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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Liquid particle composition and heterogeneous reactions in a mountain wave Polar Stratospheric Cloud" *by* D. Lowe et al.

D. Lowe et al.

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We thank Beiping Luo for his careful and generally positive review, and explain below how we have dealt with the various points he has raised. The referee's original comments are in italics, followed by our response in normal typeface.

1 Comments

1. *The impact of non-equilibrium of STS on heterogeneous chemical reaction is a key result of the present MS. I do not understand why a comparison with the*

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measuredCl_2O_2 is not possible. A comparison of Cl_2O_2 value at the upwind and down wind of MW (at the time when the aircraft crossed these regions) would be very useful and it is also possible. Two set of trajectories (with different time only) are required. I am forward to see the result of such comparison. The solar zenith angles during the period of the second mountain wave are large, so that there is no, or little, photolysis of Cl_2 to ClO, and so no scope for a comparison of ClO dimer mixing ratios before and after the mountain wave event. Data from the Halox instrument show mixing ratios of the dimer between 80 and 200 pptv during the mountain wave event, but the signal is very noisy and no trend in dimer mixing ratio is apparent.

2. *Due to the fast temperature fluctuations, the liquid particles are out of equilibrium with the gas phase shown by Figure 8a. One feature in Figure 8a has to be cleared: i.e. in the kinetic simulation, the NOy signal (blue curve of Figure 8a) begins to decrease at a time at 0.9 hr for the major maximum. At this time, the equilibrium NOy value is much higher than the kinetic value requiring a further HNO3 uptake by the STS particles. Therefore, there is a contradiction. Similar behaviour can be observed at other maxima of simulations. I guess that this difference could be caused by the neglect of the Kelvin effect for the equilibrium calculation and/or a different model parameter for the simulations. In order to separate kinetic effect from other factors, the same parameter set should be used for both kinetic and equilibrium simulation.* These differences in behaviour are indeed caused by the inclusion of the Kelvin effect in the microphysical model. However we do not believe that the inclusion of the Kelvin effect into the equilibrium calculations would greatly change its behaviour because the size-dependent variations in particle growth seen in the microphysical model do not occur in the equilibrium calculations. Any changes in the HNO3-content of the particle phase for the equilibrium calculations would also be minor, and so we concluded that the inclusion of the Kelvin effect in the equilibrium calculations would not be worth

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the extra work.

3. *The initialisation of the background aerosol is obvious too high as can be seen from Figure 9b. The maximum surface area ($2-4 \times 10^8\ cm^2$) is only about a factor 3 larger than the surface area of binary aerosol used here. A smaller value of surface area of binary aerosol has definitely some impact on the numbers given by this MS.* Please see our response to referee 2. The sulphur mass loading certainly does have an affect on the model results, but tests we have run show that our conclusions are unaffected.

2 Minor technical points

(i). *P9552 Line 19: "1999) is a short range lidar instrument" should be "1999) is a short range optical instrument".* We have changed the line to read "1999) is an optical backscatter sonde, which measures aerosol backscatter and depolarization."

(ii). *The flight tracks in Figure 1 and figure 2 differ, The location of the cloud event analysed located at totally different latitude.* Agreed. There was a plotting error during the production of Figure 2, which has now been corrected.

(iii). *Are you sure that the blue marked part are STS clouds (Figure 2)? Pls show an overview plot for MAS and NOy.* The MAS and SIOUX data do indicate the existence of particles within the blue region of the flight marked in Figure 2. We have labelled these as PSCs, and believe that they are most likely to be STS particles, but we have not investigated further to determine this.

(iv). *Is the uncertainty of 3 % of MAS data on aerosol backscattering coefficient given in Figure caption 8 correct?* Uncertainties affecting MAS data arise from: 1) data calibration procedure. 2) intrinsic noise affecting the raw data. We discuss these in turn, below.

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2.1 data calibration

Backscattersonde data, as most LIDAR, data are not absolute, in the sense that they rely on an assumption of known fixed values of physical quantities of interest (Backscatter and Depolarization ratio) in particular regions of the atmosphere. In agreement with the most recent literature we have assumed in our analysis a value of 1.03 for the 532 nm Backscatter Ratio and of 1.44 % for the Depolarization Ratio in a region of the atmosphere along the flight track were the aerosol burden was considered the least. Errors deriving from these assumptions have been neglected in the uncertainty estimation given in the manuscript.

2.2 intrinsic noise

The intrinsic noise for the backscatter ratio comes from

- uncertainties on the temperature, T
- uncertainties on the pressure, P
- uncertainties on the PMT current measurement, C
- uncertainties on the laser pulse energy measurement, L

in the formula for the BR = k * (p/T) * (C/L), where K is a calibration constant.

Relative errors for P, T, and L in this formula are well below 1 % and have been neglected. The relative error for C comes from the ratio of the amplitude of the RMS of the atmospheric background light signal to the atmospheric laser backscatter signal. That depends on the altitude and time of the day, and at the time of the observation presented in the manuscript, was 0.03

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