

Response to Reviewer 1: A multi-physics Eulerian framework for long-term contrail evolution

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We sincerely thank the reviewer for dedicating their valuable time to reviewing our manuscript and for their careful and insightful comments, which we believe were very helpful in enhancing the quality of the paper. Before responding one to one to the reviewer's comments, we would like to summarize the modifications/additions included in the revised version of the manuscript:

1. We have revised the paper's structure, in accordance with both Reviewers' 1 and 2 comments. Together with some re-arrangements, a concept figure has been added. This intends to strength the focus of the paper in our "multi-physics Eulerian framework for long-term contrail evolution", which we herein coin as MuliCon. We also hope it helps identifying the main building blocks of the model.
2. We have included a direct comparison with the CoCiP contrail model (following both Reviewers' recommendations), expanding also the discussions of the results and, thus, we believe now we have found a better balance between the MultiCon model development and the application of the model to obtain results (as suggested by Reviewer 1).
3. We have added the wind-shear effect into the MultiCon modeling framework using a tracer (characteristic) equation. This also responds to a suggestion from Reviewer 2.
4. Following Reviewer's 2 recommendation, the monodispersity assumption has been made prominent in the introduction, the modelling assumptions, and the problem statement. We have also discussed extensions to accommodate the polydispersity effect in our MultiCon model.
5. We have provided additional insight on the habit dynamics, including further justifications and explanations of the validity of the habit model. This was one of the main criticism of Reviewer 2.
6. We have derived a representative Partial Differential Equation (PDE) for ice supersaturation in detail and explained precisely how it is implemented in our simulations, including the initialization schemes employed, addressing an important comment from Reviewer 2.
7. We have highlighted the computational time required for single-plume analysis (the analysis conducted in this paper) and discussed its implications for potential future work targeting large-scale aircraft/contrail simulations.
8. We have included minor changes in accordance with both reviewers' comments.

Now, we proceed answering one by one the comments raised by the Reviewer 1. For the sake of clarity, the following criteria has been adopted throughout this response document:

- Reviewer's comments
 - [Authors' responses](#)
 - [New words added to the manuscript after revision](#)
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This work presents a novel contrail evolution model for the long-term phase of the contrail lifecycle after the breakdown of the vortex wake. The novelty of the model sits in its explicit coupling of macroscopic and microscopic physical processes in an Eulerian framework, addressing previously underexplored effects that arise out of this coupling. As such, the horizontal and vertical evolution of the contrail plume, as well as the distributions of ice particle habits within the plume, display interesting behaviors that will bring meaningful impact to the field of contrail modelling.

While I believe this manuscript has the potential to be publishable in this journal, there are several significant changes that I feel should be made before doing so. The challenges I have are related to one another, and I believe the authors can readily address them. I'll outline them here and provide further details below, including recommendations for changes that I believe would both improve the manuscript and address these issues.

[Before addressing your comments, we would like to sincerely thank you for the time you devoted to reviewing our manuscript, as well as for your positive remarks and insightful suggestions, which have been instrumental in improving the quality of our work.](#)

