

Interactive comment on “Characteristics of particle size distributions in the tropical tropopause based on optical particle counter and lidar measurements” by S. Iwasaki et al.

S. Iwasaki et al.

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Yellow-marked sentences in our revised paper denote areas we have changed.

We would like to thank the referees for their valuable comments that have greatly improved the quality of this manuscript.

The main aim of this paper was to summarize the findings of field measurements of cirrus clouds in the tropical tropopause layer (TTL), hereafter TTL clouds, by using OPCs and lidar. The reason why this was attempted was because, as reviewer #1 commented, particle number observations of TTL cloud are rare. Our results will contribute to understanding the nucleation mechanism of TTL cloud, particularly the elucidation of the freezing process by comparing aerosol and cloud particle size distributions in

TTL cloud and those below and above TTL cloud (e.g., Iwasaki et al. 2006 AGU Fall meeting). However, we omitted presenting a hypothesis of the nucleation mechanisms suggested by the observational results as we did not show our model results.

Major changes are as follows:

1. We reanalyzed OPC data after realizing that the detection angle was incorrect, i.e., "Width of collecting area" was not 44° half solid angle in Table 1, but 39.2° . Consequently, some figures, especially Fig. 5 in our first manuscript (Fig. 6 in revised one), were modified in the revised paper. We therefore rewrote the sentences related to the local maximum for particle size distribution dn/dr at $2.0 \mu\text{m}$ in TTL cloud and deleted its implications.

2. We present new reanalyzed figures and have added explanations regarding dn/dr , such as, "TTL clouds exhibit dn/dr enhancement at radii greater than $0.8 \mu\text{m}$ while clouds at 10 - 15 km do not. Thus, dn/dr enhancement is a unique feature of TTL cloud."

3. We abbreviated "cirrus cloud in the tropical tropopause layer (TTL)" as "TTL cloud." This is because a subvisual cirrus cloud (SVC) is defined as cirrus with an optical thickness of less than 0.03 (Sassen and Cho, 1992) though SVC is usually used for a cloud in the TTL.

4. We added explanations about atmospheric field conditions, such as time-longitude and time-latitude sections of TBB.

5. We added graphs to show the vertical profile of OPC and lidar data to validate our data.

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Comment 1: The title says this paper is about aerosol particle near the tropopause, the abstract aerosol particles in the vicinity of sub-visible cirrus. But going through the paper there are only 2 cases of sub visible cirrus. The paper needs to get organized

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and be consistent all the way through.

Reply: As summarized in Table 2, there were a total 6 TTL cloud observations. That is:

1. 2 OPC + lidar measurements.
2. 4 OPC measurements.

There were numerous days on which it was not possible to measure TTL clouds with lidar due to dense cloud below the TTL.

Since there were numerous kinds of particles, such as aerosol and ice cloud particles, we wrote, "Note that "particles" in this paper refer to both aerosols and cloud particles, as distinction between the two types is not possible with the present OPC." in our first manuscript (subsection 2.1).

To reduce any misunderstanding, we have changed the title to, "Characteristics of aerosol and cloud particle size distributions in the tropical tropopause layer measured with optical particle counter and lidar measurements."

Comment 2: Abstract and overall analysis: While the abstract says that this is a study using an OPC and lidar, we only see OPC data. The lidar is mentioned only for the detection of SVCs and only a maximum backscatter return is given. Inclusion of lidar data profile along with corresponding particle profiles from the OPC would be a logical addition to this paper. Similarly, given that this is a balloon, I would also expect some thermodynamic information. They do not present any soundings. This would really help the reader get some context for the measurements.

Reply: We also stated that "The mode radius of cloud particles in the TTL is less than 10 μm " and showed that "cloud heights of cirrus in the TTL varies by several hundred meters over distances of tens kilometers; hence the height is not horizontally uniform" by using lidar and OPC data in the first paper. Both of these results are important for characterizing TTL cloud particles, because no paper has reported these inhomogeneities previously. Therefore, the data presented in this study is unique.

Since a considerable amount of OPC and lidar data were collected in this study, we did not present raw data, such as a vertical profile in our first manuscript. However, since these profiles would facilitate greater understanding of our data, we now present it in Figure 3.

