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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of mountain waves and NAT nucleation mechanisms on Polar Stratospheric Cloud formation at local and synoptic scales during the 1999–2000 Arctic winter" *by* S. H. Svendsen et al.

Anonymous Referee #1

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General Comments:

This study use a microphysical box model to investigate the influence of mountain waves and nitric acid trihydrate (NAT) nucleation mechanisms on solid PSC formation in the 1999/2000 Arctic winter.

The model is forced by 6-hourly ECMWF temperature trajectories on a 1.5 x 1.5 degree longitude-latitude grid. This resolution is insufficient to capture the full spectrum of mesoscale mountain wave cooling. Mountain wave cooling is included by using 1 x 1 degree grid averaged fields of maximum temperature perturbations output from a separate mountain wave model.

The effect of mountain waves is examined under two different NAT nucleation scenarios. In the first scenario NAT nucleates heterogeneously onto pre-existing ice particles, thus requiring temperatures 3-4K below the ice frost point at some point in the back-trajectory. In the second scenario, NAT is also "allowed" to nucleate above the ice frost point following the homogeneous surface nucleation mechanism of nitric acid dihydrate (NAD) of Tabazadeh et al. (2002) assuming an instantaneous conversion into NAT. The surface nucleation rates are reduced by a factor of 10 following Larsen et al (2004).

Firstly, the effect of mountain waves and the different NAT nucleation scenarios are examined on the local scale. To this end, lidar measurements of aerosol backscatter and depolarisation at 603 nm from two DC-8 flights in January 2000 are compared with equivalent model products derived using T-matrix calculations. The percentage of NAT and ice PSCs are calculated for the two flights according to the observed and model-derived depolarisation and backscatter ratio using a simple classification scheme.

For the lidar comparison, one of the authors' conclusions is that, "the inclusion of mountain wave effects was necessary in order to be able to produce amounts of ice PSC events comparable to those seen in lidar observations". However, this is not surprising since, as the authors point out in the Introduction, the "ECMWF analyses lack sufficient resolution to resolve the full spectrum of mesoscale mountain waves that occur in the stratosphere".

The main local scale conclusion concerns the nucleation mechanism. It states that the percentage of ice or NAT particles in the model simulations shows best correspondence with that for the lidar measurements when NAT nucleation above the ice frost point is included. This conclusion concurs with a number of recent studies (Larsen et al., 2004; Pagan et al., 2004; Irie et al., 2004) which have suggested that many observed solid nitric acid containing PSCs cannot be explained by heterogeneous nucleation onto mountain wave ice PSCs and subsequent advection alone.

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Secondly, the authors go on to attempt to quantify the extent to which mountain waves and the two nucleation mechanisms impact on large-scale PSC production for the whole of the winter 1999/2000. The methodology for this is to use vortex-covering trajectories starting from 11th January both forward to the end of the winter and backwards to the start of the winter. Firstly the temperature trajectories are used directly to determine the percentage of trajectories which at some time fall below the ice frost point and NAT equilibrium temperature over the winter. Secondly, the results of the microphysical model simulations on these trajectories are analysed for the two nucleation scenarios with and without the mountain wave cooling added.

The inclusion of mountain waves are again required to produce any ice PSCs. The authors find that NAT particles are found more frequently on the large scale when mountain wave cooling is added (whichever NAT nucleation scenario is used). The authors also find that even when mountain waves are included, under the NAT-nucleation-above-Tice schenario the percentage of trajectories containing NAT particles is larger than when ice particles are required.

However, the absence of anything to compare this fraction to means it is unclear whether the larger percentage of trajectories containing NAT particles when the surface nucleation mechanism is included is more or less realistic. Consequently the final conclusions are forced to be rather weak.

The paper is sometimes quite well written but at other times is a little vague and lacks focus. The introduction section is somewhat brief and the review of pertinent papers rather thin. The conclusions section should also more clearly describe what can be drawn from the findings of the paper (see specific comments).

The topic of the manuscript is certainly of interest to the PSC community, the question of what mechanism dominates in producing nitric acid containing PSC particles is of great importance in understanding denitrification and ozone loss in the Arctic. However, the conclusions reached do not really add a great deal to what is known about the

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important topic described in the manuscript's title. The method of incorporating mountain waves into a detailed microphysical box model for vortex scale studies is new and presents possibilities for future investigations. Consequently the manuscript is worthy as a discussion paper because of these modelling methods and the sensitivity tests to the different nucleation mechanisms. However I am not convinced that the conclusions are sufficiently clear for the paper to be included in Atmospheric Chemistry and Physics without a major re-write.

