Response to the Referees

Summary of Revision

By incorporating the extensive comments by the Referee #1, the manuscript has doubled in size by revision. Most of the new materials have been presented in the Appendices, because we judge them to be mathematically too technical to average NPG readers. The main text is presented in a self–contained manner, also maintaining the readability for NPG readers as the original version, so that it can be read without referring to those Appendices.

Thanks to the Referee #1, the quality of the present manuscript has enhanced dramatically, but also maintaining the accessibility of the general NPG. We do hope that the present form will be found acceptable for publication .

Response to the Referee #1

General Remarks

In the following, the original comment texts are inserted with blue. On the other hand, the Referee's comments are quoted by »...« whenever they are quoted in the response text.

Please note that the present Referee sent us an updated referee comments separately by e–mail through the editor. The following response is based on this revised comment text.

Referee Comment Text and Response:

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).

We much appreciate a very good summary of our manuscript by the lead paragraph.

By actively responding to the following comments by the present referee, we

have extensively revised the manuscript. Yet, we also have to face a cultural gap between the present referee and the intended audience of NPG. The referee is clearly with a strong mathematical background, thus insists on importance of the mathematical, abstract formalism and strict use of logic and the terminologies. On the other hand, the intended audience of NPG are geophysicists, including atmospheric scientists, — the primary audience for this particular manuscript. This audience often lacks strong mathematical backgrounds, and most specifically, many never learn the group theory nor Lie algebra: the majority would even not wish to understand them. Simply, "*ce n'est pas mon truck*".

Thus, although the present referee suggests to introduce a more formally justifiable Lie–algebra based more systematic symmetry analysis, that would expel many NPG readers. For this reason, we decide not to introduce those advanced, abstract mathematics into the main text, so that it remains fully accessible for the NPG readership. Instead, we have decided to introduce a concise introduction to the Lie–algebra based symmetry analysis as Appendix D so that some readers can expose themselves to this new domains.

Strict use of the terminologies in style of mathematics, as the referee suggests, would also hinder the accessibility of the manuscript to the NPG audience. Geophysicists discourse in much looser style than mathematicians, and strict distinction of the terminologies would expel many readers. Yet, we also agree with the present referee that a certain standard of strictness in terminologies must be maintained, to avoid any confusion of the text.

The key issue is: various types of transformations that are referred by different names. A standard NPG reader would certainly feel expelled by finding the definitions for many different transformations are introduced in the text, when a single word, 'transformation', suffices it. As a compromise, in revision, we have followed the strict terminologies for different transformations as much as possible, but without introducing precise definitions, and with *italic* letters, suggesting that these are special terminologies that are used in mathematics (L70–71).

As suggested in the newly–introduced full paragraph of the introduction (L62–71), mathematical proficiency of average microphysicists, as the principal audience of the present manuscript, is extremely low in the present referee's standard. The original manuscript was already written *right at the edge* of the mathematical literacy of average microphysicists. If we make it further advanced, as the present referee appears to urge us, no microphysicists would dear to read. Please understand this very deep cultural gap here.

Especially for this reason, the original Sec. 5, in which an elaborated separation of variables is attempted, has been merged into the Appendix D, where the basics of Lie transformations are introduced, and have been fully expanded (Appenedix D4) by integrating the original materials into the Lie–transformation framework, as suggested by the present referee below (M14).

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:

We are glad to read that the present referee begins the general assessment by stating that »The underlying idea has merit.« As the referee specifically points out immediately afterwards, our intention is indeed to show that »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.« However, the referee's final words are rather cautious: »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.« We intend to respond negatively to this caution in the following.

• **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.

As summarized by the present referee above, the basic message of the present work is very simple: »the cloud/rain dichotomy of bulk microphysics cannot follow from the SCE alone« due to the scale–invariant nature of SCE. We present some simple similarity solutions obtained by the invariant transform (Sec. 4: Sec. 3 in revision) to demonstrate this point explicitly. However, a systematic investigation of the similarity or self–similar solutions is beyond the scope, and also, as the referee suggets, already done.

In revision, we have modified the introduction extensively for more clearly stating the 'novelty' of the present work. More specifically, the paragraph (L50–57) reviews the existing work, including the mathematical and statistical–mechanics contributions; by following up, the short paragraph (L58–61) succinctly states the purpose of the present work; this short paragraph is further expanded in the following paragraph (L62–71) to clarify the relation of the present study to the previous ones by outlining our basic strategy of the presentation.

The main purpose of the article is to introduce a simple concept of "invariance" to the microphysics, but without introducing any elaborated mathematics (L62–71). After going through the references suggested by the present referee, we find that our simple similarity solutions are already derived in a more elaborated procedure. This work presents the same, but based on a very simple invariance argument so that an average cloud microphysicist can follow.

Finally, please be assured that the phrase "too involved" no longer appears in the revised manuscript.

• **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".

By the heading, »The symmetry analysis is not a symmetry analysis«, the referee makes it clear that the present study does not constitute a full symmetry analysis, but merely focuses on the "invariant transformations". This point has been clarified by a new full paragraph in the introduction (L62–71). The term such as "symmetry analysis" has systematically been replaced by "invariance analysis" in revision.

