



Interactive comment on “Mountain-wave induced polar stratospheric clouds and their representation in the global chemistry model ICON-ART” by Michael Weimer et al.

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Response to Referee #1

Dear Referee,

thank you for your detailed review of our manuscript. Please find our responses to your comments below.

Major changes include

ACPD

- a comprehensive revision of the introduction
- moving the description of the PSC scheme in the appendix and keeping just the important information in the main text
- restructuring of the simulation description
- correcting an error in the CALIOP comparison and accordingly a revision of Sect. 5.2
- moving the reaction mechanism from the appendix to the supplementary material

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Yours sincerely and on behalf of all co-authors,
Michael Weimer

1 Major Comments

1.1 First, your PSC parameterizations limit the number concentration to 2.3e-4 cm-3 for NAT and 0.25 cm-3 for ice. There's nothing wrong to have the assumptions since this is a PSC parameterization model, but I'm not convinced by your descriptions of the number density choices for NAT and ice. For NAT particles, the GW likely booster the NAT particle concentration and significantly increasing the surface area density of NAT. Do you have any evidence that your number concentration assumption is suitable for GW conditions? You refer to Fahey et al. 2001 for this concentration choice for all your NAT particles as small as 0.1 um. However, Fahey et al. mentioned a two-mode particle concentration. The 2.3e-4 cm-3 is only for particles larger than 10 um. And particles \sim 3 um has a larger number concentration \sim 1e-3 cm-3. I don't think it's proper to quote part of their measurements and ignore others. For ice concentration, in line 350, you said the mountain wave can increase the concentration to the order of a few cm-3 but your assumptions are smaller than that. In contrast, in line 404-405, you said "ice number concentration is set to the tropospheric value of 0.25 cm-3 which is too large in comparison to measurements". So, is 0.25 too high or too low? Could you explain it more?

Our NAT formation procedure bases on van den Broek et al. (2004). Therefore, we use the size distribution provided by van den Broek et al. (2004) who show that it leads to denitrification comparable to measurements. They also provide a quite comprehensive explanation why they are using this value. We now state this clearly and cite their paper.

For ice, the used particle number concentration of 0.25 cm-3 originates from the tropospheric hydrometeor microphysics of the model. We clarified the statement in line 405. The number (from a model tuned for global performance) is too low for regional

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1.2 The second question is about ozone chemistry (section 5.4.3 and 5.4.4). The temperature fluctuation causes a regional low temperature of 10 degrees than the average value. Have you considered the impact of the low temperature on the chemistry reaction rate? The uptake coefficients of chlorine activation reactions are very sensitive to the temperature. GW probably has an even bigger impact on chlorine activation and ozone depletion than the enhanced surface area density (SAD) provided by PSCs. If your model setting has already considered the temperature fluctuation in the chemistry module, the impact on heterogeneous chemistry is not only from PSCs SAD but also from the temperature. Line 440: "At altitudes around 26 km, the additional ice PSCs activate both ClONO₂ and HCl in the lee of the mountains." But your ice PSC locations (Figure 13c) and Cl species (Figure 13e, 13f) locations are not correlated.

Yes, the whole chemistry, including PSCs and photolysis, is called in all nests using the temperature as shown in the figures. We added statements regarding the temperature dependence to the sentences where only PSC SADs were mentioned as a source. In addition, we included in the simulation description that the chemistry is called in the nests as well.

As part of our response to referee 2, we included a description of the mountain wave event from a meteorological point of view. This includes that easterly winds over the Antarctic Peninsula lead to the mountain wave. Figure 13 c) shows that the ice PSC in the simulation with the nests reaches one kilometre higher than in the simulation without the nests. We replaced the panel by a figure with a refined colour bar. It shows better that the ice PSCs and low temperatures in the lee of the mountain reach higher in the simulation with the nests. In correspondence with the winds, chlorine is activated in the mountain wave and the activated chlorine is then transported downstream.