Specific comments:

1) The Abstract

Although the abstract outlines the methodology of the paper, at no point does it state any results or conclusions of the paper. The abstract should also summarize briefly the main points to be drawn from the paper.

2) Nucleation rate used for NAT production above the ice frost point.

The surface homogeneous nucleation rate used by the authors is somewhat controversial at present (see Tabazadeh, "Commentary on 'Homogeneous nucleation of NAD and NAT in liquid stratospheric aerosols: insufficient to explain denitrification' by Knopf et al.", Atmos. Chem. Phys. Discuss., 3, 827-833, 2003). Given the uncertainty of the NAT nucleation mechanism above Tice and the fact that the surface nucleation rates from Tabazadeh et al (2002) has to be reduced by a factor of 10 to agree with observations, I think the authors should consider also using instead a simpler, more general nucleation rate expression as a sensitivity test. Perhaps the volume-average NAT production rate of Carslaw et al. (2002), which has been shown to produce number densities of NAT particles in good comparison with observations for the 1999/2000 winter, could be used.

3) The Introduction

The introduction section is rather brief and should be expanded to include a summary

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of current thinking around NAT nucleation mechanisms and other papers which analyse lidar observations of PSCs (e.g. Toon et al., 2000; Biele et al., 2001; Hu et al., 2002; Fueglistaler et al., 2003).

4) Comparison of model simulations with lidar measurements

The comparison of the observed and derived backscatter and depolarization signals is weak and the corresponding figures need considerable attention. These two DC-8 flights were over the Norwegian mountains and were specifically to investigate mountain waves. Lidar signatures from both flights have already been analysed more comprehensively by Hu et al. (2002).

A short description of each of the flights should be included, preferably with a map showing the flight tracks with areas perhaps indicating where the type 1a and type 2 PSCs were measured at one level.

On page 4589, lines 1-10 the authors compare the lidar signatures with and without the inclusion of mountain waves, and finally conclude that "When considering the backscatter ratio and depolarisation the inclusion of the effect of mountain waves apparently produces a better correspondence between the observed and calculated quantities". From my eye, it seems that some of the observed features are better reproduced on inclusion of mountain waves (e.g. the feature at 21-23km on 25th January at 15-17UT) but some features compared more favorably when mountain waves are not introduced (e.g. spurious feature at 18km on 25th January 08-09UT). A few specific regions where the comparison is visibly improved should be pointed out in the text and perhaps marked on Figures 1 to 4. Areas of disagreement should be explained. For instance the signal in the lidar indicates type 1a between 19 and 23 km on 23rd January 15-16UT, but in the model simulations the lidar signature seems to indicate Type 1b by the classification scheme used. Yet this apparent disparity is not even mentioned.

The legend for the backscatter ratio in Figures 1 and 3 goes from 0.18 to 2.5, but the classification scheme used assumes type II PSCs for 603 nm backscatter ratio

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greater than 5.0 (provided the depolarisation is greater than 2.5%). The plots would be much easier to understand by increasing the upper limit of the legend to at least 10 and marking the contour of B=5.0. Similarly in Figures 2 and 4 marking the contour D=2.5% with a thick black line would help distinguish Type 1b from Type 1a by visual inspection.

Also, areas where there is no depolarisation data in the lidar measurements should be omitted (plotted white) in the corresponding part of the model derived profiles in Figures 1-4 to avoid the eye being diverted to these areas.

5) Lidar classification

Page 4589 lines 16-19: The authors should explain why they have used this classification scheme as opposed to the more recent classification scheme of Biele et al (2001).

The paper would be improved by putting the classification information in either a table or a graph of backscatter ratio versus depolarisation indicating the parts of the graph where each PSC type is assumed.

6) Conclusions

The conclusions section is a little muddled with some conclusions appearing in the results section and a discussion section absent. The paper would perhaps be improved by removing the discussion from the conclusions and having a seperate discussion section between the results and the conclusions. The conclusions should then be rewritten with a number of short succinct points drawn to summarise the results and discussion. Vague statements like "mountain wave effects appear to be quite significant" should be avoided.

Minor comments:

1) The 4 plots in each of Figures 1-4 should be labelled a-d.

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2) Page 4588 line 22: replace "depolaristion" with "depolarisation"

3) Figures 9 and 10 should have their 6 plots labelled a-f

References:

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4581, 2004.