As emphasized therein, our intention is to introduce a very rudimentary invariant transformation analysis, without introducing full technicalities of the symmetry analysis with Lie algebra, so that the text remains accessible to standard atmospheric–science readers, as well as geophysicists in general, without further reading of the references and textbooks. For this reason, for example, the *infinitesimal generators* has not been introduced in the main text, but merely in the Appendix D, as introduction to Lie–algebra based symmetry analysis for atmospheric scientists as well as geophysicists.

• **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.

The notation redundancy has been amended by first preparing a comprehensive list of variables and symbols in the Appendix A, as suggested by the referee in the major comment, M6, and by adjusting the notations for avoiding the redundancy. Most notably, the dimensional variables are now designated by the subscript, $*$, thus there is no longer a need to add a tilde sign *tentatively* to the nondimensionalized variables as in the original manuscript.

• **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).

As suggested elsewhere (M18) by the referee, every algebra in the text has been verified by the two leading authors independently so that no mistake, or typos in equations remains. As suggested, here, some extra steps of the reductions have also been explicitly stated in revision.

Furthermore, by following the suggestion, the numbers have beeen added to more equations: however, we strongly believe that it is not a strict rule to

numbering every equation in the text. Notably, when intermediate steps are explicitly stated, it would rather be annoying to find these are even numbered with absolutely no chance to be referred later.

• **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 issue of gelation has first been remarked more formally in discussing the mass conservation in Sec. 2.1 in revision (L96–105), which is further elaborated in the Appendix B. A more direct reference to the gelation has been presented (L239–244) after presenting the similarity solution with $\lambda = 2$, which predicts a collapse of solution at a finite time, being consistent with the prediction by a more formal studies (*e.g.*, van Dongen and Ernst 1988, Escobedo *et al.* 2002).

• 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.

By following the suggestion here, we have added a new last section titled "From Cloud to Rain", in which an explicit numerical demonstration that a different slope in distribution suggesting the rain scale emerges with time when the Long's kernel is adopted. Furthermore, by taking a transformed density, $3x^2n$, a secondary peak corresponds to rain emerges, as being detailed in this new last section.

3 Major comments

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

(a) Eq. (5.16): 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.

Here, the present referee argues from a point of view of a mathematician that »modified Bessel function« found in Eq. (5.16: renumbered to Eq. D.48b in revision) is »neither involved nor obscure«, but that is not a case for average NPG readers: many never learn about special functions, even Bessel functions, not to mention modified Bessel functions.

We also emphasize that our main interest is with general asymptotic solutions that are expected to realize in long time limit, as numerically demonstrated in a later section. We do not think it easy to see the solution of the form (5.16: renumbered to Eq. D.48b in revision) spontaneously emerges with an arbitrary initial condition. For this reason, we considered this particular solution to be less useful than those more general solutions presented as Eqs. (3.11a, b).

However, we also emphasize that we did not tag this solution (Eq. 5.16: renumbered to Eq. D.48b in revision) with »excessive complexity«. Please note that

this expression never appears in the original manuscript in any variant forms.

Note that in revision, discussions concerning the solution (Eq. 5.16: renumbered to Eq. D.48b in revision) has been moved to the Appendix D, where the basics of Lie–group transformations method are introduced, and has been fully expanded in that context. Therein, we have also shown that the solution (Eq. 5.16: renumbered to Eq. D.48b in revision) reduces, in the limit of $x \to +\infty$, to a new similarity solution (D.47a) identified for the case with $\gamma = 1$ by performing a full symmetry analysis.

(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 lit- erature. The manuscript already concedes priority for Eq. (4.3c) (Srivastava 1988), Eqs. (4.4a–c) (Xie 2024) and Eq. (5.6a) (Xie 2024).

By following the general suggestion of the present referee, the link of the results obtained in the present study to the existing studies has been more carefully stated throughout the text, as also remarked in the response everywhere as more specifically raised by the referee. Most notably, the main similarity solution presented in the manuscript (Eq. 4.9: Eq. 3.10 in revision) is identical to the one more formally derived by Fournier and Laurençot (2005, 2006b: L225–226). Contributions from mathematical and statistical–physics community have also been remarked in the revised introduction, contrasting the present work with theirs (L56–57): see further L243–244, L252–253, and more in the Appendix D.

(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.

As suggested, the following references have been cited in revision: Leyvraz (2003, Phys. Rep.: L57), Menon and Pego (2004, CPAM: L728), Escobedo, Mischler and Perthame (2002: L243–244) and Fournier and Laurençot (2005/2006: L226).

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 1/2 in the gain term (each pair is counted twice); the origin of the loss term; and the interpretation of $K(x, y)$ dt 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).

By following the suggestion, the meaning of the SCE has been concisely described in revision (L89–94) immediately following Eq. (2.1). This description can also be considered a short derivation from a physical point of view. Therein, it has also been stated that $K \geq 0$ and $K(x, y) = K(y, x)$ (L94–95).

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_{ref} , t_{ref} , ... 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).

