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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Differences in Arctic and Antarctic PSC occurrenceas observed by lidar in Ny-Ålesund (79° N, 12° E) and McMurdo(78° S, 167° E)" *by* M. Müller et al.

Anonymous Referee #1

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GENERAL COMMENTS

It is well known that polar stratospheric clouds (PSCs) play a major role in polar ozone chemistry, and that PSC occurrence is related to very low stratospheric temperatures. As greenhouse gases continue to accumulate, the stratosphere will cool, which is expected to increase the frequency of Arctic PSCs and heighten the possibility of widespread Arctic ozone loss. Since PSCs occur very frequently in the colder Antarctic today, one might presume that these Antarctic PSC data can be used as a proxy for future Arctic PSCs in studies of possible ozone loss. The present paper raises a warning flag for such studies by showing that PSC properties are not a simple function of temperature alone.

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Interactive Discussion
Discussion Paper

The paper presents and compares data obtained over nine winters (1995-2003) using similar polarization-sensitive ground-based lidars in Ny-Alesund, Spitsbergen, and McMurdo, Antarctica. The data are classified according to PSC type, i.e. PSC 1a = solid nitric acid trihydrate (NAT), PSC 1b = supercooled ternary solution, and PSC 2 = H2O ice, using the well known backscatter/depolarization criteria of Browell et al. [1990]. The paper shows that there are significant differences between the PSC ensembles observed at the two lidar stations, which would not be expected based on average temperatures alone. Type 1a PSCs were observed on almost all measurement days at McMurdo, while Type 1b and Type 2 PSCs were seen in only about 10% of the measurements. At Ny-Alesund, both Types 1a and 1b were observed on 75-85% of measurement days, while no Type 2 PSCs were observed. Another very interesting finding is that Type 1a-enhanced PSCs, with presumably higher particle number densities and smaller particle sizes than the NAT "rocks" responsible for stratospheric denitrification, comprise between 30-50% of the total Type 1a data. This differs significantly from several papers in the literature that refer to Type 1a-enhanced PSCs as "rare."

I think the paper will be of considerable interest to the stratospheric chemistry community. However, I feel that the authors need to do more work to convince the reader that the lidar data are truly representative and not biased by the fact that they are collected only under clear tropospheric conditions. Several suggestions are listed below.

SPECIFIC COMMENTS

1. Throughout the paper, the authors refer to the "constant background of NAT particles" observed in the Antarctic. To me, the word "constant" implies that the backscatter and depolarization values of the background NAT do not change. A better phrase might be "persistent background of NAT particles."

2. In Figure 2, the authors interpolate from ECMWF analyses to compute the monthly averaged temperature profiles over McMurdo. But, to normalize the McMurdo lidar

ACPD

4, S2348–S2352, 2004

profiles, they use NCEP data. I'm sure this doesn't impact the results of the paper, but why were different data sets were used? For consistency, the authors could perhaps use NCEP analyses to compute the monthly averages.

3. Also in Figure 2, the authors used lower H2O mixing ratios in August and September to account for dehydration in computing the estimated TNAT profiles. They should also use lower values for HNO3 to account for the effects of denitrification.

4. One suggestion for testing the potential bias of the data sets is to compute (and show) monthly average temperature profiles using temperature data only from the days when the lidar was operated. If these are similar to the average profiles for entire months shown in Figures 1 and 2, then the lidar data can probably be considered typical.

5. It's not clear to me that the Ny-Alesund and McMurdo data sets are equivalent when discussed in terms of measurement days. The paper states that the Ny-Alesund lidar is operated continuously on days when the weather is favorable, but that only two measurements per day are taken with the McMurdo lidar. Thus a Ny-Alesund "measurement day" might include data points from multiple profiles collected over possibly many hours, whereas the McMurdo data are more likely to come from individual profiles. So it seems that the two data sets are not necessarily equivalent unless the meteorological situation over a "measurement day" is relatively stationary. The authors need to clarify and discuss this point in the paper.

6. Along the same line of thought, the McMurdo data set (2 profiles per day) would seem to have many fewer data points than the Ny-Alesund data set (continuous measurements). Could the small size of the McMurdo set bias the statistics derived about the various PSC types there?

7. It is difficult, if not impossible, to assess the overall consistency of these measurements with other PSC climatologies (e.g., the Poole and Pitts [1994] and Biele et al. [2001] papers cited in this manuscript), which are usually presented in terms of the per-

ACPD

4, S2348–S2352, 2004

Interactive Comment

centage of total observations that are classified as PSCs. Showing consistency with other data sets would also help establish that the present measurements are representative and unbiased. The authors need to add such a figure or do the calculations and discuss this point in the text.

8. On pages 6848-6849, the authors discuss the so-called “sandwich structure” PSCs, in which the presence of solid NAT particles is masked by the more prevalent liquid STS droplets. In discussing Figure 8, they proceed to state (but not show) that a plot of perpendicular-polarized backscatter ratio reveals that solid particles did indeed exist over the entire altitude range of the cloud. The paper by Biele et al. [2001] demonstrates that paired plots of parallel-polarized and perpendicular-polarized backscatter discriminate PSC type much better than the backscatter-volume depolarization ratio plots used in the present paper. The authors seem to be aware of this (R. Neuber was a co-author on the Biele at al. paper), so the natural question is: Why wasn’t that approach used in this paper? I am not suggesting that the authors redo all their analyses, but is there a chance that the results would be significantly different if the Biele et al. approach had been used? I recommend that the authors reanalyze a portion of their data to answer this question.

TECHNICAL CORRECTIONS

Page 6845, line 8: I don’t think contemporaneousness is a word. Do the authors mean co-existence?

Page 6846, line 25: The sentence beginning on this line should read “The region corresponding to the Antarctic...”

Page 6847, line 14: The phrase “In contrary...” should be “In contrast...”

Page 6851, line 18: I think the authors mean that the Ny-Alesund observations clearly differ from those at McMurdo. Ending the sentence with the word “unambiguously” is confusing.

ACPD

4, S2348–S2352, 2004

Table 1: The defining backscatter ratio range for Type 1a enhanced PSCs should read as follows: $2 < R532 < 10$

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ACPD

4, S2348–S2352, 2004

Interactive Comment