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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of particle loss to the Kelvin effect in LES of young contrails" *by* Aniket R. Inamdar et al.

Anonymous Referee #2

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Review of Inamdar et al

Summary

The study presents LES of young contrails and focuses mainly on one aspect, i.e. the treatment of the Kelvin effect in the ice microphysical model (which is also reflected in the title). The numerical model has been used before in the study of Naiman et al, 2011. Compared to other LES results, the latter study showed a rather weak sensitivity to ambient relative humidity which appears counter-intuitive. Unterstrasser, 2014 addressed this discrepancy and pinpointed the Naiman model to be an outlier model. Unterstrasser, 2016 performed a more thorough comparison of various LES models used for early contrail simulations. They found that the wake vortex descent and decay in the Naiman model is similar to that of the other models which implies that the

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discrepancies are likely associated with microphysics. The present study tackles this problem and attempts to give an explanation of the observed behaviour. Generally I appreciate this effort, the format and the intention behind this study.

Often such tests are added as appendices in more comprehensive studies. I agree that often details matter which require a somewhat longer description than possible in such appendices. Given the rather short length of the manuscript, I recommend to extend the study by a few aspects to make it a more substantial scientific contribution that is suitable to be published as independent research.

The presented results leave many questions open and I get sometimes the impression that the presented results are not fully understood. Some results seem implausible. At best, they are not explained well enough. Moreover, I think that the evaluation of some relationships or quantities is not overly useful. In general, the text touches many aspects only superficially.

I recommend publication, only after including a more careful analysis, a more thoughtful presentation and a better explanation of your results.

Major issues:

A1. Motivating your work with the low level of scientific understanding by citing a reference to Penner et al, 1999 (collecting scientific results about 20 years old) is awkward. Science has progressed since. Sausen et al, 2005, Lee et al, 2010, Burkhardt and Kärcher, 2011, Bock and Burkhardt, 2016

A2. The results section looks more like a technical report where hard facts are listed. Implications and interpretation of the simulation results are only touched in a few cases. I could live with it, if I understood all your model results. However, this is not the case which is outlined in the following.

A3. Ice crystal formation in contrails is completed after one second or so. Hence, the fraction of surviving particles as you show in Fig. 2 should be a monotonically decreasing function. It is not trustworthy when one simulation (red line) shows a late time increase after t=400s. So what's wrong, something in the model or in the post-processing tools?

A4. Several aspects of the evolution of the size distribution (SD) in Fig. 3 look peculiar.
1.First of all, your chosen colours for B0 and B2 are hard to distinguish. Please replace one.
2.Why do you change the y-range for $t \geq 60s$, when there is no need to?
3. The large droplet mode is controlled by RH_i. Hence for the right end of the SD, all solid lines are basically identical and similarly all dashed lines. For $t = 15, 30$ and 45 s it looks like this may not be the case. Can you clarify this.
4. What really irritates me is the fact the left tail (the so called sublimation tail) develops faster for higher RH_i. How can this be? There's three times more water vapour available for deposition in the $RH_i = 130\%$-cases. So why should ice crystals be more susceptible to sublimation in this case? This result is counter-intuitive, implausible and different to all other models. Hence, it must be explained in detail, why your models behaves like this. As a consequence, the ice crystal number in Fig. 2 drops faster for higher RH_i. For the red case, even the final survival fraction is lower. This result is hard to believe. Did you carefully check all your model components?

A5. It is discouraging, if you mix up things and reviewers have to disentangle them: The terminology of your simulations is misleading. Runs B0, B1, L0 and L1 use a temperature-dependent a_k which are compared to runs with constant $a_k = 1 \times 10^{-9}$ m. Your presentation implies that including the temperature dependence makes a large difference. However, the temperature dependence itself is not the reason for the observed differences (the temperature dependence is anyway weak, as you shows in Table 1 and Fig. 1). It is simply that the a_k-value is about 2.3 times higher if you use your temperature-dependent expression. So you basically compare cases with $a_k = 1 \times 10^{-9}$ m and $a_k = 2.3 \times 10^{-9}$ m.
By the way, why do list a_k-values for three different σ-values, if only one case is used

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in the simulations?

p4. l.9-10: "results in substantially lower Kelvin correction for smaller particles". This is misleading as the correction factor is constant over the whole radius range, only the absolute change is larger for smaller particles.

