

## ***Interactive comment on “Interannual variations of early winter Antarctic polar stratospheric cloud formation and nitric acid observed by CALIOP and MLS” by A. Lambert et al.***

**Anonymous Referee #2**

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This is a complicated and comprehensive over view of what may be learned by combining co-located remote measurements of gas phase HNO<sub>3</sub> from the Aura microwave limb sounder (MLS) and the CALIOP lidar measurements of particle backscatter and depolarization. The authors combine these measurements with back trajectory calculations to address questions related to the nucleation of nitric acid trihydrate (NAT) within polar stratospheric clouds.

The primary conclusion that there is significant NAT formation at temperatures well above T<sub>ice</sub> for NAT with low number concentrations and large particle sizes implies that particle observations confirming this conclusion are not possible due to the sensitivities of the lidar measurements. Thus a significant fraction of the paper is devoted

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to microphysical calculations of the sensitivities of the various instruments to particles of varying sizes, number concentrations, and shapes. This is necessary to ultimately rely solely on the MLS measurements of HNO<sub>3</sub> gas phase loss as evidence of PSC formation when concomitant particle observations provide no measurable PSCs. Thus the primary conclusion on NAT nucleation requires only MLS gas phase hno<sub>3</sub> loss and an absence of PSC detection by the CALIOP lidar.

The authors present a lot of information in fairly compact forms and it is difficult to suggest any effective ways to shorten or compress the figures. The color coding can be at times difficult to fully distinguish the various categories of particle calculations and I wonder if all the aspect ratios presented are necessary. I should think the aspect ratios bounding the range are sufficient.

It is not clear why the sensitivities to IR measurements at 12  $\mu$ m are presented. No IR measurements are used in the analysis and none are currently available for PSC measurements to my knowledge. Is this just done for completeness? In any case the slant indicating the IR measurements are somehow more sensitive to PSCs based on the graphs shown seems a bit arbitrary. The IR emission measurements will be more sensitive to larger particles since they are sensitive to volume whereas the visible scattering measurements are sensitive to aerosol cross section so more sensitive to smaller particles.

The paper should be published with minor corrections. I provide some specific comments for consideration below.

5.6-13. This is too much detail that will make sense to the very few who know what PSC\_compositions of 2 and 3 are. I would recommend a short description of what the distinction between mix1 and mix2 is and why it is important, then just explain that the separation failed to include in mix1 those observations with a scattering ratio less than 1.25. Later 1.25 is used as a threshold to identify PSC presence so overall this is a bit confusing.

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6.2. "The HNO<sub>3</sub> and H<sub>2</sub>O values (for estimating TNAT and TICE) are assumed to be the same at all back-trajectory times." This does not make sense. The same as what? Are they assumed to be constant from the start point of the back trajectory, so not updated along the trajectory, or ???

8.8. Do the authors believe there is really uncondensed H<sub>2</sub>SO<sub>4</sub> available for uptake? If so what are they using for the ambient H<sub>2</sub>SO<sub>4</sub> gas phase mixing ratio?

8.15.  $R_t=1.25$  as a threshold? This is the same value used earlier in the revised classification scheme and now here is used as a threshold to identify PSCs. Seems a bit confusing. In addition Fig. 4 c) uses 1.25 as the threshold for STS detection, so clearly NAT PSCs could appear below this level. Please clarify.

8.29-31 and Fig. 4c). This statement and the figure don't show that the IR is more sensitive than the visible to STS content. It just shows they are different. The ordinates in Fig 4c) are somewhat arbitrarily chosen and the assumed thresholds are set on somewhat subjective criteria that are not fully justified and are different. Again for this paper what is the point of showing the IR results?

9.18-21. Is this realistic? Would the large NAT particles not continue to take up HNO<sub>3</sub>, given enough time, until it was all consumed, so additional uptake of HNO<sub>3</sub> is not dependent on STS?

9.30-31. Same question as above. Why does the uptake of HNO<sub>3</sub> depend on the number density of particles? Is this some sort of kinetic limit or resident time limit, i.e. particles growing too large to stay in the air space? I am not aware there is a theoretical limit to the size a NAT particle can attain given existence at  $T < T_{NAT}$  and HNO<sub>3</sub> and H<sub>2</sub>O available.

10.9-10. I suppose this should be no surprise since IR emission will be sensitive to volume,  $R^3$  whereas backscatter is sensitive to cross section,  $R^2$ . Thus large particles cause larger changes in volume.

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10.17 How is effective radius,  $R_{eff}$ , defined? Is this  $3V/S$  so the ratio of the third and second moments?

14.25. I do not see a blue line, indicating  $T_{ice}+2K$ , on any of the panels for case 3.

15.28. I assume the authors mean 340 K (mid-June).

16.10. Where is the gray shading? I do not see any. It only appears in later figures so save the explanation for then.

16.14. It takes quite a few days for significant TTE to be observed at 21 hPa, nearly half the period shown, so the statement here comparing 21 and 46 hPa for the appearance of significant TTE is confusing.

Fig 13a). Where are the diamonds in the TTE plot?

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