Response to reviewers' comments.

We thank the reviewers for their helpful and insightful comments. These are repeated below (*in blue italics*) followed by our responses (in black). Any revised or added text is in red.

Reviewer #2

Manuscript #egusphere-2025-5346 titled "The Impact of Rocket-Emitted Chlorine on Stratospheric Ozone" takes a detailed look at how chlorine emissions from spacecraft using solid rocket motors (SRM) may impact the health of the ozone layer in the stratosphere. The authors ran the WACCM6 model, nudged with MERRA-2 reanalysis data in order to represent realistic stratospheric meteorology. This methodology also allows for one to isolate the chemical impact of chlorine emissions on stratospheric ozone and perform detailed investigations on stratospheric chemical processes. The authors investigated multiple launch scenarios ranging from a modest 10x the 2019 annual launch frequency of SRM vehicles to an extreme 120x annual launch frequency. The authors show that increased chlorine emissions from the simulated scenarios leads to modest losses in stratospheric ozone that scale linearly with increased launch rates and can potentially slow stratospheric ozone recovery.

While SRM vehicles make up a smaller portion of the present-day launch fleet, this work follows the same path as previous studies which isolate and investigate specific rocket fuel types and the potential impact of emissions from these engines on the middle atmosphere. This manuscript is timely, very well written, and it adds a new piece to the evolving puzzle that is understanding the impact of this new age of space travel on the atmosphere. Overall, I believe this to be excellent work that deserves publication, but requires specific revisions prior to publication, mainly in the motivation and discussion sections.

1) I don't see much justification for the 10x/52x/120x launch scenarios. While the 52x and 120x are clearly meant to be more hypothetical, what is the likelihood of the 10x scenario? Can you provide a probability or potential date that this emission frequency could occur? SRM is not the most heavily used fuel type relative to others such as kerosene and liquid natural gas. While there are clear projections that show both kerosene and liquid natural gas fuel usage increasing, there isn't as much information about the future of SRM. In fact, some believe it will be used less. Therefore, I'd like to see a stronger argument which supports the notion that we might experience an increase in SRM launch rates such as those studied here.

Response: Our study was motivated by Revell et al. (2025), who found an increasing impact of rocket emitted chlorine in their scenario of ambitious growth through 2030. This scenario projected an increase in Cl emissions by a factor 21 by 2030. An important motivation for our study was the headline results from that paper which reported a 3.9% decrease in Antarctic springtime ozone by 2030, under an ambitious launch scenario. Polar ozone loss is driven by chlorine chemistry and controls on stratospheric chlorine are directly related to the Montreal Protocol (MP) and recovery of the ozone layer. Our study aims to focus on MP-policy-relevant emission of Cl species.

We would like to clarify that the 10×, 52×, and 120× launch scenarios used in this article are not quantitative predictions of future rocket activity, but rather a set of amplified scenarios for sensitivity analysis, based on the number of rocket launches in 2019. 2019 was chosen as the base year because a relatively complete launch record was available to represent the reference level before the rapid expansion of commercial spaceflight. Among our scenarios, the 10× is not an extreme hypothesis. Research and industry assessments (e.g. Brown et al,.2024) suggest that the total number of global rocket launches could increase by an order of magnitude or even higher (up to about 20 times) compared to 2019 by around 2030. In this context, the 10× launch scenario can thus be considered as a modest increase to explore the possible effects on stratospheric ozone of chlorine emitted by SRM. In contrast, the 52× and 120× scenarios are primarily used to explore the characteristics and potential upper limits (and linearity) of ozone responses at higher emission intensities, and do not correspond to specific future years or probabilities of occurrence.

We agree with the reviewer that SRM is not the current mainstream propellant and that there is still uncertainty about its future use trends. However, this paper does not focus on the market share of different propellants in the total launch activity, but rather on the efficiency of SRM emissions in direct injection of MP-policy-relevant chlorine into the stratosphere under unit launch conditions and its potential amplification effect on polar ozone loss. Even if they account for a low proportion of the overall emission structure, SRMs could still have a disproportionate impact on stratospheric ozone in these specific regions of efficient loss under cold conditions.

