First of all, we would like appreciate the anonymous reviewer’s comments and suggestions. In response to the reviewer comments, we have made relevant revisions in the manuscript. Listed below are our answers and the changes made to the manuscript according to the questions and suggestions given by the reviewer. Each comment of the reviewer is listed (black) and followed by our responses (blue).

Anonymous Referee #3

Received and published: 8 June 2010

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

Following the comment here, units are put into Eq. (5)

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).

The following in the old manuscript is edited out:

Secondary production of ice occurs by the Hallet-Mossop process of rime splintering (Hallet and Mossop, 1974) and involves 350 ice splinters emitted for every milligram of rimed liquid at - 5.5 ˚C. The number of splinters per milligram of rimed liquid is linearly interpolated to zero between - 3 and - 8 ˚C.

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.

The sentence pointed out here is changes to “if one or both of the ice-water path (IWP) and cloud ice number concentration (CINC) changes.”; the change in CINC with a constant IWP leads to changes in effective radius.

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

We are not able to find references about comparisons of aerosol-cloud interactions between cirrus clouds coupled with deep convective clouds and those coupled with large-scale vertical motion. Hence, the words “are likely to be” are used in the sentences pointed out here to indicate possible differences in those interactions in cirrus clouds with different driving forces. However, the following is added to compare the differences between cirrus clouds here and cirrus clouds driven by deep convection.

(LL566-574 in p20)

The IWP simulated here is ~ one order of magnitude smaller than that in Lee et al. (2009) who simulated cirrus clouds coupled with deep convective motions. Hence, clouds here can be considered thin (as compared to cirrus clouds driven by deep convective motions) and, thus, depositional growth of ice crystals is likely to be inefficient as compared to clouds with deep convection. This contributes to the negligible role of the conversion of ice crystals to aggregates and sedimentation of hydrometeors in the IWP response to aerosols, since depositional growth and collisions among particles both play a critical role in the particle growth to a critical size for autoconversion.

P. 10434: Eq. 2 would be easier to understand if _an_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_{c2} on the rhs.

Done

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.

The coefficient “a” is omitted in Eq. (3) in the old manuscript, since “a” has a value of 1. “a” is added in Eq. (3) in the new manuscript to make a consistency in the unit; as described in the text, “a” has a unit of K^{-1}

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

Eq. (4) is corrected to make a unit consistency.

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

Corrected to make a consistency in the unit.

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?

“The simulations are performed in a 3-D framework. A uniform grid length of 100 m is used in the horizontal domain and the vertical grid length is uniformly 50 m above 10 km. Periodic boundary conditions are used on the horizontal boundaries. The horizontal domain length is set to

12 km in both the east-west and north-south directions. The vertical domain length is 20 km to cover the troposphere and the lower stratosphere." is moved before the description of large-scale forcings.

Large-scale disturbances associated with large-scale forcings have a spatial scale of 200-300 km according to ECMWF. This is indicated in the new manuscript as follows:

(LL274-275 in p10)

The model domain is considered to be small compared to large-scale disturbances which have a spatial scale of ~ 200-300 km or larger.

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

Corrected.

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

The large-scale forcing is imposed as the large-scale advective tendencies. The CSRM domain is considered to be small compared to large-scale disturbances. Hence, the large-scale forcing is approximated to be uniform over the model domain and large-scale terms are defined to be functions of height and time only. For example, the large-scale advective tendency of water-vapor mixing ratio is $(\partial \overline{q} / dt)_{LS} = -\vec{\overline{V}} \bullet \nabla \overline{q} - \overline{w}(\partial \overline{q} / \partial z)$. Here, bars indicate observed large-scale values. This tendency term is included water vapor prediction equation with a relaxation time of 1 hr. The imposition of large-scale temperature tendency follows the same methodology as that of water-vapor mixing ratio. The wind field nudging is also done following the similar methodology to that of water vapor mixing ratio.

The forcing is to maintain the horizontally-averaged fields in simulations close to the observed fields. But it is not to remove the differences in cloud and mesoscale circulations between high- and low-aerosol runs. The large-scale forcing does not entirely control results the model produces; it only determines the net total water, energy and wind-field momentum supplied to or removed from the domain. We do in fact see differences between the high and low aerosol cases. Although feedbacks from these differences onto the large-scale flow cannot be captured by this design, the controlled large-scale forcing isolates the effects of microphysics in an imposed large-scale flow. This enables one to see more clearly the particular effects of the aerosol changes on microphysics.

