Atmos. Chem. Phys. Discuss., 5, S120–S122, 2005
www.atmos-chem-phys.org/acpd/5/S120/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "3-D microphysical model studies of Arctic denitrification: comparison with observations" *by* S. Davies et al.

Anonymous Referee #2

Received and published: 4 March 2005

The manuscript addresses for the first time a non-equilibrium treatment of NAT particles within a full-chemistry integration of a global Eulerian 3D CTM. This study could perhaps serve as a benchmark for future model studies and should be published in ACP. The model validation is extensive. I do not have serious major concerns, except that I worry about the experimental setup and how the initialization and NOy scaling may affect the calculated HNO3 distribution during the integrations. If the remarks below are addressed properly, this manuscript is acceptable for publication in ACP.

General remark

The initial NOy field from the multi-year integration seemed to deviate significantly from observations, so that scaling was needed. However, the scaling was performed based on very limited observations, partly only inside the polar vortex or outside, depending on the winter. I'm afraid that the artificially created cross-vortex gradients due to this scaling by mixing affects your HNO3 fields inside the polar vortex and thus confuses the model evaluation. For example, you state that the model occasionally underestimated N2O with about 20 ppb, while NOy was overestimated with about 3 ppb. Transformed into NOy, 20 ppbv N2O would lead to a roughly 1.4 ppb overestimation of NOy, leaving an equal amount most likely caused by mixing during the integration, which is quite substantial.

Remarks page by page

Page 7, Section 3.1.1 para 2 The introduction of NOy* needs some explanation for the reader, rather than a reference, since it is a crucial quantity in the interpretations.

Page 11, section 3.1.2, final para To show that DLAPSE is better that the equilibrium approach is more convincing if the results from the equilibrium approach are added either recalculated with the same experimental setup or by discussing comparisons from previous studies.

Page 19-20, Section 4. The 1994-1995 winter seems outstanding concerning disagreement between modeled and observed denitrification. Of the examined winters, this winter was the only one still influenced by the Pinatubo eruption. Could the presence of remaining Pinatubo debris have affected the nucleation mechanism? In line with your introduction it may be relevant to discuss the potential for heterogeneous nucleation to explain the disagreement between model and observations. Why would a factor 4 increase in nucleation rate be relevant particularly for this winter, or why would mountain waves be more important this winter compared to other winters? Another exception for this winter is the scaling of NOy, which is only done with extra-vortex observations. How could this have affected your results? See also general remark.

Typo's

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page 4, para 3: efficacy => efficiency

section 2 page 6, para 2: “any one time step”??

Figure 13a+b The figure caption refers to dashed black lines, which are not present in the graph. Instead, there is a thick solid line, but it does not extend higher than approx. 20 km altitude. How could the observed denitrification be calculated for higher altitudes?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 347, 2005.

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Interactive Comment