We have added the following text in the revised paper at the second-to-last paragraph of Section2:

The 10×, 52×, and 120× scenarios are sensitivity experiments, not specific projections. They are scaled from the 2019 launch inventory, which provides a consistent reference, before rapid growth, to test ozone response under stronger emissions and to examine linearity of the impacts.

This study does not aim to predict the future of SRMs. It focuses on the efficiency of SRM-derived chlorine when injected into the stratosphere. Polar ozone loss is very sensitive to chlorine under cold conditions. Even a relatively small number of SRM launches may cause a clear impact in the polar lower stratosphere.

2) Could the authors please provide additional reasoning for choosing to run the model only between 1990-2012? Almost all of the increase in launch rates occur after 2015 with the exponential growth starting in 2019. Wouldn't it have been better to run simulations through more recent years where your stratospheric chlorine and ozone levels, as well as anthropogenic emissions are more up to date? MERRA-2 is generally up to date (at least through 2024), so the data should be available. If authors believe that the simulation time-frame doesn't in fact matter, then please provide a reason why this time-frame was chosen.

Response:

The major motivation for using this specific time period was so that we could use the nudged CCM and thereby ensure realistic stratospheric meteorology. This is important to correctly model chlorine-catalysed polar ozone loss, especially in the Arctic. In line with this, we had a focus of wanting to include a cold Arctic winter in the simulations and 2010/11 is a very good example with (at the time) near record ozone depletion. In order to include 2010/11 in the time series we started the model in 1990 to allow for spin up.

Therefore, the model dates are not linked to the particular rocket scenarios studied, and indeed we are largely looking at future large increases in launch rates anyway. Our results mainly depend on the differences between two similar simulations with relatively small perturbations to chlorine loading, so small differences in the background composition (e.g. between 2010 and 2025) are unlikely to affect this quantification.

For the corresponding change in the the revised paper please see Response #2 of Reviewer 1.

3) You might consider including a baseline control case with the default WACCM emissions somewhere within the manuscript, or supplemental information. This can help readers know that WACCM is indeed producing a realistic stratosphere and seasonal ozone. Additionally, this would help show the scale of the ozone depletion caused by the rocket emissions relative to baseline conditions.

Response:

WACCM has been widely used in numerous previous studies to simulate stratospheric ozone and its seasonal and interannual variability (e.g. Solomon et al.,2016; Eyring et al.,2016; Gettleman et al.,2019; Cuevas et al., 2022, Zhu et al., 2023, Zhang et al., 2024). Relevant research indicates that this model can reasonably simulate the structure and evolution characteristics of stratospheric ozone (see Response #5 for Reviewer 1). For the revised paper we have included a control run with no rocket emissions and include column ozone comparisons in Figure 7 and new Figure S1.

The impact of the baseline rocket emissions (Rocket ×1) is small compared to the control and so we therefore continue to use Rocket1 as the reference for the changes with increased emissions. All analyses of ozone changes in this paper are based on differences or percentage changes relative to the Rocket1 scenario.

4) In the latter half of the discussion section (line 239 onward) the authors compare the magnitude of their ozone depletion from SRM chlorine emissions to that found in the Revel et al. 2025 paper. I worry that the authors may be under representing important differences in methodology between these two studies which likely play a role in the final ozone discrepancy. Previous studies have shown a relatively strong ozone response to dynamical and temperature anomalies caused by the presence of other rocket emissions, especially aerosols (i.e. black carbon). While I think the comparison in this manuscript to the Revel et al. 2025 study is interesting and informative, there needs to be stronger mention of the fact that this work does not include the important contributions of aerosol stratospheric heating and dynamical shifts which may be driving most of this discrepancy between the two studies. This may be beyond the scope of this manuscript but quantifying the individual contributions of chlorine chemistry, stratospheric dynamics, and aerosol heating on ozone would be very interesting.

