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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of polar stratospheric clouds in the global chemistry-climate model SOCOLv3.1 by comparison with CALIPSO spaceborne lidar measurements" *by* Michael Steiner et al.

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Reply to the comment of Referee 2

We would like to thank the referee for taking the time to read our manuscript and for the helpful feedback. We have taken these comments into account and present our responses below, with reviewer comments in blue and author responses in black. The major changes to our manuscript, as suggested by the reviewer, are:

- Additional model simulations and analyses for the Antarctic winters 2006 and

2010, which represent years with above- and below-average PSC occurrence.

- Additional sensitivity simulations to investigate the impact of the model's cold bias on PSC formation: temperature for PSC formation increased by 3K.
- Extended discussion on further influencing factors for PSC formation like model resolution or temperature biases as well as of previous studies.

Steiner et al. present an evaluation of PSCs simulated with the CCM SOCOL by comparing these to the backscatter observations derived with CALIOP onboard CALIPSO. The comparison is performed for the Antarctic winter 2007 and simulation performance has been tested by using different microphysical properties to optimize the set-up for the PSC scheme.

This is a quite interesting study and could definitely be useful for the modelling community. However, I am a bit disappointed with the outcome of this study. At the end, with none of the used set-up a really good agreement with observations is found so that not really a recommendation for the modelling community can be given. On top of that the impact on the results for ozone (which was used as a motivator for this study) is not there at all. After major revisions the manuscript may be suitable for publication. Detailed comments for improvement are provided below.

In this study we performed the first in-depth evaluation of PSC occurrence and composition in the SOCOL model. We agree that the comparison with the satellite data shows that the agreement is not perfect, but we believe that the modifications in the microphysical parameters have indeed resulted in a significantly improved agreement with the CALIPSO observations. The fact that polar ozone showed little response to the modifications in the PSC scheme was also a surprise to us. From the simulations with enhanced temperatures for PSC formation we saw a later onset of the PSC-season and a reduced PSC area, both of which is in better agreement with CALIOP observations. Consequently, the onset of O3 depletion is also delayed by slightly less than one month, however still earlier than in the MLS observations. This shows that further improvements of other parts of the model are necessary to reduce the disagreement with between modeled and observed ozone.

General comments:

Why has the Antarctic winter 2007 been chosen? Is this winter representative for Antarctic winters? Why is only one winter analysed on not several? From the simulations and observations more years should be available.

We have chosen 2007 since is a typical Antarctic year, with a steady vortex and PSCs from May to September. Furthermore, CALIOP data coverage was high and there was no impact of volcanic eruptions. However, it is absolutely true that, based on available observational data and low computational costs, more winters can be analyzed. In response to the reviewer's criticism, we additionally analyzed the years 2006 and 2010 (with above-average and below-average PSC occurrences, respectively). The analysis of these two additional winters showed very similar results as of 2007 in all comparisons (geographic PSC-distribution, areal coverage, histograms, MLS-comparisons). The main results are the same as for 2007 and the analysis of these two additional winters has not lead to different conclusions. For this reason, we still show the results for the year 2007 in the result-section. However, all plots for the years 2006 and 2010 are included in the appendix of the paper.

The SOCOL data is modified so that it mimics what CALIPSO is measuring. However, since SOCOL has the much coarser resolution wouldn't it then better to try modify the CALIPSO data so that it rather mimics the SOCOL coarse resolution and therefore what SOCOL is simulating?

This is an important point. In the analysis of the spatial distribution, where we show the polar stereographic plots, this averaging of the CALIPSO measurements over the SOCOL grid boxes is already applied. Within this revision, we also apply a similar method for the areal coverage calculations. By doing so, we intend to show by how much the

area calculation from the SOCOL grid boxes contributes to a larger area compared to the method applied by Pitts et al. (2018), especially as we already mentioned in the text that this difference may most likely cause some of the overestimation. Our goal is to calculate the area from the measurements and from the simulations as similar as possible. Therefore, we average the measurements over the SOCOL grid boxes over 12 hours (the output frequency of our simulations). Since not all grid boxes are overpassed within 12 hours, we set the PSC area in the remaining grid boxes to the mean PSC area in the overpassed grid boxes along the same latitude. The areal coverage calculated with this approach is larger than calculated by the method applied in Pitts et al. (2018), which is what we expected. We show this additional Subplot in Fig. 5. However, the simulated PSC area is still larger, which is due to the cold bias of the model. The PSC area from a sensitivity run with increased PSC formation temperature is also included in Fig. 5 to show the effect of the temperature bias on PSC area.

