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Comment on acp-2021-497

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Referee comment on "Contrail formation within cirrus: ICON-LEM simulations of the impact of cirrus cloud properties on contrail formation" by Pooja Verma and Ulrike Burkhardt, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-497-RC1, 2021

The general topic of this work -- how formation within natural cirrus may affect contrail properties and thereby their potential impact on climate -- is a potentially important one. The results of the present paper concentrate on two topics: how the presence of natural cirrus changes the number of contrail ice crystals nucleated and how it changes the fraction of contrail crystals that survive the wake-vortex regime of the contrail evolution. Despite what the paper's title and abstract suggest, neither of these processes are actually simulated in the present work. The ICON-LEM simulations are employed purely to provide large and hopefully representative sets of sample atmospheric conditions with different levels of natural cirrus (which, on its own, is a potentially useful approach). Existing parameterizations taken from the literature for contrail nucleation and crystal loss are then evaluated on these sets of conditions. In the vast bulk of these cases the results of the cirrus on contrail nucleation and crystal loss are found to be essentially insignificant. This is not a new result; Gierens (2012) earlier reached the same results in more succinct and robust fashion. The authors here highlight the differences for the less common case of very heavy cirrus. Unfortunately, the parameterizations relied on here for contrail nucleation and crystal loss were not designed for, or tested on, the case of contrails forming within natural cirrus. The authors' extensions of these parameterizations to apply to this case leave out critical physics that is involved in these processes. In my opinion this makes the new conclusions drawn regarding changes in nucleation and crystal loss due to heavy natural cirrus untrustworthy and, in some regimes, even of the wrong sign. Nor are the results critically examined within the context that is really of interest here: how the natural cirrus might affect the potential impact of contrails on climate.

The text is generally clearly written, but verbose. Several statements are repeated in multiple places within the body of the paper. Further, much of the text is taken up paraphrasing old results from elsewhere rather than providing more explicit specifications of the present study.

Specific points:

(1) In assessing the impact of existing cirrus on contrail nucleation the authors only consider the contribution of cirrus ice sublimated within the jet engines. The effects of cirrus ice that is mixed into the jet plume later are not considered. Depending on the temperature of the diluting jet at the time, these crystals might sublimate (providing an extra moisture source) or grow (providing a moisture sink). The authors use the change in contrail formation threshold temperature (T_sa), occurring through a change in the slope of the mixing line G in eqn. (1), as their primary metric of cirrus impact. This approach is already problematic in the presence of significant cirrus. The usual mixing line analysis and computation of T_sa relies on conservation of the water vapor during the mixing process (so that G is constant). But with the existing cirrus it is only the total water that is constant in the mixing process, not the vapor portion alone, because of growth and sublimation of the cirrus crystals. Furthermore, the contrail nucleation parameterization employed (Karcher et al., 2015) is based around the approximation that all droplets form at the same instant, and so the effects of existing ice crystals competing for available moisture during the nucleation process are not included. A recent LES study of contrail formation (Lewellen 2020) has shown that competition between ice crystals that form earlier and ones trying to form later can, in some regimes, significantly reduce the number of contrail ice crystals that would otherwise be produced. While for thin natural cirrus this effect on contrail nucleation should be negligible (as is the sublimation contribution considered by the authors), where the authors are reporting a significant cirrus effect (i.e., for very heavy natural cirrus and/or near contrail threshold temperatures) it need not be. This neglected contribution could lead to the presence of the cirrus sometimes reducing contrail ice nucleation, rather than always increasing it as concluded by the authors.

(2) In extending the parameterization for the fraction of contrail crystals surviving the wake-vortex regime developed by Unterstrasser (2016) to include the effects of existing natural cirrus, the authors again only consider the cirrus crystals as a moisture source. Furthermore, they ignore the Kelvin effect in their implementation (e.g., in lines 283-285). This is a problem because it has been explicitly demonstrated in LES studies that crystal losses in this regime are significantly greater when the Kelvin effect is included than when it is not (see e.g., Lewellen et al 2014). The reason is that a significant portion of the crystal loss in the plume occurs in a regime where the water vapor pressure equilibrates to conditions which are subsaturated for the smaller crystals but not for the larger ones due to the Kelvin effect. Indeed it has been shown in exact analytic solutions of ice crystal populations under different conditions (Lewellen 2012) that significant crystal loss can occur even if the overall ice mass is slowly growing in time: the larger crystals grow at the expense of the smaller ones (a process known in more general contexts as ``Ostwald

ripening''). In the present application this dynamic could lead to greater losses of contrail crystals in the presence of the larger natural cirrus crystals rather than reduced losses, including losses in the secondary wake where no adiabatic heating is occurring. Again, this effect on the contrail (like those included by the authors) should only negligibly be affected by the presence of thin cirrus, but for conditions where they are reporting significant cirrus effects (i.e., high cirrus ice number concentrations and IWC), this Kelvin-dependent process could even change the direction of their reported effect, decreasing rather than increasing contrail crystal survival fractions.

(3) The authors report greater relative changes in contrail ice crystal numbers due to natural cirrus for near-contrail-threshold conditions, largely because fewer crystals are nucleated there. It must be noted, however, that contrail ice numbers prove highly sensitive to a large host of variables near threshold conditions (see e.g., the simulation results in Lewellen (2020)). As a result, simple parameterizations in this regime, including both the nucleation and crystal-loss parameterizations that the authors are relying on for their conclusions, are much less trustworthy near-threshold than in most other regions of parameter space.

(4) In this work the authors only consider the changes in contrail ice crystal numbers due to the presence of the cirrus, not the changes in cirrus due to the passage of the aircraft (e.g., in metrics like equation (4)). For assessing the impact of aircraft on climate (presumably the ultimate motivation here) what matters is the net effect on the total system of contrail plus natural cirrus. For heavy cirrus or in near-contrail-threshold conditions where the authors claim significant increases in contrail ice crystal number, to what extent are these increases offset by losses in natural cirrus in the hot jets and descending wake? Further, the methodology for conducting the ``with'' vs ``without'' cirrus comparison is never explicitly defined, e.g., by specifying what variables are held fixed in the comparison. For example it is never actually stated whether it is water vapor or total water that is held fixed in the comparison. Given the results it is presumably the former, but the latter choice would in some sense give a more robust comparison (since water vapor changes more in time as the natural cirrus ages).

(5) It seems to me that the regimes where the current work concludes that effects of existing cirrus on contrail properties may be significant (e.g., heavy cirrus or near-threshold conditions) are not in fact ones where contrail climate impacts are potentially very significant. But how different cirrus scenarios might affect contrail radiative impact or longevity are never addressed in the paper. Radiative effects of contrails are clearly of potential concern when they seed significant, long-lived contrail cirrus in otherwise clear

skies (i.e., where natural cirrus is optically thin or absent). On the other hand, the net radiative impact of a contrail shrouded within optically dense natural cirrus will naturally be expected to be much less (as well as occurring much less frequently). Likewise near-threshold contrails can be expected to have less impact because they occur less frequently, have fewer ice crystals and tend to be shorter-lived.

Additional references cited here:

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