Atmos. Chem. Phys. Discuss., 4, S1922–S1925, 2004
www.atmos-chem-phys.org/acpd/4/S1922/

Atmospheric Chemistry and Physics Discussions

ACPD
4, S1922–S1925, 2004

Interactive Comment

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 #2

Received and published: 30 September 2004

General comments

This paper uses a trajectory-based box microphysical model to simulate the formation of solid polar stratospheric cloud particles during the Arctic winter of 1999-2000. One basic goal of the study is to compare two different nitric acid trihydrate (NAT) nucleation scenarios - one where NAT nucleates heterogeneously below the frost point on pre-existing ice particles, and another in which NAT forms by homogeneous surface freezing at temperatures above the frost point. The second goal of the study is to assess the effects of mountain waves, which are included in an indirect way by applying a negative temperature perturbation derived from Mountain Wave Forecast Model results

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

to the coarser resolution ECMWF temperatures used to force the microphysical model. Calculated PSC optical properties are compared to airborne lidar data collected in the Arctic on two January 2000 flights of the NASA DC-8. Then, vortex-filling trajectories are run forward and backward from January to assess the effects of mountain waves and the different NAT nucleation mechanisms over the entire 1999-2000 winter.

The review by anonymous referee #1 gives an excellent synopsis of the results of the papers, and I don't feel the need to detail them here. The main findings are that agreement between observations and calculations of both Type 1a and Type 2 PSCs is best when NAT nucleation above the frost point occurs, and that Type 2 PSCs basically do not occur in the absence of mountain wave temperature perturbations. The findings are not surprising in light of other recent papers, and I concur with referee #1's assessment that the conclusions of this paper do not add substantially to our present state of knowledge about solid PSC formation. The paper would be stronger if alternate NAT nucleation mechanisms were considered. Some points that I wish to reiterate or emphasize are included in the specific comments below.

Specific comments

Abstract - A brief summary of the findings of the paper must be included in the abstract.

1. Introduction - The introduction should include much more discussion of current ideas on solid PSC formation and results from recent relevant publications.

2. The Microphysical Model - As referee #1 has pointed out, a number of recent papers suggest that homogeneous surface freezing may not be the mechanism by which NAT is nucleated. The authors did in fact have to reduce the nucleation rate by a factor of 10 to align their calculations with observations. I think it is a major weakness of the paper that some alternate heterogeneous NAT nucleation scheme has not been included and tested. I am also curious why SAGE and LIMS data were used to specify the background aerosol size distribution and HNO3 profile, respectively. Are there not more appropriate data available from the SOLVE-THESEO 2000 campaign, e.g. from

dustsonde and JPL Mark IV launches?

2.1. Including Mountain Wave Effects in the Simulations - It's a little confusing that both the amplitude and standard deviation of the wave-induced temperature fluctuation are discussed in the text, while only the amplitude is used in the study.

3. Comparing Model Runs and Lidar Measurements - I agree strongly with the suggestion of referee #1 that maps be included to show the location of the DC-8 flights of January 23 and January 25. It also might be helpful if the authors showed, as an example, a time history of the three temperatures (T0, TA, and Tcorr) along a trajectory on a single potential temperature surface. The authors should be careful to clarify throughout this section that they are using aerosol backscatter ratio, and not total backscatter ratio.

Figures 1-4. Why is there "banding" in the plots of calculated aerosol backscatter ratio and depolarization ratio? Is it just to make the results visible to the reader? The authors state in section 3 that the inclusion of mountain waves "apparently produces a better correspondence between the observed and calculated quantities." This is a rather weak and qualitative statement, and it's not obvious that the figures support the statement. I agree with the suggestion of referee #1 that calculated results be omitted (plotted white) in areas of the figures where no depolarization data are available. I'll go one step further and suggest that measured backscatter also be omitted in these same areas since the PSC classification scheme the authors use is based on having both backscatter and depolarization values. Also, I don't see the need for the number of colors used in the plots. For the purposes of this paper, it seems that only 3 color ranges are necessary for aerosol backscatter (less than 0.18, between 0.18 and 5.0, and greater that 5.0) and only 2 color ranges for depolarization (less than 2.5%, and greater that 2.5%).

Figures 9-10. I suggest replacing the layer number descriptors in the figure frames with the layer potential temperature; e.g. change "layer 5" to "theta=475K."

ACPD

4, S1922–S1925, 2004

Interactive Comment

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

ACPD

4, S1922–S1925, 2004

Interactive Comment