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Comment

***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.***

Anonymous Referee #2

Received and published: 3 September 2009

This paper describes and tests a parameterization of mesospheric ice clouds in terms of temperature, ice water content and effective particle radius. This is an important step towards implementing polar mesospheric clouds into global models. As compared to Lagrangian models with detailed, but computationally slow particle microphysics, this obviously requires simplifications. In order to develop necessary parameterizations, the detailed microphysical model CARMA is used, coupled to the global background fields of WACCM. This generates detailed particle populations that can be integrated to provide quantities like effective radius and ice water content. By analyzing a large

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ensemble of simulations, the relationship between local temperature, ice water content and an effective cloud particle radius is parameterized. This method of obtaining parameterizations is sound. The results are tested against both microphysical model simulations and satellite data.

I recommend this paper for publication in ACP after considerations of the points raised below.

My major concern is the use of the effective radius. It is here defined as the area-weighted mean radius of the particle population (equation 1). This is motivated as the volume growth rate of a particle is proportional to the particle surface. However, in the paper, the effective radius is also used to compare to infrared occultation measurements (which are proportional to r^3) and ultraviolet/visible scattering measurements (which are proportional to some higher power of r). This needs to be handled more carefully. Ambiguous notations like "mean radius" or "PMC radius" should be avoided.

It is stated that the effective radius used here is independent of a specific observing technique as opposed e.g. to the effective optical radius used by Karlsson and Rapp (2006). This is correct. However, these approaches are not really comparable. The very idea of the effective optical radius is that it is directly related to a measurement. As opposed to this, the model-based effective radius defined in the current paper is convenient, but it is not a directly measurable quantity.

The discussion in section 3.1 is good. The authors derive a parameterized particle number density. But they also recognize that this number density is of limited value as it is based on the area-weighted effective radius rather than a volume-weighted effective radius. The same is true for the derived cloud backscatter coefficient and albedo. The authors introduce a correction by using the detailed CARMA simulations to modify the size distribution that is the basis for the derivation of the optical parameters.

In the section 3.2, the parameterization is then applied to compare to infrared occultation data (SOFIE) and lidar backscatter data. Here it is not clear from the description

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whether corrections like the one above have been applied. It should also be discussed how the effective radii retrieved from SOFIE and lidar are defined. What assumptions on the particle size distribution are made for the simulation and the measurement analyses, respectively? Can the absence of small radii for SOFIE and ALOMAR in Figure 10 be explained by instrument sensitivities?

In summary, the authors should discuss more thoroughly how useful the definition of an area-weighted effective radius is when it comes to both modeling PMC properties and comparing to experimental results from various measurement techniques.

Some other comments:

In sections 1 and 4, clear definitions of "particle growth rate" should be given. When does it refer to radius growth dr/dt , when does it refer to volume growth dV/dt ? Equation 7 suggests volume growth, but weighted with a factor ice density / air density, which remains unexplained. In sections 1 and 4, Gadsden (1998) is given as a reference for the radius growth rate dr/dt . However, this reference does not contain a discussion of dr/dt . The original reference for dr/dt is Hesstvedt (1969).

In connection with the CARMA model, it is stated that Rapp and Thomas (2006) studied the PMC growth process based on a time-independent temperature profile only. It may be interesting to include a reference to Rapp et al. (JGR, 497, 4392, 2002) who introduced gravity waves into CARMA and thus investigated faster time variations than the current paper. Based on this, a discussion on the influence of gravity waves on the conclusions in the current paper would be useful.

In section 2, the discussion of the trajectory approach should be extended. The trajectories in Figure 1 all have starting points at relatively low latitudes ($< 60N$). Does this introduce biases? At what altitude are these trajectories analyzed? Many of them have a clear northward component, which is in contrast to the usually southward meridional component at the core altitude of PMC. Is the assumption made that the entire vertical column from 75 to 100 km stays together and moves along the same trajectory? This

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is a coarse assumption, considering the strong wind shears that usually occur in the altitude range of the summer mesopause. How do all the above questions influence the resulting parameterizations?

In connection with this, the vertical resolution of the WACCM/CARMA simulations should be stated in section 2.

In section 4, it is stated that PMC nucleation is initiated when the air becomes "extremely supersaturated". This should be quantified.

In Figure 11, the two subplots (left: parameterization results, right: satellite results) cover different geographical areas (down to 40N and 60N, respectively). For a better comparison, both plots should show the same geographic area.

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

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