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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 #1

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This paper represents in some ways a rather impressive and stimulating study, but in its present form I am not sure its conclusions are safe ones (in the sense that they do not advance our understanding of real-world climate responses). I feel that the authors may be on to an important point, about the efficiency with which radiative perturbations in the upper troposphere can be transmitted to the surface, but whether the experiments presented here are sufficient to establish that importance is not so clear. I cannot give a strong recommendation for acceptance in anything like its present form. On the other hand I do not wish to discourage the authors from pursuing this important and interesting topic.

One of my issues with the paper is that it oscillates between being a fundamental study of the fate of radiative perturbations in the climate system and being a more applied and directed study concerning contrails in particular, and it is easy for the reader to get lost amongst material that is not clearly relevant. Certainly this reviewer felt lost on several occasions, and I found myself having to go back and re-read earlier sections and still I sometimes struggled. I am sorry to say that if I had not been a reviewer, I may not have persevered with reading the paper.

So, for example, some of the approximations that are made may be appropriate to a more theoretical/illustrative study, are not so clearly appropriate if the aim is to specifically understand contrail efficacy. They might even invalidate the results. And similarly, while it might be useful to discuss the pure radiative equilibrium case in a theoretical study, that case is not really relevant to understanding contrail efficacy. I feel that the repeated presentation of the radiative equilibrium case gets in the way of understanding the real-world response. Overall, I felt the manuscript tried to be too "completist" (e.g. presenting figures and calculations that didn't need to be presented) which made the manuscript longer and more complex than it needed to be.

A central issue in this paper is the ability of real-world cirrus/contrails to distort the vertical profile of temperature in the way that is shown in figures 5 and 6. It is this stabilization that is key to the authors' results. Is there any wide scale evidence that cirrus of contrails do this, particularly away from the rather special conditions in the tropical tropopause layer? I feel the authors need to do a critical analysis of the literature on this point, as the paper would be greatly strengthened if they are able to present any such evidence.

Detailed comments. Those preceded by an asterisk are more major comments.

1:1 I have a concern about the title. I do not believe that cirrus causes radiative forcing. It certainly has a radiative effect that can change (and hence induce a feedback) but this is rather unlike the contrail case. Perhaps "contrails and contrail cirrus" would be better as these are more obviously forcings.

1:8 “basically without climate system changes” – presumably this means “no feedbacks” except for temperature change?

1:13 “Heat induced by cirrus” – since in principle there is a latent heating associated with cirrus formation, clarify that this is “radiative heating due to cirrus”

1:14 “adjusted” – is this stratosphere-temperature adjustment?

1:23 and throughout: I think it better to talk of a “cloud radiative effect”, as is now common in the literature, rather than a “net radiative forcing” of cirrus.

2:2 “heat induced” – maybe better as “changes in radiative heating”

2:7 “covers” –> “is estimated to cover”

2:26 “contrails occur mainly over land” – this could be clearer – do you mean that most flights are over land, or conditions for contrail formation are more likely over land? I think it is the first of these.

*3:8-9 As is discussed by Hansen et al. (and I think in papers by Ponater) it needs to be clear that lambda_co2 is not a fixed number even in a single climate model, as it depends on the size of the CO2 perturbation. Hansen et al are careful to define their efficacy relative to a specific CO2 change (see their para 34 and Table 1), so that other CO2 perturbations have themselves an efficacy that departs from 1 relative to their specific case.

4:1 “rating” – I didn’t quite understand this word – perhaps it is “rerouting” afflicted by an automatic spell-checker?

4:18 “similar to a dust layer” – I didn’t understand – mineral dust layers (if that is what is meant) can have a LW forcing.

5:9 And F_T is also zero in the radiative equilibrium case, I presume. If so, perhaps the

text should say this.

**5: This page needs much better structure and to establish a consistent terminology. Three cases are presented “pure radiative equilibrium”, “radiative-diffusive mixing ” and “radiative-convective mixing” . But sometimes different terminology is used. 8:12 refers to the “radiative case” (but all cases are radiative), Fig 10 caption refers to “radiative equilibrium without mixing” and “strong diffusive mixing”, Figure 11 refers to “radiative equilibrium with zero mixing” and “uniform diffusive mixing” and then Figure 12 refers to “radiative equilibrium with zero turbulent fluxes” and “moderately strong diffusive mixing”. I could go on. I hope the author will see the need to adopt a concise and consistent terminology but also to consider whether a good scientific purpose is served by presenting results for cases in almost all figures. The terminological confusion is further accentuated on page 6 by having two variants to determine the skin temperature – no separate name is given to each case, and I am frankly not sure it is necessary to even present results from both, as the zero surface turbulent heat flux case is entirely theoretical.

*5:15 “model includes a cirrus layer” – I think it is equally important to make clear that it ONLY includes a cirrus layer – i.e. no other cloud layers are included. The paper was not clear on this point but I regard this as a serious restriction when it comes to specifically looking at the impact of contrails, and so it is important that this is kept in mind. The impact of cirrus on the surface LW and SW budget, as well as the radiative heating at cirrus cloud base (e.g. Figure 7), will be considerably affected by the lower level clouds which are missing here.

*5:21 It is not clear where the value for diffusivity comes from. Some earlier study? The value plays such an important role in the analysis that it has to be justified in a more rigorous way. And it is important to again acknowledge important caveats: in this case, vertical heat transport in the real atmosphere is not, for the most part, diffusive, and so what is adopted here is a convenience for the simple model.

