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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Potential of future stratospheric ozone loss in the mid-latitudes under climate change and sulfate geoengineering" *by* Sabine Robrecht et al.

Anonymous Referee #2

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This is a well-designed study that seeks to examine the potential for and impacts of heterogeneous chlorine activation in the lower stratosphere on ozone in current and future climates. In particular, using global climate model projections of the future with and without geoengineering assumptions, the likelihood of chlorine activation is assessed over time and evaluated to assess future impacts on lower stratosphere ozone. Overall, I found the paper to be a valuable contribution and worthy of publication. My only significant criticism would be in the weight given to the results and their interpretation in the text throughout, as the narrative broadly glances over the limitations/caveats of the GLENS model that are relevant to the subject matter. This is not to say that important elements are ignored or simply not acknowledged. Rather, they are largely

dismissed when discussing the significance of the results. Either more evidence needs to be given to favor or increase confidence the GLENS results or the limitations of the model should be more routinely stated in context of the results.

Major points: One of the more concerning limitations of GLENS in my assessment is the apparent warm bias of the model in the UTLS (which is common in most models given their relatively coarse vertical resolution near the tropopause). In the paper, this is assessed using airborne observations from the SEAC4RS campaign, which are very good but ultimately too limited for comprehensive validation of the model. I would strongly recommend that the authors consider using high-resolution radiosonde observations to characterize the true temperature bias in the UTLS (by comparing tropopause-relative T) as it may be as high as 5 K based on the data shown and is a major source of sensitivity to the chlorine activation results. The authors do show what an assumed warm bias of 2 K would lead to, but even this number appears to be conservative in my opinion. Rather than the messaging throughout stating that the assessments in the paper are an "upper bound" to chlorine activation, I would argue that in many ways they are a lower bound. Better assessments of model biases will help to focus the messaging more on the expected likelihood and impacts of this important process.

The second limitation that I believe needs to be better addressed and highlighted is the representation of convection in the GLENS model. Climate models are often not classified as resolving (or even representing) convection well. Rather, global coarse horizontal resolution models such as GLENS are often better used to assess changes in convective environments. Dynamically downscaled climate simulations have become increasing used to study convection since it can be better simulated (and even resolved) over regions of interest by using the large-scale environments projected by the global model as input. Since this study relies on the global large-scale climate projection alone, the realism of UTLS water vapor and its variability due to convection is highly questionable. It is very likely a significantly underestimated reference point, which again is in contrast to the messaging throughout in the paper. I would like to see these points better highlighted and used to interpret the results. I'm not asking for additional analysis to respond to this point, but more appropriate messaging/discussion in the text and perceived importance or likelihood of chlorine activation.

Apart from these points on the under-emphasized model limitations, I don't have an exhaustive list of technical corrections - the text and figures are otherwise excellent.

Minor points: Page 11, line 4 - should cite Smith et al, 2017 (doi:10.1002/2017JD026831) and Herman et al, 2017 (doi:10.5194/acp-17-6113-2017) as well since this studies more extensively evaluate delivery of water to the stratosphere by convection during SEAC4RS.

Page 26, line 21 - contrary to this statement, I found very little discussion of the apparent temperature bias in GLENS in Section 3.1.

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