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Interactive comment

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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Dear Yunqian Zhu,

thank you very much for your helpful feedback. We appreciate your suggestions, which helped to improve our manuscript a lot. We present our point-by-point reply below, with your comments in blue and our responses in black.

Best regards, Michael Steiner

Major comments:

I'm not convinced by your reasonable denitrification and ozone simulation right now (abstract Line 20). I think adding error bars (MLS accuracy) to the observation on your Figures 7, 8, and 9 may help to see whether the modeled HNO3, H2O, and O3 are reasonable. Right now, the onset of O3 depletion seems much earlier than the observation. This is important since the onset date of O3 depletion is one of the important indicators for ozone recovery (Solomon et al., 2016). Is your early O3 depletion caused by your early PSC formation that provides more SAD (Line 259)? I think your cold bias in the model may also contribute to both early PSC formation and early O3 depletion. If your model is not consistent with the O3 and other species after you add the error bars, you may want to emphasize your conclusion on "The change of NAT scheme has minimum impact on O3 depletion." And this conclusion is supported by previous studies like Tabazadeh et al.2000 which find the denitrification impact on Arctic ozone depletion but not much on the Antarctic one.

We added the MLS error bars to the Figures 7-9. While at the beginning of winter, O3 agrees well with observations, the difference between simulations and observations for HNO3 and H2O are large and also larger than the MLS uncertainty (upper panel). However, when looking at the relative evolution of the species (lower panel of the figures), the amplitude of denitrification and dehydration is in good agreement with observations, depending on the simulation. This is different for O3, where depletion in the model occurs too early and too strong, as you correctly stated. To test the hypothesis that the early onset of O3 depletion in the model is caused by early PSC formation, we ran sensitivity simulations with the temperature in the PSC routine constantly increased by 3K, which is roughly the temperature bias in the lower stratosphere. We indeed see a later onset of the PSC-season and a reduced PSC area, both of which is in better agreement with CALIOP-observations. As a consequence, the onset of O3 depletion is indeed delayed by slightly less than 1 month. However, this is still earlier than observed by MLS. Towards the end of the winter, the differences between the simulations vanish. The relatively small impact of the PSC scheme on the underestimation of O3 likely is a result of an underestimated downward transport inside the polar vortex. This is also discussed in the answers to the referee comment 2. We added this point to our discussion and included the fact, that modifications in the NAT scheme have minimal impact on O3, to our conclusions.

You explain your mismatching of PSCs to CALIPSO is due to the wave PSCs (e.g. Line 240, Line 245). I think the explanation is not enough. Why does the mountain wave cause higher R532? It is not just because of the wave-ice cloud, since wave ice clouds are a very small portion of PSCs. The large R532 is likely to be enhanced-mix or ice clouds in CALIPSO observation. It is probably because you exclude NAT particles with higher number densities. These NAT particles are generated from ice or wave-ice cloud downwind the Antarctic Peninsula. Many observations saw or retrieved small NAT particles (~2 um) with large number densities, as well as the model simulations (see Zhu et al., 2017, and references therein, note that this is not the same paper you cited in this manuscript). Zhu, Y., Toon, O. B., Lambert, A., Kinnison, D. E., Bardeen, C., Pitts, M. C. (2017). Development of a polar stratospheric cloud model within the Community Earth System Model: Assessment of 2010 Antarctic winter. Journal of Geophysical Research: Atmospheres, 122, 10,418– 10,438. https://doi.org/10.1002/2017JD027003 Line 295, I suggest you ran a test case with increased Sn(NAT, max) but decrease the NAT size.

Thank you very much for pointing this out. We agree with you that the upper limit for NAT number densities may contribute to the underestimation of the large R532 observations, and we added this point to the discussion of Fig. 4 and Fig. 6. However, we did not perform a further simulation with enhanced N(NAT,max) and smaller r(NAT). Even with such modified NAT parameters we would need some representation of the mountain waves in the model to reproduce these peaks. The observed clouds with enhanced-mix and ice downwind of the Peninsula form on ice which forms due to the mountain waves, and since mountain waves are almost not present in the simulation

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we would not see a peak downwind of the Peninsula. We would just shift the R532-values of all grid-boxes containing NAT particles. Furthermore, enhancing N(NAT,max) would put the reasonable agreement with the thin NAT-STS mixtures at stake.

Minor comments:

In the abstract, you mentioned meteoric dust as a possibility for PSC formation, but you haven't talked about it at all in the main content. Maybe put something in the discussion session.

We removed the meteoric dust from the abstract. Meteoric dust is not included in our model, and we think that a further discussion is beyond the scope of the paper.

Line 64, please cite Wegners et al., 2012 for PSC parameterization in WACCM

Reference added.

Line 65, I think Bardeen 2013 is not relevant.

We removed the citation.

Line 65 and 78-82, This is not the newest publication from Zhu et al. Please cite: Zhu, Y., Toon, O. B., Lambert, A., Kinnison, D. E., Bardeen, C., Pitts, M. C. (2017). Development of a polar stratospheric cloud model within the Community Earth System Model: Assessment of 2010 Antarctic winter. Journal of Geophysical Research: Atmospheres, 122, 10,418– 10,438, https://doi.org/10.1002/2017JD027003. In this paper, we improved the model with ice to NAT nucleation. And the model is able to capture the small NAT particle features and compare pretty well with CALIPSO backscatter.

