## 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 #1**

*This is an interesting, timely and well written article. As rocket launch rates increase, understanding the role of rocket exhaust on stratospheric composition is crucial, since the stratospheric ozone layer is a vulnerable, recovering part of the planet. You report on stratospheric ozone changes under different chlorine emission scenarios from rocket launches. The conclusions drawn are supported by the simulations they present, but much more context and confidence could be provided by model evaluation. I also think the methods section requires clarification. I have compiled my thoughts below, comprising major revisions I think should be addressed before publication.*

1) *Why did you not include other pollutants? This is an especially important question for NOx, which Ryan et al. (2022) found to be a more important factor in stratospheric ozone depletion than SRM-derived Clx and alumina.*

**Response**:

In this study, we deliberately focus on chlorine derived from solid rocket motor (SRM) exhaust, with the aim of isolating and quantifying the role of SRM-derived Clx in polar stratospheric ozone loss, where chlorine-driven heterogeneous chemistry plays a dominant role.

We acknowledge that NOx is an important driver of stratospheric ozone loss and has been shown in previous studies to exert a strong influence, particularly at global and mid-latitude scales. However, the sources, chemical pathways, and spatial–temporal characteristics of NOx differ substantially from those of SRM-derived chlorine. Including NOx in the same set of sensitivity experiments would complicate the attribution of ozone changes and obscure the specific role of chlorine emissions from SRMs.

In addition, the relative importance of Clx versus NOx depends strongly on altitude, latitude, and season. Our study specifically targets the lower stratosphere in polar regions, where chlorine activation on polar stratospheric clouds is a key process, and where SRM-derived chlorine is expected to have a more direct and possibly disproportionate impact.

For these reasons, NOx emissions were not included in the present study and are

considered beyond the scope of this work. A comprehensive assessment of the combined effects of SRM-derived chlorine and NOx on stratospheric ozone would be an important topic for future research.

The following text has been added to the manuscript at the end of Section 1:

Other species including NOx and SRM-derived chlorine have different sources, chemistry, and spatial–temporal patterns. Adding them to the same sensitivity experiments would make it harder to attribute ozone changes and to isolate the effect of SRM chlorine. The roles of Clx and NOx also vary with altitude, latitude, and season. This study focuses on the polar lower stratosphere, where chlorine activation on polar stratospheric clouds drives ozone loss and where SRM chlorine may have a stronger impact.

2) *Your choice of time period needs further justification and explanation. Why did you choose to simulate 1990-2012? During the early 1990s, the Montreal protocol was just gaining momentum and stratospheric ozone holes were still frequently very large. In addition, the relevant rapid space industry expansion is a modern post-2012 phenomenon. I understand that you wanted to use reanalysis meteorology, which necessitates going back into the past, but it would be great to elaborate on the justification of your time period choice, and why you didn't choose to go from 2025 or 2024 back.*

**Response**:

The major motivation for using a specific time period was so that we could use the nudged CCM and thereby ensure realistic stratospheric meteorology. This is an important consideration to correctly model chlorine-catalysed polar ozone loss, especially in the Arctic. In line with this, one focus was to include a cold Arctic winter 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. This date would also allow simulations of 30 years or so if we had continued to the present day. However, due to the cost of the model simulations and the large number of sensitivity simulations, we stopped the simulations at 2012. This length of model run allows us to diagnose the desired chlorine impact.

The following text has been added to the manuscript at the last paragraph of Section 2:

This is essential for simulating chlorine-driven polar ozone loss, particularly in the Arctic. The winter of 2010/11 was included because it was an exceptionally cold Arctic winter with near-record ozone depletion.

This length of model run is sufficient for us to diagnose the targetted chlorine impact for a range of polar conditions and reasonable background chlorine

loading. Although stratospheric chlorine is decreasing, and will continue to do so, we use differences in our model runs to diagnose the relative impact of additional rocket-based emissions.

