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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the relationship of polar mesospheric cloud ice water content, particle radius and mesospheric temperature and its use in multi-dimensional models" *by* A. W. Merkel et al.

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General Remarks:

This paper presents a new approach of modelling PMC microphysics using the 3-D global chemistry climate model WACCM. The approach consists of a bulk parameterization of PMC microphysics which postulates the relationship between the PMC radius, ice water content (iwc), and local temperature. The relationship is described by an empirical formula which has been adjusted from several trajectory simulations using the

complex microphysical CARMA model. The derived formula allows for any 3-d GCCM the incorporation of PMC modelling in the summer mesopause region, at least a description of major PMC parameter e.g. the ice water content, the mean effective radius and number density of ice particles which additionally allows to derive optical quantities as backscatter signals and cloud albedo values comparable to lidar and satellite observations.

The topic is very appropriate to the journal of 'Atmospheric Chemistry and Physics'. The paper contains interesting and novel features, in particular the derivation of a simple formula to model PMC ice cloud coverage in complex climate models of the middle atmosphere. I recommend the final publication of this paper, perhaps taking into account my major comments and some minor comments in addition.

Major comments:

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1) The authors state in the abstract and section 2 that the relationship between the PMC radius, ice water content (iwc), and local temperature is of fundamental character resulting from basic microphysical modelling of PMC. Here the authors have to invest some theoretical work in order to prove this weighty statement. So far, in my opinion the authors derive an empirical formula, their eq. 2, which adjusts three constants p1, p2, and p3, by the help of two fitting functions, a power function and an exponential function which up to now have no physical meaning. Hence, at that time this is a pure empirical relationship which has been not derived by fundamental deduction from PMC microphysics. Furthermore, you should explain and deduce why the specific values of these empirical constants should be valid in a general meaning of PMC formation. When comparing your model results with satellite measurements, Fig 3 and Fig 9, respectively, you try to show that also the satellite data fulfil formula (2). Obviously, Fig 3 and 9 are different in numbers and shapes to a large extent, these data sets do not

match together. I think you ignore this fact in the discussion of Fig 9. In opposite to your discussion I would say that the values of the constants should totally change if the fitting procedure is applied to the satellite data set. Hence, your actual choice of the three constants is not of universal and fundamental character as you claim in the paper (e.g. abstract) but it will depend on each different satellite experiment, and perhaps the set of constants will even change from model to model.

2) The parameterization does not take into account any dependence on variable background water vapour, e.g. season-to-season variability. In none of the equations I see as an input variable the water vapour background. This implies that the actual conditions of the 11y solar cycle, which will modify the background water vapour due to Lyman-alpha photolysis, will not change the effective radius and number density of ice particles, respectively. But the effect of variable water vapour should be very important in modelling e.g. any long term behaviour in PMC formation. This will limit the universal validity of the parameterization. Furthermore, the absence of any actual background water vapour inside the parameterization formulas will not allow for effects by variable freeze drying. The authors should comment on this aspect.

3) Another parameter which controls PMC formation is the number of available condensation nuclei. So, what are the assumptions used in the parameterization ?

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Minor comments:

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1) Page 14574, line 21: Would you expect a significant change in your results when using the 3-d version instead 1-d of CARMA e.g. Bardeen et al, 2008, 2009 ?

2) Page 14575, line 15-20: I miss any information about the seasonal date of your 4-day trajectories. Did you test trajectories from the beginning, mid, and end of the PMC season? I assume you compute 3-d trajectories in time. What is the trajectory

path with altitude ? Do all air parcels start at the same altitude? In Fig 1 it is not possible to identify each starting point of trajectories. I identified 9 points instead of 15. Why are all these starting point located in the latitude range southward of 60N ? I cannot identify any trajectory poleward of 60N, so you miss the most important inner region of PMC formation where the lower temperatures are present. I am wondering why you do not cover the entire polar region with trajectory starting points, e.g. at least two trajectories at 80-90N, two at 70-80N, etc. In summary, I think that your choice of trajectories seems to be done in a very special way which neglects many other cases. In an accurate manner you should state that so far the fit values defined in the parameterization (computed from these special trajectories) are only valid for the cases of PMC formation at the border of the PMC area (< 60N) and for a special time period from which the 4-day trajectories had been selected (beginning, mid, end of season or what?).

3) Page 14576, line 5: Which trajectory has been used in Fig 2 from Fig 1? And, add the information of date of season.

4) Page 14577, line 26: State clearly that the input temperature is defined as a function of altitude exactly at the heights where iwc values are non-zero.

5) Page 14578, line 14: The effective radius plotted in Fig 5 is the mean radius summed up over the vertical column of effective radii, or do you plot simply the maximum effective radius inside the column?

6) Fig 6,7, and 8: Which trajectories from Fig 1 have been plotted ?

7) Page 14579, line 13-14: A typo? 'a cloud is formed between the simulated hours 2 and 3.' This happens for every run, it's simply the initial condition. Or do you mean days? I don't understand it.

8) Page 14581, line 18: I can not see that the values from SOFIE are 'slightly different'. Fig 9 shows extremely different values, and also the shape of the colored domain is

different. Ok, the effective radius increases with increasing temperature but this is a trivial argument as you discussed this a section before. So, the comparison only works in a qualitative way but not in a quantitative way. See also my major comment 1.

9) Page 14582, line 14: The discussion of Fig 10 does not satisfy me. You say that for radii larger than 40 nm all curves coincide. That's true, but it's also true that for the cases below 40 nm none of the curves coincide, e.g. blue radius (yours) maximum of 15 nm and red radius (SOFIE) maximum of 30 nm. Because brightness of a PMC is proportional to the 5-6th power of radius, brightness values will differ in orders of magnitude form one data set to the next. Such a behavior I would not call as 'slightly diverge', see major comment 1. Now one could ask how often these cases, radius smaller than 40 nm, occur. Because your y-axis describes probability one can guess that for approximately 60 percent of all measurements (cases) the effective radius is lower than 40 nm. Therefore, there exists a error probability of approximately 60 percent that the parameterization induces inconsistent results when applying the parameterization to different kind of data sets as satellite, model and lidar data, respectively. Please add some discussion which does not minimize the importance of this fact. I believe that this problem is still unresolved, namely what causes actually the different maxima by lidar, SOFIE, and model.

10) Page 14583: I have to admit that I cannot fully follow the iteration procedure. What is exactly the initial condition you use for first iwc numbers unequal zero in correct physical units in order to start the iteration? Furthermore, equation 7 predicts both Q_growth and the change of the effective radius in time through the term (delta R_eff/ delta t). But then you apparently ignore your new effective radius and recalculate it from equation 2. So, you are saying I trust the new Q_growth but I don't trust the new R_eff. May be, I misunderstand this procedure. Therefore, it would really help to describe in detail the numerical recipe of iteration including the sedimentation equation.

11) Please replace Fig 11 with a figure from the mid of the season, e.g. a snapshot from the beginning of July. You want to introduce and validate a new parameterization,

so please show us a picture which is representative for the main season and not for the very end. Furthermore, the actual Figure 11 probably raises more questions than it can answer. Firstly, at the end of the season as August 3rd, no PMC will be observed at 50N as your model does. Secondly, have a look at the CIPS plot in Fig 11b, where only some weak clouds appear northward of 70N. So, it is advisable to replace the figure with a plot which shows a better coincidence of model versus observation.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14571, 2009.