

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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Author response to comments of Referee 1

We thank the referee for taking the time to read the manuscript and for the helpful feedback. Although we think that many most of the points raised are already described in the manuscript, it became clear that some clarification is necessary. In response to Referee 1, along with other revisions in response to Referee 2, we extended the description of our PSC scheme (Sect. 2.1) and of our evaluation approach (Sect. 2.4).

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We hope that our answers and revisions help to clarify the description. We present our responses below, with reviewer comments in blue and author responses in black.

The authors present an interesting study of comparison of the model outputs of the SOCOL model with observations by the satellite-borne lidar CALIOP. The approach is to test if a CCM without a detailed microphysical model for the formation of PSCs can be used to calculate PSCs in the polar regions. The advantage of such an approach is the reduced time for calculations wrt more sophisticated models including microphysical schemes. To demonstrate the merits and deficits of such an approach the model output is processed to obtain optical parameters which allow PSC classification similar to that used by CALIOP. The authors compare the optical constants measured by CALIOP with those obtained from the SOCOL model. How are these optical parameters obtained? The authors state “From the simulated SADs and the assumed microphysical parameters, we calculate the number density and/or radius for each particle type.”.

The idea of our study is to evaluate the PSCs simulated by SOCOLv3.1 with the help of backscatter measurements by CALIOP onboard the CALIPSO satellite. For that purpose, we converted the simulated PSCs quantities, namely the SAD of STS, NAT and ice, into a size distribution and calculated the optical signal CALIPSO would measure. This is described in Sect. 2.4.

The general procedure is the following: In SOCOL, NAT and water ice are calculated as soon as the partial pressures of HNO₃ and water vapor, respectively, exceed supersaturation. From the excess HNO₃ and H₂O, the surface area density of NAT and water ice is calculated. Herein we assume for NAT a fixed radius and a maximum number density. The latter assumption prevents that all excess HNO₃ goes into NAT particles at the expense of STS formation. This accounts for the fact that NAT and STS clouds are mostly observed simultaneously (e.g. Pitts et al., 2011). Conversely, for water ice we assume a fixed number density and calculate the radius from the total ice volume.

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The rationale behind the different treatment of NAT and water ice in the model is the following: For water ice, the time to reach equilibrium between the gas- and particulate phase is very short. That means that, once ice has formed, the ice number density stays quite constant and further cooling leads rather to particle growth than to additional nucleation. In the case of NAT, however, the equilibrium between the gas and particulate phase is hardly reached, as shown by observations (Fahey et al., 2001), and additional particles can nucleate upon further cooling. Therefore, we do not fix the NAT number density, but the radius, which has been optimized to match observed sedimentation/denitrification. We are aware that this bulk parameterization is a simplification of the real world, but it helps keeping computational effort low. To various extents, this is done in most CCMs. Thus, the strongest point of the present analysis is the comparison with the state-of-the-art satellite data.

The basis for STS droplets are binary H₂O-H₂SO₄ aerosol particles, whose distribution is prescribed from a monthly mean observational data record, mainly based on SAGE-observations. The data record provides SAD, volume density, mean radius and H₂SO₄ mass of the binary aerosol. STS droplets form in the model when gas-phase HNO₃ and H₂O is taken up by the binary aerosols, following the expression by Carslaw et al. (1995). The uptake of HNO₃ and H₂O leads to a change in aerosol mass, from which a growth factor of the binary aerosol and therefore the radius of the ternary aerosol can be calculated.

For all three PSC type we outputted the individual surface area density for each model grid point. For the present study we used a 12 hourly output frequency.

To mimic the satellite measurements we proceeded as follows: From the outputted SADs of the three PSC types and the above mentioned assumptions on NAT radius and water ice number density as well as with the STS-radius resulting from the above mentioned growth-factor for ternary aerosols, we calculate the missing parameter, i.e. the number density or radius. These quantities are then used in Mie and T-matrix scattering codes (Mishchenko et al., 1996) to compute optical parameters of the simulated PSCs,

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i.e. R532, $\delta_{aerosol}$ and β_{\perp} . As shapes we assume ellipsoids with an aspect ratio of 0.9 (diameter-to-length ratio) for NAT and ice. STS are liquid particles and therefore assumed to be spherical with a depolarization ratio $\delta_{aerosol} = 0$.

They also state that the radius of the NAT and STS particles is fixed (5 micron for NAT but we don't know for STS), and that ice has a variable radius, but we don't know how this is obtained. ("The variable radius of ice particles results in a variable $\delta_{aerosol}$ value."). Since the conversion of SAD to particle size distribution and number density has an important impact on the results, the authors should dedicate a paragraph on how this is done.

As mentioned above only the radius of the NAT particles is fixed, not the radius of STS-particles. We noticed that our formulation in the manuscript was indeed very misleading and rephrased the sentence "STS and NAT, due to their spherical shape and fixed radius, appear at constant $\delta_{aerosol}$ -values of 0 and 0.167, respectively." to "STS (due to their spherical shape) and NAT (due to the assumed fixed radius) appear at constant $\delta_{aerosol}$ -values of 0 and 0.167, respectively."

Why don't they use a size distribution for all particles, instead of applying observational uncertainties to the results of the Mie calculations? This is of course an artificial way to obtain some scattering of the results but it is not equivalent to using a size distribution. Also by fixing the radius for NAT, the sedimentation velocity is the same for all NAT particles, while for a size distribution the sedimentation velocity would be also a distribution. . . . So to my opinion, the inclusion of a size distribution for all PSC particles would give a more realistic approach and would not make the calculations much more time consuming.

Thanks for the question. The reason for adding the noise level of the satellite data to the results of the Mie and T-matrix calculations is that the satellite observations include significant uncertainty, i.e. a CALIPSO measurement of even a monodisperse PSC distribution would show a lot of scatter. To mimic these observational uncertainties we

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added the instrumental noise to our results.

Concerning the distribution of sedimentation velocities, we agree that there might be better approaches to describe the size distribution of PSC particles. However, the purpose of this paper is not to come up with a microphysically fully consistent PSC size distribution, but to evaluate and optimize the existing parameterization. Instead of fixing the NAT radius, other models fix the NAT number density (e.g. Wegner et al., 2013, Nakajima et al., 2016), which results in varying NAT radius and sedimentation velocities. However, the value for NAT number density is the model dependent and acts as tuning parameter. In reality, the actual value for NAT number density is far from being constant, because the active sites for NAT nucleation themselves show a wide distribution of efficacies (Hoyle et al., 2013). Therefore, we follow a different approach and choose a NAT radius to reasonably simulate the observed sedimentation/denitrification features.

I don't understand "but at the end of each chemical time step all condensed HNO₃ and H₂O evaporates back to the gas phase."

This means that the NAT and water ice particles are not themselves prognostic variables and 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 returned to the gas phase 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, and the particles are re-established within this equilibrium scheme. The same holds for water ice and the partial pressure of H₂O. This procedure prevents numerical diffusion of within and between model grid cells of HNO₃ and H₂O, as PSC clouds are regionally limited and show strong gradients. We will rephrase the sentence for clarification.

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