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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solid particles in the tropical lowest stratosphere" *by* J. K. Nielsen et al.

Anonymous Referee #2

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The paper by Nielson et al. presents very interesting observations of particles in the lower tropical stratosphere. These observations were obtained during the HIBISCUS campaigns at Bauru/Brazil in 2001 and 2004 with different instruments, a ground based lidar and a balloon borne backscatter detector. Two cases are presented, where these instruments detected solid particles at about 19 km altitude which are interpreted as ice clouds. Based on radar observations and trajectory calculations the authors argue that a large amount of water was transported by convective overshooting into the lower stratosphere. The paper is well written and provides a very valuable contribution to the current discussion on tropical troposphere-to-stratosphere exchange. I recommend publication in ACP once the following issues are addressed:

The calculation of the hydration of the lower stratosphere is based on the assumption that the particles are pure ice in equilibrium with the ambient water vapor. I strongly doubt that this is the case, because the amount of condensed water is so low (40 ppbm

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according to the authors), that changes in temperature of less than a tenth of a Kelvin was enough to either evaporate the cloud completely or increase the backscatter by an order of magnitude. Since the temperature in the lower stratosphere is not that stable, even on the time scale of minutes, such thin clouds should be transient objects and can not exist longer than a few ten seconds. The fact that several layers of these thin clouds were observed at a time (fig.1) in my opinion precludes the interpretation that these are ice clouds in equilibrium with water vapor. There are two other possibilities. Either the ice is not in thermal equilibrium, because of low sublimation rates for example, or the particles are not ice. The authors point out the possibility that the particles are Nitirc Acid Trihydrate (NAT), however, there are a number of additional possibilities. Ice particles could be stabilized by small amounts of HNO3 or other acids (see i.e. Delval et al. ACP, 3, 1131f). Immler et al. (ACP, 5, 345f) observed particles in the lower stratosphere at mid-latitudes with depolarization and color-characteristics very similar to what was reported here. However, they provided evidence that this stratospheric aerosol layer is aged smoke from biomass burning. In any case, the quantitative analysis provided by the manuscript under review is no longer valid if the particles were not ice in equilibrium. The authors argue, that their main conclusion does not depend on the nature of the particles. I tend to follow the idea, but suggest that this should be worked out in more detail. It is not obvious that the observations support mixing and sublimation (hydration) versus particle growth and sedimentation (dehydration) as the dominant processes following the convective penetration if no information on the water vapor is available. The most important conclusion of the manuscript is that the detected particles were introduces by convection. In this respect I wonder why case O2 was not presented with the same diligence as case O1. It leaves the reader with the presumably unsubstantiated suspicion that a detailed analysis of O2 is not supporting the conclusions.

Minor comments: The backscatter ratio and the color ratio as defined here, strongly depend on the different wavelengths that are used by the instruments because of the λ^{-4} dependence of the molecular scattering. For the sake of comparability of the

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two presented cases it would be helpful if the particle backscatter coefficients and the (backscatter related) Angström coefficients were used instead. p. 9008 l.3 A few more details would be helpful for the reader: At what time was this observation made. How large is the entire dataset obtained during the Hibiscus campaign with the Micro-Lidar, are there other cases of thin clouds near or above the tropopause, what is the detection limit, etc.

p. 9008 l. 16 "See fig.5" in brackets.

p. 9009 l. 16 ff. The question of sublimation rates is crucial for this analysis, please provide references for the claimed time scales.

p. 9010 l. 2 ff. Other groups have reported higher concentration of HNO3 at the tropical tropopause (i.e. JensenDrdla, 2002, JGR, 29, 62). Moreover, it is conceivable the NOy in the outflow of deep convection is enhanced compared to ambient values due to flash induced NOx formation inside the convective system.

p. 9011 l. 24. ff. Why would you not provide the same analysis for O2?

p. 9014 l. 6 ff. dito

p. 9016 l.16 H_2O (2 as subscript)

p. 9018 l.13 As described above the small size and low number density of the particles does in my opinion not support that they are composed of ice.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9003, 2006.

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