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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol effects on ice clouds: can the traditional concept of aerosol indirect effects be applied to aerosol-cloud interactions in cirrus clouds?" *by* S. S. Lee and J. E. Penner

Anonymous Referee #3

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1 Introduction

This paper describes a modelling study on the effect of aerosol changes between preindustrial (PI) times and present day (PD) on cirrus cloud development and properties. The results are put into context with the well-known Twomey and Albrecht effects, and it is found that for the assumed cold situation the Albrecht effect is very weak. The increase of the aerosol loading between PI and PD particularly leads to higher IWP, an effect that is probably not represented in most current large-scale models with unresolved clouds. Consequences for the radiation budget are discussed as well.

The results are interesting, but perhaps not very general. Nevertheless, it should be published after some minor changes, according to the following suggestions.

2 General comments

Below are a number of comments on equations. There is a bad practice among cloud physicists to write equations without considering the units. I know this has a long tradition, but nevertheless it is bad practice. Usually it is implicitly understood that the quantities have to be taken in certain units that the reader has to look for in the text, which is uncomfortable. The reader can also never be sure whether the units inconsistency is due to an error or due to the bad practice. Much worse is it when one wishes to use these formulae with another set of units; the exercise to recompute the prefactors is very prone to errors. It is much easier and safer when the units are incorporated in the equation itself, for instance

instead of a v-to-D relation, as eq. 5:

$$v_t = a_{vt} D^{b_{vt}}$$

where even the units that should be used are not mentioned, one should write

$$\frac{v_t}{\mathrm{m/s}} = a_{vt} \left(\frac{D}{\mathrm{m}}\right)^{b_{vt}} .$$

The section on the model's microphysics could be slightly shortened, as it describes processes that do not play a role in the subsequent analysis (e.g. Hallett-Mossop).

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3 Specific comments

P. 10430, ll. 3-4: There is also an impact when IWP is constant but the crystal sizes (effective radius) change. The word "if" seems to indicate that a change of IWP is the "only" mechanism.

P. 10431, ll. 19-21: If possible, give examples of the mentioned differences.

P. 10434: Eq. 2 would be easier to understand if $\rho_a n_c/q_c$ would be combined to an average droplet mass. As it stands, the reader is puzzled in particular by the occurrence of n_c^2 on the rhs.

P. 10435, Eq. 3: The units of the rhs are inconsistent with the lhs of the equation. The argument of the exp function is not non- dimensional.

P. 10435, Eq. 4: Again the units are inconsistent. Please check!

P. 10435, l. 20: rhs has units K, lhs is non-dimensional.

P. 10437, l. 16,17: Could you please be more specific: How large is the model domain, what kind of "large-scale disturbances" do you mean?

P. 10437, l. 27: better say again "potential temperature and specific humidity".

P. 10439, l. 5: Isn't there a contradiction between nudging the wind field and periodic boundary conditions?

Sect. 4.1 needs a bit more discussion. First, as the cloud thickness is almost identical in both runs, any difference in IWP is a difference in average IWC. This is analysed in the subsequent sections. However, how much larger is the 10% difference between the MODIS observation and the high-aerosol run compared to the difference between the two runs? Although the 1-2 g/m^2 are large compared to the domain-time average, the time-individual differences in figure 5 seem moderate. At least you should indicate the observed value in figure 5. Additionally, if the model would be initialised with

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some kind of turbulent fluctuations, how would this affect the significance of the IWP differences? The answer to this question is also important as background information for the following discussions. Please clarify also that in your definition of IWP only the cloud-ice is counted (otherwise, eq. 7 would not make sense). Therefore, if all kinds of ice would be counted in IWP, how would numbers change in fig. 5, and is it really clear that MODIS sees the cloud-ice only, and not the precipitating ice?

P. 10440, eq. 6 and l. 10: Of course, A cannot be "any" variable, rather it must be a "mass mixing ratio" type variable (because of the air density in eq. 6).

P. 10442, l. 10: S should have a lower index h.

Sects. 4.3 and 4.5: I suggest to make the cause-effect relationships clearer, here and in the summary and conclusion section. First, more crystals are nucleated in the PD run than in the PI run (more aerosol -> more crystals). As the crystals start to grow they consume the water vapour. More crystals consume the supersaturation more quickly, hence there is higher in-cloud supersaturation in the PI run than in the PD run. However, more latent heat is released in the PD run, therefore the PD run yields stronger updrafts than the PI run, which in turn leads to new nucleation events,..., a positive feedback. It might be an idea to draw parts of Sect. 4.5 on nucleation before section 4.3, since you must have nucleation before you have crystals.

Sect. 4.5: The results concerning the relation between IN and CCN effects (or heterogeneous vs. homogeneous freezing) are probably not generally valid. A cirrus simulation at 8 km altitude in a more polluted air mass could give significantly different results.

Sect. 4.6: You might give an indication of the optical thicknesses of the clouds somewhere in this section.

P. 10447, ll. 16-27: In-situ collections of cirrus ice crystals often show a non-negligible fraction of aggregates. These are certainly more frequently found in relatively warm

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cirrus clouds while the present simulations treat a very cold cirrus. Again, it is not quite sure how general your results are. Hence I suggest to include some cautionary notes already here.

P. 10448 l. 1-2: From table 2 I infer that about 10 W/m^2 more are radiated away from Earth in the PD case than in the PI case. Why does this mean an "enhanced warming effect"?

I suggest to interchange the paragraphs P. 10447 l.28 to P. 10448 l. 4 with P. 0448 l. 5 to 13, in order bring the two paragraphs on aggregation together.

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