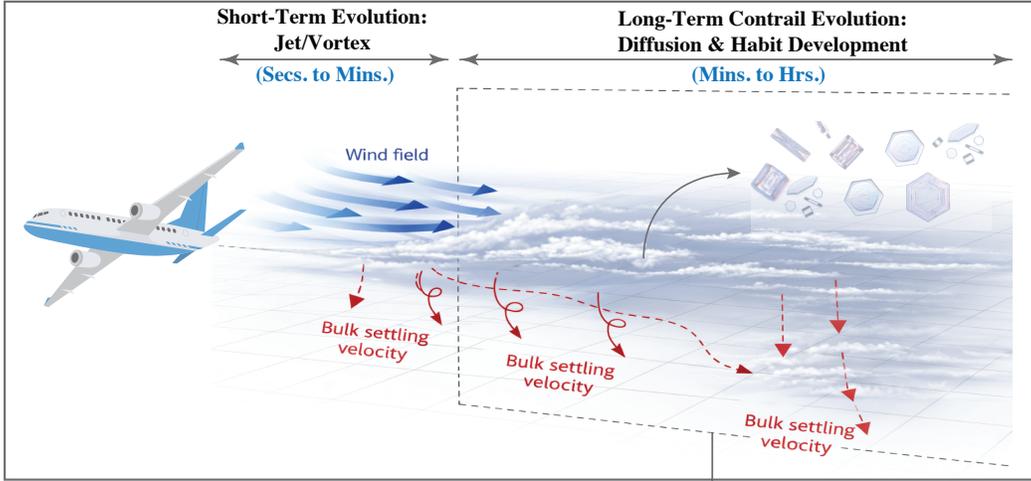
Comment 1: The impact of the work (the learnings on physical behaviors described above) is buried within it, making it unclear as to what the main purpose of the paper is. The current version primarily presents a new contrail model, but without either a comparison to the existing state of the art or to observational data, the paper is currently much better suited for Geoscientific Model Development given the stated scope of ACP. The narrative of the work is insufficiently focused. Because it is unclear whether the main aim of the paper is to present the new model or to demonstrate the findings of applying it, neither is emphasized and it is difficult to interpret what is an advance in the field and what is a restating of known phenomena.

Response: [In the revised version of the manuscript, we have updated the sectional structure and clarified the scope of the present work \(see Fig. 1\). In particular, in response to your comment, the added paragraphs read as follows:](#)

In this research, we present a unified Eulerian framework for contrail evolution that rigorously couples macro- and microphysical processes within a single computational domain. Macro-scale dynamics are described by moment equations derived from the Population Balance Equation (PBE), which include a nonlinear diffusion term representing the (possible) diffusion-blocking mechanisms and also includes polydispersity (mass space) by construction. Microphysics are represented by Eulerian field equations obtained by translating the Lagrangian growth kinetics of individual ice crystals into spatially and temporally resolved moment fields. These microphysical fields are further coupled to a habit-dynamics field equation that solves shape-evolution, enabling a continuous representation of ice-crystal geometry throughout the contrail life cycle. Within this framework, we distinguish the particle-scale terminal velocity (function of crystal mass and projected area-known as Stokes formula) from the ensemble-scale bulk settling velocity. By performing an analysis of the multi-phase flow equations under high turbulent mixing, we derive a first-order Burgers'-type equation that accounts for the collective bulk settling velocity. In addition, we demonstrate that, under mild assumptions, the governing equations exhibit separability, making the model particularly well-suited for large-scale simulations with a favorable balance between accuracy and computational cost.

Notably, although the presented Eulerian framework is general, the corresponding micro- and macro-scale field equations are derived primarily under the assumption of monodispersity for simulation purposes. Extension to polydispersity is straightforward. However, because the main focus of the present work is to introduce the framework itself and to study the effects of habit dynamics and the ensemble-scale bulk settling-velocity closures, the simulations reported here employ the minimal (monodisperse) representation of the Eulerian framework, following a mathematical separation ansatz.

