Atmos. Chem. Phys. Discuss., 4, S3102–S3107, 2004
www.atmos-chem-phys.org/acpd/4/S3102/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Polar stratospheric cloud observations by MIPAS on ENVISAT: detection method, validation and analysis of the northern hemisphere winter 2002/2003" *by* R. Spang et al.

R. Spang et al.

Received and published: 22 December 2004

First we like to thank Michael Hoepfner for his helpful comments. Below you will find our point-by-point reply.

The referee addressed three main topics in his general comments, which we would like to comment on first:

General comments

1) The intercomparison of some parameter are missing

In the revised version of the manuscript we will add comparisons with the Lidar stations for the cloud top height analysis. For the PSC types analysis we have restricted the analysis to the SAGE data due to various reasons. (a) The problem of comparison with other datasets for PSC type is the limited number of coincident cases, particularly considering the appearance of the NAT feature apparently depends on the particle size. Therefore we chose the dataset with the largest statistics for the comparison. For the Ny Alesund lidar case, only 2 coincidences for the NAT enhanced measurements were available of which both were in agreement. However both events showed a large backscatter ratio (BSR$<$2) and in contrast coincidences with low BSR ($<$1.2) or mixed type clouds (STS/NAT) show no indication for an enhancement at 820cm-1. We will add a comment in the text regarding this point. (b) So far we have no access to the POAM PSC type analysis.

2) It might have been worthwhile to try to tune the cloud index limits

The threshold value of 4 is already the result of a tuning process, considering effects like excluding artefacts, signal to noise limitations and the temporal/latitudinal/trace-gas dependence of the CI-background values. CI $<$ 4 is the condition for which definitive and consistent detection of PSCs can be made over the whole height range from ˜14 to 30 km. A potentially more sensitive approach is under investigation, based on a mean cloud-free CI-profile and a sigma-value of the variability of the CI (similar to the SAGE extinction method); This might help to detect optically thinner PSCs, but it will require a very carefully validation.

3) The term 'analysis' in the title is a bit ambitious

The term 'analysis' is to my mind not restricted to comparison with model results. It highlights the presentation of observations in respect to meteorological conditions and the temporal and spatial evolution.

Specific comments:

P6287L12: continuum emission should be changed to continuum radiation - changed

P6287L20-21: cloud emission -> cloud radiation (see above) - changed

P6288L7: ... radii $> 1\ \mu$m change to ... radii $> 1\ \mu$m in channel A of MIPAS. - changed

P6288L13: What means under conditions with no PSCs? ... Perhaps examples of definitely PSC-free regions, like mid-latitudes should also be shown in Figure 2. - We will modify the text accordingly. In addition we will modify Figure 2 by an exchange of the mean profile (not really used in manuscript, like mentioned by the referee later) with a mid-latitude (50-55deg) mean profile.

P6288L26: threshold value to 4 : How has this value been derived? Could it be tuned for even better sensitivity? - see general comment (2) above.

P6289L7-15: Too much discussion of winter 2002/03 here in the section where the cloud detection method is described. Perhaps better move to section 5. - we will move this part to section 5.

P6289L15-22: There is a small error in the given MIPAS pointing corrections - corrected

P6289L15-22: - we agree with the referee comments of the field-of-view effect on the CTH error, and the text will be changed accordingly.

P6290L9: How has the threshold of 10% been determined? - The intention for the applied method was to detect only events with a clear/obvious radiance enhancement at 820 cm-1 and to exclude very weak enhancements and artefacts. We must also consider that STS can also produce a slight enhancement in this wavelength region. We have tried different approaches, like absolute and relative enhancement at 820 cm-1 or the method introduced by Spang and Remedios (2003) for the CRISTA data. The chosen method might be not the most sensitive but turned out to be the most robust one in comparison with a visual inspection of the detected spectra (exclusion of artefacts). The method is not working on a statistical basis and therefore we will change the corresponding sentences. As well we will include the wavelength regions the referee asked for.

ACPD

4, S3102–S3107, 2004

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

Section 4.1: I miss some statistics about comparison of MIPAS and lidar cloud top heights. - This will be incorporated. The results show a good agreement in respect to the error estimates of the MIPAS CTH, e.g. a mean CTH difference Lidar - MIPAS of +1.3 km (standard deviation = 1.6 km) for Ny Alesund and a slightly larger difference of 2.5 km (sdev = 0.9 km) for the Alomar Lidar. A backscatter ratios $>$ 1.1 have been used to derive the lidar CTH.