3) *Moreover, it is unclear to me whether your simulations for 1990-2012, with 2019 rocket emissions, have underlying changes in anthropogenic emissions - especially ozone depleting substances - that were occurring during 1990-2012. You mention that greenhouse gas levels are constrained to 2020 levels, but the inclusion of other anthropogenic emissions needs clarification. ODS emissions have changed significantly between 1990 and 2012 following the Montreal Protocol.*

**Response**:

The chemical boundary conditions do indeed need some correction and clarification and we apologise for the confusion. Our plan had been to use constant 'present-day' (2020) surface mixing ratios for ODSs and GHGs. However, we later discovered that the nudged version of WACCM was automatically picking up time-dependent boundary conditions. Hence the model runs have used halogen levels which are slowly decreasing. This has been clarified in the revised paper in Section2 and a figure illustrating this (Figure S1) has been included in the new Supplementary Information

4) *You say that you "focus solely" on the Clx emissions but can you please clarify whether that means you include only Clx emissions from rockets, or include all emissions but only vary Clx/only focus on changes caused by varying Clx.*

**Response**:

We clarify that in this study only chlorine-containing emissions from solid rocket motors are included in the model. Other rocket-emitted species, such as NOx, BC, alumina are not included. The phrase "focus solely on Clx emissions" was intended to describe this idealised experimental design, which is used to isolate the chemical impact of rocket-derived Clx on stratospheric ozone.

We have replaced the sentence 'Here we focus solely on the Clx emissions which are produced by solid fuel. by:

Here we consider only Clx emissions from solid rocket motors, and do not include other rocket-emitted species.

5) *To build confidence in your choice of model and reanalysis data, validation plots of stratospheric ozone are needed. This would provide strong context especially to your discussion of interannual variability. How well does your model do at capturing observed total column ozone and vertical profiles in the Arctic*

*and Antarctic? Without this, there is no way to assess the significance or uncertainty in the results you present. So, I think an extra "control" simulation is needed with realistic emissions for 1990-2012 to allow for model evaluation.*

**Response**:

We agree that it is important that results are obtained with a validated and well-evaluated stratospheric model. Here we are using the US NCAR Whole Atmosphere Community Climate Model (WACCM) which is very well established and widely used internationally in the community (e.g. Solomon et al.,2016; Eyring et al.,2016; Gettleman et al.,2019). Moreover, the WACCM-SD (reanalysis driven) configuration adopted in this paper has been widely used in studies of stratospheric ozone, and its ability to reproduce the total ozone column, ozone vertical structure, and interannual variation in the polar region has been systematically evaluated and verified in a large number of existing studies.

Several previous studies have shown that WACCM-SD can well characterize the spatial distribution, vertical profile, and interannual variability driven by dynamic and chemical processes in the Arctic and Antarctic stratosphere, and these studies often evaluate model performance by using satellite observations (e.g. Cuevas et al., 2022, Zhu et al., 2023, Zhang et al., 2024). Our paper aims to build on these published studies rather than repeat basic model evaluation. To illustrate that our implementation of WACCM performs as expected based on the literature, we have added comparisons of column ozone to Figure 7 (and additional text) in the main paper and in the new Figure S1 in the Supplementary Information.

The following text has been added to the third paragraph of Section2:

WACCM is very well established and widely used internationally in the community (e.g. Solomon et al., 2016; Eyring et al., 2016; Gettelman et al., 2019). Several previous studies have shown that WACCM6-SD can well characterise the spatial distribution, vertical profile, and interannual variability driven by dynamical and chemical processes in the Arctic and Antarctic stratosphere, and these studies have evaluated model performance by using satellite observations (e.g. Cuevas et al., 2022; Zhu et al., 2023; Zhang et al., 2024).

6) *Line 38: "the hydrocarbon" probably should be "hydrocarbons".*

**Response**: Corrected.

7) *Line 46 onwards: The discussion of Ryan et al (2022) needs some refinement: that paper examined all rocket pollutants, not just NOx, and in addition, it would be fair to say they found launch NOx "relatively less important", but not "not important" as you state. It is worth pointing out here that the ozone impacts in*

*that paper were most significant in the upper stratosphere.*

**Response**:

The text has been replaced by: Ryan et al. (2022) used information on 2019 rocket launches and re-entry events to investigate the impacts of multiple rocket-emitted pollutants, including NOx, on stratospheric ozone. They found that ablative NOx production during re-entry can have a significant effect on stratospheric ozone, while NOx emissions from launches were relatively less important although not negligible. The ozone impacts reported in their study were most pronounced in the upper stratosphere.