Contrail Evolution



Modelling Framework for Long-Term Contrail Evolution

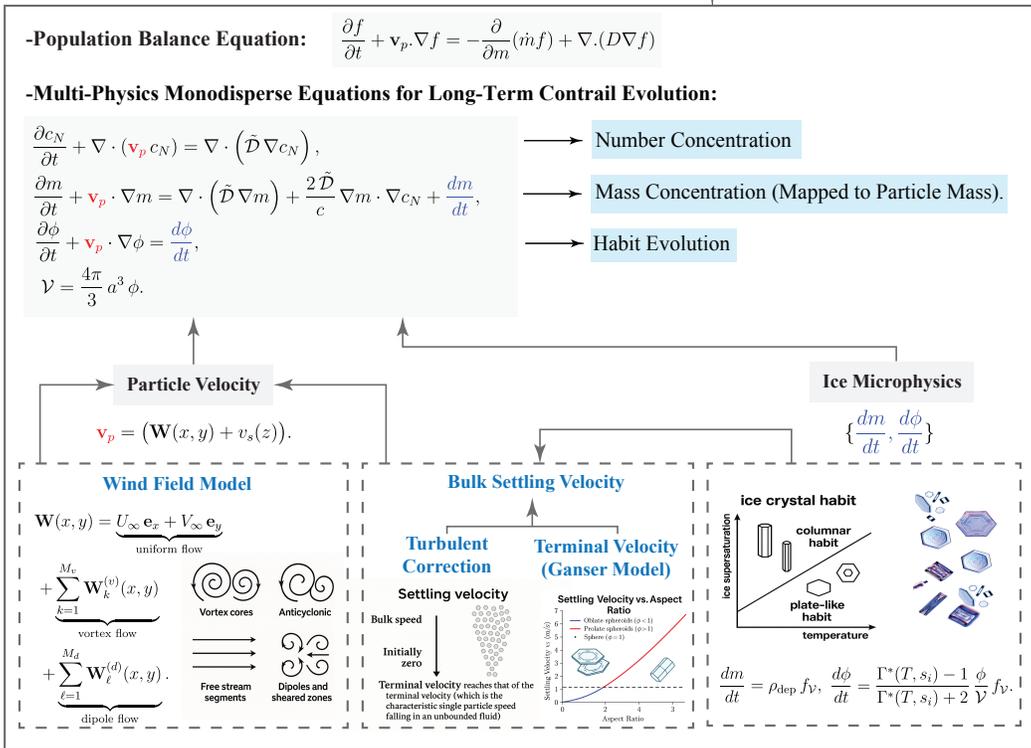


Figure 1: Schismatic diagram for the flow of the study focusing on the primary building blocks

In addition, we have included a dedicated section comparing our model with CoCiP and examining in detail how the new physical processes introduced here modify in-cloud structure, producing changes in the spatio-temporal distributions of ice water content (IWC) and ice number concentration. That section also presents a comparison of center-of-mass settling behaviour, highlighting