(a) In revision, the subscript, 0, is exclusively used for designating the nondimensionalization scales in Eqs. (2.2a, b, c, d: Eqs. 2.4a, b, c, d in revision) in the main text. The invariant transformation constants are now designated by the subscript r in revision (Eqs. 3.1a, b, c, d).

(b) For the reason explained in the beginning of the whole response, we choose *not to* introduce a more advanced »scaling factors«, but rather stick to rudimentary "transformation form" to maintain the accessibility of the text to average geophysical audience. The scaling factor has been instead introduced in the newly–prepared Appendix D.

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

(a) $dm/dt = 0$ 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.

A short discussion on the derivation of $dm/dt = 0$ has been added in Sec. 2.1 in revision (L96–105). Therein, the possibility of gelation has also been remarked due to the nonvanishing transfer of mass by coalescence towards infinity. A more elaborated analysis has also been presented in the Appendix B.

(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 mean- ingful backward self-similar approach to t_{gel} , which strengthens the paper. Note also that the kernel truncation at 106 (l. 370) regularises exactly this singularity (M15b).

The finite time collapse by gelation with $\gamma > 1$ has been remarked after presenting the solution (4.10c: Eq. 3.11c in revision) in revision (L239–244), also citing the relevant references (Leyvraz 2003; Escobedo *et al.* 2002; Fournier and Laurençot 2006).

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.

The heading given by the referee is misleading: of course, »nondimensionalisation« implies »scale invariance«, but *only if* the nondimensionalization does not lead to introduction of any nondimensional parameters.

(a) Treating the kernel, K, as if one of dependent variables, both in nondimensionalization and transformation, is an intuitive heuristic that physicists would be able to understand as well as accept. Please understand that we are following this standard, rather than following the mathematical rigor that the present referee appears to insist.

Yet, in revision, a *misleading* nature of this transformation has been more explicitly remarked (L153–159), that makes it a *form transformation* rather than a *invariant transformation*. Advantage of this *heuristic* transformation has also been more explicitly exploited by a more formally stating the relation of the invariant transformation constant, K_0, for the kernel to those for the other variables, as shown by Eq. (3.3b).

M6. Notation must be made unambiguous.

This is my most important presentational request, and in places a matter of correctness.

Notation has been made as less ambiguous as possible in revision by following the referee's following list of »collisions«. Please also note that a list of variables and symbols has been provided in revision as the Appendix A so that the readers can directly consult it whenever they are not sure.

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.

In revision, to avoid the confusion, the dimensional variables have been designated with the subscript, $*$, and the tilde sign is no longer added for designating the nondimensional variables, even tentatively. Consequently, the tilde sign is used exclusively for designating the dependencies arising from the separation of variables in revision. It also follows that »The warning at ll. 90—91« has been removed in revision.

• 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.

In the revised manuscript, ν has been listed in the Appendix A as "normalized mass–distribution density: defined by Eq. (4.8b: Eq. 3.9b in revision) when $\gamma \neq 1$ and by Eq. (4.12: Eq. 3.12b in revision) when $\gamma = 1$".

Very frankly, we are afraid that we do not follow the referee's meticulous rigor: notably, Eq. (4.9: Eq. 3.10 in revision) follow directly from Eq. (4.8b: Eq. 3.9b in revision). Please also note that Eq. (4.10: Eq. 3.11 in revision) directly follows from Eq. (4.9: Eq. 3.10 in revision).

(a) Please note that ν is not really density in Eqs. (4.9–4.10: Eq. 3.10–3.11 in revision), but it becomes a density only by multiplying a given factor.

(b) Please note that n (as well as n_* as its dimensional counterpart) is only the number density throughout the text.

• 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).

(a) Note that ξ is defined by Eq. (4.7a: Eq. 3.9a in revision). The expression (6.3b) in the originally manuscript (Eq. 4.3b in revision) automatically follows by setting $\gamma = 1$ in Eq. (4.7a: Eq. 3.8a in revision): this fact has been more explicitly stated by Eq. (3.12a) in revision. In other words, Eq. (6.3b: Eq. 4.3b in revision) is nothing other than a special case of Eq. (4.7a: Eq. 3.9a in revision).

(b) In Revision, the variable, $\alpha(t)x = Nx$ in Eqs. (5.1b) and (5.5b: Eqs. D.26b and D.30b in revision) have been introduced as a consequence of a symmetry transformation in the Appendix D4, thus it is now more appropriate to designate it as ξ than any other symbol for consistency.

• 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.

We intend m to be the total mass throughout the text. Appearance of m in Eq. (5.16) in the original manuscript was a mistake; m in the latter has been replaced by x in revision (Eq. D.48b).

• 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.

The separation constant/eigenvalue has been designated by χ in revision, thus λ appears only as a parameter of the gamma distribution.

• t_0 : group parameter in Eq. (4.1b) but collapse (gelation) time in Eqs. (4.10c) and (6.4).

In revision, we no longer use the notation, t_0, in the main text: the group parameter and the collapse time are, respectively, now designated as t_r and t_c. It is merely introduced as a translation constant of time in the Appendix D.