Major to Minor issues:

B1. The paragraph (p.3 l.18- l.20) sounds like a perfect motivation to carry out sensitivity simulations varying the initial size distribution. If the true initial size distribution is not known, a model offers the unique opportunity to vary this parameters. This is particularly interesting in this study. The Kelvin effect has a prominent effect on the shape of the size distribution as you show in Fig. 3. So a variation of the initial size distribution is directly relevant to the main aspect of your paper. This may be also a reason for discrepancies between models.

B2. Similarly, I recommend to carry out the L4 and L5 simulations. You say, those simulations are not necessary, as Picot et al, 2015 showed that no crystal loss occurs. One main motivation of your work was the discrepancy between the various models. So in this sense, referring to another model is a bit contradictory. It would be interesting to know, if your model behaves similarly.

B3. To me it is unclear, what you want to demonstrate with the bottom row of Fig. 2. Mean particle size is mainly controlled by growth of detrained ice crystals being outside of the vortex system. The crystal loss, on the other hand, occurs inside the vortex system. For me it makes no sense to link those to quantities, as they are not really physically connected. I recommend to remove the paragraph from p.4 l.32 to p.5 l.5 and the sentence in the abstract/conclusions.

B4. Personally, I think that analyses of optical depth of young contrails are not overly useful, as this quantity is linked to radiation and climate aspects. LES of young contrails are not directly relevant to this. For this, contrails must be simulated over a much longer time (at least several hours). Optical depth decreases substantially over the first half an

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hour, as the contrail gets usually tilted by vertical wind shear (a process absent in your simulations). So the given optical depth values are only a snapshot. The differences you find may not be long-lasting. Indeed, Unterstrasser et al, 2016 presents contrail-cirrus simulations over 6 hours and switching off the Kelvin effect had barely an effect on contrail properties (all simulations were initialised with the same 5 min old contrail, though)

B5. A point-to-point comparison between various models as done on p5 l21 is not leading anywhere. Contrail optical depth depends on a multitude of parameters. So you always find simulations with similar, yet not identical parameters which leave enough room for arguing that for this or that reason the optical depth is similar or smaller/larger in the one case. Unterstrasser, 2016 presents a more rigorous evaluation exercise that accounts for the multi-parameter nature of the problem and that is also able to disclose systematic model differences as mentioned in the introduction of this review.

B6. Naiman speculated that they might have used too few computational particles and that this could have led to the discrepancy with other LES models. How many particles did you use? Do your results depend on this numerical parameter? Did you check convergence of your results?

Minor Issues:

C1. I don't want to downplay the possible effect the early temperature difference by including/excluding the exhaust enthalpy has on contrail properties. Nevertheless, it is noteworthy that after 100s the excess plume temperature is not affected at all by this model aspect.

C2. I recommend to split Fig. 1 for clarity reasons. The left column shows LES results and the right column shows simple physical relations without a connection to your LES results.

C3. You cite several Inamdar papers from the recent past. I am not sure, if all those

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are peer-reviewed contributions. If not, I recommend to reduce references to them and instead repeat the results in this study. For example, p.6, l.9/10 cites an important result of your recent work. Has it gone through peer review?

C4. Can you add the expression for σ? Do you vary it independently of T? The legend of Fig. 1 right alludes to this.

C5. p.2 l.27: What do you want to say here? Can you make a clearer connection between the availability of measurement data and what you say in the rest of the sentence.

C6. p.3 l.33: The plume temperature is constant!? I do not understand this statement. The plume temperature increases due to adiabatic heating. It may help if you describe in more detail how you compute the excess temperature. How is your reference temperature determined?

C7. p.6 l.4: Be more specific about how measurements can help. Otherwise this statement is pointless.

Technical points:

T1. Many author names are mis-spelled (often missing german umlauts or french accents): Sölch, Görsch, Kühnlein, Kärcher, Helie, Nybelen, Schäuble, peter J. Newton to name only a few!

T2. For units the regular font is usually used.

T3. # is no SI unit. I guess you can just remove it.

T4. The numerical treatment of the Kelvin effect in the Sölch Kärcher model is described in more detail in Unterstrasser et al, 2016 and can be cited for reference.

T5. The axis annotations and legends are too small in many plots. In Fig. 1, the legend misses the unit m for a_k. In Fig. 3 it suffices to show the y-axis on the left column. Inserting the time label in each plot would save a lot space.

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T6. p.3 l.3: better write Δx_{jet} and Δx_{vort} separately.

T7. p.2 l.3 RHi is clearly not ice supersaturation.

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