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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Observations and analysis of polar stratospheric clouds detected by POAM III and SAGE III during the SOLVE II/VINTERSOL campaign in the 2002/2003 Northern Hemisphere winter" *by* J. Alfred et al.

J. Alfred et al.

Received and published: 12 March 2007

We thank the referee for his/her comments and respond to each on a point-by-point basis.

Anonymous Referee #1

Received and published: 1 January 2007

General Comments:

The paper by Alfred et al. presents the first integrated POAM and SAGE III analysis of

Polar Stratospheric Cloud (PSC) occurrence by using a unified method of PSC detection. The method is presented in detail and has been applied very carefully to the two datasets. Despite the sensitivity for PSC detection of both instruments looks slightly different the unified method results in consistent statistics, as well in comparison to other analyses and satellite observations in the literature over the same winter. The temperature dependence of the PSC observation frequency in respect to the Ni- tric Acid Trihydrate saturation point have been used to infer irreversible denitrification over the entire winter, with levels up to 80% by early January between 400 and 550 K potential temperature. The paper is clearly structured and the presented results are well described. However, parts of the section on denitrification can be improved and additional references should be addressed. Some figure captions are not completely clear to me. As well some more detailed questions to different topics should be addressed. Details and suggestion are described below.

Main Comments:

It is very positive that the authors discuss in detail the PSC temperature relationship and their uncertainties. However parts of the discussion are confusing and overloaded by details. Inferring the dentrification by the change PSC occurrence is a very nice tool. However, this method has limits. It would be very valuable to compare the presented analysis to the work of Davies et al. (2006), where the authors have compared model results including denitrificaton and microphysical formation and sedimentation of particle with HNO3 observations of the MIPAS instrument on ENVISAT for exact the same winter. To use the method as a tool for other satellite measurements to analyse long Arctic denitrification record - like suggested in the summary - it would be necessary to validate the method in more detail, e.g. in comparison to more global-like HNO3 measurements by satellites (MLS or MIPAS) and models. The latter might help to close the gap to regions of the polar vortex not observed by the satellites but definitively important regions to get the total budget of denitrification correctly. Because such an extensive analysis is out of scope of the paper, the authors should address the limits of

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the denitrification analysis method more carefully (e.g. in the last section of summary).

Authors Response: We have added comparisons with MIPAS-E measurements to Sect. 4.2. While it is beyond the scope of this paper, a validation of this methodology has been conducted using MLS HNO3 [Santee et al., 2004] and POAM water vapor climatologies as part of an expanded study using the SAM II, POAM II data in both the NH and SH and shows promising results.

A major limitation of this methodology is that many measurements – both PSC and non-PSC, and at a wide range of temperatures– are required to infer denitrification. Both the POAM and SAGE instruments sample a wide range of equivalent latitudes and temperatures throughout the vortex however there is no way to extract denitrification on a spatial scale. All measurements are required and only a vortex average can be obtained. Also in order to obtain enough measurements to conduct a statistically significant analysis requires at least ten days of measurements at cold temperatures. This limits the amount of information that can be obtained on the temporal evolution of denitrification. We have added a discussion of these limitations in Sect. 4.2.

In terms of expanding this analysis to other instruments it is more difficult to quantify the extent to which other circumstances could contribute to a temperature shift. In the SAM II data, there is only one wavelength so it is not possible to conduct the cloud composition analysis and determine to what extent a shift in Type 1A and 1B PSCs could contribute to such a shift. The CHAMP-RO data begin in 2002, so prior to that point there are less data to help determine any possible bias in the MetOffice temperatures.

Santee, M. L., G. L. Manney, N. J. Livesey, and W. G. Read (2004), Three-dimensional structure and evolution of stratospheric HNO3 based on UARS Microwave Limb Sounder measurements, J. Geophys. Res., 109, D15306, doi:10.1029/2004JD004578.

Specific comments:

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Referee Comment: The paper is missing a more general introduction on the PSC types and e.g. why T_NAT is an important threshold temperature.

Authors Response: We have expanded the introduction to provide an overview of the different PSC types.

Referee Comment: P11396: A maybe more detailed description of the winter 2002/3 than in the Manney et al. 2005 paper is given in Naujokat and Grunow (2003).

Authors Response: This reference has been added.

Referee Comment: P11397: It is not clear to me how the authors defined a background profile (BG). Any enhanced extinction profile due to a PSC will enhance the BG values. Is there any cloud clearing beforehand the BG computation? For example, in the SH winter it might be very difficult to find inner vortex non-cloudy profiles at all, because part of the winter the whole vortex will be filled with PSCs.

