Response to reviewer's comments – review 2

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We thank the reviewer for the comments, leading to an improvement of our manuscript.

We extended our manuscript by several details, including time scale analysis; thus, the manuscript now contains more novel results than before. Many of the arguments below are now included into the manuscript.

1. Novelty of our results:
   The referee stated that our results are not novel, thus not worth for publication in ACP. We respectfully disagree, and present several points in order to document the results consistent with former studies as well as novel results.

   (a) General treatment of aggregation:
       For a parameterisation of ice aggregation in bulk models, the starting point must be the stochastic collection equation; further, the equation must be solved, either analytically or numerically. This is the given roadmap, which was already followed by Smoluchowski (1916, 1917) for investigating Brownian motion. Thus, it is a common treatment to solve the double integrals, this is of course not a novel part in the work; however, for investigating ice aggregation, we have to state the problem, and for our further investigations we have to reformulate the double integrals, similarly to former concepts.

   (b) Solving the double integrals – former attempts:
       As stated above, the key issue is to solve the double integrals for deriving a bulk aggregation parameterisation. Certainly, there were some earlier attempts for solutions. Actually, two different treatments can be distinguished (as we also state in the text):

       i. Passarelli (1978) assumed exponential size distributions; this enables him to find an analytical solution for the double integrals.
       ii. Ferrier (1994) and Mitchell (1988) started similarly to Passarelli (1978); however, because of the use of more complicated types of distributions (gamma or extended gamma distribution), half-analytical solutions are only possible using some additional assumptions (Mitchell, 1988). Ferrier (1994) solved the double integrals numerically and calcuated a lookup table.

       Both treatments are different from our model as they solve moment equations for droplet spectra (mass and radar reflectivity), which makes the formulae a bit cumbersome. Additionally, these aggregation parameterisations concentrate on warm temperature regimes, i.e. $T > -30\,°\mathrm{C}$.

       All other schemes rely on these two different approaches (e.g. Morrison et al., 2005), and beside the scheme by Schumann (2012), there is no other parameterisation for ice aggregation in the low temperature range. We compare our approach with Schumann (2012), see below and new text in the manuscript.

   (c) Our approach for an aggregation parameterisation:
       As in former studies, we started with the stochastic collection equation, confronted by the problem of solving the double integrals (which is not novel, but standard). In contrast to former, quite cumbersome formulae, our Smoluchowski type differential equation is very simple. Similarly to the approach by Ferrier (1994) we solve the integrals for the collection kernel offline numerically. However, instead of using a lookup table, we constructed polynomial fits, which are much more comfortable for the use in models. This is a new result, nobody has done this before, especially not for the cold temperature regime. Additionally, this new parameterisation is consistent with our bulk model parameterisation, developed for cold cirrus clouds (Spichtinger and Gierens, 2009a) – and this is an important issue, because using just any parameterisation "from the market" would create inconsistencies in the model.

   (d) New results – parameterisation:
       Despite the fact that our starting point is quite common and our approach is only partly new, there are some new results just resulting from the parameterisation:

       • The interpretation of $\langle K \rangle(t)$ as an expectation value of K given a 2-dimensional mass (or size) probability density is new, this was not done before.
       • The discussion on the scaling properties is new as well and worthwile since other authors recently seem to take these scaling properties for granted which is wrong.
       • We added a careful analysis of the impact of the width of the distribution on the aggregation efficiency; although, these results are as expected, this was not done before. Here, also a comparison with the only parameterisation for the cold regime that we are aware of is added (Schumann, 2012). Form the comparison, we can see that both parameterisations differ by only a factor of about 5. Since the value of $\langle K \rangle(t)$ varies over about 14 orders of magnitude, we can state that there is good aggreement, also considering further uncertainties for the process of aggregation.

- We added also a time scale analysis for ice aggregation, showing the dependence on size and number concentration; from this analysis we can derive different regimes for natural cirrus clouds, where aggregation might be important and where not. This was not done before for aggregation parameterisations.

(e) New results – simulations:
Certainly, the general effect of aggregation (less and heavier particles) is quite well known. However, since it was often stated that aggregation might not play any role for cold cirrus clouds (see, e.g., Kajikawa and Heymsfield, 1989), we have to demonstrate the impact. For this purpose we have carried some idealized simulations with aggregation as the exclusive process. Such tests are also missing in literature, thus also this very simple approach is a new result. Additionally, we carried out a whole set of box model simulations for a wide range of temperatures and vertical velocities. We extended the section on the box model simulations, especially by introducing the aggregation factor f_{agg}, measuring the maximum impact of aggregation (not done before). Finally, since sedimentation is a key process changing impact of aggregation we carried out idealized 2D simulations of stratiform cirrus clouds at different temperature regimes. Again, the results are not surprising in terms of general effects of aggregation. Nevertheless, they are new, because the impact of aggregation on mean ice number and mass concentration, as well as on relative humidity over ice was not shown before.

