91 is not a solution; use a different decoration.
- ν**:** the rescaled density in Eq. (4.8b) and the similarity profile in Eqs. (4.9)–(4.10), Eq. (4.12), Eqs. (6.2)–(6.4) and the figures. Since it is a density, use $\hat{n}$ or n'. Same for Eq. (5.1a). As printed, the reader must track three densities (n, $\tilde{n}$, ν) differing only by t- or x-dependent factors.
- ξ**:** $tx^{\gamma-1}$ in Eq. (4.7a), $\alpha(t)x = Nx$ in Eqs. (5.1b) and (5.5b), and $\xi = t$ in Eq. (6.3b).
- m**:** total mass in Eq. (2.3b) and Eqs. (4.4a,b), but droplet mass (i.e. x) in Eq. (5.16), which is therefore unreadable as printed.
- λ**:** separation constant/eigenvalue in Eqs. (3.3), (5.7)–(5.10), but also the free parameter of the initial distributions Eqs. (6.1a,b) and of every figure caption.
- t_0**:** group parameter in Eq. (4.1b) but collapse (gelation) time in Eqs. (4.10c) and (6.4).
- **Subscript** 0**:** used for group parameters, reference scales, initial values and the Gaussian centre Eq. (6.1a).

I encourage the authors to add a symbol table, as an appendix or even at the beginning of the manuscript.

M7. Section 3 should be moved to an appendix. It establishes only that a strictly separable ansatz is inconsistent with mass conservation — a useful negative result and a natural stepping stone to Eq. (5.1a), but it interrupts the argument. Move it to an appendix, keeping two or three sentences in the main text, and while doing so:

- make ll. 107–108 explicit: “not self-consistent” is asserted before the argument is given. The inconsistency is between Eq. (3.4) and Eq. (3.5), i.e. between the separable ansatz and mass conservation;
- note that Eq. (3.3b) is a nonlinear eigenvalue problem in $\tilde{n}$ with eigenvalue λ; its solvability is a separate question and should not be conflated with the mass-conservation obstruction;
- note that the obstruction relies on mass conservation and hence does not apply directly to gelling kernels (M4).

M8. Eqs. (4.1a–d) are (most likely) an equivalence transformation, not a symmetry. The transformation acts on K, which is a fixed function specifying the equation rather than a dependent variable; it therefore maps one member of the SCE family to another. A genuine point symmetry of a *given* SCE survives only on the subgroup leaving K invariant, i.e. the condition $K_0 = x_0^\gamma$ imposed later in Eq. (4.5b). Please (i) state the distinction, (ii) identify equivalence group and symmetry subgroup separately, and (iii) correct the terminology throughout, where “invariant transformation”, “symmetry” and “scale invariance” are used interchangeably. This is not pedantry: which transformations may be used to generate new solutions from old ones depends on it.

M9. Use the exponential (Lie) parametrisation and determine the group dimension. With $x^* = e^{\varepsilon_x} x$, $t^* = e^{\varepsilon_t} t$, $n^* = e^{\varepsilon_n} n$, $K^* = e^{\varepsilon_K} K$, the multiplicative constraint Eq. (4.3a) becomes the linear relation $\varepsilon_n + \varepsilon_K + \varepsilon_x + \varepsilon_t = 0$, i.e. manifestly a hyperplane in a four-dimensional parameter space. Written this way it is apparent — as should be verified and stated — that Eq. (4.3a) defines a **three-parameter** group. Please:

(a) present the generators (maybe in an appendix), e.g. $X_1 = x\partial_x - K\partial_K$, $X_2 = t\partial_t - n\partial_n$, $X_3 = n\partial_n - K\partial_K$, together with the time translation $X_4 = \partial_t$ that is used ad hoc in Eq. (4.10c) but never listed; verify the count and give the commutator table (abelian for the scalings, $[X_2, X_4] = -X_4$);

(b) show how each constraint reduces the dimension: mass conservation Eq. (4.4b) removes one parameter, homogeneity Eq. (4.5b) another, leaving the one-parameter group that generates Eqs. (4.7a) and (4.8b);

(c) derive Eqs. (4.7a) and (4.8b) as invariants of that group (from the characteristic equations of the generator) rather than by inspection. This would also deliver the similarity variable Eq. (5.1b), currently pulled out of thin air (M14a).