The details of the forcing can be found in Krueger et al. (1996: GEWEX Cloud Systems Study Working Group 4: First Cloud-Resolving Model Intercomparison Porject CASE 2).

The temperature, humidity and wind fields affected by the large-scale forcings advect with winds and these fields can move out of domain. The purpose of this study is to examine aerosol-cloud interactions for a given large-scale forcings or large-scale environment. This also means that we want to conserve energy, water, and wind-field momentum budget imposed by the large-scale forcings. In other words, we want the domain net energy, water and momentum variations to be controlled by the large-scale forcings only but not by boundary conditions; this way, we are able to say energy, water and momentum are imposed on model domain by large-scale environment or large-scale forcings. This is why we used a periodic boundary condition which enables energy, water, and momentum moving out of one side of domain to move into the domain though the other side of the domain by allowing the temperature, humidity and wind fields moving out of one side of the domain to move into the domain through the other side of the domain. This enables the conservation of energy, water and momentum imposed by the large-scale forcings.

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/m2 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 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?

1. The observed IWP is indicated as follows (note that the MODIS observations are available only as averaged values over the simulation period):

(LL338-340 in p12)

The difference between the domain-averaged IWP in the high-aerosol run (2.68 g m^{-2}) and MODIS-observed IWP (2.86 g m^{-2}) is less than 10 % relative to IWP observed by the MODIS.

2.The difference in the domain-averaged IWP between the MODIS and the high-aerosol run is ~3 times smaller than that between the high- and low-aerosol runs.

3. We put “random” humidity perturbations only at the first time step for the initiation of clouds. These random perturbations are designed not to impose any organized structures on the convection when it develops. This prevents these perturbations from affecting statistically mean properties of clouds. Hence, when we increased and decreased the magnitude of the random perturbations by a factor of 5, we found that the qualitative nature of results did not vary with this varying magnitude.

4. Only cloud ice is included for the calculation of IWP. This is indicated as follows:

(LL331-332 in p12)

Here, only cloud ice is included for the calculation of the IWP.

5. Since most of cloud mass is accounted for by cloud-ice mass, including all kinds of ice does not change lines in figure 5 significantly.

6. The IWP from the MODIS does not include solid hydrometeors formed from aggregation among ice crystals or from riming between liquid particles and solid particles. The following is added:

(LL340-341 in p12)

ice crystals are only included in the calculation of the MODIS-observed IWP.

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).

“A represents any of the variables” is replaced with “A represents variables of mass mixing ratio”

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

Following a comment of one of the other reviewers, S_h is removed for the simplification of Eq. (9). Instead of defining S_h, C/D is directly put into Eq. (9) which simplifies Eq. (9).

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.

Parts of section 4.5 in the old manuscript moved before section 4.3 in the old manuscript. These parts moved form section 4.2 in the new manuscript and the remaining parts of section 4.5 in the old manuscript is now section 4.6

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.

The following is added:

(LL578-580 in p20)

However, note that the heterogeneous nucleation can be more important in cirrus clouds located lower than those simulated here, since homogeneous freezing gets less efficient as background temperature gets warmer.

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

The following is added:

(LL530-536 in p19)
The averaged effective size (optical depth) over cloudy areas is 30 μm (0.43) and 33 μm (0.29) for the high- and low-aerosol runs, respectively. The percentage difference in the effective size between the high and low-aerosol runs is ~ 3 times smaller than that in IWP. Hence, the difference in IWP accounts for the difference in the optical depth more than that in the effective

size and the radiation difference is associated more with the IWP variation than the effective-size variation between the high- and low-aerosol runs.

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 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.

The following is added:

(LL592-598 in p21)

However, it should be pointed out that the conversion of ice crystals to aggregates can be more efficient in less cold cirrus clouds (or located lower) than clouds simulated here. Thus , it is possible that a role the conversion of ice crystals to aggregates plays in the response of cloud mass to aerosols is more important in the less cold clouds than that simulated here. The varying role of the conversion of ice crystals to aggregates with varying cloud temperature or cloud altitude merits future studies.

P. 10448 l. 1-2: From table 2 I infer that about 10 W/m2 more are radiated away from Earth in the PD case than in the PI case. Why does this mean an "enhanced warming effect"?

The following (LL29 in 10447- LL2 in 10448 in the old manuscript) is removed:

Thus, the increase in IWP and CINC in cirrus clouds with increasing aerosols can enhance the so-called infrared warming effect of these clouds.

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.

Done.