Authors Response: One of the fundamental needs in the development of the Unified PSC database [Fromm et al., 2003] was the determination of an objective aerosol background loading that did not require cloud-clearing in its calculation, since the Unified time-frame covers both very high and very low aerosol loading conditions. This was achieved by assuming that in the presence of PSCs the total distribution of aerosols will be quasi-Gaussian mode comprising non-PSC aerosols and a tail of PSC extinctions [Fig. 5 from Fromm et al. illustrates this]. The BG and sigma are determined from this quasi-Gaussian mode, for both an inside and outside of the polar vortex air mass. We have amended the text to briefly describe the calculation of BG and sigma.

As the referee points out, this does cause some complications in the SH winter. In the NH there are always enough non-PSC extinction profiles to calculate a BG but in the SH the majority of the vortex is filled with PSCs. In the SH the BG and sigma are determined by interpolating the calculated values from early-June to late-September.

Referee Comment: P11398: because the Randall et al reference is so far not published

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and the results are very important for applied method it would be helpful to present a few more details about the reasons of the POAM/SAGE bias.

Authors Response: The POAM /SAGE bias is not well understood and we already speculate to possible causes such as the effect of aerosol cleansing and that in low aerosol measurements, small pointing errors can result in large fractional aerosol extinction errors.

Referee Comment: P11399: Could any disagreement in the profile comparison caused by systematic altitude offsets/errors. Is there a noticeable tangent height error for the instruments. If yes, then please specify.

Authors Response: There was an error in this Fig. The corrected version does not show a significant altitudinal offset between the SAGE and POAM profiles. There is no known systematic pointing error in the POAM [Lumpe et al., 2002].

Lumpe, J. D., R. M. Bevilacqua, K. W. Hoppel, and C. E. Randall (2002), POAM III retrieval algorithm and error analysis, J. Geophys. Res., 107(D21), 4575, doi:10.1029/2002JD002137.

Referee Comment: P11400: Fig. 5 description, can not a different viewing direction for both instruments to the coincidence region cause differences in the extinction profile shapes and absolute values?

Authors Response: We presume the referee is referring to Fig. 6. There would indeed be differences caused by different viewing directions between SAGE and POAM. Given that SAGE conducts measurements during local sunset, and POAM during local sunrise, the viewing geometries are different. The viewing footprint of each instrument is up to 200 km, so even when both have the same tangent point there may be different air masses sampled from each instrument. These differences arise primarily from line-of-sight inhomogeneities, and are unquantifiable in the type of measurements that both SAGE and POAM conduct. However, the differences between the two measurements caused by line-of-sight inhomogeneities is likely less than any difference caused by temporal and spatial differences between the two measurements. We have added the following sentence: “Differences in the shapes of the PSC measurements could be attributable to either the different viewing geometries between SAGE and POAM, or the evolution of the PSC over the span of three hours.“

Referee Comment: Fig. 7.: The SAGE statistics show a kind of bimodal distribution with a local maximum at T-Tnat = 0 K, not obvious in the POAM data. Any idea why this peak has formed, is it a specific region of the vortex and why it is not in the POAM data?

Authors Response: The mode in the SAGE statistics at -2K $<$ TNAT $<$ 1 K yet does not appear in the POAM statistics is due to two factors: i) To reduce wear on the azimuth motor POAM conducted measurements only in sunset mode (i.e. In the Southern Hemisphere) on 2 January. SAGE conducted measurements of PSCs on that day in this temperature region. ii) On the days that POAM was conducting measurements in the NH the PSCs that were detected by SAGE were of modest enhancement levels only a few sigma above the 3-sigma detection threshold. The corresponding POAM measurements do show small features in the aerosol extinction but only reach enhancement levels of near 2 sigma, thus they are not categorized as PSCs.

Referee Comment: P11402: That Poole et al. observed a similar behaviour is not surprising, due to the fact that they used SAGE III data as well, but Spang et al. analysed MIPAS data. To my mind the different instruments should be noted, to highlight the differences in the measurement technique and/or analysis method.

Authors Response: This point is now noted in the text.

Referee Comment: P11403: The PSC type differentiation method is not well described. To my knowledge the method is mainly sensitive to the size of the particles, or not? If so, then this should be mentioned. Finally, can the authors draw some conclusions from the PSC type occurrence over the winter? Is the method sensitive to NAT rocks?

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This would give direct indication for denitrification. Is the method able to differentiate clouds of small NAT particles from STS clouds? If not, the drawn of conclusions from the analysis are limited.

