Reply to Reviewer#3:

We thank the reviewer for the helpful comments and suggestions on the manuscript. Please find below the point-by-point response and the changes in the manuscript. Replies are presented using times roman fonts. New or reworded text passages in the revised version are highlighted in *italic*.

Comments:

(43) "PSC... and are formed of particles that are classified into three types..." as the particles are of three types, but coexist, giving rise to PSC classes more than three.

We changed the text to: "*PSCs are located in the cold polar vortices in both winter hemisphere, and are formed of three types of particle, which can also coexist: …"*

(56) maybe here is "typology" instead of (or in addition to) "concentration"
We changed accordingly: "…, *where the rates depend on surface area, particle, and typology of the particle (...)."*

(60) Here, for completeness, the authors may also wish to quote the extensive dehydration that PSCs can induce in Antarctica.

We added a sentence on this topic.

(80) ". . .faster. . ." unclear. Faster than what?

We modified the corresponding sentence:
*Further, although reaction rates have been adjusted in recent model studies (e.g. Wegner et al., 2012), there is still a substantial uncertainty on the rates of heterogeneous reactions on NAT (Carslaw et al., 1997, Wegner et al., 2012) which makes determining the type of PSC present in the atmosphere important.*

(169) ". . . with altitude, and with altitude. . ."

Repetition is now deleted!

(191) "600000 modelled spectra with varying PSC types:" here it is unclear to me whether this forward model account for external mixtures of the three PSC particles. It seems otherwise, also in view of the following discussion, but maybe this this should be made clearer here.

Mixed type spectra have been not modelled in the database. Mixed type clouds will create spectra with mixed spectral characteristics and will make it more difficult to establish separation lines / thresholds with the Bayesian classifier. We added a comment on this restriction to `pure' PSC types in the model calculations.

(372) Typo. Corrected.
(439) Typo. Rephrased.

(440-450) The comparison shows good agreement, but I had some difficulty following the reasoning that tries to explain the only evident discrepancies between the two datasets: NAT and ICE in the low altitudes at the beginning of the season. While the absence of ICE at low levels in MIPAS may be due to the vertical truncation of its data, and while the detection of NAT around 12 km is commented for both datasets, the increased presence of NAT in the MIPAS measures between 12 and 15 km seems to me not well addressed. The authors could similarly comment on that small discrepancy.

We agree with the concerns of the reviewers (#2 and #3) and revised the whole paragraph. The NAT and ice partitioning was not represented correctly in the former description.

(542) Unfortunately in fig. 8 the years 2007 and 2008 are displayed with shades of green too close for me to be able to distinguish them.

Colour code has been changed in the new version of the manuscript.

(543 and following) Here, and elsewhere, the authors focus their attention on a particular Arctic winter, rather than reporting the climatology of mean values. I do not agree with this choice, given that the particular winter is, as highlighted in the text, exceptional, and therefore not very representative. It is also true that, given the

high arctic variability, even the average values are not very significant, but this can be highlighted, thus commenting on the low representativeness of the average conditions, while reporting the climatology over the whole dataset and transferring the discussion on the specific winter 2011 in a supplement.

We considered this before we submitted the paper. As the reviewer points out, there are arguments for and against a multi-annual mean of NH PSC seasons. We already presented an extended list of items characterising the mean NH conditions in the submitted manuscript version, but without showing a figure. Consequently, we followed the reviewer suggestion and replaced the plots for the winter 2010/11 in Figure 12 with the mean 2002-2012 statistic and highlighted the limited representativeness in the corresponding section in the text. The very specific winter 2010/11 is still described in section 4.1.2 (Fig. 8), whereby we omit repetitive parts of the description for this specific winter in 4.4.2. We also think, that a presentation of only one the specific winter in a supplement is not adding substantial information to the paper, if the main finding are already summarised in other sections of the article.

(612) Here the author may also quote that, to a lesser extent, also the denitrification and dehydration play a role in the downward propagation of PSC occurrence.

We added a corresponding sentence:
*Dehydration and denitrification processes and the corresponding redistribution of $H_2O$ and $HNO_3$ over the course of the winter have also influence on the downward propagation in PSC occurrence.*

(631) See comment for (543 and following).
See reply above.

(685-689) This interesting difference between Arctic and Antarctic seem to be explained by an artefact, but I did not get the explanation in full. Is this difference arising because high, thin arctic clouds are warmer that the corresponding Antarctic ones, at the same altitude and with the same optical thickness? If so, the authors may consider to rephrase the paragraph to make such statement clearer.

We rephrased the paragraph and left out misleading statements:
*This difference may be caused by a signal to noise ratio issue for altitudes of ~30 km and above (Spang et al., 2004, 2012). At these altitudes the cold stratospheric temperatures yield only very weak radiance signals in the atmospheric window region (close to the detector noise level) used in the cloud index approach. Consequently, cloud index profiles start to get noisy above ~30 km and cloud detection becomes more difficult. This effect is stronger in the SH than in the NH, with a larger and colder polar vortex in the SH. This may cause an underestimation in PSC occurrence at ~30 km in the SH.*

(690-700) Interesting feature. Have the authors tried to apply their classification algorithm in non-polar, volcanically contaminated stratospheric regions and see whether there too, the algorithm recognizes presence of NAT?

So far we have not applied the algorithm for mid and tropical latitudes. The classification algorithm is guided by the modelled IR spectra where we applied temperature and trace gas profiles representative for polar winter conditions. Applications at low latitudes would need to take realistic background profiles into account and some adaptation of the classification approach would be necessary.

(701-762) The author may consider to shift the whole paragraph in a supplement, and to quote only the main result in the manuscript.

We prefer to leave this section in the main part of the manuscript. This part is highlighting potential application of the new PSC database and gives a hint to the limits of proxies used to estimate the ozone loss potential over an entire winter like $A_{PSC}$ or $V_{PSC}$. In combination with Section 4.2.4, where additional smoothing and vertical averaging show a rather good correspondence of the simple temperature based proxies with the MIPAS observations, Figure 13 is stressing the limitations such simple proxies have compared to real observations of PSC (see also change notes (756) and (806) below).

(711) ". . . overall MIPAS PSC. . ." all? Antarctic?
Changed accordingly

(795) ". . . would certainly. . ." I would use ". . . could. . ." as you don't know unless you try. . .
Changed accordingly

(803) this statement seem in conflict with an earlier one at (753). Maybe one of the two should be rephrased.

We changed the statements (753) and (803) highlighting that the smoothing and vertical integration of the quantity Amax results in a better agreement:

(756) *(c) the simple temperature-based method is not accurate enough to describe the occurrence of PSCs with respect to vertical distribution and temporal evolution over the winter.*

…

(806) *The results of the comparison between the temporally smoothed and vertically integrated maximum area of PSC coverage of MIPAS with the simple temperature based PSC proxies show that the overall winter evolution can be modelled reasonably well. Although, more detailed and less smoothed analyses for individual winter show significant differences (see Figure 13). However, a similar approach applied to the output parameters of global models could be a valuable tool to quantify the quality of PSC related processes in CCMs and GCMs.*