## 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.

**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):


Cuevas, C. A., Fernandez, R. P., Kinnison, D. E., Li, Q., Lamarque, J.-F., Trabelsi, T., Francisco, J. S., Solomon, S., & Saiz-Lopez, A. (2022). The influence of iodine on the Antarctic stratospheric ozone hole. Proceedings of the National Academy of Sciences, 119(7), e2110864119. https://doi.org/10.1073/pnas.2110864119.

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