M10. $K_0 x_0 = 1$ (l. 141) is a symmetry breaking and needs justification.

(a) Which subgroup is selected, and why? In the language of M9 this is the restriction to the subgroup generated by X_2 alone — a deliberate reduction, not a consequence of anything.

(b) What is the physical content? $K_0 x_0 = 1$ combined with $K_0 = x_0^\gamma$ forces $\gamma = -1$, inconsistent with all three kernels of Sect. 6. Hence Eq. (4.3b) and Eqs. (4.5a,b) appear to describe mutually exclusive situations, and the reader cannot tell whether Eq. (4.3c) is meant to hold with the rest of Sect. 4. If Eqs. (4.3b,c) merely quote the Srivastava (1988) rescaling for completeness, say so and separate it typographically.

M11. Specify the kernel as $K(x, y) = \ldots$; homogeneity, support, divergence.

(a) Up to Eq. (4.5b) the kernel appears only as “K” or “K_0”, so its arguments and scaling are invisible. Always write $K(x, y)$ and state the homogeneity assumption in the standard form $K(ax, ay) = a^\gamma K(x, y)$ for all $a > 0$ *before* using it. Eq. (4.5b) is a statement about the group parameter, not about the kernel, and is easily misread as a definition of the kernel itself.

(b) A homogeneous kernel with $\gamma > 0$ grows without bound and has no compact support. Discuss the consequences: for $\gamma \leq 1$ harmless, with global mass-conserving solutions; for $\gamma > 1$ this is exactly the cause of gelation; and physically no real collection kernel is homogeneous over all scales, since collision efficiency and terminal-velocity difference saturate. As the central conclusion is conditional on exact homogeneity at *all* scales, the range of validity must be stated honestly, ideally also in the abstract.

(c) The large-x divergence interacts with the numerical truncation (l. 370) — see M15b.

M12. The regularised integrals of Sect. 4: the conclusion does not follow.

(a) Substituting Eqs. (4.16a,b) into Eq. (4.15) I obtain

$$S_0 = -1 + \frac{x}{X} + 2\ln\frac{x}{\epsilon} - \ln\frac{X}{\epsilon} = -1 + \frac{x}{X} + 2\ln\frac{1}{\epsilon} - \ln\frac{X}{\epsilon} + 2\ln x,$$

i.e. the x-dependence is $2\ln x$, which is *not* removed as $x \to 0$ but diverges. Eq. (4.17) therefore does not follow by "retaining only the leading terms", and the stated justification is the opposite of what the algebra gives. Please redo this calculation and state precisely which terms are retained and why.

(b) Typographical error in Eq. (4.16b): "xp/y^2" should read "x/y^2".

(c) The difficulty has an identifiable origin: the ansatz Eq. (4.12), $n = \nu(t)/x^2$, is not normalisable — both $\int n\,\mathrm{d}x$ and $\int xn\,\mathrm{d}x$ diverge logarithmically. The cut-offs ϵ and X are thus not a technicality but a signal that the $\gamma = 1$ scaling solution has no finite zeroth and first moments — exactly the case described by van Dongen and Ernst (1988).

(d) State what the reader should take away from ll. 228–239. At present the passage ends with "this final result is unclosed" and the argument is dropped. Either close it, or state that the $\gamma = 1$ similarity solution exists only as an intermediate asymptotic on a finite mass range set by the initial condition.

M13. The kernel on l. 242 is unsupported and not admissible as written.

(a) Number the equation (see m2).

