

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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Interactive comment on referee 2 - T. Peter

We thank the referee for the review of the paper.

General comments

The authors are grateful for the appreciation of their work as discussed by the referee in the introduction, and for situating this work within the field of current research.

Specific comments

1. Mathematical and numerical conditions for the coupling of the models

Assessing the mathematical and numerical conditions for the coupling of two models as different as a 3D CTM and a microphysical box model is indeed an important issue. We are still analyzing the best way to handle this problem, but as the referee mentions, we have already performed several sensitivity tests which can give at least some indication of how well the models are coupled, in particular the sensitivity test regarding the number of size bins, and the different horizontal resolutions.

For what concerns the horizontal resolution, it is a bit surprising that increasing the resolution by 4 has only a small influence on the calculated extinction. We conclude that even in the lower resolution case, the polar grid cells are already smaller or at least comparable in size to synoptic-scale PSCs. Increasing the horizontal resolution by 4 may lead to local temperature refinements, but will not lead to major changes in the (synoptic-scale) PSC field. Therefore we conclude empirically that, as in the case of 36 size bins, a resolution of $3^{\circ}.75$ by 5° is in principle sufficient to study synoptic PSCs in the Antarctic. However as stated before the main benefit of the resolution increment lies in the improvement in numerical diffusion. (In this context, we can mention Strahan and Polansky [2006] who showed that our high resolution is sufficient to isolate the Antarctic polar vortex.)

The issue regarding the influence of the semi-Lagrangian transport on the micro-physics is a very interesting, yet a tough one. Perhaps the main problem with our present approach lies in the fact that clouds are not treated as entities but that all size bins are treated independently under transport. This leads to modeling errors such as in the following example. A specific size bin with only a limited number, let's say 2, of large particles may under one step of transport end up divided between separate grid cells. If the conditions are right, they will take up water vapor and nitric acid in those grid cells and grow larger than they would if they would have ended up together in one grid cell. In that case they would have

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had to share the available amounts of H_2O and HNO_3 .

This makes it also difficult to think of proper NAT mass conservation tests, as the referee proposes. This is definitely one of the challenging tasks in this domain and in our opinion it would be better to address this problem in detail in a separate paper.

2. Comparison to DLAPSE approach

The fundamental difference between our approach and Lagrangian-based approaches like DLAPSE by Carslaw, Man and Davies or CLaMS by Grooss et al, lies in the Lagrangian versus Eulerian description. In fact the DLAPSE approach - coupled to the Slimcat CTM - is an approach to some extent comparable to ours but avoids the direct influence of transport on the microphysics as described in the previous topic. Therefore it is better suited to study individual size distributions, e.g. in the case of so-called NAT rock particles [Fahey et al, 2001].

This is of course an important advantage, but we think our approach also has a reason of existence because it is a straightforward, simple and full Eulerian approach, including the full description and transport of binary and ternary aerosols, and ice particles, with a proper implementation of sedimentation, and the possibility to calculate optical properties from size distribution and hence to directly compare results with aerosol and PSC measurements.

The problems of interference between transport and microphysics need to be further addressed but at present the model is already capable of producing reasonable results as presented in the paper.

3. Nucleation of NAD/NAT from STS

This issue has been addressed partially in the comment to referee 1.

We agree with the referee to omit the discussion of whether or not reducing the Tabazadeh freezing rates and simply assume that they are too high and cite the

relevant papers mentioned by the referee. In its revised version the paper will present only results from high and low resolution simulations with the Tabazadeh freezing rates reduced by 100, and will mention also results of a low resolution simulation with the surface dependent freezing of NAD switched off.

Technical

- The effect of reducing the NAT nucleation rate is indeed small. In our opinion reducing the nucleation rate has rather an impact on the distribution of HNO_3 within the different particle types, but not so much on the combination of denoxification and denitrification and hence on gas-phase HNO_3 as a whole.
- We show only model data on exactly the same time and location as the available observations, so gaps in observational data will also show up in model curves.

New references

Fahey, D.W., Gao, R. S., Carslaw, K. S., Kettleborough, J., Popp, P. J., Northway, M. J., Holecek, J. C., Ciciora, S. C., McLaughlin, R. J., Baumgardner, D. G., Gandrud, B., Wennberg, P. O., Dhaniyala, S., McKinney, K., Peter, T., Salawitch, R. J., Bui, T. P., Elkins, J. W., Webster, C. R., Atlas, E. L., Jost, H., Wilson, J. C., Herman, R. L., and Kleinböhl, A: The detection of large HNO_3 -containing particles in the winter Arctic stratosphere, *Science*, 291, 1026-1031, 10 2001.

Strahan, S. E. and B. C. Polansky: Meteorological implementation issues in chemistry and transport models, *Atmos. Chem. Phys.*, 6, 2895-2910, 2006

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