Atmos. Chem. Phys. Discuss.,
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mechanism of ozone loss under enhanced water vapour conditions in the mid-latitude lower stratosphere in summer" *by* Sabine Robrecht et al.

Anonymous Referee #1

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Review of the paper: Mechanism of ozone loss under enhanced water vapour conditions in the mid-latitude lower stratosphere in summer, by S. Robrecht et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1193, 2018.

This paper presents a detailed chemical study for a potentially significant ozone depletion in the lowermost stratosphere, using a chemical box-model, with 7-day and 19-day back-trajectory analysis. The study is conducted under conditions of low temperatures (<205 K) and an elevated water vapour mixing ratio, up to 20 ppmv (resulting from convective overshooting events, rather frequent for summertime mid-latitude conditions). These convective events can transport ice crystals into the lowermost stratosphere,

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where the ice evaporates leading to a local water vapour increase. The sensitivity to high Cly mixing ratios is also addressed. The authors analyze with plenty of details the catalytic chemical cycles involving ClOx, NOx, HOx and leading to a perturbed balance of ozone production and destruction taking place in the lowermost stratosphere. The study takes inspiration from previous published works (Anderson et al., 2012; Anderson and Clapp, 2018); the authors conclude that the combined effects of temperature, water vapour and chlorine on the ozone loss process are consistent in their study with respect to these previous ones.

I think that this study may help clarify important points regarding the ozone sensitivity to elevated water vapour conditions in the lowermost stratosphere and for this reason I recommend publication on ACP. In my opinion, a few improvements could be made in the manuscript, mainly for completeness and for improving readability.

Specific points

(1) Chemical cycles are essentially those leading to polar ozone depletion and widely described in previous literature. For this reason, I suggest moving large part of chemical details from the main text to a specific Appendix or in supplementary material. In particular, section 3.2.2 is in my opinion way too detailed and should be simplified focusing of the evidences of Fig. 5 (which is clear and exhaustive).

(2) The authors clearly state that their box-model ignores mixing with outside air poorer of water vapour, so that their calculated ozone losses should be interpreted as an extreme case. They also suggest a possible use of their findings in global modelling of the atmosphere for future experiment of sulphate geoengineering under changing climate conditions, or in case of major volcanic eruptions. It should be mentioned that in these cases the large scale latitudinal mixing of atmospheric tracers in the lower branch of the Brewer-Dobson circulation could be significantly affected in sulphate-perturbed conditions due to geoengineering or major tropical eruptions, both leading to a different level of isolation of the tropical pipe with the mid-latitudes. Eddy heat

fluxes could also be perturbed, thus affecting mid-latitude temperatures in the lower stratosphere. Recent works have addressed these specific points (e.g., Visioni et al., 2017).

Visioni, D., et al.: Sulphate Geoengineering Impact on Methane Transport and Lifetime: Results from the Geoengineering Model Intercomparison Project (GeoMIP), Atmos. Chem. Phys., 17, 11209-11226, doi:10.5194/acp-17-11209-2017, 2017.

Minor points

Page 25, lines 6-8: the sentence sounds odd and should be modified: “In summary, the combination of low temperatures, enhanced sulphate concentrations and high water vapour mixing ratios promotes an ozone decrease of up to ∼10% (max. -30 ppbv O3) for high water vapour mixing ratios, low temperatures and enhanced sulphate conditions.”

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1193, 2018.

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