(b) Give the derivation, or verify the claim. As it stands a rather baroque kernel is asserted to make S_0 constant, nothing follows from it, and the section simply ends. Either show the calculation and draw a conclusion, or delete it.

(c) More seriously, $K(x, y) = [(A - 6S_0)x + (A + 6S_0)y]\, x^2y^2/(x+y)^4$ is *not symmetric* under $x \leftrightarrow y$ unless $S_0 = 0$. A collection kernel must satisfy $K(x, y) = K(y, x)$, otherwise Eq. (2.1) does not describe coalescence. Please check the algebra; presumably a symmetrised form is intended.

(d) If a kernel is being reverse-engineered to make the ansatz exact, say so, and discuss whether it has any physical relevance (it is homogeneous of degree 1, at least consistent with the surrounding discussion).

M14. Section 5: provenance of the similarity variable, undetermined α, missing derivation, algebraic errors.

(a) Eq. (5.1b), $\xi = \alpha(t)x$, is introduced as a guess with a reference to Friedlander (1961). Since Sect. 4 has just constructed the scaling group, it should be *derived* as the group invariant (M9c); this would also show why α must be a power of N.

(b) α is never determined independently: Eq. (5.5b) gives $\alpha = N$, and N follows from Eq. (5.7a) with an eigenvalue λ that itself depends on the unknown profile through Eq. (5.10). Spell out this closure — it is a nonlinear eigenvalue problem in which $\tilde{n}$ and λ must be found simultaneously from Eqs. (5.7b) and (5.10) subject to Eqs. (5.3a,b). As written, the reader will believe α has been determined when it has not.

(c) Eq. (5.10) is said to follow "by directly integrating Eq. (2.1) over x", but the interchange of integration order, the symmetrisation, the cancellation of the gain against half the loss term and the substitution of the scaling forms are all suppressed. The result fixes $\lambda = 1/2$ for $K = 1$ and $\lambda = 1$ for $K = x + y$, so it carries real weight.

(d) **Algebraic errors:**

- In Eq. (5.6a) the right-hand side should be $N^{3-\gamma}S(\xi)$, not $N^{-\gamma+2}S(\xi)$: with $\tilde{N} = N^2$, $\alpha = N$ the gain term carries $\tilde{N}^2\alpha^{-1-\gamma} = N^{3-\gamma}$ while the left-hand side carries $N\dot{N}$. Eq. (5.7a), $\dot{N} = -\lambda N^{-\gamma+2}$, is *correct* and consistent only with the corrected exponent, so the printed chain is inconsistent.
- In the unnumbered formula on l. 306 the integrand should be $\xi\,\partial\tilde{n}/\partial\xi$, not $\xi\tilde{n}$ (whose transform is simply $-\mathrm{d}\bar{n}/\mathrm{d}p$).
- In the Laplace transform of the source term (unnumbered, l. 322) the first term should be $-\frac{1}{2}\,\mathrm{d}\bar{n}^2/\mathrm{d}p$, not $-\frac{1}{2}\,\mathrm{d}\bar{n}/\mathrm{d}p$; the square appears in the next equation.
- In the unnumbered equation on l. 324 the left-hand side should be $2\bar{n} - (\bar{n} + p\,\mathrm{d}\bar{n}/\mathrm{d}p) = \bar{n} - p\,\mathrm{d}\bar{n}/\mathrm{d}p$. With the printed left-hand side one does *not* obtain the following equation; with the corrected one, one does.

(e) **Admissibility of the two branches in Sect. 5.2.** The solution $\bar{n} = 1 - p$ is called "rather speculative", but it can be excluded on principle: the Laplace transform of a non-negative measure must be completely monotone and tend to zero as $p \to \infty$ (Bernstein/Widder), which $1 - p$ does not. The other branch, $\tilde{n} = \delta(\xi)$, violates Eq. (5.3b). Hence *neither* branch is admissible: the ansatz Eqs. (5.1a,b) with the normalisations Eqs. (5.3a,b) has no solution for $\gamma = 1$. This is consistent with the numerical failure at ll. 351–353 and with the known additive-kernel profile $\sim \xi^{-3/2}e^{-\xi/2}$, whose zeroth moment diverges — so Eq. (5.3a) is the offending assumption. Rewritten this way, a reported failure becomes a clean result.

