Review of the revised version of

Investigating the Development of Persistent Contrails in Ice Supersaturated Regions with Cloudy Backgrounds Using ICON-LEM

by Sajedeh Marjani et al.

General comment:
The authors responded to my major issues and added some text to the manuscript. However, the manuscript only slightly improved. Actually, the main issues, as the inadequate microphysics model (classes and representation of ice cloud processes) and the lack of a quantitative evaluation remain. The explanations are still not satisfying, and to my opinion, the main issues cannot be resolved by just providing some more explanations.
I still think that the manuscript should not be accepted, as long as the methods and the evaluation are not improved. However, it is certainly the decision of the editor, how to proceed.

Major issues:
The manuscript explicitly addresses contrails INSIDE cirrus clouds, implying that the contrail can be placed into a pre-existing cloud, and of course accepting the fact that ice crystals from cirrus clouds above can sediment into the contrail. Without a conceptual separation of the two "clouds" the investigation must remain incomplete. The argument about the high vertical/horizontal resolution does not help, since even in a super high resolution one CAN place a contrail into an ice cloud; without a separation of the microphysical properties (i.e. different mean masses) the resulting cloud is neither contrail nor cirrus (for instance, the sedimentation velocity is changed completely, if ice crystals from a cirrus cloud above fall into the contrail – no ice can fall out to lower layers).

It is quite clear that most of the ice microphysics scheme can be used for both, the contrail and the natural ice cloud (e.g. growth equation, formulation of the terminal velocity). However, the formation of ice is certainly different. For the contrail, this is just an instantaneous placement of the ice crystals, which is fine with me. The "natural" ice formation pathways as homogeneous freezing of solution droplets and heterogeneous ice nucleation is a different issue. To my opinion, the Kärcher et al. (2006) scheme must be adapted for high time steps (as e.g. carried out in Köhler and Seifert, 2015); probably the nucleation events can be resolved directly. The authors state that the scheme was already used in many studies before. This argument is not convincing, since all studies cited in the response are investigations of warm or mixed-phase clouds (except of the study by Verma and Burkhardt, 2022, which uses a coarser resolution and an adapted cirrus-contrail scheme). The authors have to give arguments according to the physics of the ice clouds, that this scheme can be used.

My concern about the resolution was mostly driven by the anisotropy of the atmosphere (vertical vs. horizontal scales). The authors have clarified the used configuration (resolution) of the model. However, the explanation is lacking arguments about the different horizontal and vertical scales. If they use a horizontal resolution in order of 150m, then the vertical resolution should be much smaller (in order of few 10m), if we think about the thin layers of the atmosphere. The authors should again argue with physics; again, referring to former studies is not enough if there is no convincing physical argument.

The investigations lack a clear quantitative evaluation. It would be interesting how the contrails modify the environment and thus the relevant cloud processes in order to produce a different time evolution and other physical states as compared to the control run. An analysis of time scales would be very appropriate and can be done offline (i.e. after performing the simulations). I am completely sure that such investigations would result into a clear understanding how the contrail inside the cirrus cloud influences the further evolution.

The issue of quantitative evaluation is strongly related to the choice of the scenarios. The author state that the scenarios are chosen “to represent a range of realistic, cloudy, and ice-supersaturated environments”. How do they know that these scenarios are really representative? They are just case studies from a former investigation, and at least in this study there is no statistics or whatever evaluation that these scenarios are “representative”. If they can show with a concise evaluation based on physics (e.g. time scale analysis) that the outcome of the time evolution with contrails is based on the same general effects on the natural cloud processes, then this investigation would lead to a better understanding. At the moment it is just a collection of cases, which were randomly chosen and evaluated. However, I appreciate the attempt to collect the main features as found in the evaluation thematically.