differences attributable to the newly introduced microphysical processes embedded in a consistent

Eulerian framework employed in our model. The associated figures are presented below.

Moreover, the accompanying .gif files show animations for the single-plume evolution by MultiCon over 9 hours simulation Examples of Single-Plume Evolution by MultiCon. The details associated with the evolution of the plume are given in **Sec. 6.4** of the revised version.

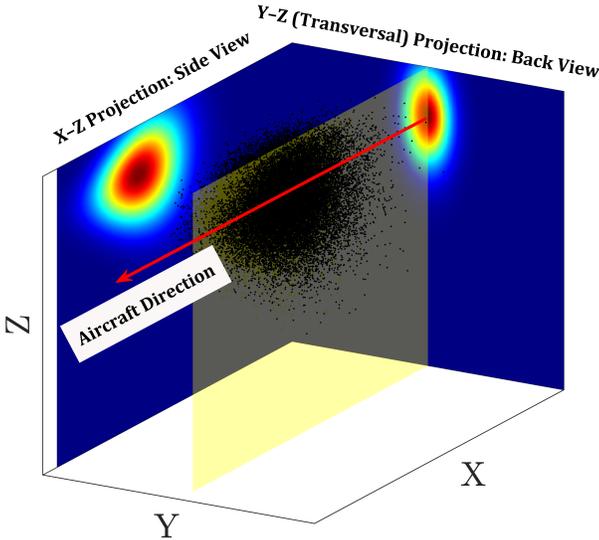
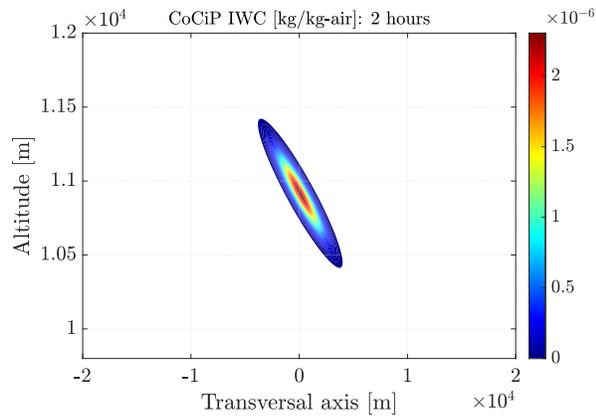
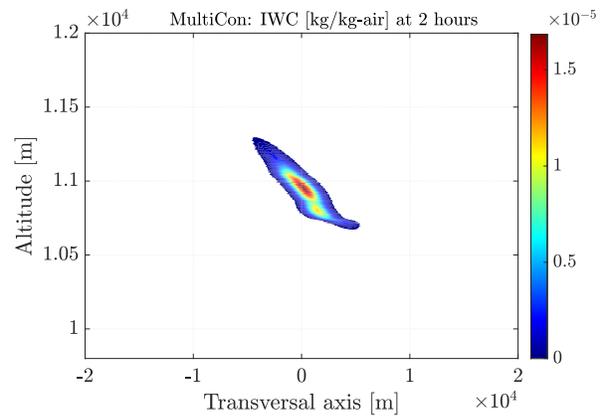