Authors Response: The method takes advantage of the fact that Type IA clouds are composed of a small number of large particles, while Type 1B clouds are composed of a large number of small particles. As the cloud grows from the interstitial aerosol, the method cannot discriminate between 1A and 1B, and are categorized as undetermined. These particles are unlikely to influence denitrification, as they are not large enough to sediment. However as the clouds grow they separate in extinction- Ångström exponent space and can be distinguished. The particles in the clouds categorized as 1A are large enough to sediment and as they continue to grow will form NAT rocks. Some clouds are comprised of both Type 1A and 1B clouds. These are reported as mixed. While the majority of clouds lie near the 1A or 1B regions in extinction- Ångström exponent space, there is a continuum in mixed-phase clouds. As with any classification method we had to assign thresholds to delineate different cloud types.

The fact that the discrimination method is only sensitive to particle size is an important one that should be made more explicitly. Particle composition cannot be determined - only inferred – with this method. Clouds that are designated as 1A likely contain a significant amount of NAT particles, as it is unlikely for STS to grow to such a large size. Clouds that contain NAT rocks would be designated as 1A with no other distinction. The following has been added to address this point: “It is important to note that labeling PSCs as Type 1A or 1B does not definitively mean NAT or STS composition as the method is only able to differentiate particle size. Consequently PSCs labeled Type 1B may in fact be partly composed of small NAT PSCs, although Type 1A PSCs are unlikely to contain STS, as STS droplets are unlikely to grow to such large sizes.”

There is no way to determine whether the clouds designated as 1B are composed of all STS particles or a large number of small NAT particles. In situ observations have shown that when NAT particles are observed in PSCs, their number is several

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orders of magnitude smaller than STS particles. This small number will not give the levels of extinction associated with 1B clouds. However, the conclusion that any shift in PSC occurrence-temperature is not a result of a change of phase in PSCs is still valid. The scenario outlined in the text outlines an upper limit to the change in PSC occurrence-temperature that can be attributed to a change in PSC phase under the assumption that all 1As are composed of NAT and all 1Bs are composed of STS. Since the PSCs designated as 1B probably contain small NAT clouds, the effective T1B would be slightly warmer than TSTS. This would result in an effective shift to warmer PSC occurrence-temperatures from a change in PSC phase.

We have added more detail to describing the discrimination method in Sect. 3.3 and added to the discussion in Sect. 4.1.1

Referee Comment: P11405-07: The discussion on the Type 1a to 1b ratio is confusing and the type differentiation might have limits, which causes error in the analyses. I would suggest to condense the discussion and to focus on the main results.

Authors Response: This section has been changed with the adding of subsections (as Referee #2 suggested) to make it more lucid. Specifically the section discussing the Type 1A/1B ratio has been rewritten. While there are limits to the extent that we can quantify the effect of any change in the Type 1A/1B ratio would have on a change in PSC occurrence temperature we have not suggested that this is a rigorous enough approach to attempt to use this in a correction for PSC occurrence temperature shifts. We only wish to thoroughly outline the other possible causes for a shift in PSC occurrence temperature.

Technical corrections:

Referee Comment: The abstract should include the wording for NAT

Authors Response: Changed.

Referee Comment: Introduction last sentence: ‘ temperatures are used TO infer’

Authors Response: Changed.

Referee Comment: P11408: You describe the short cut HAGAR but not SIOUX, this is inconsistent.

Authors Response: To our knowledge, SIOUX is not an acronym. No other references in the scientific literature define SIOUX.

Referee Comment: P11407: What's HiZmin? Please clarify.

Authors Response:We have added: "Ěwhere the air becomes sufficiently opaque in the presence of ice that a measurement profile terminates at an abnormally high altitude."

Referee Comment: Fig.8: In my print-out I cannot find blue vertical lines for saturation in respect to ice.

Authors Response: We have remade Fig. 8 enhancing the blue vertical lines.

References:

S. Davies, G.W. Mann, K. S. Carslaw, M. P. Chipperfield, J. J. Remedios, G. Allen, A. M. Waterfall, R. Spang, and G. C. Toon, Testing our understanding of Arctic denitrification using MIPAS-E satellite measurements in winter 2002/2003, Atmos. Chem. Phys., 6, 3149-3161, 2006

Naujokat, B. and Grunow, K.: The stratospheric arctic winter 2002/03: Balloon flight planning by trajectory calculations. Proceedingsof the 16th ESA Symposium on European Rocket and Balloon Programmes and Related Research, St. Gallen 2003 (ESA SP-530), 421-425, 2003. S5859

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11391, 2006.