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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling of cirrus clouds – Part 1: Model description and validation" *by* P. Spichtinger and K. M. Gierens

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1 Reply to reviewer 1 (J. Kay)

1.1 Ad General comments:

The referee asks a very general question, that would be more suited to a general paper on modelling or a textbook. The question what level of complexity is needed for a useful simulation cannot be answered in general. It is possible to get useful results with very simple model settings, but for other applications one needs a very detailed and complex model. It is far beyond the scope of the present paper to deal with such questions.

The question of speed vs. accuracy was not a question for us. As we did only 1–D and 2–D simulations, speed was not an issue; our main goal is accuracy.

The other questions overlap with the specific comments of all referees, and are answered there.

1.2 Ad Specific comments:

p 604, l 2–4: One can trace back this statement to the papers by Sassen and Dodd(1988) and Heymsfield and Sabin(1989) and many papers that followed. An indirect observational evidence is provided by Haag et al.(2003b) who show frequency distributions of relative humidities from the INCA campaign. The in–cloud distributions show a cut–off at the approximate critical humidity for homogeneous nucleation in both hemispheres. In the polluted air of the northern hemisphere they find a clear–sky cut–off at a lower relative humidity (about 130%), pointing to heterogeneous nucleation; however, the in–cloud cut–off at the homogenous nucleation threshold shows that the number of heterogeneously formed ice crystals is, on average, not sufficient to inhibit the later homogeneous formation. Another aspect is that undisturbed homogeneous nucleation produces much more ice crystals than heterogeneous nucleation, because there are much more aqueous solution droplets than appropriate ice nuclei in the upper troposphere (see also Spichtinger and Gierens, 2008)

We add the references from above in the text.

p 604, l 8–18: We think this paragraph is necessary in order to put the new model into context. What is new or unique in our model is shortly indicated right in the next paragraph and described in more detail later in the text. We agree, that a few more words would be useful, and we give them in the next paragraph.

p 609, l 14–15: Even if ice crystals would simply be spheres, the notion “crystal size” could be either radius or diameter. For crystal habits that actually occur in nature, there

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is even more freedom for a choice between various size parameters. The only unique measure of crystal size is therefore crystal mass. We think this is evident and does not need more justification. Additionally, it is water mass that is transferred between the vapour and the ice phase, not size.

Ice crystal shape is important for depositional growth (Stephens, 1983), sedimentation (Heymsfield and Iaquinta, 2000) and radiative properties (Wendisch et al., 2005; 2007) of ice crystals. The ice crystal shape depends clearly on temperature and ice supersaturation (e.g. Bailey and Hallet, 2004; Libbrecht, 2005) but columns seem to be a frequent habit below -40 °C in a variety of field measurements (Heymsfield and McFarquhar, 2002, their table 4.1.) . Therefore we assume columnar ice crystals in our model.

p 616, l 19: Equilibrium is a good assumption as long as the supersaturation changes on a longer timescale than the radius of the liquid aerosol particles. In the validation runs we have shown when the equilibrium assumption starts to make problems (Sect. 4.1.1). We have also shown that these problems get less relevant in situations with realistic number concentration of liquid aerosol particles (Sect. 4.1.2). Unless a complete aerosol dynamics model is added there is no possibility to avoid the equilibrium assumption, which is implicit both in the Koehler theory and in the Koop parametrisation. Note also that our primary application of the model is the investigation of heterogeneous nucleation processes on cloud evolution. This effect is important primarily in situations with weak dynamical forcing, when the equilibrium assumption is valid.

p 617, l 18: We made some calculations with a binned size distribution of the background aerosol (sulphuric acid) and compared these findings with our new bulk approach. However, it turns out that our approach of assuming a lognormal distribution (i.e. without changing the shape in case of nucleation) bears errors. For sensitivity studies, we have used some different values for α. All of them fit fairly to different parts of the binned distribution. Regarding these results we decided to use $\alpha = 1.33$.

The impact of shifting the modal radius of the background aerosol distribution leads to smaller radii of solution droplets during the ongoing nucleation event. The impact strongly depends on the amount of nucleated ice crystals (i.e. on temperature and vertical velocity). For low vertical velocities ($w < 0.20\ \mathrm{m\,s^{-1}}$) and/or high temperatures ($T > 210$ K) the impact of shifting the modal radius is negligible. For stronger updraughts and/or colder temperatures the shift of the distribution during nucleation events results into a (slight) reduction of the amount of formed ice crystal number density. This is due to the fact, that smaller solution droplets need higher supersaturations to freeze than larger ones. This behaviour is slightly different to the reference simulations with an initially smaller but fixed modal radius. In case of shifting the distribution, first nucleation takes place at supersaturations characteristic for large droplets, i.e. large solution droplets will form ice crystals beginning to deplete the water vapour. During this process the distribution is shifted such that suddenly higher supersaturations are needed to nucleated further ice crystals; this leads to less ice crystals compared to simulations without shifting the distribution. In case of an initially smaller modal radius, nucleation already starts at high supersaturations.

