

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

Anonymous Referee #1

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Weimer et al. investigate the polar stratospheric cloud (PSC) formation during an Antarctic mountain wave event using the ICON-ART model. The model applies local grid refinements (nesting) with two-way interaction to simulate the temperature fluctuation and wind shear during the mountain wave event. The model has a detailed PSC parameterization of three types of PSCs. The results are compared with CALIPSO and ARIS satellite data for PSC classifications and brightness temperature for validation. The manuscript also analyzes the impact of PSCs on ozone-related chemicals with and without mountain waves. Although more work needs to be done in the future regarding the model validations, this two-way nesting approach provides a new and useful pathway for future PSC and stratospheric ozone studies, because the localized

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GW PSC formations usually cannot be treated in the global model properly.

I have four major concerns that I recommend further discussions in your manuscript.

First, your PSC parameterizations limit the number concentration to $2.3 \times 10^{-4} \text{ cm}^{-3}$ 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 $0.1 \mu\text{m}$. However, Fahey et al. mentioned a two-mode particle concentration. The $2.3 \times 10^{-4} \text{ cm}^{-3}$ is only for particles larger than $10 \mu\text{m}$. And particles $\sim 3 \mu\text{m}$ has a larger number concentration $\sim 1 \times 10^{-3} \text{ cm}^{-3}$. 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?

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 ClONO_2 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.

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.

Question4: Figure 2: why do you do the free-running in the third step (i.e. July 19 – July 29)? 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?

General comments:

Line 44-46: Is the $\pm 15\text{K}$ 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.

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?

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Please rephrase the sentence.

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

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

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 μm^{-1} . Is it dN/dlnr ?

Line 304: how about the refractive indices of STS?

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.

Line 306: I thought you calculate the R and beta 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).

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

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

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

Line 332: "XH₂O = 5 ppmv and XHNO₃ = 10 ppbv" These values feel like before denitrification and dehydration (early winter), not in July. Are you sure you've considered

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denitrification and dehydration here?

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

Line 392: “where also H_2SO_4 is enhanced”. Why H_2SO_4 is enhanced? Do you mean aerosols or gas? If you are using a prescribed H_2SO_4 , why does the H_2SO_4 increase when you do the nesting?

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 gas phase? If you don’t have sulfate aerosol, it might be a problem for ozone chemistry since sulfate provides surface area too.

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.

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.

Appendix A: some reaction rates are not listed i.e. “ $k_{\text{HO}_2\text{HO}_2}$ ”.

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