

Interactive comment on “Possible influence of anthropogenic aerosols on cirrus clouds and anthropogenic forcing” by J. E. Penner et al.

Anonymous Referee #1

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1 General comments

The authors study the effect of anthropogenic aerosol emissions on cirrus cloud ice crystal number concentrations and the effect of these microphysical changes on radiative forcing at the top of the atmosphere. Pre-industrial background aerosol concentrations are compared with present-day conditions, and then specific sources are switched off to isolate their effect. Several model configurations are employed as well as several parameterisations.

Unfortunately it turns out that the sign of the forcing depends on the model configuration. In fact, in section 4 I have the impression that every single sensitivity test proves the computed radiative forcing highly volatile. This alone makes it very probable that

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the authors had not paid due attention to questions of statistical and physical significance. The reported results may be noise, even if some of them seem plausible. In its current state, I would recommend to not publish this paper.

2 Major comments

1) In Figure 1 the authors have cirrus clouds at the ground, and in figure 2 they show crystal number concentrations at 140 hPa in the mid-latitudes and at the poles, i.e. far above the tropopause. What is that? Noise? How far apart must the curves in figs 2 and 3 be to be significantly different?

2) The description of the method (parts of section 1 and section 3) is not clear enough. While reading I had never the impression I understand the strategy and the how they ran the models. For instance, it is unclear what “fixed off-line meteorological fields” (p. 13907, l. 1) are and how they are used to model cirrus formation and evolution. Is it so that model output (every 6 hrs) is taken to represent fixed conditions for cirrus formation and subsequent evolution, that the cloud is abandoned after 6 hrs or earlier when the next model output is taken, and so on? When in the cloud evolution are the number concentrations recorded? Section 3 should be rewritten, such that the selected procedure gets clear.

3) Why is it required to use different parameterizations (KL, LP, KLm) when the only effect of this seems to be the variation in the threshold for heterogeneous nucleation? If this variation is all that is intended here, why cannot that be achieved with LP alone, isn't this just a free parameter? This would also render the presentation in section 3 clearer. Different parameterizations certainly involve further subtle differences that may cause effects not considered in the paper. I do not understand this strategy.

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3 Minor comment

1) In the introduction (p. 13906, l. 20 ff) it should not only be stated that Hendricks did this and that, but also their main conclusions should be reported and then the expected progress of the present paper over the state of Hendricks (2005) should be stated.

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