Comment 3: Section 2.1: Details on the OPC instrument could be beefed up. For example, this appears to be a closed celled system, but are there any heating or inlet issues? For example, how confident are the authors that aerosol particles are not modified in the sampling train. Also, they say there are 8 channels, but except for the total instrument minima and maxima, our only indications are from grabbing data in the figure. It would be nice if it was listed. They need to defend their index of refraction and their assumption that these particles are sulfuric acid. I can see ammonium sulfate and or smoke being pumped up to these altitudes. And (in relation to comment 2), what is the RH in the SVC layer? It is not necessarily 100%. If these particles are measured at near ambient conditions, I would expect the index of refraction to be lower than what was assumed. How sensitive is the instrument to refractive index or particle shape effects? Because the authors admit that they cannot differentiate between sulfate and ice particles, they assume channel 8 is ice, and all others are sulfuric acid. How do they justify this? Can they give us some indication or reference on why this was chosen? Getting ahead, it looks like from Figure 5 this is not necessarily a good assumption.

Since there are some comments in Comment 3, we divide them into Comment 3-1 ~ 3-5.

Comment 3-1: Details on the OPC instrument could be beefed up.

Reply: Since an explanation of OPC would increase the length of this manuscript markedly, we have omitted them in our first manuscript. However, we have added the details below to subsection 2.1:

Note that the integrated number concentration changes induced by impaction on a wall and heat inside an OPC are negligible for particles with radii of less than 5 μm . This is

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because the stokes number (Eq. 14-11 in Pruppacher and Klett, 1997) in the OPC flow system is 1 for a particle with a radius of 5 μm where the dimensions of the stainless steel inlet tube is 4.5 mm in diameter and 5 cm in length, the flow rate of the sample air is 3 m/s, and the shrink ratio of particle size is less than 0.01 for a particle with a 0.5 μm radius.

We have also rewritten the calibration method in subsection 2.1.

Comment 3-2: how confident are the authors that aerosol particles are not modified in the sampling train.

Reply: Since this system is a through-flow system and not a closed system, two types of sampling errors can be expected: deposition loss on the inlet tube wall and thermodynamic modification of number concentration.

[Deposition loss on inlet tube wall by diffusive and inertial impactions]

Diffusive impaction is not effective for accumulation of mode particles ($r_{\text{mode}} \sim 0.1 \mu\text{m}$) and primary mode particles ($r_{\text{mode}} \sim 1 \mu\text{m}$), where r_{mode} denotes the mode radius of a particle.

Modification by impaction for particles with radii less than 10 μm is also negligible; the reasons are as follows:

(1) Stokes number (Eq. 14-11 in Pruppacher and Klett, 1997) in the OPC flow system is estimated (result is not shown). It shows that the number is 1 for a particle with a radius of 5.0 μm and the dimensions of the inlet tube is 4.5 mm in diameter and 5 cm in length, and flow rate of sample air is approximately 3 m/s. In addition, the inlet nozzle is designed to have a smooth flow line. These characteristics show that the impaction rate of particle is negligible for particle with a radius of 5 μm or less.

(2) It is well known that solid particles do not adhere to the wall of any hard surface easily, especially conductive and hard material, such as the stainless steel pipe that we use as the tube.

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[Thermodynamic modification]

Since sample air passes through the interior of the OPC which is warmer than the environment outside, it is necessary to consider this modification and the effect is examined here for a spherical ice particle. Assuming the temperature and environment water vapor density to be 0 °C and 0 kg/m³, respectively, the shrink ratio, $\Delta r / r$, relative to the radius of an ice particle r is less than 0.01 for particles with a radius of 0.5 μm , where the length of the warmer environment is 5 cm and the air passes through this area in 17 ms (result is not shown). Therefore, this effect is also negligible.

Comment 3-3: Also, they say there are 8 channels, but except for the total instrument minima and maxima, our only indications are from grabbing data in the figure.

Reply: These were listed below Table 2 in the first manuscript and have now been added to another table (Table 3) which shows detection size for the channels.

Comment 3-4: If these particles are measured at near ambient conditions, I would expect the index of refraction to be lower than what was assumed. How sensitive is the instrument to refractive index or particle shape effects?

Reply: A typographic error resulted in us reporting that the refractive index was 1.4 + i 0, corresponding to spherical sulfuric acid solution with a concentration a 70 wt.%, when this should have been 40 wt.%.