M15. Numerical experiments (Sect. 6).

(a) **Initial conditions violate the normalisation used in the theory.** Line 355 states $\int n\,\mathrm{d}x = 1$, whereas Sects. 4 and 5 assume $m = \int xn\,\mathrm{d}x = 1$ (Eqs. (2.4b), (4.4c), (5.5)). For the Gamma distribution Eq. (6.1b) with $\lambda = \mu = 1$ one has $\int xn\,\mathrm{d}x = (\mu + 1)/\lambda = 2$, and 5/4 for $\lambda = \mu = 4$. The exponents are unaffected but prefactors are not, which may explain part of the residual spread in Figs. 2(b), 4(b), 5(b). Renormalise, or state how the comparison is made.

(b) **Kernel truncation.** Truncating the $\gamma = 2$ kernel at 10^6 destroys the homogeneity on which the analysis rests and regularises the gelation singularity. Quantify the effect (vary the truncation level) and show that Sect. 6.3 is truncation-independent.

(c) **Numerical artefacts are not discussed.** Figs. 7 and 8 show pronounced oscillations and spurious secondary structures at large mass — precisely where the claim "no second, rain-like peak appears" is tested. Identify them as artefacts, explain their origin (grid resolution, truncation, flux limiter) and demonstrate their irrelevance; otherwise a sceptical reader may conclude the opposite of what is intended.

(d) **No quantitative diagnostics.** Convergence is judged by eye on log–log plots, and the authors themselves warn (ll. 395–397) that this misleads. Add the time evolution of $\int xn\,\mathrm{d}x$ (verifying the scheme for $\gamma \leq 1$, diagnosing gelation for $\gamma = 2$), of $N(t)$ against Eqs. (5.8)/(5.12a), and a norm such as $\|\nu(\xi, t) - \nu_\infty(\xi)\|_{L^1}$ versus t. A resolution and time-step sensitivity test is also required.

(e) **The central claim is never tested.** All three kernels are homogeneous, so only the "no rain peak" branch can be confirmed. One additional experiment with a piecewise Long (1974)-type kernel ($\propto x^2$ below, $\propto x$ above a transition mass) would test the main thesis directly. This is the single most valuable addition the authors could make.

(f) The mass range $x = 1.2 \times 10^{-9}$ to $1.2 \times 10^{+36}$ is extraordinary and unmotivated. Justify it, or restrict it and show insensitivity.

(g) Figures: panel (b) is labelled $n(x,t)$ in Figs. 4–6 although it should be ν; the abscissa is ξ in Figs. 1–3, 7, 8 but "X" in Figs. 4–6; no legend identifies the individual curves; the displayed dynamic range of 14–16 decades far exceeds the meaningful range.

M16. Contradictory assignment of kernels to cloud and rain. Line 190 states that $\gamma = 2$ ($K = x^2 + y^2$) is expected for cloud droplets and l. 222 that $K = x + y$ suits the rain scale — both consistent with Long (1974). But l. 364 states that "the last two forms [$K = x + y$, $K = x^2 + y^2$] are conceptually applicable to the cloud and rain droplets, respectively", which is the reverse. As the cloud/rain distinction is the subject of the paper, this is not a trivial slip.

M17. Conclusions (Sect. 7).

(a) The opening sentences (ll. 430–431) on diffusional growth beyond the activation point introduce material appearing nowhere else. Motivate it in the Introduction or delete it.