The results should be put in the context of results derived from other studies for discussing and understanding the differences between simulation and observation (e.g. Khosrawi et al. 2018 for the Arctic and the papers by Orr et al. 2015 for the Antarctic). Also the efforts done by the WACCM community to improve the PSC scheme could be helpful for the discussion (Wegner et al. 2013; Brakebusch et al. 2013).

We agree that so far our manuscript was mainly focused on the presentation of our own results. We have now extended our discussion section substantially and discuss several of the mentioned papers. However, it should be mentioned that all these models and their PSC scheme are (slightly) different or the studies focused on different winters/hemispheres. So a one-by-one comparison with our study is not always possible.

The underestimation of denitrification seems to be a general problem in GCMs. This underestimation was also found in Khosrawi et al. 2017 and 2018 comparing EMAC with MLS and still remained even with a higher resolution. In general you blame to often the coarse resolution, but forgot to consider that also deficiencies in the model physic play a role as the representation of dynamics (e.g. descend) and the interplay of the chemistry.

As mentioned above we have extended our discussion section substantially. We now compare our studies with previous papers and discuss the potential impact of various model deficiencies. We fully agree with the reviewer. The studies by Khosrawi et al. provide very important insights also for our analysis, especially since the EMAC model and the SOCOL model are based on the same general circulation model, namely MA-ECHAM5. EMAC was found to suffer from an underestimation of downward transport inside the polar vortex, and Khosrawi et al. (2017) suggested this as likely reason for the underestimated polar vortex O3. We now compare our studies with previous papers and discuss the potential impact of various model deficiencies. See also below.

Specific comments:

P1, L2: The process of denitrification (namely sedimentation of PSC particles and thus HNO3 removal from this atmospheric layer) should be quickly explained (as has been done in the introduction).

Done.

P1, L18: Which resolution has been used? Add here T42L39.

Yes, T42L39 has been used. Information has been added.

P2, L31: I think there are even newer references. There is at least the paper by Nakajima et al. (2016).

We added the reference Nakajima et al. (2016).

P2, L40: Is there really no newer version of the PSC scheme? Please check.

To our knowledge there is no newer version. The paper by Nakajima et al. (2016) explicitly states that their results confirm the possibility of an ice-free nucleation mechanism of NAT involving solid particles as suggested by Hoyle et al. Furthermore, Nakajima et al. do not take sedimentation into account. However, we will add the citation.

P4, L 99: With satellite observations from which satellite? MLS? Please add this information.

Yes, from MLS observations. Information will be added.

P4, L108: Why is the hydrolysis of N2O5 important? This should be explained.

In general, the heterogeneous hydrolysis of N2O5 is an important and efficient loss process for NOx as it forms HNO3. The respective reaction in the gas phase is comparatively slow. The heterogeneous reaction is important in the troposphere in aerosol particles and cloud droplets, but also in the stratosphere on binary aerosol and PSC particles. The sentence in our manuscript explicitly refers to the N2O5 hydrolysis on tropospheric aerosols, and was mainly added for the sake of completeness. For the present study, it is not of importance. Therefore, we removed the sentence.

P4, L113: PSC types. . .. . -> this is a repetition. This has already mentioned in the previous paragraph.

Sentence will be deleted.

P4, L128: ". . ..but at the end of each chemical time step all condensed HNO3 and H2O evaporates back to the gas phase". What do you mean with that? This is not realistic at all.