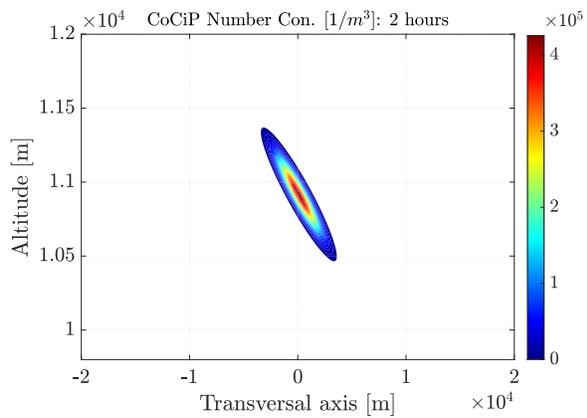
Figure 2: Schismatic for the plume projections used for the visualization purpose



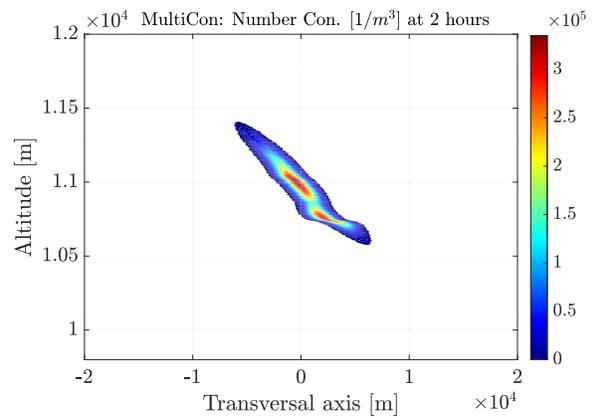
(a) CoCiP IWC: 2 hours



(b) MultiCon IWC: 2 hours



(c) CoCiP number concentration: 2 hours



(d) MultiCon number concentration: 2 hours

Figure 3: Comparison with CoCiP

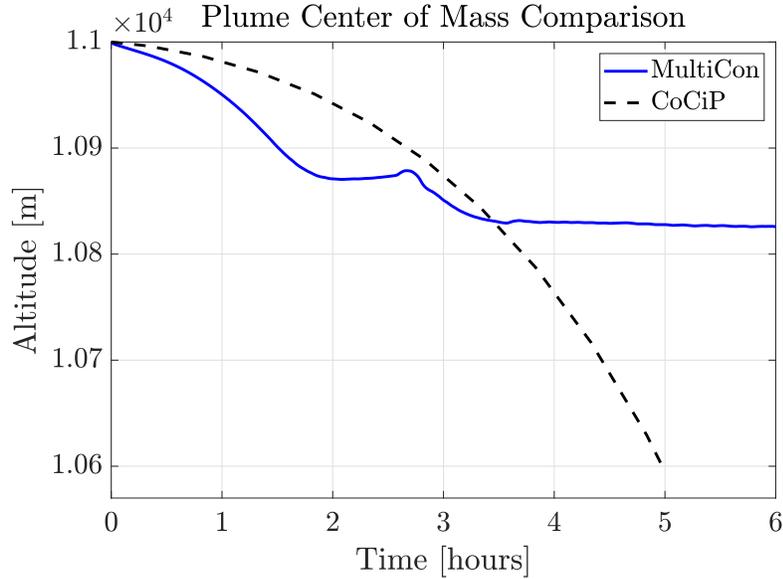


Figure 4: Center of mass comparison between CoCiP and MultiCon

Comment 2: The work is imbalanced between model setup and model exploration/evaluation. Results occupy a smaller part of the presented work than they should, either to demonstrate the new model’s capabilities or to highlight the scientific learnings made by applying it.

Response: In the revised version, in addition to the figures and discussions presented for comparison with CoCiP, we have included additional results highlighting the convergence of the monodisperse model used in our simulations, as well as the computational time of the vertical solver and its significance. With these new figures and discussions, we believe that a better balance is now achieved between model setup and model exploration/evaluation, addressing the concern raised by the respected reviewer. These additions are primarily included in the **Results Section, Sec. 6 of the paper**. For convenient reference, a portion of the new material is presented here:

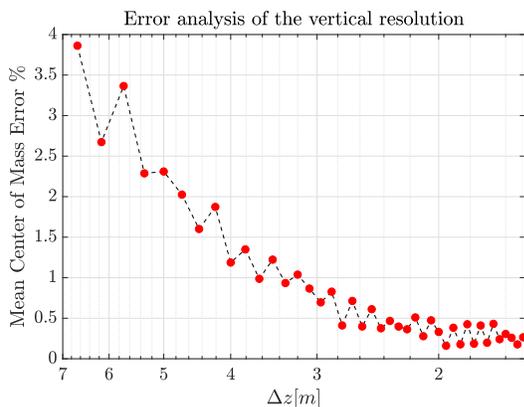
We first assess convergence with respect to vertical resolution and establish a corresponding resolution threshold. As a metric sensitive to vertical discretization, we consider the time-averaged center-of-mass height, defined as $\bar{z}_{\text{cm}} = \frac{1}{T} \int_0^T z_{\text{cm}}(t) dt$, where $T = 10$ h. In discrete form, this is approximated as $\bar{z}_{\text{cm}} \approx \frac{1}{N} \sum_{n=1}^N z_{\text{cm}}(t_n)$.

A high-resolution simulation with $\Delta t_{\text{ref}} = 1$ s and $\Delta z_{\text{ref}} = 0.5$ m is taken as the reference solution, denoted $\bar{z}_{\text{cm}}^{\text{ref}}$. The relative error for a given discretization $(\Delta z, \Delta t)$ is defined as $\varepsilon(\Delta z, \Delta t) = \frac{|\bar{z}_{\text{cm}}(\Delta z, \Delta t) - \bar{z}_{\text{cm}}^{\text{ref}}|}{|\bar{z}_{\text{cm}}^{\text{ref}}|} \times 100\%$.

