

Interactive
Comment

Interactive comment on “Composition of cirrus-forming aerosols at the tropical tropopause” by K. D. Froyd et al.

Anonymous Referee #2

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General Comments

This paper presents data of subvisual cirrus chemical composition (as measured by residual particles from a counterflow virtual impactor) in the tropical upper troposphere. This is a cloud type with heretofore unsampled composition, so while the number of particles sampled are small, this is an important original contribution. The measurements raise some interesting questions in terms of ice nucleation mechanisms, with some potential ideas given to help explain them. Overall, the paper is well written, although there are a few areas that should be expanded or clarified as given below.

Specific Comments

1) Title: Since these measurements are related to subvisible cirrus and its formation

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and chemistry and may be very different from other (more studied) types of cirrus, it would be useful to include the term “subvisible cirrus” somewhere in the title.

2) p. 20349, 1st paragraph: Since heterogeneous and homogeneous freezing are also dependent on temperature as well as supersaturation, please include typical temperatures at which subvisible cirrus forms in this paragraph.

3) p. 20350, line 25: crystal “size” should be further defined: I assume it is an optical diameter (not radius), but is it maximum length, mean diameter, or something else?

4) p. 20351, lines 10-15: Some more details would be useful to better understand the authors’ reasoning here. The discussion is about artifacts in convection and anvil cirrus. The main measurements presented in the paper, however, refer to subvisible cirrus. The authors seem to imply earlier that the crystals in such clouds are small enough to be efficiently sampled (presumably) without contact with inlet surfaces. So, it would be useful to note initially in this discussion that other types of cirrus were also sampled (as inferred but not really detailed with respect to Fig. 2.) Also, it is stated that secondary particle generation was “significantly reduced” by plating the inlet with gold. It would seem difficult to prove this without sampling with and without the gold plating in clouds with similar crystal sizes and types (and airspeeds). Was this done, and if so, is it presented elsewhere? If not, those data should be presented here to support the authors’ statements.

5) Fig. 1 gives an example of chemical spectra for three different types of contamination particles. Fig 1c shows trace metals (Cu and Sn) that are given as an indication of contamination with mineral dust, yet these are also present in the earth’s crust; if these are assumed to be from the inlet, what is their source within a gold-plated inlet?

6) Fig. 2 gives the percentage of total particles influenced by contamination as a function of ice water content, which is interesting. Since the authors discuss several different potential types and sources of contamination, to aid in understanding the results, please include the relative percentages of these different contamination types detected.

7) p. 20353, line 23: Particles with ammonium to sulfate ratios of 3:2 or 2:1 are given as the most common, but Table 1 says the ratios for all samples were greater than or equal to 2. Do you mean less than or equal to 2?

8) p. 20354, line 20-21: I don't understand why it is stated that mineral dust was not enhanced in cirrus residues – unless I am misinterpreting Table 1, dust is actually a higher percentage of cirrus residue samples than of unfrozen aerosol samples. Is it because the number of dust particles are insufficient to be statistically significant? If so, the statistics should be presented.

9) p. 20354, line 25: Suggest inserting “heterogeneous” IN, if that is what you mean here.

10) p. 20355, line 15-17: Wouldn't the Murphy et al. 2006b results be from a different field project? Perhaps they are assumed to be relevant since sampling was conducted in the same region, but this should be clarified.

11) p. 20356, line 19 (discussion of cirrus concentrations): Fig. 5c is helpful in showing the size of particles detected by PALMS, which is only the far end of the accumulation mode. Can the authors give an estimate, based on the sampling rate and efficiencies, of what fraction of the subvisible crystal number concentration was actually sampled by PALMS and so is actually represented by these results?

12) p. 20357, lines 9-15: Is there any evidence, through in-situ aerosol shape/phase measurements or environmental SEM, that the ambient particles actually were dry in this environment? That would be something interesting to measure in the future.

13) Fig. 3 caption: How was IWC calculated from both 2D-S channels? Were they just averaged?

14) Fig. 4b: Please include the number of anvil cirrus residuals represented by the pie charts, as these don't seem to be given in Table 1.

Minor Technical Corrections

15) p. 20357, line 22: Delete extra “and”.

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