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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.

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Hello,

It is really exciting to see someone working on PSCs as I do. Sorry to put extra work on you but I hope my comments can help you prepare your paper better.

I mainly have two major comments on the manuscript.

1. I'm not convinced by your reasonable denitrification and ozone simulation right now

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Discussion paper

(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.

2. 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 ($\sim$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.

And Some small comments, mainly need more citations:

1. 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.

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

3. Line 65, I think Bardeen 2013 is not relevant.

4. 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.

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

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

7. 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.

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

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

10. 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.

11. Line 224 and 283, It is not due to "orography". It is due to the "lack of orographic

gravity representation in the model".

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

12. Line 265, “contributes to the larger PSC area and longer period”.

13. 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.

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

15. Line 320, “underestimates the HNO3 compared to MLS”

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