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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosols in the tropical and subtropical UT/LS: in-situ measurements of submicron particle abundance and volatility" *by* S. Borrmann et al.

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Reply-letter to Reviews for the manuscript:

Aerosols in the tropical and subtropical UT/LS: In-situ measurements of submicron particle abundance and volatility, by S. Borrmann, D. Kunkel, R. Weigel, A. Minikin,T. Deshler, J. C. Wilson, J. Curtius, C. M. Volk, C. Homan, A. Ulanovsky, F. Ravegnani, S. Viciani, G. N. Shur, G. V. Belyaev, K. S. Law, and F. Cairo.

General remarks: We prepared a major revision of the manuscript. The main changes are enumerated below and in our reply to the individual reviewers we refer to these items:

(0.) We very much thank all three referees for their extraordinarily constructive comments, which caused us to very thoroughly revise the manuscript.

(1.) A new section (2.1) is included on the connection between the aerosol measurements and atmospheric dynamics. One additional figure with correlations between submicron particle data and trace gas (CO, N2O, O3) measurements is added and discussed (Figure 8 of the revised manuscript). For this we included four new co-authors and text/references on their instruments. (2.) In the meantime a number of relevant publications appeared or was submitted and we included 29 new references. (3.) In addition to the parameterization for the tropical profiles we supply now a second parameterization from the Figure 9 (of the revised ms) for the profiles in mid-latitudes. However this parameterization is given in terms of particle number concentrations instead of mixing ratios because of lack of adequate temperature data for several flights. (4.) In order to demonstrate the particle concentration maximum is indeed a maximum with decreases below and above we integrated the data from the DLR Falcon-20 into Figure 6 for altitude levels below 350 K.

Response to the Anonymous Referee #3:

Comments from the first two paragraphs of this review: With the changes described in Remark #1 and #4 above and with what we say in response to Question 3 of Reviewer #1 we hope to have improved the paper adequately.

Specific comment – typical ranges for relevant tracers: We have prepared such a table for each of the campaigns. In the end we decided to rather comprehend the data into the new Figure 8 as the tables are quite space-consuming. If the editor asks us, we can supply the tables too.

Specific comment – Upper right panel Figure 2: Thanks for this comment. The analysis was redone and most of the points were removed. There seemed to have been a problem with the flow rate of the pump during the rapid ascents and descents. Yes the (very rare) cloudy segments were removed based on the data of our cloud particle

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instruments. In general “Geophysica” tried to avoid clouds during the transfers.

Specific comment – Figures 1 and 2: We cannot give the altitude above the tropopause because the MTP instrument was not on board.

Specific comment: “sections are too long with inconsequential details”: Agreed. Section 3.5 was rewritten completely. Especially the descriptive part was shortened. However, after quite a few discussions with people during the 2010 RECONCILE campaign in Kiruna we added a section pertaining to the causes of the minimum in f. Clearly this is speculative, and marked as such, but we think these considerations are useful. It could be removed, if required.

Specific comment: “Page 24604 line 16”: This information was added in the instrument section (2.2) of the revised ms. We say now that MAS is sensitive to particles with size diameters above roughly 0.2 μm.

Specific comment: “Page 24595 line 5”: The exposure time of the particle flow to the heater is between 3 and 6 seconds, depending on the flow rate. (We added this information in the revised ms.) The evaporation point of H2O- H2SO4 particles under pressure conditions at 10 to 20 km is roughly 150 C according to Rosen, 1971. We heat to 250 C in order to compensate somewhat for the lower ambient temperatures at the tropical tropopause. The figure "Weigel_et_al_2009_Fig.pdf" in the supplement shows laboratory test measurements with COPAS under ambient pressures of 300 hPa, 15 hPa and 70 hPa. Here binary solution droplets of known size were subjected to the COPAS channels and as result the total “disappearance” of the particles after the 250 C heating is evident even for high number densities. Thus we believe the f-measurements are valid.

Specific comment: “Page 24598 line 26”: Definition of nucleation event. Since we did not want to go too much into details on this point we quote two papers including a new reference. Also in response to Referee #1 we shortened this discussion and moved it into the instrument section 2.2. The n6 > 1.5 n14 formula has been removed from the

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ms for requiring a too detailed explanation. Briefly: The flow rates and counting electronics have uncertainties and fluctuations, especially during rapid altitude changes. To be sure (for the identification of a nucleation event) n6 really and significantly differs from n14 outside of these error margins we demand a “minimum discrepancy” between the two channels of 50%. Only if n6 is 50% higher than n14 we believe that particles with sizes between 6nm and 14nm were detected. This is the background of the factor 1.5.

Specific comment: “Page 24600 line 18-23”: As mentioned in General Remark #4 above, we added the DLR Falcon-20 data for the three campaigns. With these Figure 6 now clearly shows the existence of the maximum. Also in the balloon data from Figure 9 (now Figure 10 in the revised ms) exhibit this maximum for SCOUT-AMMA. The Brock et al., 1995, maximum also was below the tropopause. The interpretation is that particles in this altitude band below the tropopause and within the TTL are slowly transported upward with the ascending air in the tropics. Clearly, this still may be considered as hypothesis and for this reason we phrase the interpretations in our paper carefully. However, indications and even quantitative estimates for such ascent are found in these references: * Plöger et al., Impact of the vertical velocity scheme on modeling transport in the tropical tropopause layer JGR, 115, 2010. * Fueglistaler et al., Tropical troposphere-to-stratosphere transport inferred from trajectory calculations JGR,.109, 2004. We added a few sentences and quote these references.

Specific comment: “Page 24603 line 8”: “Larger” vs “supermicron”. We say now here: “particles with sizes above the accumulation mode”. Because The OPC data from the Hoffman, 1990, paper start at 300 nm diameter.

Specific comment: “Page 24606 line 3”: Not really. For SCOUT-AMMA the cold point tropopause was between 365 and 370 K, for SCOUT-O3 near 375 K and for TROCCINOX at 380 K. This information –forgotten in the originally submitted ms- is now added in connection with Figure 6, and mentioned in the text.

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Specific comment-Figure 1: Yes, we added squares designating the areas where the local flights were performed.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/9/C12314/2010/acpd-9-C12314-2010-supplement.zip

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24587, 2009.

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Figure 1 (FIGURES-100430)

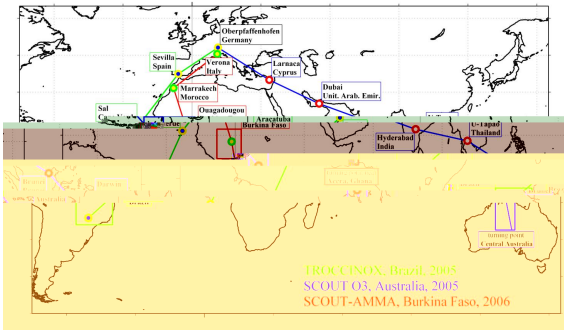

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Fig. 1.

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Interactive Comment