8) *Line 81: It is worth putting the longitude of Korou too.*

**Response**: Done.

9) *Line 81: Link to table 1 here as this is where you outline the emission inventory scaling year on year.*

**Response**: Done

10) *Table 1: if you're keeping the focus as Clx emissions only, it would be helpful to add a column to the table detailing the magnitude of the Clx emissions (i.e. Tg Clx). This would help the reader understand quickly the extra Clx in each simulation.*

**Response**: A new column "Mass of rocket emitted Clx" has been inserted into Table 1.

11) *Section 3.1: You show Cly increases. Could you elaborate briefly (in the text is fine) on which species dominate the Cly changes in your rocket scenarios?*

**Response**:

Cly represents the sum of inorganic chlorine species in the stratosphere, including both reservoir species and reactive radical forms. Once chlorine is emitted from rockets, the Cl atoms will be partitioned among the Cly species in response to the background stratospheric chemistry and meteorology. Hence, the additional Cl will adopt the usual model Cly partitioning. Thus, in the rocket emission scenarios examined here, the simulated increase in Cly is largely associated with enhanced HCl concentrations, which account for most of the total response. In addition, increases in reactive chlorine, particularly ClO, are also apparent at high latitudes

in the upper stratosphere (approximately 1–3 hPa). This clarification has now been added to the end of Section 3.1 of the manuscript.

The simulated increase in chlorine becomes partitioned between the Cly species based on the background model chemistry and meteorology. Hence, the increase in Cly is dominated by enhanced HCl concentrations, while increases in reactive chlorine, particularly ClO, are also evident at high latitudes in the upper stratosphere (~1–3 hPa).

12) *Line 137: "These are the regions where chlorine chemistry is expected to have an impact on ozone" - this sentence (and possibly the one after it too) sounds like it needs a reference.*

**Response**:

Several relevant references have been added to support the following sentence:

These are the regions where chlorine chemistry is expected to have an impact on ozone (e.g. WMO, 2019; Farman et al., 1985). The upper stratospheric loss occurs through the catalytic cycle involving ClO + O (Stolarski and Cicerone, 1974), while loss in the polar lower stratosphere occurs through reactions involving ClO + ClO (Molina and Molina, 1987) and ClO + BrO (McElroy and Salawitch, 1986).

13) *Section 3.2: in your discussion of ozone decreases (e.g. of a certain amount of ppb, or DU), it would provide great context to readers slightly less familiar with stratospheric ozone if you included what these changes amounted to as a percentage. (You do this at some points but it could be more widespread.)*

**Response**:

The percentage changes have now been added into Section 3.2 especially for the content of Figures 2 and 4.

14) *Figure 8: one advantage of your simulation period is that there were some significant ozone holes in there. When you talk about interannual variability, you have the opportunity to contextualize the significance of the ozone depletion due to rockets (and each year's meteorology) against the size of the each year's actual ozone hole. This could either be as a percentage in the text, or incorporated as a timeseries into Figure 8.*

**Response**:

The percentage contributions corresponding to each ozone depletion value have been added.

15) *Line 250 onwards: could you hypothesize further on why you think you model a smaller Cly concentration change than Revell et al (2025)? What differences in deposition, chemical scheme or other model factors might give rise to this? This seems like a pretty significant difference in conversion to reactive chlorine, which it would be good to understand (and again make me wonder how well your model performs relative to observations at converting other ODS to reactive chlorine and simulating past ozone holes).*

**Response**:

WACCM is a well-established community model with a strong track record in simulations of stratospheric ozone (e.g. Cuevas et al., 2022, Zhu et al., 2023, Zhang et al., 2024). The model simulates well the destruction of ODSs and the production of inorganic product chlorine (e.g. Villamayor et al., 2023). As we only have our results to analyse in detail we cannot be sure of the differences in our results compared to Revell et al based on the limited information available in their published paper (i.e. their Figure 3c). We do see a smaller apparent increase in stratospheric Cly (e.g. their Fig 3c versus our Figure 1) for similar scaled emissions. Thus we have aimed to be careful to document our methodology, especially the processing the Cl emission rates. If we consider the stratosphere as a 'box', then the additional Cly loading should simply depend on the emission into that box and the transport out of that box. There are many chemical details (e.g. partitioning within the conserved Cly family) which are not relevant. If both models are using consistent stratospheric emissions that would leave the stratospheric transport timescale (or residence time) as a possible source of differences. Further investigation of the differences would best be achieved by a community model intercomparison effort.

We have expanded the discussion to further hypothesize potential reasons for the smaller Cly response simulated in this study compared to Revell et al. (2025) along those lines. The following lines have been added:

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 slow Brewer-Dobson circulation, and thus their residence time in the stratosphere, will depend on the circulation which can vary between models. Further diagnosis of this is beyond the scope of this paper and would need formal model-model intercomparisons.

**New References** (included in revised paper):


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