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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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Penner et al. examined the forcing of anthropogenic aerosols (aircraft soot, surface soot and sulfate) on cirrus clouds using two different nucleation schemes in an off-line calculation of the ice crystal number concentrations. These results are interesting insofar as this is the first estimate of the forcing of anthropogenic aerosols on cirrus clouds alone (previous estimates only provided aerosol effects on all clouds). Therefore I recommend the paper for publication after revisions as suggested below.

Major comments:

1) I agree with the referee 1 that the sensitivity studies seem a bit arbitrary. I would also question their rational for using the KL scheme, which, in the paper, is motivated by its 140% RHi threshold. However, in the Kaercher et al. (2006) paper also the 130% RHi

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threshold is used for heterogeneous nucleation. Thus, comparing these 2 schemes might make sense because the assumptions behind the competition between homogeneous and heterogeneous freezing are different, but that needs to be stated. I would second referee 1 that you could vary the RHi threshold within one parameterization if that is what you use the two different parameterizations for. Otherwise if you compare them because of different assumptions, more detail on these schemes needs to be added.

2) I have a problem with your comparison of the 3-mode scheme with a mass-only scheme, which seems to yield larger differences in forcing. I am in particular worried that these estimates are treated equally in terms of their likelihood. I would argue that the 3-mode scheme should be superior and therefore its results should be more reliable. Also, at the very end of the article the authors state the cirrus scheme has been used online in another publication. This yields larger positive forcings than discussed here. This to me questions the whole paper. Online simulations should be more reliable because the ice crystals can then sediment, which has been omitted in the offline simulations. Thus, what is the point in presenting simulations that likely overestimate the impact of anthropogenic aerosols on cirrus clouds? This point should be made much clearer

Detailed comments:

1) What is the rational for varying the updraft speed but not the RHi threshold? 2) Why are the authors comparing their 3-mode scheme with a mass-only scheme? If the purpose is to reduce the high accumulation mode number concentrations, then why not vary the collision kernel? At the end of the paper the authors compare their results with the results by Liu et al., which uses the mass-only scheme. If that is their motivation, it should be made clearer. 3) Why are you doing offline simulations? Is that to rule out any impact on water clouds? 4) P. 13911, line 8: do you really mean T $>$ Tcrit_het or is that a typo? If not, I do not understand this as you probably do not allow T to be larger than 238K for cirrus formation 5) How do you allow for

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homogeneous and heterogeneous freezing in between the temperature thresholds? Do you calculate both freezing types and then take a linear combination? If so, that is not what the KL parameterization suggests. It suggests to only calculate homogeneous nucleation if heterogeneous nucleation with subsequent growth to ice crystal size is not sufficient to deplete the available supersaturation. 6) P. 13914, lines 26-28: This is something that was also found in Lohmann and Kaercher (2002), and which can be understood because there you have the high updraft velocities so that the ice crystal number concentration in pre-industrial times might be limited by the number of sulfate aerosols.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13903, 2008.