As a general remark about the novelty of research articles, we want to note that progress is usually slow and revolution events are rare. It is of high interest to state that our results are in line with former developments. If this would not the case, we would either have carried out something wrong or everybody else would have been wrong in former studies. Thus, we have to represent our treatment in comparison to others. On the other hand, we hope that we could demonstrate that many of our results are really new and worth for publishing.

2. Representation of work:
Generally, we extended the text in some sections in order to describe our developments and results in more detail. We hope that now the text can be understood better.

- p 23978, l 24: We changed the sentence into: "We use a crystal mass distribution $f(m)$ instead of a size pdf, thus we use the zeroth and first moment of $f(m)$." Next sentence deleted.

- p 23980, l 11: Yes, this is correct. We introduce I1 and I2 in eq. 2. and then it should be clear.

- p 23982, l 5: The fact that a size distribution starts to deviate from a log-normal when aggregation happens cannot yet be taken into account in our bulk model. Generally, the shape of the size/mass distribution in one class cannot be changed during the simulations. However, there might be another way to treat aggregated ice crystals seperately. Some steps have to be taken in order to do this: (1) aggregation must open a new ice class with its own size distribution (exponential or similar), (2) the aggregation framework has to be extended to aggregation between crystals from the original class and the aggregates class. This is not so difficult to do, but new double integrals have to be computed that depend then on the mode mass of the log-normal pdf (and its sigma) and the mean mass of the aggregates class. This is future work and not topic of the present paper. At least, this is now stated at the end of the section.

- p 23982, l 19: We delete the colon.

- p 23983, l 3: Changed "further" into "other" (possibly Germish).

- p 23983, l 7: In order to produce aggregates one has to start with something simpler, in our case hexagons, since parameterisations for hexagons are implemented in our model. Again, we can in principle include a new ice class "aggregates", but this is not topic of the present work (see above).

- p 23982, l 14, eq. 9: The text says "surface", thus it is the surface area. This is used in order to convert size dependence into mass dependence consistently. The surface is used in order to translate the crystals into equivalent volumes of spheres. As stated in the next sentence, random orientation has been assumed.

- p 23983, l 23, eq. 12: The pressure and a temperature correction for the fallspeeds is now given explicitly. Since this correction is the same for both crystals, it can be taken out of the integral.

- p 23986, l 3: Having updated the crystal number we can compute an updated mean crystal mass (mean mass $\bar{m} = q_c/N$ with q_c constant during the aggregation process). The updated mode mass is then diagnosed from the updated mean mass (σ_m is fixed or given as a function of the mean mass). We have clarified this in the text, an extra equation for this simple thing is not required.

- p 23986, l 26: Yes, but results are valid only down to $-30°$C, too warm for pure cirrus clouds. Levkov et al. (1992) and Lin et al. (1983) do not provide explanations for this parameterisation but it is explicitly used for ice and snow (which does not necessarily imply $T < -35°$C). Unfortunately there is a lack of laboratory data for cirrus temperatures.

- p 23989, l 7: The section has been rewritten, more details are given and we hope it is clearer now. Anyway, sedimentation changes the size distribution in the box, since larger crystals fall faster than smaller ones. This implies a change in the modal mass. Simply taking out a fraction of all crystals of each size is not equivalent.

- p 23993, l 26: "but not otherwise" has been deleted.

- p 23994: Indeed, Tian et al. (2010) provide good arguments to use a log-normal distribution. Westbrook et al. (2004) use a homogeneous collection kernel (constant exponents) and thus they get a scaling solution. As stated in our paper, our exponents change with mass so we cannot get a true scaling solution. Thus, logically, Westbrook et al.s results cannot be used to corroborate or challenge ours.

  Please note also that the term "scaling" in Tian et al. (2010) and several similar papers quoted there on the one hand and in Westbrook et al. (2004) and in our paper on the other means different things. In Tian et al. (2010), it is a method to reduce general size pdfs to the most simple member of their distribution families (exponential or standard normal); this is merely a parameter fitting method. In relation to aggregation, scaling is meant as a property of the process itself, when it would lead to a self-preserving size distribution.

- fig 8: The contour labels habe been omitted because they are merely intended to indicate where the cirrus is. The increments for ice water content isolines and isentropes are given in the caption. Values of ice water content are given in figure 10.

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