Reply to reviewer E. Remsberg: (reviewer #1)

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*.

Reviewer Comments:

The authors are not consistent in this regard. For example, at line 803 they say "that the overall winter evolution of PSCs can be modelled reasonably well by the simple temperature based estimates of the PSC proxies". But, earlier on line 753 they conclude that "the simple temperature-based method is not accurate enough, to describe the occurrence of PSCs". At line 413 they say that "V_{PSC}(T) is a very useful proxy for ozone destruction potential (Rex et al., 2004). . .", but they then follow at line 714 by implying that a more detailed comparison with CLaMs including a sophisticated microphysical model (Grooss et al., 2014) was necessary as presented in a parallel study by Tritscher et al. (2017).

We revised those potentially misleading and contradictory statements. We had discussed the advantages and limitations of the V_{PSC} proxy already in the original manuscript (now lines 416-424) but also added an rephrased in Section 4.2.3

(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.

And at the end of Section 4.2.4:

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 winters of A_{TYPE} 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.

So how should a modeler proceed? Does one need to employ GCM output or assimilate data on temperature, nitric acid, water vapor, and winds (at a minimum) to determine threshold conditions for PSC type, and then use that information to calculate ozone loss potential for future years? Are you recommending that the models include a parameterization for PSC occurrences and types and that one should validate its calculated PSC distributions against the MIPAS findings for a specific year and winter before going ahead with multi-year predictions of ozone loss? Or is it still to be determined how best to employ this climatology for such studies?

The best approach to compare our PSC climatology with mode results still needs to be determined. It is also not clear, if a single approach serves all needs. The approach depends strongly on the model to be validated, i.e. how detailed the model treats the PSC processes and which parameters are to be 'validated' (e.g. overall column ozone depletion, ozone trends, PSC coverage or volume). For more sophisticated PSC schemes in CCMs, which are taking the formation of PSC types into account, it is meaningful to compare detailed PSC type distributions. We noted, that this is done in the parallel study (Tritscher et al., 2018). In our study, we only like to show the potential of the new database to improve and validate CCMs or CTMs. Further studies should explore and develop detailed procedures on the specific scientific question to be addressed.

Specific comments:

1) I find that the PSC class PDF distributions as a function of T-Tice ought to be most useful (Figures 3 and 4), although you do not indicate what is the related atmospheric pressure. Figure 2a of Pitts et al. (2013) shows that the threshold T for formation of STS versus NAT depends on pressure, as well.

The pressure dependence is already considered by applying $T\text{-}T_{ICE}$ instead of T in the PDF distributions. This approach is frequently used in PSC studies and is also illustrated in Figure 2b of Pitts et al. (2013).

2) Further, at line 370 you say that "a similar temperature analysis of PSC classes by Pitts et al. (2013, Fig.8 and Fig. 9) . . . shows comparable results with respect to the PDF maxima location and shift between the PSC types"

(in your Figures 3 and 4). However, I find that T-Tice for the center of the NH PDF of STS in their Fig. 8 is more like 1 K, while in your Fig. 4 it is nearer to 5 K. Those two results do not agree, in my opinion!

This is a justified objection. Both results for STS do not seem to agree very well for the maximum location. We refined this section by highlighting the discrepancy. In addition we deleted the sentence:"This is an independent endorsement in the reliability of the new MIPAS approach."

Because there is so far no explanation for this difference (see following paragraph), we avoid to present details on speculative options and just mentioned the larger difference in maximum location of the PDF between ice and the other types in Figure 4:

A similar temperature analysis of the PSC classes by Pitts et al. (2013, Fig. 8 and Fig. 9) with the CALIOP lidar data shows comparable results with respect to the PDF maxima location for ice ($T-T_{ICE}$ ~ -1 to 0 K) and a systematic shift to warmer temperatures for the other PSC classes. However, the shift for example for the CALIOP STS events is only in the range of 1-2 K, but the MIPAS analysis shows a shift of ~4 K. In addition, the CALIOP T-TICE histograms show significantly smaller PDF distribution widths than the MIPAS analysis.

A quantified comparison between the MIPAS and CALIOP $T-T_{ICE}$ PDFs is partly affected by the different meteorological datasets used for both analyses (GEOS-5 for CALIOP and ERAi for MIPAS). Although the temperature biases in the winter polar region for most meteorological datasets have substantially improved over the last decade (Lambert and Santee, 2018), there are still significant differences in mean temperature biases between various assimilation systems. Regarding this comparison, this effect has the right tendency, with a warmer bias for ERAi than for GOES-5 (~0.5 K, taken radio occultation measurements for reference, Lambert and Santee, 2018). However, the different biases are not able to explain to the full extent the difference of 2-3 K of the $T-T_{ice}$ distributions and may also effect the maximum of the ice distribution in a similar manner.
A specific restriction of the MIPAS analysis is the caveat that NAT clouds with large particles (r > 2-3 microns) are difficult to distinguish from STS. The spectral characteristic spectral signature of NAT particles at 820 cm^{-1} is for larger radii. As a consequence, these clouds may be misclassified as STS. This may cause a broader PDF and a shift to warmer temperatures (considering a difference in existence temperatures of $T_{NAT}-T_{STS}$ ~ 2-3 K).
Furthermore, due to a lack of coincident gas phase HNO_3 and H_2O measurements for MIPAS in PSCs, it is unfortunately not possible to quantify their effects in detail, like in Pitts et al. (2013). Finally, the missing coincident H_2O measurements may also create uncertainties in the T_{ICE} estimates.

3) I am familiar with trying to identify PSCs from a precursor dataset to that of MIPAS, i.e., from the Nimbus 7 LIMS experiment. For the LIMS profiles I was able to determine the occurrence of PSC-like emission signatures as a function of threshold and/or existence temperature, but not its composition or type (see Remsberg and Harvey, Atmos. Meas. Tech., 2016). Your Figure 4 indicates that the predominant PSC type for NH winter is STS with its temperature threshold of ~192 K. I found a similar threshold temperature for PSC occurrences from LIMS, but could not verify that it was most likely STS based on Pitts et al. (2013, Fig. 8). Information in the literature at that time about likely composition was not as precise as you imply, in my opinion, and may still not be. Some guidance on this point would be welcome.

It is the strength of the MIPAS data, due to its spectral resolution and coverage, to identify and separate characteristic spectral signatures in the IR spectra. Consequently, the classification is temperature-independent and very robust, especially for small NAT and ice particles. But there is the caveat to miss-classify large NAT particles as STS, which is highlighted frequently in the manuscript (line 267, 635, or 676). In a coincidence comparison with CALIOP (Spang et al., 2016) we found for the MIPAS STS class large contribution of the CALIOP mix-types classes (STS+NAT). Only around 20% of the CALIOP coincidences were classified as 'pure' STS events. PSCs detected by MIPAS and classified with STS might be mixtures of STS and large NAT particles.

References:

Pitts, M. C., Poole, L. R., Lambert, A., and Thomason, L. W.: An assessment of CALIOP polar stratospheric cloud composition classification, Atmos. Chem. Phys., 13, 2975-2988, https://doi.org/10.5194/acp-13-2975-2013, 2013.

Spang, R., Hoffmann, L., Höpfner, M., Griessbach, S., Müller, R., Pitts, M. C., Orr, A. M. W., and Riese, M.: A multi-wavelength classification method for polar stratospheric cloud types using infrared limb spectra, Atmos. Meas. Tech., 9, 3619-3639, doi:10.5194/amt-9-3619-2016, 2016.