• **Subscript 0:** used for group parameters, reference scales, initial values and the Gaussian centre Eq. (6.1a).

In revision, throughout the main text, the subscript, 0, is used only for designating the nondimensionalization scales, as introduced in Sec. 2. the group parameters and the initial values are, respectively, now designated by the subscripts of r and i.

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

Following the suggestion, the list of variables and symbols has been provides as the Appendix A in revision.

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:

By following the suggestion, the analysis of a solution with the separation of variables has been moved to the Appendix C in revision. Merely a short summary of the result has been given in Sec. 2.3 in revision.

• 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;

In revision it has been stated "not consistent with the mass conservation", rather than "self–consistent" (L513).

• 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;

Concerning Eq. (3.3b: Eq. C.3b in revision), in revision, it has been stated that "The size distribution, $\tilde{n}$, is also obtained by solving the diagnostic integral equation (3.3b: Eq. C.3b in revision) with χ as an eigenvalue of the problem" (L511–512).

• note that the obstruction relies on mass conservation and hence does not apply directly to gelling kernels (M4).

We note the point that the inconsistency of the separable solution applies only when the mass conservation is satisfied, and this inconsistency does not apply to the gelling kernels. However, we refrain from this explicit remark in the revised Sec. 2.3, because the gelation is to be more explicitly pointed out only later in Sec. 3 (originally Sec. 4).

On the other hand, the separable solution turns out to be useful for interpreting the numerical results with $\gamma = 2$ using a finite domain, as has been discussed in the Appendix E2.2 (L882–883).

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 "invariance 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.

Indeed, Eqs. (4.1a, b, c, d: Eqs. 3.1a, b, c, d in revision) constitutes an *equivalence transformation*, and it has been explicitly stated so in revision (L159). As for more details:

(i) Distinction between the *equivalence transformation* and the *invariant transformation* have been remarked in the context of discussion Sec. 3 (see notably L153–159)

(ii) Explicit allusion to »equivalence group and symmetry subgroup« are considered beyond the scope even in the newly–prepared Appendix D. Yet, the relevant materials have been extensively discussed in the Appendix D in revision.

(iii) More careful distinction between different transformations has been made throughout the text *as much as possible*. However, please refer to our position on the mathematical rigor in response to the Summary of the Manuscript: see L62–71 more on this.

M9. Use the exponential (Lie) parametrisation and determine the group dimension.

With $x^* = e^{\epsilon_x} x$, $t^* = e^{\epsilon_t} t$, $n^* = e^{\epsilon_n} n$, $K^* = e^{\epsilon_K} K$, the multiplicative constraint Eq. (4.3a) becomes the linear relation $\epsilon n + \epsilon K + \epsilon x + \epsilon_t = 0$, *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).

We find that the present referee's recommendation to add more rigorous results based on the Lie algebra is a good one, and we have added this presentation as the Appendix D in revision. Therein all those results listed by the referee above have been presented. Especially, for the purpose of derivation of »the similarity variable Eq. (5.1b)«, the materials of the original Sec. 5 have been moved to Appendix D, where its full exposition based the Lie–group transformation method have been provided.

Most importantly, please note that, as already emphasized in response to the Summary of the Manuscript, we would like to retain a more physically–oriented presentation in the main text.

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 X2 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.

This problem is due to the first author's sloppy mathematical logic, in which he simply stated $K_0 x_0 = 1$, when it must be actually stated as $K_0 = 1$ and $x_0 = 1$. We believe this simple modification resolves all the issues raised by the referee here.

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

The present referee's request of specifying the kernel as $K(x, y) = \ldots$ *at onset* is absolutely correct, *if* one wishes to construct a paper by following a strict axiom system, and to be in *mathematically* logical order, as the present referee tries to cast the manuscript into. However, this manuscript is directed to the physicists, or more precisely geophysicists, who do not think in terms of such an axiom system. It is far more intuitive for the physicists to think in terms of rescaling of a certain physical process (here the coalescence), that is represented by the kernel, here. See L153–159 in revision.

We sincerely, request the understanding of the present referee on this point: we would not wish to turn our physical article into a mathematical thesis by simply following the present referee's all the requests.

(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.

To avoid a possible confusion the following remark has been added in revision: "Importantly, the transformation of the kernel given by Eq. (4.1c: in revision: Eq. 3.1c) is a provisional one, or alternatively put it, hypothetical one. In reality, the kernel is transformed by following the transformation of mass, given by Eq. (3.1a), thus the transformation constant, K_r, cannot be chosen freely. Yet, by transforming the kernel in this manner with a constant, K_r, we can identify contribution of the kernel, that is required to maintain the invariance, in the equation more explicitly. The constraint on the constant, K_r, due to its dependence on the mass, x and y, is given later by Eq. (4.5b: in revision Eq. 3.6), which is incorporated into the other constraints already obtained. This type of "incomplete" transformation rules are more precisely called the *equivalence transformations*: see Ch. 6 of Bluman ad Kumei (1989) for importance of this concept." (L153–159)

(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 phys-

ically 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.