This means that the NAT and water ice particles are not explicitly transported by the model's advection scheme. This is a common approach in CCMs. At the end of the chemistry routine, the condensed HNO3 and H2O is re-evaporated and the transport occurs via the gas-phase. At the next call of the chemistry scheme, NAT is freshly formed if the partial pressure of HNO3 exceeds supersaturation. The same holds for water ice and the partial pressure of H2O. This procedure goes back to times when tracer transport was computationally expensive, with the goal to keep the number of prognostic tracers small. Furthermore, it prevents numerical diffusion as PSC clouds are regionally limited and show strong gradients. We will rephrase the sentence for clarification.

P5, L134: Using 39 vertical levels is really coarse and what is the motivation for doing this. Several studies show that much better results are derived with a higher resolution and computer resources nowadays allow doing such simulations. Especially since you only consider one winter you could have easily done the simulations with a much better resolution. Especially, the coarse vertical resolution is a drawback. Why have you not used 90 levels? Using 90 levels significantly improves the results in the stratosphere.

Whether L90 leads to improvements compared to L39 clearly depends on the quantity. We do not see large differences in the simulated Brewer-Dobson-Circulation (w*) between L39 and L90. Furthermore, for the present study we used SOCOL in specified dynamics mode, and in SD mode there are no large differences in stratospheric dynamics between L39 and L90. The cold bias in the polar lowermost stratosphere is very similar in both vertical resolutions. Therefore, we do not expect large differences in the simulated PSCs between L39 and L90. This is also supported by the study of Khosrawi et al. (2017), who applied the EMAC model in L90, in T42 and in a much higher horizontal resolution of T106. Both model versions showed very similar differences to observations. This shows that a higher resolution is not necessarily the remedy for all model deficiencies.

Khosrawi, F., Kirner, O., Sinnhuber, B.-M., Johansson, S., Höpfner, M., Santee, M. L., Froidevaux, L., Ungermann, J., Ruhnke, R., Woiwode,W., Oelhaf, H., and Braesicke, P.: Denitrification, dehydration and ozone loss during the 2015/2016 Arctic winter, Atmospheric Chemistry and Physics, 17, 12 893–12 910, https://doi.org/10.5194/acp-17-12893-2017, https://acp.copernicus.org/articles/17/12893/2017/, 2017.

P9, L225: Much is attributed to the coarse resolution. However, why has such a coarse resolution been used? Why has not one of the used set-up been used for a simulation with a higher resolution to check what impact this would have?

See above. Furthermore, a change to higher horizontal resolutions would require a

complete re-tuning of the model, which is out of the question, also because we are currently working on a new model generation, which will apply T63 as default horizontal resolution. However, even with T63 we will not fully resolve mountain waves.

P9, 234: PSC formation depends strongly on temperature. How well is temperature simulated in SOCOL?

As the majority of chemistry-climate models, SOCOL experiences a cold temperature bias in the polar stratosphere. A comparison with ERA-Interim on four different pressure levels is shown in the Figure below (Fig. 1 of this reply). Mostly, the temperature difference is between 2 and 4 K. To address your question and investigate the impact of the cold bias on PSC formation in the model, we ran two further sensitivity analyses during this revision. In both simulations, temperature for PSC formation was increased by 3K, once for the reference simulation and once for the $S_{n(ice),n(NAT,max)}$ simulation. A discussion of the former simulation in terms of areal coverage and of latter simulation regarding the MLS-comparison has been added to the manuscript. The simulation is denoted as $S_{T,n(ice),n(NAT,max)}$. With the increased PSC formation temperature, PSCs occur later and their area clearly decreased, as expected. The area of this new simulation agrees very well with the observed PSC area (with a similar method as for the simulation; see above). Figure 5 has been extended with these new results. The simulations with increased PSC formation temperature further show a later onset of denitrification and ozone depletion, both of which also is expected with PSC occurring later. However, towards the end of winter, the difference in HNO3 and O3 between the simulations with and without increased PSC formation temperature vanish. Further, the new simulation show almost no more dehydration since ice rarely forms with the formation temperature increased by 3K. The Figures 7-9 now include the new simulation. It is important to highlight, that we didn't increase the temperature of the model itself, but just for the PSC formation (i.e. the tropical tropopause temperature and the related dehydration remain unchanged).