We add some words in the text.

p 617, l 1–5: We have reformulated this section and shifted some parts concerning details of the heterogeneous nucleation parameterisation to Part 2 (Spichtinger and Gierens, 2008)

p 620: In the model comparison of Lin et al.(2002) all models exept one have a deposition coefficient exceeding 0.1. This agrees with the finding of the referee herself. We add the references.

p 628: The nice thing with modelling is that one can make academic experiments with unrealistic assumptions that allow, however, to get rid of certain complications that always arise in nature. The choice of an unrealistically large concentration of background aerosol particles is such an academic experiment. It show us the performance of the parametrisation of homogeneous nucleation without the complication of possibly exhausting the aerosol reservoir. It is exactly that academic experiment that reveals the weakness involved in the necessary assumption on equilibrium (see above), and that moreover hints at directions to improve cirrus modelling further.

p 629, l 12: In principle we agree, but unfortunately we used a different set of vertical velocities than Kärcher and Lohmann (2002). But even without a difference plot and having a log–log plot (necessarily! — otherwise we could not cover three orders of magnitude in w) we see clearly where our parameterisation reproduces the results of the bin model and where not.

p 629–630: That is true, and also stated in the paper. The reason for using a very high number of aerosol particles (hence ice crystals) is already explained above.

p 631: Surely, the 400 hPa lines at T=200 and T=215K could be left out from the figure, since such parameter combinations do hardly occur in the atmosphere. However, for consistency we still show these values in our figure although they might hardly reached in the real atmosphere.

p 631: We have not tested that. According to Kay and Wood(2008) this should be a possibility when the deposition coefficient was very small. In our model it is not very small.

p 632: In principle, yes. But of course, with only 4 different time steps, the figure would be a bit coarse and not very helpful. We think the figure is clear enough and the question does not warrant additional simulation series (each over the complete w range) with additional time step choices.

p 635, l 5–7: At least, hexagonal columns are more typical than spherical ice crystals. However, as stated above, there is no simple answer on the dominant shape of ice crystals inside cirrus clouds. At least in most of in situ and laboratory measurements (e.g. Bailey and Hallet, 2004) columns can be found for low temperatures ($T < -40°$C).

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The shape of hexagonal columns (aspect ratio depending on size) in our parameterisations is derived from measurements (Heymsfield and Iaquinta, 2000) , thus it should be representative.

p 636: Our model does not include the whole atmospheric column. Certain layers can of course be lifted more or less uniformly, e.g. in an atmospheric conveyor belt (Wernli and Davies, 1997; Spichtinger et al., 2005) or along atmospheric fronts. Although the whole concerned column might not be lifted with an uniform updraught, this assumption can be justified by investigating updraughts in the upper troposphere (e.g. Spichtinger et al., 2005) . However, this approach is often used for simulating (cirrus) clouds in 1D models (Lin et al., 2005; Kärcher, 2005; Jensen and Pfister, 2004 and many others). That we lift in our model not only the ice supersaturated layer, but also the remaining part of the domain is due to technical convenience. Since these layers have $RHi \ll 100$%, this is completely irrelevant for the results.

p 637: We agree that a more detailed investigation of competing processes using time scales could be interesting. But this would be the topic of another paper.

p 638: At least it is evident that a model neglecting any fluctuations is unrealistic in this respect. Fluctuations of temperature and humidity in the upper troposphere have been studied by Gierens et al. (2007), albeit on scales corresponding to grid boxes of large–scale models. On these scales, σ_T ranges from 0.7 to 1.2 K. Hence the assumption of $\sigma_T = 0.1$ K for the scale of a cloud sounds reasonable. Added the reference.

p 638, l 25–30: Part 1b now contains a thorough discussion of the probable mechanisms that cause the effect. In order to quantify it, it would be necessary to run an ensemble of simulations with different realisations of fluctuations (i.e. different set of random numbers). Then it would be possible to determine averages and standard deviations. With only one realisation it makes no sense to quantify it. An ensemble run is however not in the scope of the present paper. If this is considered interesting enough, it would rather warrant an own paper. However, we made an additional run with a dif-

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ferent set of random numbers for the case of $w = 0.08 \mathrm{m\,s^{-1}}$ including fluctuations and wind shear for testing our results.

p 639, l 10–12: The chosen wind shear is weak, but in the range of what is observed in the UT. We add a sentence stating results from Dürbeck and Gerz (1996) and Birner (2006)

However, as stated in the text, a stronger wind shear would damp these fluctuations even more, unless the Richardson number is higher than the critical value of $Ri = 0.25$; the latter case could lead to instabilities which are not subject of our investigations. We choose a relatively moderate wind shear to show that with a change in the environmental conditions an intermediate scenario between the extreme cases single column simulation (1D) and fluctuations without damping by wind shear can be reached. We add some text here.

p 641: The results you mention are obtained from box–model runs. It seems however that the spatial component of the fluctuations is important here, and this cannot be represented in a box model. The discussion has been extended a bit (similar explanation in Part 2).

p 642, l 5: We agree certainly, and add the reference again to clarify this.

p 643, l 13–15: It is nice to find consistency. We add the reference.

1.3 Ad Editorial comments/typos:

We do not agree that using phrases like "note that" etc. really thwarth the scientific character of the manuscript, therefore we did not change these phrases.

p 603, l 5–14: We have added some references (although some of the suggested references were still in the manuscript) and changed some parts in the discussion part.

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p 612, l 8, etc.: An ansatz in an educated guess. This is one of the German words that are sometimes used in English language. See footnote on page 335 in von Storch and Zwiers (1999).

p 603, l 10–12: Part 2 has been published in ACPD Spichtinger and Gierens (2008).

p 636: “updraught” is the correct British English spelling.

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