We take the given comment as a suggestion of the referee to discuss about possible breakdown of mass conservation with the SCE system. It has been briefly remarked in revised Sec. 2.1 in outlining the derivation of the mass conservation (L96–105), and a fuller exposition has been presented in the Appendix B. Therein, we have also suggested that when $\gamma > 1$, the mass conservation can break down, and gelation can happen (L96–105).

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

We thank the present referee for pointing our attention to the numerical issues arising from »large-x divergence«. See our response to M15(b) on this issue more specifically.

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.

Please note that we have found a sign error in the definition of S_0 (notation change to S in revision) during the proof reading for this revision. Thus, after correcting this sign error, we agree with the result:

$$S = 1 - \frac{x}{X} - 2\ln\frac{x}{\epsilon} + \ln\frac{X}{\epsilon} = 1 - \frac{x}{X} - 2\ln x + \ln \epsilon X.$$

From this point, we have next realized in revision that we are actually making a wrong assumption of the ordering of the parameters: instead of $\epsilon \ll x \ll 1 \ll X$, we need to assume $\epsilon \ll 1$, $X \gg 1$ and $x \sim 1$. Based on this ordering, the first three terms on the right–hand side above may drop out, because those are either of the order unity or less. Furthermore, if we assume, $\epsilon X \gg 1$, the last term still remains, and:

$$S \simeq \ln \epsilon X.$$

See L278–286 in revision.

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

»Typographical error in Eq. (4.16b)« has been corrected from "xp/y^2 " to "x/y^2" as suggested.

(c) The difficulty has an identifiable origin: the ansatz Eq. (4.12), $n = \nu(t)/x^2$, is not normalisable – both $\int ndx$ and $\int xndx$ 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).

The singular nature of the solution (4.12: Eq. 3.12 in revision), $n = \nu(t)/x^2$, has been remarked immediately below the solution (L250–253) in revision, and also reiterated by inserting a phrase "for the reason already suggested" later in discussing the integral for S (L272). Please also note the original remark about the singularity over L218–220 (L263–264 in revision). In revision, van Dongen and Ernst (1988) has also been cited in discussing the solution with $\gamma = 1$ (L253)

(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.

The original »passage ends with "this final result is unclosed"« has been modified to end with a more neutral remark that "Note that this constant can be specified only by specifying the range, $[\epsilon, X]$, over which the asymptotic distribution (3.12) is valid" (L285–286).

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 dis- cuss whether it has any physical relevance (it is homogeneous of degree 1, at least consistent with the surrounding discussion).

We much appreciate extensive comments by the present referee on the kernel on L242, $K(x, y) = [(A - 6S_0)x + (A + 6S_0)y]x^2y^2/(x + y)^4$. However, as pointed out, this kernel is not symmetric, unless we assume $S_0 = 0$. The latter case is not much of interest, because the system becomes steady. Based on those considerations, the allusion to this kernel has been totally removed in revision.

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 will be determined when it has not.

(a), (b): In revision, the original materials of Sec. 5 have been moved to the Appendix D, where its full exposition from a point of view of the Lie–group transformation method has be enprovided. Those include »why α must be a power of N« as well as spelling out »a nonlinear eigenvalue problem in which $\tilde{n}$ and λ must be found simultaneously from Eqs. (5.7b: Eq. D.33b in revision) and (5.10: Eq. D.34 in revision) subject to Eqs. (5.3a,b: Eqs. D.28a, b in revision)« (L675–677).

(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.

First of all, in revision, it has been more explicitly stated that Eq. (5.10: Eq. D.34 in revision) follows from the integral of Eq. (5.7b: Eq. D.33b in revision) over ξ in revision (L695–696). Note that this is essentially equivalent of »integrating Eq. (2.1) over x«, amounting to the mass–conservation law. As already stated in response to M11(b), the issues of mass conservation have been fully discussed in the Appendix B in revision.

(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.

Algebraic error of Eq. (5.6a: Eq. D.32a in revision) has been corrected as:

$$(2\tilde{n} + \xi\frac{\partial\tilde{n}}{\partial\xi})\dot{N} = N^{-\gamma+2}S(\xi)$$

as suggested by the referee (L685).

• Unnumbered formula on L306: 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 $-d\bar{n}/dp$).

This error was obviously due to a sloppy editing of the first author: it has been corrected as

$$\int_0^{+\infty} \xi\frac{\partial\tilde{n}}{\partial\xi}e^{-p\xi}d\xi = -\int_0^{+\infty}(1-p\xi)\tilde{n}e^{-p\xi}d\xi = -(\bar{n} + p\frac{\partial\bar{n}}{\partial p})$$

also showing an intermediate step (Eq. D.40b, L719).

• Laplace transform (unnumbered, L322) In the Laplace transform of the source term (unnumbered, l. 322) the first term should be $-(1/2)d\bar{n}^2/dp$, not $-(1/2)\bar{n}/dp$; the square appears in the next equation.

The equation in concern has been modified as

$$\bar{n} - p\frac{d\bar{n}}{dp} = \frac{1}{2}\frac{d\bar{n}^2}{dp} - \frac{d\bar{n}}{dp} + \bar{n}$$

in revision, showing an immediate first step of reduction (L738–741).