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1.3 Question3: Section 5.2: Could you please explain why your model forms PSCs at higher temperatures than observed (all the three figures, Figure 5, 6, 7)? Is this because your model did not denitrify or dehydrate properly before July? There are several places related to this problem: Line 327: What is “this bin” referring to. If you are referring to temperature bins 184K and 188K. I think your model is overestimated and it is not negligible. Conclusions for section 5.2 (near Line 353). In addition to “some differences in NAT at low temperatures and the “Wave-ice” category”, the simulation also has the problem that forms PSCs at high temperatures. Line 497-498: “The comparison with all CALIOP measurements within the Antarctic Peninsula nest demonstrated that the general formation of most of the PSC types in ICON-ART is similar with respect to temperature.” I cannot agree with this statement since you form the STS and NAT at these higher temperatures that are not in CALIPSO.

We found an error in our analysis to derive optical properties from the modelled PSCs. We corrected this, included updated figures and revised the manuscript accordingly.

This correction clearly shows that no PSCs are formed at temperatures higher than TNAT.

1.4 Question4: Figure 2: why do you do the free-running in the third step (i.e. July 19 – July29)? Is it because the generation of temperature fluctuation from gravity wave needs a free-running model? As you mentioned in many places in your manuscript that you cannot directly compare with the observations (like CALIPSO and AIRS) because of the free-running. Why don't you do a nudged run instead?

We opted for a simpler approach to allow the model to track largely the general meteorological development, yet leave it as free as possible to develop the waves and the corresponding composition changes. The reason is that we are interested in the im-

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pact of the higher resolution domain's feedback on the global domain, which can only be done in a free-running system. A global nudging would interfere with this aim.

2 Minor Comments

2.1 Line 44-46: Is the +-15K near the altitude of PSC formation or for higher altitude. If it is for higher altitudes, it is not related to PSC formation or ozone depletion.

The range is taken from Carslaw et al. (1998b), now cited in this context, and are related to ozone depletion.

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2.2 Line 47-49: This sentence is confusing. Are you talking about Arctic denitrification is closely connected to the heterogeneous nucleation of NAT on meteoric dust and NAT formed in mountain wave activity? How about other NAT formation pathways by previous studies, like homogeneous nucleation [Tabazadeh et al., 2002]? If NAT forms in the mountain wave, it still forms through a microphysical process like nucleation. Is it the heterogeneous nucleation of NAT on meteoric dust or it could be other processes? Please rephrase the sentence.

We removed the first part of the sentence and rephrased it.

2.3 Line 60: what's "the effect"? Are you passing the temperature fluctuation to the PSC formation module? Or you also pass other variables?

We added more information about how the two-way nesting is applied in the model in the introduction.

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2.4 Line 92-93: What are these “several nucleation processes” referring to? Do you have more than one nucleation pathway for ice formation?

We replaced this statement by “heterogeneous nucleation of cloud ice, nucleation of cloud ice due to homogeneous freezing of cloud water and depositional growth and sublimation of cloud ice”. Further information can be found in the cited technical description of Doms et al. (2011).

2.5 Line 95: Zhu et al., 2015. This approach is originally described in a WACCM paper by Wegner et al., 2013. Please cite Wegner et al. 2013 instead. Figure 1: the y axis is not particle number concentration since it has um^{-1} . Is it $dNdlnr$?

We replaced the citation. The notion “particle number concentration” originates from the distributions shown by Figure 3 of van den Broek et al. (2004). We added $\Delta N/\Delta r$ to Fig. 1.

2.6 Line 304: how about the refractive indices of STS?

The refractive index for STS is assumed to be 1.44 (Krieger et al., 2000). We included this in the manuscript.

Ulrich K. Krieger, Juliane C. Mössinger, Beiping Luo, Uwe Weers, and Thomas Peter, “Measurement of the refractive indices of H_2SO_4 - HNO_3 - H_2O solutions to stratospheric temperatures,” Appl. Opt. 39, 3691-3703 (2000)

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2.7 Line 305: These are not boundaries between STS, NAT, and ice. These are STS, NAT mixtures, and ice mixtures since particles are internally mixed. Also, for your simulated data, are you considering a mixture of different types also? Please specify here.