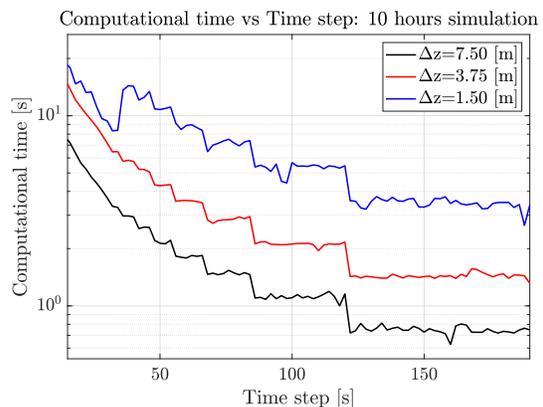
We note that the vertical solver exhibits weak sensitivity to the time step for $\Delta t \lesssim 50$ s. Accordingly, convergence test is performed with $\Delta t = 10$ s while varying Δz . We observe that reducing the vertical grid spacing to approximately $\Delta z \approx 3$ m yields convergence within 1% relative error (see Fig. 5a). Therefore, for the simulations presented in this section, the z -domain from +1000 m to -2500 m is discretized using 2000 grid cells, corresponding to a uniform grid spacing of $\Delta z = 1.75$ m.

Figure 5b shows the computational time of the vertical solver over a 10-hour simulation as a function of the solver time step, for several vertical resolutions. When the present Eulerian framework is deployed in large-scale analyses involving many plumes (e.g., using tube-based methods), the vertical solver remains the dominant contributor to overall computational cost. Although this study does not target large-scale applications, the timings in Fig. 5b indicate that the model is promising for such scenarios. As can be seen, for moderate vertical resolution and solver time step, the computational time of the vertical solver can fall below one second.

Nevertheless, further discussion of tube-based solvers is beyond the scope of the present work; however (as we presented in [the ECATS conference Jan. 2026](#)), we would like to refer to the following link as a demonstration of ongoing activities on this topic ([An Example of Continuous Aircraft-Plume Evolution by MultiCon](#))



(a) Vertical-resolution convergence of the 10-hour mean center-of-mass position



(b) Computational time of the vertical solver for a 10-hour simulation as a function of time step

Figure 5: Vertical-resolution convergence and computational cost analysis.

Before detailing major and minor recommended changes, I would like to address some of things I enjoyed about the work:

The review of existing contrail models and simulations in the introduction is thorough and well written, with a clear outline of what current standard implementations are, what they offer, and how they could be improved. The same goes for the sub-sections on habit development and bulk settling velocities, which I found to well justify some of the work that has been undertaken.

The structuring of equations and explanation of terms is sensibly carried out, with the complexity of notation well-handled throughout the work. With the amount going on that there is, it would have been easy to lose the reader in this aspect, but I didn't find this to happen at all. Where choices have been made to simplify or eliminate terms, they have generally been justified and explained to a satisfying level. Section 3 was quite enjoyable to read, in fact.

The writing style is, in general, excellent, handling relative complexity with relative ease in the model development sections, showcasing the authors' clear knowledge and understanding of the systems they're dealing with.

We would like to express our sincere gratitude to the reviewer once again for their encouraging comments on our manuscript.

Comment 3: I recommend that the paper should undergo restructuring, deciding what its main narrative is, what is essential to that, and what is consequently peripheral. If the work's intent is to present a new contrail model, then it should spend time evaluating that new model (see more below). If the work's intent is to showcase the physical behaviors derived from application of the new model, then less time should be spent on outlining and detailing aspects of the internal workings of the model. To do all of this in a single publication would make the work exceedingly long, which is why I understand the authors having written the work as presented. However, by trying to do all of it, several problems arise, including the 1/2/3 outlined above. Once the choice of intent is decided, the gaps that this choice leaves behind should be addressed directly by the authors in the work, clearly outlining the scope of the present work and what they intend to do in future publications or encourage others to examine.