Section 4.2.1: To estimate the cloud-top height intercomparison it would be nice to have the absolute accuracy of the POAM measurements. - Due to the solar occultation technique the absolute accuracy of the tangent height is extremely high (no bias) and the leading error in the determination of a cloud top height is the vertical resolution and sampling respectively, which results in error estimate of +/- 1 km.

P6293L1: 'I have problems with the given length of the measurement volume ...' - We agree with the comments of the referee, but these data are given here in the sense to introduce to the reader a quantity of the measurement volume in relation to the miss-distance and miss-time approach. We will caution the readers that these lengths apply only to very thin clouds at the tangent height.

P6293L15: Can you give also the standard deviation of the mean differences? - this will be incorperated.

P6293L24: Would it be possible to try to correlate the not-detected PSCs by MIPAS with differences in the T-field at the MIPAS and POAM tangent points? and P6295L11: Perhaps check T-fields also here. - This is an interesting idea but out of scope of the paper. However, for such an analysis you have to cope with potential temperature biases, due to interpolation and altitude errors, as well the temperature history play an important role and should be considered (e.g. if the air parcel is moving from warm to cold conditions or the other way round). Both T-check and T-histories will be addressed in future analyses.

P6295L23: Any idea why POAM PSC top heights fit much better to MIPAS than SAGE?

ACPD

4, S3102–S3107, 2004

Interactive Comment

Just statistics? POAM less sensitive than SAGE? Latitude dependence? - The slight inconsistency between both solar occultation experiments is currently under investigations in the expert teams of both experiments. It is out of scope of this paper to cross-validate both instruments and we would prefer not to comment on this in the paper.

P6296L11: Can the CI-values be adjusted to match more of the SAGE/POAM PSC observations? - A simple increase of the threshold values didn't result in a better statistic. A more sensitive approach based on a mean cloud-free CI-profile and a sigma-value based on the variability of the CI values (similar to the SAGE extinction method) might be more sensitive in the detection of optically thin PSCs, but requires carefully validation.

Chapter 4.3: I miss comparisons between MIPAS and lidar/POAM PSC-types. - please, see general comments

Somewhere it should be pointed out that the Type detection of SAGE/POAM is based on different size - we agree and will incorporate this.

P6298L28: 'Otherwise it is not clear why a CI$<$3 is mentioned here...' - This sentence will be changed to: After a short warming of the polar vortex between 30 December and 4 January, where only two optically thin events with 3$>$CI$<$4 were observed, weak PSC activity started from 5 January onwards.

P6299L5: Fig. 7 indicates no PSC events at all in February. - This figure is replaced with a correct version which includes Feb events.

P6300L13-22: Can you identify such an effect in the MIPAS PSC data? From Fig. 8 (d) I think there is no clear signal that (meanCTT - Tnat) is decreasing. This should be mentioned. - Denitrification has been observed in the MIPAS HNO3 data, but this will be analysed in detail in a different paper. A short comment on the MIPAS HNO3 observation will be given. In addition we will alter the paper to say that denitrification

ACPD
4, S3102–S3107, 2004

Interactive Comment

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

EGU

has been observed, but that we cannot observe the corresponding decrease in NAT existence temperatures within the scatter of the data.

P6302L21: It could be misleading to compare formation processes here, since the statistics for MIPAS is rather small and since CRISTA data have been measured in August and not during the initial period of PSC formation over Antarctica. - We will delete this sentence.

P6303L5: New calculations showed that $1.3<CI<1.5$ can also be STS (or large, but also dense NAT), however, more probably it is ice. $CI<1.3$ is very probably ice. - We think, the statements in the manuscript are in agreement with this.

P6304L9: In contrast to what is said here, Fig. 7 shows no PSCs during mid- February. - This figure is replaced with the correct version including Feb events.

Figure 2: Mean profile of log(CI) and standard deviation of log(CI): what are these necessary for? Are these used somewhere? - Not really, therefore we followed the suggestion of the referee (above) and included now a mid-latitude mean profile instead, which shows more clearly how cloud-free CI-profiles looks like. We are using log(CI) because the linear variation of CI at one altitude step (especially above 25 km) is producing an unrealistic mean and standard deviation for the linear scale.

Technical corrections: All technical corrections will be incorporated in the new version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6283, 2004.