Atmospheric Chemistry and Physics Discussions

Interactive Comment

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 #2

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This paper is dealing with the comparison of PSC lidar profiles from McMurdo, Antarctica (Southern polar vortex) and Ny-Ålesund, Spitzbergen (Northern polar vortex), taken among almost a decade of data from both stations, in order to state on the differences in PSC occurrences between the two hemispheres and infer the prediction of future Arctic PSC occurrence from present Antarctica PSC occurrence. First, the temperature conditions in Arctic and Antarctica are compared. Then the instruments and data sets are described. A statistical analyse is given before discussing the differences in PSC lidar profiles from each station. This paper is well in the scope of ACP. Many lidar observations of PSC have been published, but for each hemisphere separately.

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The originality of this paper is to make a correlative study of PSC lidar data from quasi symmetrical stations in both hemispheres. The authors have at their disposal long time series of excellent lidar observations. Unfortunately, the study is only based on data description. The level of analysis is not sufficient at all and the authors can certainly do better. In addition, the structure of the paper is extremely confusing: there is no order in the discussion and it introduces a lot of repetitions. This really has to be worked further. Consequently, this paper should be acceptable, subject to major revisions.

General comments:

1 - One of the main problem concerns the objectives of the study. From such a simple description of lidar profiles and a little statistical analyse, the authors can only infer comparisons of PSC characteristics and occurrences. But they certainly cannot say anything about future PSC occurrences in Arctic. Such a prediction requires to study the relationship between observations of each PSC type and the conditions of influence prevailing for these observations, that is, not only temperature conditions at the observation time, but also location in equivalent latitude with respect to the polar vortex, thermal histories of the air masses in which PSC particles form... So, there are two options. Either the authors can do some more work in order to fit the objective of Arctic PSC occurrence prediction or they revise the objectives of the paper.

2 - The fact that what happens in these two stations does not represent the global hemispheric situation is not a real problem: nevertheless, the comparison of stations with a similar position with respect to each polar vortex is interesting in itself. The main point is to state this similar position, for instance by showing the position relatively to the vortex edge at different time during winter or by showing how can these stations be influenced or not by orographic waves propagation.

3 - In order to classify the PSC types, the authors use the well known Browell-Toon method of backscatter-depolarisation. Meanwhile, from many observations both in Arctic and Antarctica, a number of type 1 PSC measurements do not fit to this classifi-

cation (Rosen et al., 1997, Shibata et al., 1997, David et al., 1998, Tsias et al., 1999, Stein et al., 1999, Toon at al., 2000Ě). These non-classified type 1 PSC have been referred to as small NAT particles in high concentration (type 1a enhanced), transition or mixed types (type 1c or 1d or 1x). In fact, they lie between type 1a and 1b characteristics. In this study, the author should be consistent on the way they account for their type Ia enhanced PSC: sometimes they use them as a distinct category (but do not show the corresponding figures) and sometimes they include them within the type 1a category. How the threshold of backscatter ratio for type 1a and type 1a enhanced is chosen? I mean, if type 1a enhanced have $2<R<10$ and type 1a have $R>2$, and both have depol>1.44, how can we distinguish them?

4 - Concerning the use of the datasets, there is a point on how the authors account for the PSC occurrence. As the numbers of measurements days are very different from a station to another, the total number of PSC observed is not significant for the comparison. The percentage of detected PSC with respect to the number of observation days is the suitable parameter (this is used in all the PSC climatology studies); moreover, the authors use this percentage in the paper.

5 - A clear explanation of the methodology used to count the observed PSC has to be given before describing the statistical analyse, in order to understand it better. Some typical profiles (1 to 3, not more) could be shown in order to lighten the presence of several clouds on a same profile.

6 - The statistical analyse is very poor. Almost a decade of data in both stations can certainly give much more information than the compared frequency of PSC occurrence and of co-presence on the same day of solid and liquid particles. For instance, with such a huge data base, some statistical characteristics of type 1a enhanced PSC (or non-classified type 1 PSC) can withdraw: a figure could show their frequency of occurrence. Do they appear at anytime during wintertime? Rather at high altitude or at low altitude?.. The same for classified type 1 PSC and for type II PSC: when? where? which evolution during winter?..

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7 - In the introduction, most of the references concerning PSC observations and climatologies are German or Italian. The authors should be aware that there are some more relevant publications from other groups, in atmospheric specific journals.

Line by line comments:

p6839, line 5: "from inactive reservoir gases through heterogeneous reactions" instead of from inactive reservoir gases in heterogeneous reactions".

p6841, line 1: ice clouds occur every winter only in the inner part of the vortex, but not at the edge.

p6841, line 18: Figures 1 and 2 could be put into a unique figure, in order to compare easily both stations.

p6842, line 4: "warming ever observed" instead of "warming observed ever".

p6843, line 12: "dataset includes" instead of "data set comprises".

p6844, line 4-8: This paragraph is not clear. Could the authors be more explicit (see general comments)?

p6844, line 11: "according" instead of "geared".

p6844, line 21-22: I do not understand this sentence.

p6845, line 8: "co-existence" instead of contemporaneousness.

p6845, line 12: suppress "by far".

p6846, line 2: "highlight" instead of "illuminate".

p6847, line 11: Change for "Even without significant denitrification due to PSC sedimentation, the continuous presence of NAT particles over a broad spatial range during late Antarctic winter has a large impact onĚ"

p6847, line 14: "In contrast" instead of "In contrary".

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p6847, line 29:"like" instead of "resembling".

p6848, line 28: Suppress "In principle".

p6849, line 5: "decrease" instead of "diminishment".

Figure 6: Could the author give a reference on their TSTS calculation?

References: Waugh et al., 1999 and Zhou et al., 2000 are missing

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6837, 2004.

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