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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lagrangian analysis of microphysical and chemical processes in the Antarctic stratosphere: a case study" *by* L. Di Liberto et al.

Anonymous Referee #2

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The manuscript 'Lagrangian analysis of micro-physical and chemical processes in the Antarctic Stratosphere: a case study' by Di Liberto et al. analyzes the chemical and micro-physical composition of stratospheric air in between two in-situ balloon measurements over McMurdo in the Antarctic. Chemical species and PSC particles are constrained with information from a first balloon sounding and subsequently modeled along trajectories. When the previously probed air masses came close to McMurdo a second balloon was launched to measure the same air masses again. Modeling results are compared to observations and the sensitivity of stratospheric chlorine chemistry towards NAT sedimentation, ozone mixing ratios, and particle surfaces discussed. Such

process studies are a valuable contribution to our understanding of chemistry since they allow to test models on small scales. The current manuscript is of interest to the scientific community and adds to the on-going discussion about the sensitivity of chlorine activation towards different particles in the stratosphere. I suggest publication pending the authors address the following issues in the revised manuscript.

Major:

The study is based on two vertical profiles which are connected through trajectories and initialized from in-situ and satellite observations. The uncertainty of such calculations needs more discussion and quantification. On p32643 l23 the authors note that limb measurements average over large horizontal distances which makes analyzing processes in small portions of air difficult. This also applies to the initialization of the trajectories. It might be helpful to look at daily maps of MLS or CTMs to assess the homogeneity of the vortex in the vicinity of the trajectory initialization. Especially, the uncertainty in ClOx is of interest in modeling ozone loss. Quantifying the uncertainty here will add value to the authors calculation of ozone loss.

In this context, please add the in-situ measured ozone mixing ratios to Figures 6 and 8. From Figure 2 it appears that on 20 September ozone at 400K is at about 100 ppbv. Is the calculated ozone loss along the trajectory in agreement with this? Figures 6 and 8 appear to end before the 2nd balloon observations. Please discuss how good the model reproduces the observed ozone loss. The authors have observations of all relevant species and can add value to the discussion concerning the parameters determining the ratio of ClO/Cl2O2. Are the recommended values for the Cl2O2 recombination constant and photolysis frequency in agreement with ozone loss observations (for details see von Hobe et al. (2007), doi: 10.5194/acp-7-3055-2007)?

Another major point concerns the accuracy of the trajectories. Similar studies (MATCH, see Rex et al. (1998), doi: 10.1029/97JD03127) which use point to point comparisons of measurements use several parameters to determine if the same air masses have

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indeed been sampled and need a significant amount of measurements to achieve statistically robust results. The authors should discuss the uncertainty of their trajectories. From Figure 3 it appears that after 10 days trajectories which had been started around the same altitude (400K, cyan) spread some hundred kilometers. The authors could start several trajectories around the initial balloon profile and monitor their spread after ten days. This should allow to assess the prevailing atmospheric flow situation and help to determine trajectory accuracy.

Is the 400K isentrope representative for the 380-420K range where the air mass returned over McMurdo after 10 days? From Figure 5 it appears that 400K is at the upper edge of the PSC and the authors note on p32645 l9 that sedimentation prevents NAT particle existence after two days. At 380K NAT appears to be present until the PSC evaporates, how does this influence heterogeneous chemistry?

Minor:

p32635: Add information about H2O and ClO MLS products to the discussion. MLS v2.2 has a known bias in ClO (Santee et al. (2008), doi:10.1029/2007JD008762), has this been accounted for in the initialization? Also, has the MLS averaging kernel been taken into consideration?

p32640: Add a Figure comparing ZOMM results (backscatter and depolarization) to intersecting CALIPSO overpasses (red circles in Figure 4). How does ZOMM perform in reproducing the PSC several days after the initialization?

p32642: How does sedimentation of NAT particles change the abundance of HNO3 within the PSC? Please add a panel showing total HNO3 to Figure 5 so the layers of denitrification and re-nitrification become visible. The vertical extent of the PSC alone is rather inaccurate to quantify the extent of particle sedimentation and evaporation.

p32647

l15: So far it has only been shown that ZOMM agrees with observations that after 10

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days the PSC has evaporated. To conclude that the evolution and type have been reproduced an intermediate comparison with CALIPSO should be shown.

l23: How does the modeled ozone depletion rate compare to the measured O3-change between both sondes and how does this rate compare to previous studies like MATCH campaigns?

Figure 6: Describe how the error bars on the satellite observations were determined

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