A restructuring and refocusing should also help with the balance between model setup and model exploration. Currently, there are too many (albeit excellently written) pages of model derivation for

the reader to get through (see Minor Points for recommendations), with a then quite short results section. The reader feels rushed towards the end of the work and takes less meaning away than the work deserves. After a reduced outline of the model, I would thus recommend including an examination/evaluation of the choices that were presented throughout the setup, demonstrating that the model produces contrail shapes and lifetimes that are in keeping with expectation, before moving on to exploring the impactful results related to the novel physics. A section where such model outputs are briefly compared to existing models as reviewed in the introduction (e.g., CoCiP, APCEMM, LES from Simon Unterstrasser and collaborators), or even to the limited observations that we have, is essential to demonstrate these points to the reader. Highlighting where the model outputs are similar to or differ from these other models, and why, helps set the context for interpreting the physical behaviors on show. I have a feeling that the authors have already pursued such comparisons and intended to perhaps place them in an additional manuscript, so hopefully these additions shouldn't be a large volume of work, but they are necessary here to demonstrate the capabilities of the model.

Response: As the reviewer has already mentioned, the above points are related to one another. As we outlined in response to **Comment 1**, in essence, the scope of the present work is mainly to introduce the Eulerian framework itself, demonstrating how the long-term contrail problem, considering the novel micro/macro-scale physics introduced, can be represented within a unified Eulerian framework. However, to assess the general performance of the Eulerian framework, we had to carry out simulations. To this end, in this research, we carried out simulations under some simplifying assumptions, such as monodispersity and single-plume evolution, rather than solving for continuous aircraft-plume release.

Therefore, to summarize, this paper presents the Eulerian framework itself and simulates a minimal representation of the framework (monodisperse plus single-plume evolution). The first-level comparison of the model is performed against CoCiP, which is also a monodisperse model. Subsequent work may address a) comparing single-plume evolution between the polydisperse version of MultiCon and APCEMM, which is also a polydisperse model, and b) comparing the evolution of an entire continuous aircraft plume with observations (if possible/available) or with CoCiP per-flight contrail evolution. In the latter case, we will introduce a tube-based solver that easily extends the application of the Eulerian framework from single-plume evolution to analysis of contrails from an entire aircraft fleet.

Throughout the structural changes we made in the revised version, we changed the title of **Section 5** to something which we believe addresses possible confusions about the scope of the present work. Now it reads:

Section 5: Solution Methodology for Single-Plume Evolution

Comment 4: In terms of the results as presented, Sections 6.1 and 6.2 feel appropriate as they focus on specific physics introduced as novel and offer interpretation/discussion. The start of Section 6.3 offers some exploration of the columnar crystal settling phenomenon, but then figures are presented in rapid fire, with insufficient explanation to the reader as to their intended meaning, why they’re significant, or whether their content is what we might expect given the model parameterizations and initial conditions. All of these results are interesting and potentially impactful, but without the accompanying discussion, they don’t add the value that they should.

I realize that by asking for this additional analysis and discussion, the length of the work is starting to creep upwards. However, I strongly believe that, as discussed above, there are critical elements which are missing, and several sections which could be reasonably moved to an appendix.

Response: We have narrowed down the narrative and structure to present the Eulerian framework, compared our results (simulations, based on some simplifying assumptions already discussed) with those of CoCiP (**Section 6.4**), and enriched the discussions in **Sections 6.1, 6.2, and 6.3**.

Minor Points:

Comment 5: Citation style needs fixing throughout (in-line citations vs. parentheses), such as “(e.g. (Sundqvist, 2003))”.

Response: We have rechecked citations carefully in the revised version.

Comment 6: Ensure that all acronyms are properly defined when used for the first time, such as “RANS” or “UTLS”.

Response: Absolutely, we have tried to ensure consistency when using acronyms.

Comment 7: The placement of figures is quite frustrating throughout. I regularly found myself reading text, thinking “a figure that explains this would be great” (e.g., both Figures 1 and 2), only

to find that figure several pages later. The set of figures at the end of the work have the same effect. However, in this case, it's the lack of accompanying text which makes effectively spacing out the figures challenging (i.e., this may correct itself naturally during restructuring).

Response: In the revised version, this issue has been addressed through the inclusion of additional contextual explanation as well as improved \TeX rendering.