Response:

We agree that methodological differences between this study and Revell et al. (2025) may contribute to the differences in the simulated ozone response. As noted above, we are focusing solely on the chlorine-ozone impact. While we agree that Revell et al. included more factors which will affect ozone, we note that our model seems to produce a smaller chlorine enhancement for similar emissions. This will decrease our modelled ozone impact.

We have revised the discussion section to more explicitly acknowledge these differences and to clarify that aerosol-induced heating and dynamical shifts, which have been shown in previous studies to exert a strong influence on stratospheric ozone, may play an important role in the larger ozone response reported by Revell et al. (2025).

The following paragraph has been added (see also Reviewer 1 Response #15):

The smaller Cly response simulated here compared to Revell et al. (2025) may reflect differences in detail of the location of the rocket emissions and the stratospheric circulation in the respective models. For example, the spread of the emissions through the stratosphere by the relatively slow Brewer-Dobson circulation, and thus their residence time in the stratosphere, will depend on the circulation which can vary between models.

This will affect the modelled ozone impact from the chlorine enhancement. We also acknowledge that the Revell et al. (2025) study includes other forcings that contribute to ozone depletion, notably circulation changes due to black carbon, which we have not considered in our runs.

5) *Line 40: this sentence reads awkward to me. Please consider rewording*

Response:

The sentence has been replaced by:

The benefits of SRMs include easy storability, high reliability, and design simplicity. However, once ignited, they cannot be turned off and are therefore typically used only in the first stage of launch vehicles. The exhaust from SRMs contains a number of compounds of environmental concern, in particular hydrochloric acid (HCl) and alumina particles.

6) *Table 1: is it possible to add an estimate year or future scenario in which these numbers of launches may occur?*

Response:

These scenarios are used to explore the model sensitivity and are not specific projections. Instead, they represent a set of idealised emission masses used for sensitivity tests to quantitatively explore the response of stratospheric ozone to changes in the scale of solid rocket motor emissions. The objective of our study focuses on process attribution and relative response rather than predicting future space activity pathways or specific time points. Indeed, by analysing the model impact for the different scenarios (including extreme cases) we demonstrate a linear response over a range of emissions, which then allows estimations of many other scenarios.

Since actual rocket launch numbers may vary significantly from year to year and strongly depend strongly on technological developments, commercial activities, and policy environments, mapping our idealised launch rates to a specific year or future scenario would introduce unnecessary assumptions and potential misunderstandings. Therefore, we choose not to assign specific years to these settings in Table 1. In the revised paper we clearly position them as idealised experimental configurations for exploring the mass–response relationship of emissions.

7) Line 100: *Please provide which fields were specifically nudged by the reanalysis data. Were only SST's, winds, and temperature nudged? Or did you nudge chemically as well?*

Response:

In the specified dynamics (SD) configuration of WACCM, horizontal winds and temperature are nudged toward meteorological reanalysis fields. Chemical variables are not included in the nudging process.

The following sentence has been added:

In this study, WACCM is nudged to MERRA-2 reanalyses (Hurrell et al., 2008; Feng et al., 2013; Molod et al., 2015) to ensure realistic lower-stratospheric meteorology, which is critical for accurately simulating chlorine-induced polar ozone loss. The nudging is applied only to winds and temperature (Gettleman, et al. 2019); the chemical species evolve freely.

8) *Figure 9-10 caption: I would mention that the contour scale changes amongst the panels*

Response:

The sentence: 'Note that the contour scales differ among the panels' has been added to the caption of Figures 9 and 10.

9) *Table 2: These are annual averages, correct? If so, please state that in the table or table caption. Could you please include the error range which represents the year-to-year model variability in ozone alongside your ozone depletion values? Or please provide a calculation of statistical significance relative to the model variability. This can help show whether the ozone depletion values are outside model noise or not.*

Response:

For near-global region, the numbers are annual averages and for Antarctic region, they are SON averages (see the first column).

The error range is now added into Table 2.

New References (included in revised paper):

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