P12, Table 1: How are these values motivated? Have these been derived from the

CALIPSO measurements or are these based on what is used in the literature (based on other observations or other experience with model simulations)?

The default setting for the microphysical parameters has been adopted from the previous model version SOCOL 2 (Schraner et al., 2008). The parameter settings for the sensitivity simulations have been defined based on the evaluation of the reference simulation with CALIPSO measurements and a stepwise modification of n_{ice}, $n_{NAT,max}$ and r_{NAT}. We did many more simulations than presented in the paper, modified one parameter after the other and analyzed the impact of the respective parameter on the model result. It is clear that in reality PSCs are very heterogeneous in space and time, while a model like SOCOL has a coarse resolution, therefore, the "optimal" parameter setting is always a compromise and requires some testing and tuning. Furthermore, the "optimal" parameter setting will most likely depend on the model resolution or might change in future model versions with different dynamics, treatment of binary aerosol etc.

Schraner, et al., Technical Note: Chemistry-climate model SOCOL: version 2.0 with improved transport and chemistry/microphysics schemes, Atmos. Chem. Phys., 8, 5957–5974, https://doi.org/10.5194/acp-8-5957-2008, 2008.

P12, L274: Add references. At least there is a publication by Grooss et al. where a certain value for the ice number density has been used.

For example, Nakajima et al. (2016) also used 0.01 cm-3. Tritscher et al. (2019) used 10 cm-3 for homogeneous ice nucleation under mountain wave conditions. For heterogeneous ice nucleation they use a look-up-table (their Fig. 1) as done in Grooß et al. (2014) for NAT nucleation.

Grooß, J.-U., Engel, I., Borrmann, S., Frey, W., Günther, G., Hoyle,C. R., Kivi, R., Luo, B. P., Molleker, S., Peter, T., Pitts, M.C., Schlager, H., Stiller, G., Vömel, H., Walker, K. A., andMüller, R.: Nitric acid trihydrate nucleation and denitrificationin the Arctic stratosphere, Atmos. Chem. Phys., 14, 1055–1073,https://doi.org/10.5194/acp-14-

1055-2014, 2014.

Tritscher, I., Grooß, J.-U., Spang, R., Pitts, M. C., Poole, L. R., Müller, R., and Riese, M.: Lagrangian simulation of ice particles and resulting dehydration in the polar winter stratosphere, Atmos. Chem. Phys., 19, 543–563, https://doi.org/10.5194/acp-19-543-2019, 2019.

P13, L312: I do not really understand how this is done. How do you account for the heterogeneity of the MLS data by using area-weighted concentrations for SOCOL? How does that mimic the MLS measurements? Why not using the averaging kernels of MLS or just using the SOCOL output at the locations of the MLS measurements (thus along the satellite orbits)?

We average the MLS measurements over each SOCOL grid box, so that it makes the measurements comparable with SOCOL. To calculate the "polar mean" concentrations, the averaged MLS-values as well as the SOCOL concentrations are area-weighted to take into account the different areas of the grid boxes. We changed the sentence to: "To account for the spatial heterogeneity of the MLS measurements, we averaged the measurements over the SOCOL grid boxes from which area-weighted polar mean concentrations are calculated."

P16, L354 and 358: As stated in my general comments. The differences in agreement are partly caused by the coarse resolution. There are many other factors playing a role as well.

Agreed, and now considered in our discussion. See also next point.

P17, L369: But what is then the usefulness of this study? What would you recommend the modelers to do to improve their simulation results?

First, the main goal of the study was to evaluate the PSCs in the SOCOL model, which has never been done before to such an extent. As mentioned above, the fact that O3 did not react very much to the PSC modifications was also a surprise to us and suggests that other processes than heterogeneous chemistry play an important role for O3 in the polar stratosphere during winter/spring. As pointed out by Khosrawi et al. (2017) model deficiencies in downward transport inside the polar vortex are a promising candidate. As EMAC and SOCOL are both based on MA-ECHAM5 as underlying general circulation model, this might also hold for SOCOL. We mention this now in our discussion. Second, all models are different. It is difficult to come up with a general suggestion for all modelers. Each has to be evaluated individually. We make this point when we put our results in context with other studies.

