Responses to Reviewer 2.

We thank the reviewer for his thoughtful and detailed review. The review comments lead to considerable changes and several improvements.

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.

We agree. We now add "and efficacy relative to CO_2 changes" in the abstract to follow your interpretation.

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:

We are pleased that the results are of interest.

We agree that the results are based on a simple model. That was the purpose.

As you know, our team also runs more comprehensive climate models. The problem is that such models often do not allow identifying reasons for certain results. Therefore, we looked by purpose for the most simple model we could think off to study the relative importance of mixing and radiation in clear isolation from other processes. In the conclusions, the importance of the model simplifications is now stressed. Further the abstract says: "Since the results of this study are model dependent, they should be tested with a comprehensive climate model in the future. "

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?

We agree that the limiting result cannot be guaranteed to be fully relevant for real atmospheres and we now say this in the conclusions. But as noted in the introduction recent research indicate that strong SW contributions are getting more and more realistic. Certainly, this needs further studies and this paper may trigger such studies.

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?

The reviewer addresses important issues, which we cannot answer strictly without running far more extensive models. Our point should still be valid that mixing is important. The question whether our study gives correct quantitative result cannot be answered without further research. We now say this in the abstract and in the conclusions.

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?

You are right that the cirrus cover is important for stabilization and we mentioned that. We now deleted this part to reduce the complexity of the paper.

4) What does Fig 11 look like with radiative and convective adjustment? we expect the CO_2 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.

We now show the results also for convective adjustment. This discussion is part of the discussion on model dependence. We now point out that global models often show a rather smooth profile of temperature increase in the troposphere, partly perhaps because of strong mixing on coarse grids.

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

Thank you for your suggestions. We revised the paper accordingly.

- 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?

We keep less cases, with 3% cover (the 100 % case is kept in the comparison to Meerkötter et al (1999) who run the test cases with 100 % contrail cirrus cover). We discuss the importance of cirrus properties. The strength of the cirrus forcing is important mainly for convective mixing. The diffusive mixing is linear in this model and less sensitive to the contrail details. The main issue that contrails have positive RF at TOA and negative RF at the surface is robust and independent of such details.

- 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.

We agree. We now relate the diffusivity to the studies by Stone on baroclinic adjustment by large scale eddies.

- 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.

We do not follow this suggestion because we will never find a 1-d model that includes all known effects. (A future model version should be 2-dimensional and include diural and seasonal cycles – coming closer to a full climate model.) Instead, by purpose, we simplify the study even further and skip some results of variants for clarity The test against reality has to be done within comprehensive climate models. We say this in the Conclusions. However, we show and explain the robustness of the results to parameter changes.

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.

The term “basically” was used since we had a model variant with fixed relative humidity. But we now skip this and simplify the paper.

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

We thought that our study is relevant beyond contrail cirrus. That caused part of the complexity and apparently misleading wording. We now decided to reduce the paper to the mid-latitude case and talk about contrail cirrus only (with a short remark on generalization potentials in the Conclusions).

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?

Your are right in your interpretation. We now added “route changes” to clarify this question.

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.

We now changed the text to avoid such misunderstandings.

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.

The reference lapse rate Γ drops out for constant diffusivity. This can be seen when taking the difference of Eq. (1) for $\Delta T=T(t,z) - T_0(z)$. The corresponding equation for ΔT would make the text more lengthy without providing much insight. Basically this is of theoretical importance only. The code includes the full set of equations. Therefore, we now deleted this sentence.

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?

We changed the values to mid-latitude values. The results are robust to these changes.

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.

We decided to reduce complexity by setting the surface temperature equal to the temperature in the lowest model layer, throughout the paper. Again, a more realistic model would require further model parameters, which we want to avoid, because any parameter requires a discussion on its validity and limitation and this would make the paper just more complex without much gain and without changing in the basic conclusions.

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?

We now explain that Hansen et al. (1997) used a GCM with rather coarse resolution.

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?

We use 3 % because that is representative for mid-latitudes. We now explain this.

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?

It is well known that the radiative heating in a cloud layer may drive convection above it, and this is what the model simulates. This does not mean that all contrail cirrus cause convection, and we do not say that. The text got modified for avoid this misunderstanding.

Ulrich Schumann and Bernhard Mayer, 22 August 2017