

## ***Interactive comment on “A 3D-CTM with detailed online PSC-microphysics: analysis of the Antarctic winter 2003 by comparison with satellite observations” by F. Daerden et al.***

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### General comments

Daerden et al present the microphysical PSC simulations which are performed online within the framework of a global 3-D chemical transport model (CTM) with full chemistry. A coupling of a model with detailed PSC microphysics with a CTM is a challenging task. Most CTM approaches circumvent the difficulties involved by making some very rough approximations, e.g. by assuming monodisperse particle distributions and/or by assuming the condensed matter to be in equilibrium with the gas phase. Neither of these approximations is doing justice to the microphysics of PSC particles, and con-

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sequently there can be very undesirable effects of such simplifications on modeled reactive gas phase species including ozone. The approach chosen by Daerden et al is therefore a laudable one.

While I find the paper interesting, the results important, and the writing clear, I do have some specific concerns which should be addressed before publication in ACP.

#### Specific comments

(1) The mathematical and numerical conditions for the coupling of the two models remain unaddressed.

First the authors use standard CTM resolution of 3.75 deg x 5 deg with 30 min time stepping, then they double spatial and temporal resolution. But is this sufficient? How much of the effort spend into detailed PSC modeling is lost because the semi-Lagrangian transport smears it out in the next time step?

The Lin-and-Rood transport scheme is good, but has its limitations as Daerden et al show themselves for the case of N<sub>2</sub>O. The authors might get away with arguing that their test of reducing (increasing) the number of bins to 12 (96) provides an indication of how much the CTM leaves the microphysical world intact. But tests of NAT mass conservation during a transport time step would be a better measure. (Such tests would also be of interest to the currently running activities of CCM validation (CCMval) by Eyring and colleagues.)

(2) The relationship of this approach and the one by Carslaw, Mann and Davies should be discussed.

Carslaw and colleagues undertook the only existing (to my knowledge) serious attempt to couple full scale PSC microphysics to a CTM. They developed a detailed NAT particle model which advects the particles along trajectories and takes full account of sedimentation. They are cited by Daerden et al., but it should be stated clearly that their trajectory-based treatment of the PSC particles does not have a problem with

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point (1) because of its Lagrangian nature. The same is true for Grooss et al., ACP, 5, 1437-1448, 2005, who apply a fully Lagrangian approach (for particles and for gas phase species). However, it is correct that these studies have been used primarily for denitrification, i.e. neither ice particles and dehydration, nor the transport of binary and ternary aerosols is provided.

(3) The paper takes a zigzag approach to the issue of surface-induced vs volume-based nucleation of NAD or NAT from STS.

While the paper starts out by saying that NAT may nucleate heterogeneously on ice and homogeneously via surface-dependent nucleation (according to Tabazadeh et al., 2002), the authors later on basically resort to reducing the homogeneous surface-dependent nucleation rate by 2 orders of magnitude. This is confusing the reader. Also, in doing so they ignore other work that has clearly pointed to a strong overestimation of NAT nucleation by the parameterization of Tabazadeh et al. (e.g., Knopf et al., Hoepfner et al., Voigt et al. as also mentioned by another reviewer) or laboratory work suggesting that for ice the nucleation is likely volume-dominated (Duft and Leisner, ACP 2004). I would suggest that Daerden et al. should simply take a confident approach and say from the beginning on that the Tabazadeh parameterization is by far too high. Just quoting the Larsen and Irie papers instead of the much more relevant papers mentioned above means coming through the back door and does not give the study the degree of independence that would be desirable.

Technical:

I am perplexed by Figure 6. Why is the influence on HNO<sub>3</sub> of reducing the NAT nucleation by 2 orders of magnitude so small?

Why are not only the observational curves in the figures but also the modeling curves showing these gaps?

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