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Interactive comment on "A climatology of polar stratospheric cloud composition between 2002 and 2012 based on MIPAS/Envisat observations" *by* Reinhold Spang et al.

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General comments: This manuscript is a comprehensive summary of findings from ten years of MIPAS data on PSC area and composition and for separate northern and southern winters. I commend the authors on their choice of figures and for their concluding remarks about each one throughout the text. The channel placement on MIPAS is, perhaps fortuitously, well suited for teasing out composition and/or PSC type. It is very likely that this will be the only satellite climatology on PSC composition for years to come. They also point to earlier publications that give more details about their methods for identifying PSC type. It would be helpful to have an additional paragraph

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Discussion paper

and references about uncertainties for this climatology.

After reading the excellent introduction section of the manuscript, I was hoping for some guidance in Section 5 on how to use the present climatology for predictions of future ozone loss from chemistry-climate models. 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 “VPSC(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).

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?

Specific comments: 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. 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!

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.

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