Comment 8: The introduction, while nicely setting the scene and being well-written, is longer than necessary (5 pages including the start of Section 2) which contributes to the feeling of imbalance between setup and results/discussion. I recommend shortening this to 2-3 pages and focusing specifically on the elements necessary to support the paper's objectives.

Response: The introduction has been restructured to place greater emphasis on the novel elements of the study and is now approximately 2-3 pages in length. Owing to your valuable comments and suggestions, the manuscript now presents a more balanced structure in terms of figures, methodological setup, and discussion.

Comment 9: Section 4.1 on the wind-field could be more clearly spelled out, with the current text feeling like it was trimmed down. For example, I would more explicitly describe the transitions between Equations 5 and 6 and the approximated polynomials in the text. However, unless I'm missing how it's critical to the narrative, I would recommend moving the section to the appendices, where it can be more thoroughly flushed out.

Response: Section 4.1 has been revised in two respects. First, although a sink/source term was introduced theoretically in the inviscid wind model, its performance was not assessed in our simulations/analyses; therefore, the corresponding term has been removed. Second, the presentation has been streamlined to improve the logical flow: transitions between equations are now clearer, and the polynomial fitting procedure is explained in greater detail. These changes read as follows:

Furthermore, as shown in Appendix A, the wind model gives the following number of tunable parameters:

$$\text{Parameter Count} = 2 + 4M_v + 5M_d. \tag{1}$$

For time-dependent reconstruction we first fit the composite model to each instantaneous

measurement. For snapshot t_i the parameter vector \mathbf{a}_i is obtained by:

$$\mathbf{a}_i = \arg \min_{\mathbf{a}_i} \mathcal{J}(\mathbf{W}_{\text{obs}}(\cdot, t_i), \mathbf{W}(\cdot; \mathbf{a}_i)). \quad (2)$$

Promotion to continuous time functions. Let $\mathbf{a}_i = (a_{1,i}, \dots, a_{p,i})^\top$ be the parameter vector at snapshot t_i , where $p = 2 + 4M_v + 5M_d$. Each parameter component is then promoted to a continuous function of time and approximated by a polynomial:

$$a_j(t) \approx \sum_{k=0}^n c_{j,k} t^k, j = 1, \dots, p. \quad (3)$$

The coefficients $c_{j,k}$ are obtained by a least-squares fit to the discrete data $\{(t_i, a_{j,i})\}_{i=1}^{N_s}$ where N_s is the number of snapshots. Therefore, the overall time-dependent wind field reads $\mathbf{W}(x, y, t) = \mathbf{W}(x, y; \mathbf{a}(t))$.

Comment 10: Sections 4.2 and 4.3 should be trimmed down where possible. They again contribute to the feeling of imbalance between setup and results. For example, is it necessary to fully outline the boundary conditions in Section 4.2? Or, in Section 4.3, is it required for understanding of the work to derive the terminal velocity to this detail? I do, however, understand that they are important components that contribute to the novelty of the model, and so should remain in the main text as Sections. The moving of Figure 2 to being next to Section 4.2 could also help add some level of interpretation to the presentation of the theoretical model.

Response: In this version, we have moved the subsection on terminal velocity to Appendix E. However, since the bulk settling velocity is one of the main contributions of the paper, we believe it should remain in the main text. Accordingly, immediately after introducing the corresponding equation—which depends on the terminal velocity—we refer the reader to Appendix E for a detailed description of the approach used to compute it. Moreover, in the revised version, all figures are placed at their appropriate locations in the manuscript.

Comment 11: In Section 5.1, it would be nice to outline a couple of the advantages that the directional-ODE approach has over conventional implicit schemes

Response: In this version, we have made the advantages of the directional-ODE approach clearer. The relevant part now reads as:

Notably, the directional-ODE approach has been shown to offer advantages over conventional discretization schemes in terms of accuracy, stability, and computational efficiency. These advantages stem from the use of analytic update formulas rather than the purely discrete update formulas typical of standard explicit or implicit methods. In our implementation, we adopt a first-order predictor-corrector algorithm, following