• In the unnumbered equation on l. 324 the left-hand side should be $2\bar{n}$ - ($\bar{n}$ + p d$\bar{n}$/dp) = $\bar{n}$ - p d$\bar{n}$/dp. With the printed left-hand side one does not obtain the following equation; with the corrected one, one does.

It has been corrected as suggested.

(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.

We much appreciate the referee's mathematically very precise comments here, that makes the matters clearer. Yet, we would like to present the same in a more down–to–earth manner: recall that the Laplace transform is defined by

$$\bar{f}(p) = \int_0^{+\infty} f(\xi)e^{-p\xi}d\xi$$

for a given function, $f(\xi)$. Thus, when f is positive definite, since the whole integrant for the transform is positive, the resulting transform, $\bar{f}$, must be positive definite. Conversely, when $\bar{f}$ is negative somewhere, f must be negative somewhere, too. Thus, $\bar{n}$, which becomes negative at certain values of p, cannot be consistent with the positive definite nature of the number density, n. In this manner, the branch with $\bar{n} = 1 - p$ is rejected (L750–754)

Unphysical nature of the solution, $\tilde{n} = \delta(\xi)$, is already stated in the original manuscript as "all the particles just stay at the state of the zero size: it is not physically meaningful." (L748) Nevertheless, as suggested by the present referee, the following statement has also been added in the revision: "It does not satisfy the mass conservation, either." (L748–749)

Some special nature of the SCE solution with $\gamma = 1$ has also been remarked in revision by citing van Dongen and Ernst (1988: L755).

M15. Numerical experiments (Sect. 6).

In revision, more serious testing of the code by Andreas Bott has been performedas discussed in the Appendix E1. It is emphasized that extensive testing of this code is already presented in the original paper by Bott (1998). Our further testing simply proves how robustly the code was constructed: the result

changes hardly by reducing the time step by factor of ten or reducing the mass grid increment rate. In some cases, the time step has been reduced by several orders of magnitudes, but without showing any sensitivity (L809–812).

A major exception to this rule is the case with $\gamma = 2$ for the reasons explained below with the standard settings (L817–821).

(a) **Initial conditions violate the normalisation used in the theory.** Line 355 states $\int ndx = 1$, whereas Sects. 4 and 5 assume $m = \int xndx = 1$(Eqs. (2.4b), (4.4c), (5.5)). For the Gamma distribution Eq. (6.1b) with $\lambda = \mu = 1$ one has $\int xndx = (\mu + 1)/\lambda = 2$, 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.

In revision, the initial condition is set in a manner consistent with the constraint, $\int_0^\infty xndx = 1$, by setting, $\lambda = \mu + 1$ for the gamma distribution (Eq. 4.1b). Note that the Gaussian distribution is no longer considered in revision in response to the comment m8.

(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.

The kernel truncation has been removed in testing the nuimerics in revision.

The numerical difficulty of running the case with $\gamma = 2$ is, indeed, due to the fact that the kernel goes up very fast with increasing x and y with the rate of x^2 and y^2, respectively. Essentially at, say, $x = 10^3$, we need a time step less than $\delta t = 10^{-6}$, because the contribution of the kernel to this scale is 10^6. Thus, it is hard to perform a computation with the maximum mass size very large. For this reason, we have decided to limit the maximum mass to be $x = 10^3$ in revision. Even for this case, after testing, we need to set time step as small as $\delta t = 10^{-7}$. Otherwise, the code produces a negative number density due to a huge conversion rate at large masses. (L817–821)

(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.

Figs. 7,8: Figs. 5,6 in revision: pronounced oscillations and spurious secondary structures: this was due to the artificially introduced the truncation of the kernel at $K = 10^6$. As a result, by setting the kernel zero above about $x = 10^3$, coalescence growth no longer happens above this scale. A simple consequence is that the droplet mass begins to accumulate just at $x = 10^3$, generating a secondary peak. This secondary peak, in turn, pumps up smaller particles to larger masses efficiently, leading to pronounced minimum just below. The loop of those processes leads to an artificial oscillation as a whole. This artificial oscillation is likely to have been further aggravated by negative number densities

arising from a large time step adopted.

In revision, the whole calculation have been truncated at $x = 10^3$. In this case, we still see transfer of mass to the scale beyond $x = 10^3$. However, a tendency of the mass to accumulate to the vicinity of $x = 10^3$ remains, and though not as pronounced as before, with much less oscillatory behavior. The adopted drastically–reduced time step also helps to curb the oscillatory tendency. Nevertheless, the tendency is so weak now that we believe it no longer necessary to discuss it in the text any more.

(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 $\int xndx$ (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.

The two major quantifications have been performed in revision:

(i) The case with $\gamma = 1$: Verify whether the whole distribution decreases by the rate with $(1 + t/t_0)^{-1}$, as suggested by Eq. (3.14): Appendix E2.1

(ii) The case with $\gamma = 2$: Verify whether the whole distribution decreases by the rate with $(t - t_c)^2$, as suggested by Eq. (3.11c): Appendix E2.2

These results as well as other analyses suiggested by the present referee have been presented in the Appendix E, because they are considered to be advanced for averaged readers due to a need to quoting advanced results with the formal applications of the Lie–group transformation method presented in Appendix D.

