The reviewer's comments are repeated in bold, my replies use normal font.
I thank the reviewer for his/her thorough reading of the manuscript.

The manuscript presents a parameterization of young contrails based on an extensive data set of large-eddy simulations available in the literature. The goal is to come up with simple relations for the geometric and microphysical properties of contrails (such as vertical extent and ice crystal number at the end of the vortex phase) that can be easily incorporated into global models like GCM. I found this study is a remarkable effort to collect and condense data from detailed, small-scale LES in an intelligent and compact formulation that is manageable to use by global modelers.

I appreciate this positive comment.

However, it doesn't bring new insights into current understanding of contrail physics nor discuss new simulation results and so it does not quite fit publication in ACP in my opinion. Given its technical nature, it would be perfectly suited for a GMD paper with essentially no additional effort and in such a case I would support publication without hesitation. The final decision lies with the editor but in any case the author should address the following points:

Reviewer 2 had a similar comment. See my answer in the reply to Reviewer 2.

- it is instructive to explain how the proposed parameterization can be made consistent with the GCM where it is plugged into. For example, how do the parameters H and N (or the corresponding normalized functions) enter in the conservation equations solved by a GCM that has its own physical assumptions and numerical approximations? In other words, which terms of the GCM (and how) should be modified? Of course the details depends on the specific code but can you provide a general strategy for implementing this parameterization in practice?

In several GCMs, contrails are initialized by prescribing a certain ice crystal number concentration over a certain contrail cross-sectional area. This implicitly determines the number of ice crystals per meter of flight path. I recommend that the contrail initialization is formulated in terms of ice crystal number per meter of flight path as this is the property of interest for later contrail-cirrus transition.

One step further one can prescribe meteorology and aircraft dependent contrail depth H and ice crystal number N.
Prior to the integration of the proposed parametrisation in a global model, several issues should be addressed.
1. It is clear, that consideration of variable H and N in a global model makes only sense, if the contrail treatment in this model depends on those contrail properties.
2. The used air traffic inventories may not be detailed enough, e.g. may not provide information on the wing span of the aircraft.
3. How is sub grid scale variability of the meteorological variables considered in the global model? Does the microphyiscal formulation allow grid mean supersaturation?
4. The GCM-quantity contrail cover usually represent a large ensemble of differently aged contrails. How are newly generated contrail added? How does this change the mean contrail depth?. Certainly, some simplifying assumptions have to be made.

All aspects depend on the specific model and can not be generally answered. Thus, how the proposed parametrisation is incorporated into a global model depends on many specific aspects.

In the following this is outlined with a few examples:
Burkhardt and Kärcher, 2009 rely on a one-moment scheme, hence their contrail evolution is insensitive to the initial N. On the other hand, they simulate contrail spreading by vertical wind shear. Thus, taking into account a more realistic initialization of contrail depth appears reasonable. Integrating the latter parametrisation in a two moment scheme was established lately and a publication on this is in review. In the updated version, inclusion of the proposed N-parametrisation is meaningful and also planned in the future.

As Chen and Gettelman (2013) do not explicitly inlcude shear induced spreading in their contrail model, H is not an important parameter, whereas N enters their equations.

In contrast to GCM applications, where usually the bulk of all contrails in a grid box is simulated, the Lagrangian approach of Schumann (2012), where many individual contrails are tracked, offers conceptual advantages in the sense, that more detailed air traffic data is processed and can be used as input data for the parametrisation.
Using traffic data composed of flight tracks of individual aircraft, several input parameters are specified in more detail (wing span, water vapour emission, Eisoot) .

In general, not all input parameters may be given or they are uncertain in some future application. Thus, section 4.2 highlights and ranks the importance of the various input parameters. This points out which parameters should be provided with least uncertainty.

- There is a mistake in Eqs. 5 and 7. In the absence of phase transition, what is conserved is vapor mixing ratio whereas vapor concentration changes because of air density change (expansion/compression due to heating/cooling). Considering the process adiabatic, one has p/T^k = const with $k \equiv \gamma/(\gamma - 1) = 3.5$ and $\gamma = 1.4$ the ratio of specific heats. Using the auhtor's notation this yields […] which differs from Eq. 5 by the exponent k in the denominator. The same correction has to be made to Eq. 7. The author should evaluate the impact of this correction in the parameterization or comment the choice of conserving vapor concentration (note the same issue would appear in terms of ice concentration which also changes because of plume dilution).

The reviewer is right, that inclusion of k would lead to definitions of z_{atm} and z_{emit} which would make more sense in physical terms.
However, a re-definition of the two length scales would not improve the quality of the parametrisation. Using redefined length scales, the parametrised values would not fit the LES results qualitatively better. z_{atm} and z_{emit} mainly control the sensitivity to RH_i and T. Block 1 of Fig. 5 treats those sensitivities and shows an excellent agreement between the parametrised and the LES-values.
Thus, it is acceptable to keep the current definitions.
Note that this has already been discussed in the original manuscript (starting from p. 28954,l. 24).

All cited papers can be found in the reference list of the original ACPD publication.