Thank you for the hint. We updated the reference and extended the description of the results for WACCM/CARMA.

Line 94, the equilibrium scheme is only for STS, but not for NAT and ice. Please rephrase here.

This is not quite correct. SOCOL does indeed assume equilibrium for STS, NAT and ice. Only if the NAT number density exceeds a certain threshold value, NAT deviates from equilibrium conditions. Since the details of the PSC parameterizations are explained in Sect. 2.1, we decided to leave the respective sentence as is.

Line 119, could you provide a citation for "observational evidence"?

Observational evidence is for example given by the CALIPSO measurements, which show that NAT and STS occur at the same time (Pitts et al., 2011), which would not be possible without HNO3 supersaturation with respect to NAT. Further evidence is shown in the in situ study by Fahey et al. (2001): Fig. 5 D shows that the HNO3 supersaturation ratio with respect to NAT (Snat) was around 15 at the time the NAT particles where sampled (time = 0).

Line 140, instead of "average year", you may say that 2007 is a typical Antarctic year with a steady vortex and observed PSCs from May to September. It is a year without the impact of volcanic eruptions. I think it would ask one question from another referee.

Thanks for the suggestion. We included this statement. Furthermore, we extended our analysis by the years 2006 and 2010, which represent years with above- and below-average PSC occurrence, but also without volcanic influence.

Line 194, please list citations for these refractive index numbers.

1.31 is simply the refractive index for water ice. For NAT, we added the following reference:

Middlebrook, A. M., Berland, B. S., George, S. M., Tolbert, M. A., and Toon, O. B.: Real refractive-indexes of infrared-characterized nitric-acid ice films – implications for optical measurements of polar stratospheric clouds, J. Geophys. Res., 99, 25655–25666, doi:10.1029/94JD02391, 1994.

Line 213, I think you mean "Figure 3d"

Corrected.

Figure 3a-c: these three figures have very different color bars. I cannot tell if you have a good comparison with CALIPSO or not. Instead of R532, you may use 1/R532 so you don't have to compensate your color bar due to the wave-ice cloud. It's up to you.

We adjusted the figure and use now 1/R532 as suggested.

Line 224 and 283, It is not due to "orography". It is due to the "lack of orographic gravity representation in the model".

Corrected.

Line 257, Is your CALIPSO figure identical with Pitts 2018? if so, Pitts 2018 says 77.8 rather than 77.4.

Thanks for spotting this mistake, we fixed it.

Line 265, "contributes to the larger PSC area and longer period".

We adjusted the text accordingly.

Line 267-269, This sentence is not logical enough. Even you filtered it when you comparing to CALIPSO observation, you still count them into the SAD in your model, right? You may want to say "these STS clouds contribute to negligible SAD to the ozone chemistry in the model" if this statement is true.

You are absolutely right, the SAD of all PSC particles is counted in the model. With this sentence we wanted to state that the fact, that these large-scale STS clouds are almost entirely filtered out by the thresholds, shows that these clouds must be teneous. We rephrased this sentence and tried to make the statement more clear:

"Those large-scale STS clouds are very tenuous since they are filtered out when applying the conservative PSC detection threshold. The contribution of those STS clouds to SAD is negligible."

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Line 292, need a citation here for ice PSCs are less important for stratospheric ozone chemistry

We rephrased this statement and added a citation: "NAT PSCs play a twofold role in stratospheric ozone chemistry: Besides halogen activation on their surfaces, the sedimentation of NAT particles leads to denitrification, which hinders deactivation of reactive halogens and facilitates catalytic ozone loss (Peter, 1997)."

Line 320, "underestimates the HNO3 compared to MLS"

We adjusted the text accordingly.

References:

Fahey, D. W., Gao, R. S., Carslaw, K. S., Kettleborough, J., Popp, P. J., Northway, M. J., Holecek, J. C., Ciciora, S. C., McLaugh-lin, R. J., Thompson, T. L., Winkler, R. H., Baumgardner, D. G., Gandrud, B., Wennberg, P. O., Dhaniyala, S., McKinney, K., Peter, T., Salawitch, R. J., Bui, T. P., Elkins, J. W., Webster, C. R., Atlas, E. L., Jost, H., Wilson, J. C., Herman, R. L., Kleinböhl, A., and von König, M.: The detection of large HNO3-containing particles in the winter arctic stratosphere, Science, 291, 1026–1031, https://doi.org/10.1126/science.1057265, 2001.

Peter, T., Microphysics and heterogeneous chemistry of polar stratospheric clouds, Ann. Rev. Phys. Chem., 48, 785–822, 1997.

Pitts, M. C., Poole, L. R., Dörnbrack, A., and Thomason, L.W.: The 2009–2010 Arctic polar stratospheric cloud season: a CALIPSO perspective, Atmos. Chem. Phys., 11, 2161–2177, https://doi.org/10.5194/acp-11-2161-2011, 2011.

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