(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.

Transition from cloud to rain droplets has been presented as the newly–added last section.

(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.

A rather excessive range for the mass: 10^{-19} to 10^{+36}: to understand a need for such an extensive range, we should realize that the coalescence is a "nonlocal" process in *spectrum space* of mass. Even when our interest is limited to a very narrow mass range, say, $[x_1, x_2]$, the mass of this narrow range interacts with all the possible mass sizes ranging from $x = 0$ to $x \to \infty$. Unless we take this range wide enough (as designated by "y" in SCE), we cannot evaluate the evolution of the mass over even a finite range, $[x_1, x_2]$, properly. Here, we should also emphasize that even though the evaluation range is very large, the code is designed in such a manner that the actual computation is performed only over a range that a significant size of mass is found: this criterion is rather severe, and set as $3x^2 n > 10^{-60}$. Since an exponential distribution tails off very fast, nevertheless, the code can run in a very efficient manner under such

a circumstance.

For these reasons, the adopted computation range of »$x = 1.2 \times 10^{-9}$ to $1.2 \times 10^{+36}$« is *not* »extraordinary« nor »unmotivated«, but completely justified.

(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.

In the original Figs. 4–6 (b), the horizontal axis is x rather than ξ as suggested by Eq. (6.4a, b: Eqs. 4.3a, b in revision). Note that $\xi = t$ in this case. In the caption, x was too large that it looked like X.

The legends (inserts) for individual curves have been added in revision.

Note that the computations are performed with the double precision, thus a valid dynamical range is safely larger than »14–16 decades«. Recall that the Bott code deals the number density above 10^{-60} to be significant. Nevertheless, in revision, the number of decades shown in the vertical axis has been limited to less than 14 for the number density, n, although the re–scaled ν tends to span a wider 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.

Contradictory assignment of kernels to cloud and rain at L364, as pointed out by the referee, has been corrected in revision (L301).

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.

»The opening sentences (ll. 430–431) on diffusional growth« haven be moved to the beginning of the introduction, as suggested. Further edit of the text has been applied therein as a result.

(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.

The phrase, "the equilibrium droplet–size distribution", has been modified into "the asymptotic tendency of droplet–size distribution" (L384–385)

(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).

Of course, more realistic kernels would no longer satisfy the homogeneity. However, we can expect that they can still be approximated by a certain homogeneity tendency, especially when an analysis range is limited. To that extent the application of the present approach to "more realistic settings" would still be feasible. The presentation of the formation of cloud to rain in the revised last section has indeed suggested that such a local analysis is possible (L425–426).

In revision, the given discussion has been expanded as: "Although only the idealized cases are considered in the study, we emphasize the generality of the methodology, which can equally be applied to more realistic settings. Note that in those more realistic applications, the adopted kernels may no longer homogeneous in any strict sense. Thus, the first question to be investigated in degrees that those kernels satisfy the from (3.6b). More generally, the present study suggests the ways of obtaining physical insights to microphysical problems with more analytically–oriented approaches than currently attempted." (L388–392)

Note that the last remark is far more important than the question of whether a specific method works more generally or not: this is the key message that we would like to convey.

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

The following sentence has been added to the end of the abstract in revision: "As a corollary, the present study points out that a deviation of a natural collision kernel from homogeneity is a key ingredient for understanding the formation of rain from cloud: a simple demonstration is presented adopting Long's kernel assumption." (L7–9)

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.

As suggested, careful verifications of the mathematical reductions over Secs. 2–5 have been performed in this revision. Errors in signs as well as typos, some pointed out the present referee, have been identified and corrected. Please note that there is no further algebraic reduction in the remainder of the original text.

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.

The phrase in concern has been modified in revision as "we seek a transformation of the time from $t = t_1$ to $t = t_2$" (L188). The phrase of "(invariance itself)" has been modified into: "because the total mass does not change with time" (L189).

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).

More equations have been numbered in revision, as suggested here. However, we believe that there is no strict rule to number all the equations. That would become rather cumbersome, especially when the equations merely present intermediate steps.

In revision, it has also been ensured that the equation numbers are incremented by unity in exact manner.

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.

In revision, immediately following Eqs. (4.6a, b: 3.7a, b in revision), it has be explicitly remarked that those formulas are valid only when $\gamma \neq 1$ (L205). Also please note the remarks over L172–174 (L207–209 in revision) about the case with $\gamma = 1$: this means that as a consequence, we did not dismiss this singularity until L204 (L227 in revision) as the referee suggests.

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).

Please note that we *do not literally* flip $\nu(t - t_0)$ to $\nu(t_0 - t)$. In revision, it has been more carefully stated as "re–defining the resulting $\nu(t-t_c)$ as $\nu_1(t_c-t) = \nu(t-t_c)$, and re–setting the latter as $\nu = \nu_1$" (L237).

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.

Remarks on the misnomer of SCE, that actually does not contain *stochasticity* has been remarked in Sec. 2.1 in revision, along with an allusion to the super–droplet method (L106–108).

