

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.

Anonymous Referee #1

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General Remarks

Daerden et al. use a detailed microphysical aerosol model from DMI and couple it to a global three-dimensional CTM to perform global simulations of aerosol and PSC development as well as stratospheric tracer transport and chemistry. They compare various quantities like N_2O mixing ratios, aerosol extinction, HNO_3 , H_2O etc. with observations on a vortex average basis. While some quantities show good agreement, others show quite significant differences., especially above 500 K. The authors attribute those differences to model resolution and possible temperature offsets in the meteorological

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analyses.

This study is also to my knowledge the first study in which such a detailed aerosol model is coupled to a global CTM. While some of the presented results are promising, I do not agree with the interpretation of the shown differences between model and observations. Especially the two papers by Höpfner et al. (2006a and 2006b), hereafter referred to as H06a and H06b, are cited but not well explained in this context, especially as they seem to contradict the presented results. I think a major revision of this paper is needed before it may be published in ACP.

Major points

1. The authors use the DMI model for the Antarctic winter 2003. The same model for parts of this period was used in H06a. It should be stated in detail, how the model adjustments differ from those used in H06a. Especially H06 show that the surface-based NAD nucleation mechanism by Tabazadeh (2002) is not reproducing the observations. Also H06b show no evidence of NAD in the MIPAS data. How are the model assumptions, e.g. about the nucleation of particles and also the conversion of NAD to NAT justified? (8517.16ff)
2. The authors show only vortex average comparisons (and variability), although they should be able to show more detailed comparisons, for example the "NAT-belt" that was shown by H06a. It would be of interest to see also the distribution of parameters within the vortex, especially as meridional differences are discussed for example in section 3.7
3. There is still a scientific debate for the causes of the low NAT nucleation rates that are for example reported by Voigt et al. (2005). In this context the simulated values of the nucleation rates in the Antarctic would be interesting

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4. The meaning of the last sentence of the abstract is unclear

Minor points

1. In cases where there is significant HNO_3 uptake into the PSC particles, there are two problems with the comparison of the data with the model, first that the uptake is very temperature-sensitive and second that the MIPAS data may be influenced by PSCs. Was that taken into account in the comparisons?
2. The description of current simulations of denitrification (p. 8514) is not completely correct. The recent version of the CLaMS model (Grooß et al., 2005) uses the same principle as the DLAPSE model (Carslaw et al. 2002) in which single representative particles are followed in a Lagrangian way that dynamically grow and evaporate depending on the available gas phase HNO_3 .
3. CTM model description: The use of forecasts instead of analyses may give smoother wind fields but they would be less consistent with reality. Especially for a 6-month integration the advantage of this procedure is not clear. As not every reader is familiar with the ECMWF model, please clarify which vertical levels are chosen, i.e. the vertical resolution. How is the vertical velocity derived from the ECMWF data?
4. Figure 2: What is the source of the shown parameters?
5. Figure 3: The deviation in N_2O may be caused by too much diffusion through the vortex edge, but it is also possible that the diabatic descent is too slow.
6. 8524.9ff Aerosol extinction, figure 4: The average extinction is good but its variability is much under-estimated in the model, especially below 500K. What is the reason for this?

7. Figure 7: "small black dots" should be "small black line"; The figure shows that the simulated NO_y increases above the values of the correlation by Popp et al. which was determined for the Northern hemisphere. This may be real or an model artefact. However, the deduced denitrification should then be compared to the higher reference.
8. 8526.26, figure 8: What is meant by "rate of denitrification"? Is this the slope of the shown graph?
9. Figures 9/10, dehydration: The offset between the two H_2O sensors is evident. It is however not clear why the MIPAS data show almost no dehydration, while it is clearly evident in the POAM data and the model fits both quite well, at least below 500 K. If this is only a matter of latitudinal coverage, it would be interesting to see the latitudinal dependence.
10. It would be desirable to provide a web link to the DMI report (Larsen, 2000), if available since it is referred to for most of the aerosol model assumptions.

Corrections

8516.10: remove "before downloading"; It is not clear how an "average in a mass conservative way" is determined

8516.18: remove "the latest" as there is now a newer JPL compilation

Typos:

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8513.12	progressively
8514.9	successful
8515.14	four-dimensional
8518.13+18	non-spherical
8519.21	Antarctic
8520.22	occurrence
8521.4	spuriously
8524.19	whether
8527.25	occurring
8532.9+10	10 minutes, 50 minutes

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