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Interactive comment on "Sensitivity of surface temperature to radiative forcing by cirrus and contrails in a radiative-convective model" *by* Ulrich Schumann and Bernhard Mayer

Anonymous Referee #2

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The authors investigate the extent to which top-of-atmosphere forcing from jet contrails are able to influence the surface temperature using a simple radiative-convective-diffusive model. This may be phrased as the "efficacy" associated with such forcing, and the authors show that this efficacy is strongly dependent on the assumed mixing within the model.

The results of this study are of interest, but they are derived from a very simplified model, and because of its simplicity I am a bit unclear on the implications of this study for Earth's atmosphere. In particular:

1) The authors find that in the limit of weak tropospheric vertical mixing, the effect of

upper tropospheric forcing like that of contrails can be to cool the surface. This seems to run counter to GCM studies of Hansen et al. (2006) and Ponater et al. (2006), which show a more constant tropospheric response, presumably because they have some vertical mixing. Does this mean that the weak vertical diffusion case in this study is simply not relevant to Earth's atmosphere?

2) In the mid-latitude case, convection is hardly active because the large-scale forcing Q_0 stabilises the atmosphere. But in Earth's atmosphere, convection acts intermittently and the convective mixing is therefore underestimated by this model. Further, I think it is unreasonable to expect Q_0 to remain unchanged in response to the forcing. The thermal stratification of the midlatitudes is set by this large-scale forcing, and a change in this thermal stratification will likely have an influence on the midlatitude eddies. Is the vertical diffusion meant to be a parameterisation of these missing processes? If so, what level of vertical diffusion is relevant for Earth's atmosphere?

3) In the "tropical" case ($Q_0 = 0$; Fig. 6) the convective adjustment is controlling the lapse rate, as is the case in Earth's tropics. But here, the forcing applied is very strong: 100% Cirrus cover. In this case, the Cirrus produces an inversion in the upper troposphere, and drives a second convective cell above the tropopause. I'm not sure this is a plausible outcome of contrail forcing. What happens if the forcing is reduced to a cloud cover of 0.2-0.5%? Do you still get the same decoupling from the surface? How does this depend on the height of the forcing?

4) What does Fig 11 look like with radiative and convective adjustment? we expect the CO2 response to warming to be relatively uniform in the troposphere. This is true with high diffusion, but does not seem to be true in the radiative-convective case. To me this suggests that the no diffusion limit is not relevant for the Earth.

My suggestions to improve the manuscript in light of these comments are as follows:

- More consistent forcing levels across the experiments. In some cases the Cirrus cloud cover is set to 3%, in others 100%. Why is this the case? And how was 3%

chosen? It seems much larger than the 0.2-0.5% quoted in the introduction for Contrail fraction. Does the response depend on the size of the forcing? What about the height of the forcing?

- Some more discussion on how the results from the simple model should be interpreted. In particular, what is the level of vertical mixing relevant for Earth's atmosphere in midlatitudes and in the tropics? How does the assumption of diffusive mixing affect the results.

- I think the study would benefit from using single-column model with a more realistic description of convection than the simple model used here (e.g., the single column model of a IPCC-class GCM). While this does not ameliorate all the problems with using a 1-D description of the atmosphere, it will ensure the convective response given the mean state will be somewhat realistic, particularly for the "tropical" case in which Q_0 is zero.

Minor comments:

page 1: Line 8: What does "basically without climate system changes" mean? Does this refer to the dynamic heating in the model? This should be clarified here and in the other places where this statement is made in the manuscript.

page 2: line 33: Here it is argued that contrails do not behave the same as high clouds, but later the forcings you apply are described as either thin cirrus or contrails. This contradiction should be resolved

page 3: Line 32: I am not sure what it means to avoid warming contrails. Does this mean that one mitigation option is to move flight paths to regions in which the effects of contrails is a cooling?

page 5: Line 1-10: The discussion here is very confusing. At one point it is stated that Q_0 is the sum of the divergence of F_R and F_T, but it is a bit unclear whether this statement is supposed to only apply for $T = T_0$ or more generally. Later it is stated

that the Q_0 = 0 case is "pure radiative equilibrium", but I think this should be Q_0 = 0 and F_T = 0.

page 5: line 20: I don't understand why \Gamma drops out of the equation for \Delta T, or why the contribution from \Gamma affects Q_0. Isn't Q_0 fixed? I think the equation for \Delta T should be presented for clarity.

page 6: line 10: Setting the cosine zenith angle to 1/4 biases the solar radiation to have a high zenith angle, this will increase the reflection from clouds and bias the results. For the global mean, one should use the insolation weighted zenith angle (Cronin 2014). But I do not see why the global mean insolation is necessarily desired. The temperature profile used is one of the mid-latitudes, so presumably that is the focus. Why not use a diurnally varying solar insolation for e.g., 45 deg?

page 6: line 20: The radiation only boundary condition for T_skin is unphysical for cases with turbulent fluxes. Perhaps it would make more sense to use an assumed value of the surface enthalpy exchange coefficient and wind speed that are typical of Earth's surface conditions.

page 8: line 32: The Hansen et al. (1997) result needs explaining. What type of model were they using? Does this indicate that the strong mixing limit is the appropriate one?

page 10: line 25: Here 3% Cirrus coverage is used, but the global cover mentioned in the introduction is 0.2-0.5%. Does the magnitude of the Cirrus cover have any effect on the results?

page 11: line 9: It appears that the Cirrus drives convection above it to the tropopause. Is this likely for the forcing from Jet contrails in the next century?

References:

Cronin, T.W. (2014), On the choice of average solar zenith angle, JAS.

Hansen, J., M. Sato, and R. Ruedy: Radiative forcing and climate response, J. Geo-

phys. Res., 102, 6831-6684, 1997.

Ponater, M., S. Brinkop, R. Sausen, and U. Schumann: Simulating the global atmospheric response to aircraft water vapour emissions and contrails. - A first approach using a GCM, Ann. Geophys., 14, 941-960, 1996.