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.

The codes and data have been made available through CNRS deposit site (https://hal.science/).

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.

Figure captions have been improved in the manner that they are suggested here: with an explicit statement for the form of kernel as well as an insert legend for describing the curve types.

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.

This case with Fig. 3 in the original manuscript was indeed, exceptionally, with the time step of $\delta t = 10^{-2}$. The text should had been clearer on this. However, as the referee points out here, the Gaussian distribution extends to $x < 0$ with nonzero density at $x = 0$. Thus, the case with the Gaussian distribution is no longer considered in revision. More specifically, the original Fig. 3 has been removed in revision as a consequence.

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

The integral range (0 to ξ) in Eq. (5.13: Eq. D.32b in revision) has been supplied in revision, as suggested.

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).

In revision, it has been added that "Menon and Pego (2004) show that the tendency to $\nu(\xi) \to e^{-2\xi}$ is a unique asymptotic tendency." (L731–732)

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.

The following sentence has been inserted in revision: "Here, $x/(1+t/2) \to 2x/t$ as $t \to \infty$, and recall that $\xi = x/t$." (L730–731)

References

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

Various mistakes in references listed below by the present referee have been applied in revision. Furthermore, it has been checked that every reference cited appears in the list and vice versa. The literature requested in M1c has also been added.

- "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. 753–759
- Xie (2024): page range "3196–3120" reversed. 3196–3200.
- "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". edited differently

• l. 111: "connstraint".

• l. 84: "nondimesionalization".

• 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".

All the above list of typos, etc has been corrected in revision. Other language issues have also carefully been proof read in revision.

• Throughout: "Eq.." (l. 316); inconsistent "Sec."/"Sect."; en-dashes in page ranges; commas after "i.e." and "e.g.".

An only typo "Eq.." identified (L316) has been corrected to "Eq."; the sections are always quoted as "Sec." throughout, thus this is consistent. Note that when multiple sections are quoted, we put "Secs." instead (but not "Sect."); page ranges have been systematically in en-dashes in revision; the commas after "i.e." and "e.g." have been removed.

• The list at ll. 378–383 is mislabelled: “i)”, “i) ii)”, “i) iii)”.

The mislabelled list at L378–383 has been corrected (L298–300).

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. Two further remarks, offered constructively. First, the manuscript reads as if it had not been proof-read before submission. Reversed page ranges, misspelled author names, an unexpanded LATEX macro in the reference list, a missing equation number, a non-symmetric collection kernel, an exponent error in a central equation and two mutually contradictory statements about cloud and rain kernels all cost the reader time and shift onto the referee work that belongs to the authors. In an era in which spelling and grammar checkers, reference managers, computer algebra systems and AI-based proof-reading and symbolic-verification tools are freely available, a large fraction 0of these errors could and should have been removed before submission; every displayed equation here is short enough to be verified symbolically in minutes. Second, 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 quation 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. Finally, I would ask the authors to consider that all three of them have established reputations in this field. A manuscript carrying this many avoidable errors into the public discussion phase – where it remains permanently visible – does that reputation no favours, and a careful revision is in the authors' own interest quite apart from the referees'.

We are glad to read that the present referee begins this section by stating that »The idea behind this paper is worth pursuing«.

We also agree that »the manuscript in its present form is not ready for publication«: this manuscript had been sitting on the first author's desk for years, and when he finally decided to make it a publication, his main effort was to remove the dust on top of it. We are afraid that we had submitted the manuscript rather in a premature manner, as a result. We much appreciate the present referee's extensive list of editorial remarks, also for this reason. In this revision,

we have performed more thorough proof readings and extensive edit.

The first author also much appreciates the present referee for proving him a chance for further extensive self–education. Along with going through all the references suggested in M1c, he also went through the two books suggested by the present referee for building up his own basic knowledge on the integral equations. As suggested by the referee, more specifically, he closely followed: »successive approximation methods«, »Adomian decomposition and its modified forms, the variational iteration method«. Other methods listed by the referee was either not discussed at all or not sufficiently enough in either of books.

However, in the end, the first author is rather left puzzled: how those methods are to be used in the present manuscript? Here, the main purpose of the present work is *not* of solving an integral equation called SCE. Its main goal is to point out the scale invariant nature of SCE and present the implications of this nature in actual cloud–droplet evolution, and more precisely, to demonstrate the relevance of similarity solutions arising from the the scale invariance of SCE. We find it not necessary to pursue more sophisticated solution methods for this purpose.

Finally, we are particularly touched by the way that the present referee concludes the review with very special encouragements: »I would ask the authors to consider that all three of them have established reputations in this field. A manuscript carrying this many avoidable errors into the public discussion phase – where it remains permanently visible – does that reputation no favours, and a careful revision is in the authors' own interest quite apart from the referees'.«

Response to the Referee #2

We much appreciate a very positive evaluation of our manuscript stating that "The paper deals with a very interesting problem, on the stochastic background of rain drops formation." The present reviewer leaves the details with extensive comments by the Reviewer #1 for now, thus we do not have any specific agendas to respond either.