We added “mixtures” to NAT and ice. In the model, the PSC particles are externally mixed. These external mixtures lead to the optical properties (R and β) for each grid point.

2.8 Line 306: I thought you calculate the R and β but not the threshold. The dynamic threshold is determined by the denitrification and dehydration status. I don't think you need to calculate the threshold when it's a fixed boundary (for example, the STS category).

As mentioned above, we had an error in the method and revised the whole paragraph.

2.9 Line 308: Are you adding the uncertainties to the threshold or adding the uncertainties to the backscatter coefficients and backscatter ratio?

The uncertainties are (1) added to the thresholds and (2) used as standard deviation for the normal distribution (now Eqs. 1 and 2) that is applied to introduce noise to the calculated optical properties.

2.10 Line 325: what do you mean by “The development of pure STS particles is similar”? What does “development” mean?

This sentence was removed as part of the complete revision of the section.

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2.11 Line 326: It seems the figure shows ICON-ART “overestimate” the STS at higher temperatures compared with CALIPSO, not “underestimate”.

We added more information about the temperature bins to make this clearer.

2.12 Line 332: “ $XH2O = 5 \text{ ppmv}$ and $XHNO3 = 10 \text{ ppbv}$ ” These values feel like before denitrification and dehydration (early winter), not in July. Are you sure you’ve considered denitrification and dehydration here?

These values are only used to have a reference temperature (TNAT / Tice) that can be applied to both CALIOP and ICON-ART and vary with pressure. We have clarified this in the text. In addition, we adapted the input values to 2.5 ppmv and 2 ppbv, based on MLS measurements shown in Tritscher et al. (2019).

2.13 Line 350: please mention this value (0.25 cm^{-3}) near Eqn (1).

We added it below the equation (now Eq. A1).

2.14 Line 392: “where also $H2SO4$ is enhanced”. Why $H2SO4$ is enhanced? Do you mean aerosols or gas? If you are using a prescribed $H2SO4$, why does the $H2SO4$ increase when you do the nesting?

This is a small artefact of the setup because $H2SO4$ is prescribed in the global domain, only. We added this to Sect. 3.

As pointed out in the description of the PSC scheme, the total gaseous $H2SO4$ is assumed to be in the aerosol. Therefore, the first row of Fig. 11 (now Fig. 12) shows the gaseous $H2SO4$ that is used to form binary aerosol/STS.

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Since H_2SO_4 does not have a sink in the setup used, these enhancements are most probably a result of this missing sink and advection processes.

2.15 Line 395: “In contrast to the literature, the NAT volume concentration decreases when the air masses approach the mountain wave.” Is this sentence referring to your simulation or some other articles? Figure 11: You don’t have sulfate aerosol in the model? H_2SO_4 are all in the gasphase? If you don’t have sulfate aerosol, it might be a problem for ozone chemistry since sulfate provides surface area too.

We included Carslaw et al., 1999 and Svendsen et al., 2005 as literature reference in the manuscript.

The module by Carslaw et al. (1995) accounts for binary aerosol, too, and the resulting particle volume concentration does not distinguish between sulfate and STS particles. Therefore, we changed all notions of STS to “liquid” in the manuscript and figures, where it is needed.

2.16 Line 417: “Due to the missing sink of H_2SO_4 by sedimentation of aerosols (see Sect.3), the mixing ratio accumulates in the nested domains (two right columns).” Why the missing sink only affects the nested domain, not the global case.

As mentioned above (your comment on Line 392), H_2SO_4 is prescribed only in the global domain.

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2.17 Line 497-498: Again, if you compared with CALIPSO, your model output is not simply STS, NAT, and ice. They are all mixtures that fall into different PSC classification categories. You may want to check the wording through the whole content.

We replaced the notion “Type” by “Category” throughout the manuscript to distinguish between the NAT, STS and ice types of ICON-ART and the categories of CALIOP.

2.18 Appendix A: some reaction rates are not listed i.e. “ k_{HO2_HO2} ”.

We included the reaction rate constants in the table (now Table S1 in the supplement) and also listed all abbreviations in the caption.

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