P17, L374: This is nothing new. This has also presented in other studies (e.g. Orr et al., Wegner et al., Brakebusch et al., Khosrawi et al .)

Agreed. We rephrased this sentence, pointing out that such simplified PSC schemes are widely used in the CCM community, however, with a wide range of assumptions concerning the microphysical parameters.

P1, L3: concentrations -> occurrences

Here we refer to polar ozone, not PSCs, so we think “concentrations” is correct.

P2, L41: PSCs are observed -> I that context I would rather write PSCs can be observed

Sentence has been changed.

P3, L84: The acronym MIPAS has not been introduced.

Acronym is now introduced.

P5, L140: 01 -> 1 (that should be changed in the text throughout the manuscript)

Corrected.

P6, L183-184: repetition of vertical resolution

Corrected.

P8, L213: 01 -> 1

Corrected.

P11, L250: 77.4-90âŮę -> 77-90âŮę

We decided to stick to the notation of Pitts et al. (. . . .), which is 77.8-90. Please note that the 77.4 has to read 77.8. This was a typo, which has been corrected.

Figure A1 caption: 1st -> 1

Corrected.

Figure 2-9, A1: The resolution of the figures is not good enough. On my printed version the plot frames are missing in several occasions.

Thanks for this hint. We did not experience any problems with the figures, but we will clarify this issue with the GMD production office.

Figure 4: Swap the upper panels with the lower ones, so that the order is July, August. Why do you use SREF as figure title? Why not using “SOCOL” as figure title?

We used S_{REF} since the reference simulation (and not one of the sensitivity-runs) is shown. But it is absolutely correct, in the paper the Figure is shown before those sensitivity-simulations are introduced, so we changed the title. We further noticed that the plot were actually in the correct order (upper panel: July, lower panel: August), but the labels were swapped. This has been corrected. Thanks for pointing this out.

Figure 5: Here I would suggest to change the figure titles as follows: “CALIPSO”, “SOCOL with thresholds”, “SOCOL without thresholds”.

Done.

Figures 7 and 8: Observations and model simulations are difficult to distinguish. I would suggest to use a thicker line for the observations and maybe a different color.

We revised Figures 7-9 and changed the line thickness and colors.

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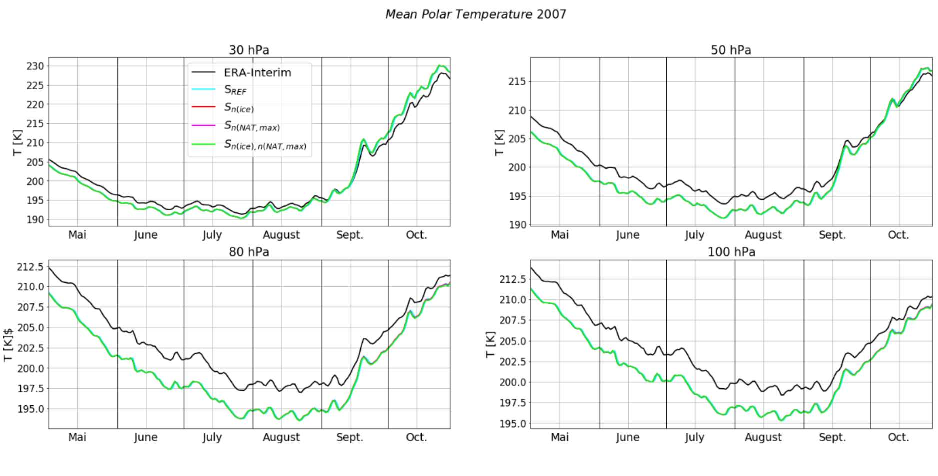

Fig. 1.