We estimated observational error induced by particle shapes and refractive index in Appendix A in the first manuscript. In addition, we have also plotted error bars in Figures 3-5 in our first manuscript (Figures 4-6 in revised one). To clarify this further, we have added these data to a new table (Table 3).

Comment 3-5: Because the authors admit that they cannot differentiate between sulfate and ice particles, they assume channel 8 is ice, and all others are sulfuric acid. How do they justify this? Can they give us some indication or reference on why this was chosen?

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Reply: As explained in Appendix B in the first paper, it is clear that the particles detected in channel 8 ($r > 3.3 \mu\text{m}$) are ice particles. Therefore, we assumed that the particles in channel 8 were ice cloud particles. On the other hand, smaller particles ($< 1 \mu\text{m}$ radius) are liquid particles corresponding to the known Junge layer. Therefore, we concluded that the liquid particles became frozen at the submicrometer to 3 μm -scales. We do not assume that other particles are water ice or liquid (sulfuric acid solution), because we cannot differentiate the phases, which is why we applied error bars. We have added, "We do not assume that the other particles are either liquid or solid because it is not possible to differentiate between phases using the OPC, Consequently, in presenting OPC data, we have applied error bars to indicate this variation in size (see Appendix A)." in subsection 2.1.

Comment 4: Section 2.2: In this section the authors state that the lidar sensitivity is 3×10^{-7} /m/str at 14 km. However, for the two cases of SVC presented the backscatter value is 2.5 and 4 3×10^{-7} /m/str at 17 km. This seems too close to the background noise. This is where showing the lidar profile couple with the thermodynamic profile from the balloon would be helpful in supporting their claims.

Reply: We now show the vertical profiles for lidar and OPC data in Figure 3. In addition, beta in Table 2 is averaged every 1 hour and 100 m, and an explanation is provided below Table 2.

Comment 5: Section 3.2: I really don't understand the inclusion of relatively low quality GMS geostationary data here. If you could show an image that detects cirrus that would be one thing. But a simple black body temperature is very difficult to make any argument. The authors don't list the channel, and if it is 8-12 (which I suspect) how do they back out surface and other contributions. Looking at Figure 2, I don't see any connection between. What about MODIS or MISR cirrus detection. Can you get anything out of that?

Reply: To validate the relationship between the nucleation mechanism of a subvisual

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cirrus cloud and deep convection is not easy since amount of water vapor and vertical velocity were not measured. In fact, even if these were measured, the proof would still be difficult. Therefore, we have edited our paper to state, "the cirrus clouds in the TTL are observed frequently during the rainy season" and show a figure of the time-longitude and the time-latitude sections TBB (K) in subsection 3.2. We then add, "Both figures show that deep convection during the monsoon extended northward into the Sri Samrong from late in May, and that the horizontal scale in longitude is 1-2 x 10³ km" and conclude that "TTL clouds are observed frequently in the vicinity of deep convection conditions."

We did not obtain other data such as MODIS or MISR.

Comment 6: Section 3.3-3.5: Instead of showing integrated number distribution, how about showing actual profiles? Also, show volume distributions as well-that is much more telling as to what is going on.

Reply: We now show the vertical profiles for lidar and OPC data in Figure 3.

Comment 7: Section 3.6: Regarding "data not shown." Why is it not shown? Already they make a marginal case for sensitivity. They should come clean.

Reply: Given the volume of OPC and lidar data that were collected, we did not present these in the first manuscript.

In Figure 4 we show that cloud height measured with the lidar is not higher than that recorded by the OPC. The figure suggests that there is no height bias between OPC and lidar.

In addition, we referred two figures in our paper. These also show horizontal inhomogeneities of cloud height though they do not mention about it. I talked with Dr. Martucci and confirmed that they had corrected for cloud height changes associated with flight attitude.

Therefore, we believe the difference in the spatial variations of cloud heights is true.

Comment 8: Section 4: Again, the claim that cirrus clouds were detected by the OPC would be much stronger if actual profiles were shown. Right now this is not entirely clear. Most of their discussion is unsupported with the data given.

Reply: We now show the vertical profiles for lidar and OPC data in Figure 3.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1595, 2007.

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