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Geoscientific Model Development Discussions

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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.

Anonymous Referee #2

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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.

General comments:

Why has the Anatarctic 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. 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?

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).

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.

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).

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

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

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

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

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

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

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.

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

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have you not used 90 leveles? Using 90 levels significantly improves the results in the stratosphere.

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?

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

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)?

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.

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)?

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.

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

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 .)

P1, L3: concentrations -> occurrences

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

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

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

P6, L183-184: repetition of vertical resolution

P8, L213: 01 -> 1

P11, L250: 77.4-90° -> 77-90°

Figure A1 caption: 1st -> 1

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.

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?

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

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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.

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