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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence for ice particles in the tropical stratosphere from in-situ measurements" *by* M. de Reus et al.

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There are very few measurements of stratospheric clouds in tropical regions and those presented in this manuscript are an interesting addition to the current data base of such measurements. Perhaps the most interesting feature of these measurements are the large ice crystals that were measured yet these are only mentioned in passing. A large part of this paper is devoted to the discussion of instrumentation, intercomparisons of IWC and discounting the contribution of shattered crystals - all very important and necessary aspects of a study that uses measurements as the basis for addressing scientific questions; however, there is very limited analysis in this paper that contributes to expanding our understanding of stratospheric hydration by overshooting convection. Granted, the number of samples of cloud particles in the stratosphere is limited. This is all the more reason to extract from the available observations as much information as possible. As discussed in the comments and questions below, there are a number ways to further identify the source and age of the air in the stratospheric clouds as well as better estimating the potential rate of stratospheric hydration by convective overshooting of Hector. In addition, the aerosol information is used very qualitatively whereas it could be used more quantitatively to distinguish the origin of the air masses where the stratospheric clouds are found.

Perhaps it is sufficient to just report the measurements as the authors have done here and leave it to the theoreticians and modelers to incorporate the results in transport models that can better describe the formation and evolution of the clouds. These measurements could also be useful to satellite scientists in developing more accurate algorithms for extracting cloud properties. It seems odd, however, to have a paper with 14 authors with so little analysis.

Finally, there are numerous grammatical errors that should have been corrected before submission. Given the number of co-authors who are native English speakers, it is unfortunate that they evidently did not take seriously their co-authorship and assist the principal author by correcting these errors.

Other comments follow.

Part 2.2:

The FSSP-100 measures over the forward scattering angles of 4-12, not 3-15 degrees.

It is not clear how aspherical particles are being analyzed. The T-Matrix approach that was used by Borrmann et al. resized particles measured with the FSSP-300. How is that being applied to the FSSP-100? The 0.3 - 20 um size range of the FSSP-300 was reduced to 0.3-16 um. What is the new size range of the FSSP-100?

"...the size distributions presented in this paper have been comprehended into 7 size bins" I think that what is meant here is "...the size distributions presented in this paper

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have been combined into 7 size bins".

Part 2.3:

Change "At 190 ms-1, the typical cruising speed of the Geophysica in the upper troposphere and lower stratosphere, this corresponds to a sampling rate of 8 MHz"

to

"At 190 ms-1, the typical cruising speed of the Geophysica in the upper troposphere and lower stratosphere, this corresponds to a sampling rate of 7.6 MHz at the 25 um resolution of the CIP"

An image can have more than 62 slices.

How was it determined how to reconstruct the missing slice at the start of an image?

The Korolev correction should be done for out-of-focus particles. It is applicable to ice as well as water. It is an important correction, particularly with respect to deriving IWC from the CIP. There are two corrections: the crystal size is decreased and there is also an associated correction in sample volume, i.e. the Korolev correction allows an estimation of the DOF as well as the size. These two corrections lead to a decrease in derived IWC.

Change "The intensity of the laser light on a diode has to decline by more than 50% to be recorded as shaded"

To

"The intensity of the laser light on a diode has to decrease by more than 50% to be recorded as shadowed"

Part 2.4 This section is entitled "Combined number and mass size distributions from the modified FSSP-100 and the CIP" but only the number distributions are discussed, not the mass. How is the mass derived?

Part 2.5

Isn't the oversampling factor also a function of the ice crystal size, density and shape? Wouldn't this lead to a larger uncertainty than 6% in the IWC derived from the FISH?

Part 2.7

Was there a measurement of vertical velocity?

Part 3.2

Why not use more recent studies to derive the IWC from the CIP, such as that by Baker and Lawson (2006) who analyzed actual measurements of mass related to measurements with the CPI to dimensions less than 100 um?

Refs: Baker, B. and P. Lawson, 2006: Improvement in Determination of Ice Water Content from Two-Dimensional Particle Imagery. Part I: Image-to-Mass Relationships, J. Appl. Metor and Clim., 45, 1282-1290.

The non-linear relationship between the IWC derived from the particle probes and the total water sensors can be more easily explained by the uncertainty in the oversampling factor. Assuming that a constant factor of nine is being used, then the best fit will be where the median volume diameter of the size distribution matches the enhancement factor of nine and when the median diameter is smaller or larger than the enhancement factor will become steadily more in error. It is likely that the IWC has had too small of an enhancement correction applied when the majority of the water is less than 30 um. Another more obvious cause of the error is that, on one hand the IWC is being derived from the FSSP assuming that the particles are spheres, yet apparently the size thresholds have been set assuming that the crystals have a 2:1 aspect ratio. This means that a 45 um crystal is being classified as a 30 um crystal. This is a huge underestimate in the IWC.

Part 3.3 Table 2 IWC can't be correct if indeed the units are g/L. 1.7 g/L would be 1700 g/m3.

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Why not show potential temperature or equivalent potential temperature as a possible marker for tropospheric air? It is not at all clear from Fig. 6a that ozone is decreasing markedly in the cloud events. The variance is quite large and during events 5 and 6 there is either an increase or no change. It might be instructive to create a Paluch type mixing diagram, i.e. q vs Thetaq from the ascents and descents then plot the in cloud q and thetaq points on this diagram to see if they fall within the envelope of mixing lines. It is possible that this won't work for stratospheric air but might provide a better indicator of where the air possibly came from.

Part 3.4

The utility of evaluating the relationship between interstitial aerosol and cloud particles is probably of questionable value except maybe as an indicator of cloud age. Older clouds will have scavenged more of the interstitial aerosols than younger clouds but, on the other hand, as the smaller, higher concentration crystals sublimate, there will be additional interstitial aerosols. As acknowledged by the authors, this is a complex relationship. Given that it does not contribute to the understanding of where the stratospheric clouds originated, or from what aerosol population, it would be best to remove Fig. 9 and the associated discussion. On the other hand, little is said about the non-volatile aerosols yet these might be the best marker of identifying the possible source of the stratospheric clouds. Perhaps Fig. 8 would be more informative if ratios between N14 and N6-14, N14 and Nnv were shown as a function of the potential temperature.

Part 4 It seems that the lidar measurements are underused in the limited analysis that is done to look at hydration in the stratosphere. The vertical information is invaluable for linking the cloud microstructure at the flight level to that below the aircraft. Given that there appears to be a vertical profile of the cloud properties as shown in Fig. 3, the backscatter ratio could be computed from the size distribution then plotted as a function of IWC. This rough relationship could then be used to estimate the IWC profile below the aircraft to get a better estimate of the total mass of IWC in the sector of the atmosphere that was sampled. If the vertical velocity is also measured on the

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aircraft, then the vertical water flux could be estimated. If not, there are other methods to estimate vertical motion with lower resolution but still as a means to estimate the vertical flux of water. The approach that is taken in the paper of calculating terminal velocities does not offer much insight into the importance of Hector as a source of stratospheric hydration.

The second important piece of information that seems to be overlooked is the presence of the large ice crystals in the stratosphere. The 25 um resolution of the CIP limits how much of the structure of the crystals can be identified, but the shape of the 400 um crystals should be evident and might help identify if they were formed solely by diffusional growth or if they are aggregates.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19313, 2008.