

Review Report for Rapid simulation of 2-D contrail cross sections through Contrail Layer Approximation using Multi-crystal Particles (CLAMP) v1.0

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This paper presents CLAMP v1.0, an intermediate-fidelity model for simulating the evolution of two-dimensional contrail cross-sections. The model couples multi-crystal particle clusters with a gridded water-vapour field and introduces a treatment of thermodynamic consistency and stochastic gravity-wave forcing.

However, since the physical processes governing contrail evolution are strongly and nonlinearly coupled, introducing individual physical processes without accounting for their interactions may improve the overall model but may still fail to represent the complete physical picture accurately. Further iterations (subsequent publications) may therefore be needed before the model becomes sufficiently mature to be considered a standalone contrail model. Nonetheless, given the many uncertainties in contrail modeling, the lack of experimental and observational data, and the complexity of the modeling problem, any new contribution or insight should be appreciated, and the paper is worth publishing. Therefore, I recommend acceptance following major revision to address the following issues.

I would also recommend softening the claims made in the Abstract.

The comments below should be responded as much as possible or if not; explicitly highlighted in the discussion/conclusion section.

monodisperse assumption:

It appears that each cluster represents a collection of physical ice crystals rather than a computational grid cell. However, all crystals within a given cluster are assumed to have the same radius. What is the physical and numerical justification for this assumption, given that the crystals represented by one cluster may experience different humidity histories and growth/sublimation rates? Moreover, please clarify how the number of clusters affects the resolved size distribution and whether the results are sensitive to this assumption.

Vertical fluctuations and coupled loss mechanisms

CoCiP already accounts for ice-crystal number losses associated with turbulent humidity fluctuations, mesoscale turbulence, and gravity waves through its E_{meso} loss term (Schumann, 2012), which incorporates the Brunt–Väisälä frequency. CLAMP also uses this frequency, but to determine the autocorrelation timescale of stochastic gravity-wave temperature fluctuations, whose effects on ice growth and sublimation are subsequently modelled. However, CLAMP does not include an explicit equivalent parameterisation of crystal-number losses due to these unresolved fluctuations. Moreover, CoCiP includes separate turbulent-mixing (E_T) and sedimentation-induced aggregation (E_A) loss terms, representing additional mechanisms affecting crystal survival. Please clarify how CLAMP’s treatment captures the coupled effects of vertical fluctuations on temperature, RH_i , turbulent humidity mixing, and crystal-number loss. In particular, could the lack of such coupling affect the predicted contrail lifetime and radiative forcing?

settling velocity and coupling with the thermodynamic:

The settling velocity is calculated using Stokes' law for spherical ice crystals, while vertical diffusion seems to have been neglected. Consequently, differences in fall speed associated with crystal habit and unresolved vertical transport are not represented. Settling is particularly important because it determines how long crystals remain within particular temperature and relative-humidity conditions. Given that the paper investigates the influence of adiabatic temperature changes and gravity-wave forcing, please discuss the importance of settling formula. Specifically, a simple settling formula may, in principle, ruin the attempts made to capture contrail properties due to gravity-wave-induced thermodynamic!

crystal habit and non-spherical microphysics:

Ice crystals are mostly non-spherical, and their habit can substantially affect depositional growth and sublimation, terminal settling velocity, and ultimately contrail evolution and radiative forcing. Even in contrail studies, models like CoCiP already acknowledge different habits; but due to the modeling complexities, CoCiP considers habit classes only in RF computation.

Although the authors acknowledge the simplified treatment of crystal habit in CLAMP, its implications for the reported results should be discussed more explicitly in the discussion and conclusions. In particular, Dedekind et al. (2026, *Atmospheric Chemistry and Physics*) demonstrate the importance of deposition kinetics for simulating ice supersaturation and contrail persistence, while recent work by Jafarimoghaddam and Soler (2026, JECATS preprint) introduces a reduced-order parameterisation (using similar deposition kinetics model) of evolving crystal habit that affects growth, sublimation, and settling. Although dynamically predicting crystal habit remains challenging given the limited experimental constraints under contrail conditions, these studies illustrate the relevance of accounting for habit-dependent microphysics. Please clarify how the spherical-crystal assumption may bias the predicted contrail lifetime and radiative forcing, and discuss the potential for incorporating habit-dependent deposition and settling parameterisations in future developments of CLAMP.

consistency of advection and diffusion for ice crystals and water vapour:

The manuscript employs different numerical treatments for the transport of ice clusters and the gridded water-vapour field, with the water-vapour field being transported using Strang operator splitting for advection and diffusion, the ice clusters are transported independently, with stochastic horizontal velocity perturbations used to represent the effects of diffusion.

Given that diffusion can substantially influence the spatial distribution of water vapour, local RH_i , and hence ice growth and sublimation, please clarify how the consistency of these two transport treatments is ensured.

Moreover, there seems to be two additional issues: 1) vertical diffusion is assumed zero, and 2) advection and diffusion in horizontal direction may not be properly coupled when a splitting technique is applied rather than resolving the PDE field directly. the second comment is perhaps outside the scope of the present work; however, it would be nice if authors could discuss the limitation of zero vertical diffusion; because even slight difference in vertical diffusion may override some of the turbulent time/length scale!