(b) "the form of the collection kernel defines the equilibrium droplet–size distribution" (l. 447): the SCE has no equilibrium; what is meant is the asymptotic self-similar (intermediate-asymptotic) state.

(c) The claim that the methodology "can equally applied to more realistic settings" is unsupported: realistic kernels are precisely the non-homogeneous ones for which the scaling group does not exist. Temper the claim, or sketch how the analysis would proceed (e.g. locally homogeneous kernels with slowly varying γ).

(d) The abstract should carry the caveat that the result holds for kernels homogeneous at *all* scales, and that real collection kernels are not.

M18. Please re-check the remainder yourselves. I read Sects. 1–5 line by line and Sect. 6 less closely. In the parts checked closely I found errors at a rate of roughly one per page (M12a, M13c, M14d, M15a, M16). I therefore have no confidence that the later sections are free of comparable problems, and I ask the authors to re-derive every displayed equation systematically — ideally with computer algebra — before resubmission, rather than leaving the remaining slips to the referees.

4 Minor comments

Structure, equations, figures

m1. Line 154, "an invariance transform of the time from $t = t_1$ to $t = t_2$" is unclear. Presumably what is meant is the group element mapping the state at t_1 onto the state at t_2, so that the evolution itself is realised as a group action. If so, say it in those words and explain why this requires m_0 to be invariant (because mass is an invariant of the evolution, not of the group). As written, "m_0 must be kept constant (invariant by itself)" is circular.

m2. Number all displayed equations. Currently unnumbered: the invariant relation on l. 176, the antiderivatives on l. 226, the kernel on l. 242, the Laplace formulas on ll. 299–

306, the hand-labelled equations in Sect. 5.2 (ll. 320–326) and the time scale $\tau = 1/K_0N_0$ on l. 357. Also, Eq. (4.11) is missing: the numbering jumps from (4.10c) to (4.12).

m3. Line 173: a reader reaching Eqs. (4.6b,c) will immediately ask about the singular case $\gamma = 1$; this is answered only at l. 204. Add a forward reference.

m4. Eq. (4.10c): "the time inversion transformation is actually not applied in the derivation" — replacing $\nu(t - t_0)$ by $\nu(t_0 - t)$ *is* a reflection. The honest and stronger statement is that for $\gamma > 1$ the relevant solution is a *backward* self-similar solution approaching the gelation time from below (M4b).

m5. Terminology: the SCE is a deterministic mean-field equation; "stochastic" is historical. One sentence would prevent confusion, given that genuinely stochastic (Monte Carlo / super-droplet) collection schemes are now widespread.

m6. Code availability: "available by request to the first author" does not satisfy the EGU code and data policy, which requires deposition in a persistent repository with a DOI. Please deposit the code and, preferably, the output used for the figures.

m7. Figure captions use λ, μ for the initial-distribution parameters and ν for the rescaled density; combined with the eigenvalue λ this is easy to misread (M6). Captions should also state the kernel and the number of curves shown.

m8. Fig. 3 caption gives $\delta t = 10^{-2}$ whereas l. 373 states $\Delta t = 0.1$ by default and 10^{-3} only for $\gamma = 2$; please reconcile. Also, the Gaussian Eq. (6.1a) has support at $x < 0$, which for small λ is a substantial unphysical contribution and is the likely cause of the instabilities reported at ll. 404–405.

m9. Eq. (5.13): the limits of the convolution integral are omitted (they should be 0 to ξ).

m10. Line 314: Eq. (5.14) is attributed to Schumann (1940); it is worth noting that $e^{-\xi}$ is the unique mass-conserving self-similar profile for $K = 1$ (Menon and Pego 2004).

m11. Line 315: for "$\nu(\xi) = e^{-2\xi}$", show the factor 2 explicitly; it arises from $\xi = x/t$ versus $x/(1 + t/2)$ and is easily lost.

References

The list needs careful checking; I noticed the following, and there are probably more.