5:25-29 It is a little hard to follow this – given the signs shouldn't the "max" on line 26 be a "min"?

*6:10 Using a surface albedo of 0.3 is a very crude way of mimicking low level clouds, and of course only does so in the SW (and so the LW surface budget is more sensitive to atmospheric perturbations than it would otherwise be). It is not quite clear to me why other clouds are excluded – is it an attempt to simplify or a methodological difficulty in including them? And why 0.3? I recognise this is the planetary albedo, but a surface albedo of 0.3 does not yield a planetary albedo of 0.3, because of atmospheric absorption (pushing one way) and Rayleigh scatter (pushing the other). It would be reassuring to know what the control top-of-atmosphere radiation budget is, as this would help determine how realistic the forcings (especially the longwave) are.

*6:10 "cos(SZA)=0.25" – this surprised me too. I understand that this yields the correct incoming solar radiation at top of atmosphere, but the high zenith angle (75 degrees) will significantly bias the SW effect of contrails to be more negative – indeed it is the zenith angle close to the most negative radiative forcing, according to the excellent Schumann et al. (2012 - 10.1175/JAMC-D-11-0242.1) paper and this may significantly affect some of the section 3.2 results . In radiative convective models (such as Manabe and Wetherald) it is common to assume a cos(SZA) of 0.5 and to assume a fractional day length of 0.5, although it may be more preferable to integrate over zenith angle.

*7.10 Following on from the above comment, I am now a bit further confused. In the caption of Figure 2, it refers to the daily mean at 45N on 21 June. How does this relate to the cos(SZA)=0.25? And why is a surface albedo of 0.2 used here when it is 0.3 in the text? I guess Figure 2 is trying to justify the use of the 2-stream hexagons scheme used in the radiative-convective model, but it seems to me that it is not testing it for the conditions applied in that model. I am sorry if I misunderstand. And I have a similar query about Figure 3. Since, from my understanding, the radiative-convective model does not integrate over the diurnal cycle, this plot leaves a somewhat misleading impression and I am not sure of its purpose here. My bigger question is whether the

choice of cos(SZA)=0.25 leads to a bias in the SW budget of contrails. Also since a cirrus optical depth at 500 nm of 0.3 (10:25) is applied in the experiments, it is not clear why a value of 0.5 is used in Figure 3.

7:19 The figures show 360-720 ppb, the text says 300-600 ppb

7:17 Figure 4: While it is useful for the authors to have performed this calculation, I see no reason for including it in the paper – it is a result that is over 50 years old and in my view just inflates the paper. I feel something of the same way about Figures 5 and 6, since they are referred to only in passing. The inversion in Figure 6 may be something of an artefact resulting from the exclusion of lower level clouds

8:6 The expression for heating rate is textbook physics and doesn't need including – I am not sure the value for the lowest level is in any case correct, if the surface pressure is really 1013 hPa (I get 0.64 K/day).

8:12 "radiative EQUILIBRIUM case"

8:13 "smaller vertical scales" – it is hard to see this when the plot is presented in linear pressure.

8:17: I agree that the 8-13 micron window is "more transparent" than neighbouring spectral regions, but it is hardly transparent, because of water vapour continuum absorption in this region.

8:19 "stratosphere" – this sentence only makes sense to me if it is the "lower stratosphere"

8:21 "rather stable" – it is unclear what measure of stability is being used in making such a statement

9:23 Perhaps 2 significant figures are enough in this and later paragraphs?

10:5-20 The experiment described here (100% cirrus, 150% perturbation to humidity) feels very contrived and in my view was a distraction. I suggest it be removed.

*10:24 Why 3% given the 0.2-0.5% at 2:7? But I am concerned that the assumed cirrus amount will ultimately impact the radiative heating in the upper troposphere and hence the extent to which that region can be decoupled from the atmosphere below. In addition, I suspect that the impact is also highly dependent on the height of the cirrus as well as the assumptions about underlying clouds.

11:1 “weak turbulent mixing” – which case is this referring to? See my comment **5. If you mean zero-mixing, the text should say this.

11:6 “only for strong” – but as I understand you have only performed the experiment for zero or strong, so there is no intermediate case? I then get further confused by the discussion of convective mixing later in the paragraph, partly for the reasons discussed above, but partly because it is not shown on Figure 11. I suspect the result is also highly sensitive to the assumed cirrus height. Comparing Figure 5 and 6, it seems clear that convective mixing is impacting the temperature profile throughout the depth of the troposphere so it confused me to say that “convective mixing is weak”

*11:10 The discussion at 6:10 about the chosen solar zenith angle calls into question this result, and I suggest it is revisited.

12:18 I can see no such plot in Ponater et al. (2006) – I am sorry if I miss it. Perhaps the text should refer to Figure 2 of Ponater et al. (2005) (see also 14:25) but even there I am a bit doubtful whether the point being made is the full story; the maximum in upper tropospheric warming may be a result of well-known moist adiabatic processes (in which a surface perturbation is amplified at upper levels via the divergence of moist adiabats with height).

13: I found this discussion rather conjectural and suggest it could be removed

*14: Although the central idea of this paper may indeed prove to be correct, this conclusions need to draw attention to the many caveats about the simple model that is adopted and how these may impact on the final result.

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