- **"Meleshoko" should read "Meleshko"** — ll. 46, 122, 344 and the reference list.
- "Strivastava, R., 1988" → **Srivastava, R. C.**; the journal is given as "jas" (an unexpanded LaTeX macro) and should be *J. Atmos. Sci.*; "distrirbution" → "distribution"; "1091-102" → "1091–1102".
- "Preppecher and Klett (1997)" (l. 314) → **Pruppacher and Klett**.
- Friedlander (1961): page range "753–739" reversed.
- Xie (2024): page range "3196–3120" reversed.
- "*Quator. J. Roy. Meteor. Soc.*" (Schumann 1940; Xie 2024) → "*Quart. J. Roy. Meteor. Soc.*".
- Bott (1998): "solutoin" → "solution".
- Smoluchowski (1917): the title should read "Versuch einer mathematischen Theorie der Koagulationskinetik kolloider Lösungen"; the journal is *Z. Phys. Chem.*, printed as "Z. Phs. Chem.".

- Golovin (1963): “Bull. Accd. Sci. SSSR” → “Bull. (Izv.) Acad. Sci. USSR, Geophys. Ser.”; the volume number is missing.
- Long (1974): the author is **Long, A. B.**; use an en-dash in “1040–1052”.
- Bluman and Kumei (1989), *Symmetries and Differential Equations*: published by **Springer**, not by Elsevier.
- Grigoriev and Meleshko (1995): please verify the journal name.
- Check that every reference cited appears in the list and vice versa, and add the literature requested in M1c.

Language and typography

A thorough language edit is needed; a non-exhaustive list:

- l. 6: “no separate peak in distribution identified as rain emerges” — consider “no separate rain peak emerges in the distribution”.
- l. 26: “no distinguished distribution identified as rain never emerges” — double negative.
- l. 43: “In contract” → “In contrast”. l. 44: “as as approximations”.
- l. 63: “coalescence into the droplet” → “coalesce into a droplet”.
- l. 80: “This results suggest” → “This result suggests”. l. 84: “nondimesionalization”. l. 111: “connstraint”.
- l. 114: “this conclusion is contradicting” → “this leads to a contradiction”.
- l. 127: “cannot be chosen arbitrary” → “arbitrarily”.
- l. 222: “yet with a difficulty of arriving a desired final result” — rephrase.
- l. 365: “mas” → “mass”. l. 386: “distributing” → “distribution”.
- l. 424: “do not show that the distribution literally collapses” — rephrase.
- l. 452: “which can equally applied” → “which can equally be applied”.
- Throughout: “Eq..” (l. 316); inconsistent “Sec.”/“Sect.”; en-dashes in page ranges; commas after “i.e.” and “e.g.”.
- The list at ll. 378–383 is mislabelled: “i)”, “i) ii)”, “i) iii)”.

5 Concluding remarks

The idea behind this paper is worth pursuing, but the manuscript in its present form is not ready for publication. The recommendation is therefore **major revision**.

The paper confines itself to two solution strategies (scaling reduction, and separation of variables with the Laplace transform). The SCE is a nonlinear Volterra integro-differential equation of convolution type, and a substantial toolbox for this class is not exploited. I encourage the authors to consult

- A. D. Polyanin and A. V. Manzhirov, *Handbook of Integral Equations*, CRC Press, 1998 (2nd ed. 2008), and
- A.-M. Wazwaz, *A First Course in Integral Equations*, 2nd ed., World Scientific, 2015,

and to survey which further techniques — resolvent-kernel and successive approximation methods for Volterra equations of convolution type, Adomian decomposition and its modified forms, the variational iteration method, moment and generating-function methods, and Laplace-convolution techniques beyond the special cases of Sect. 5 — can be applied to Eq. (2.1). Even a negative outcome (a paragraph explaining which standard techniques fail and why) would strengthen the paper and place the similarity approach in a proper methodological context.

Recommendation: major revision.