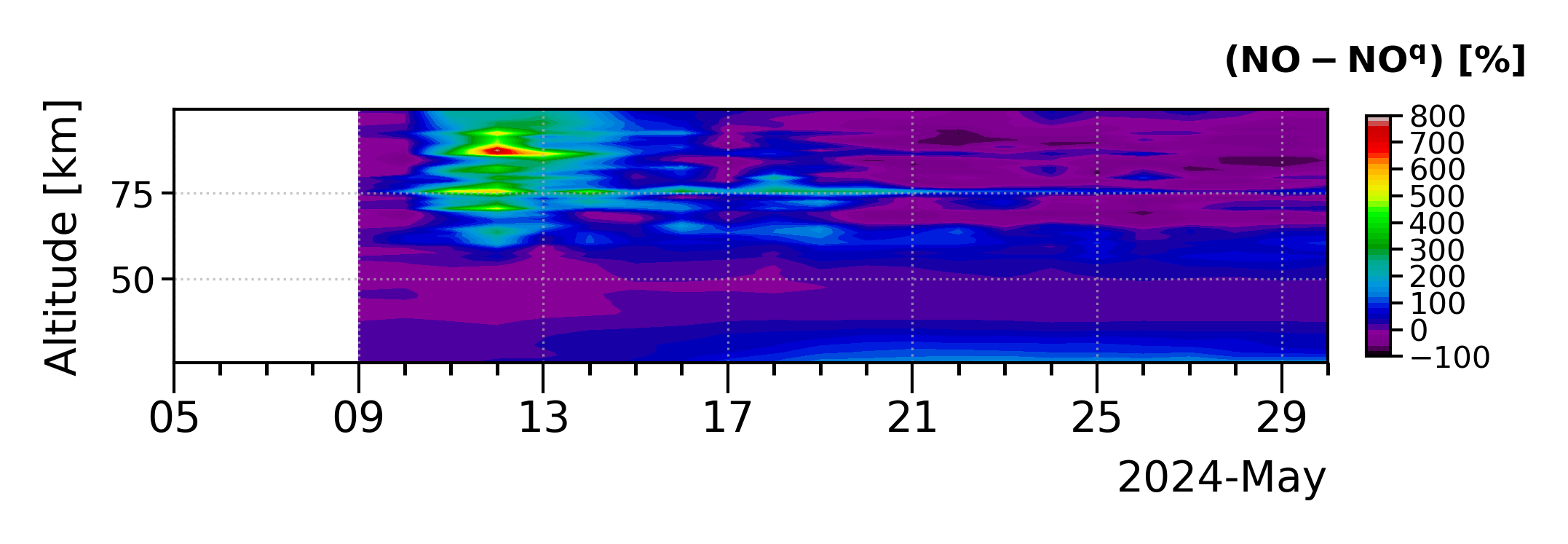
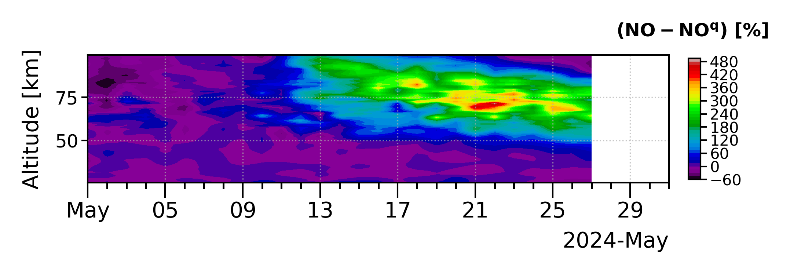
This paper uses MLS observations to investigate the response of ozone in the polar regions of the middle atmosphere to two solar storms that occurred in May and June of 2024. For both storms, it finds no response in the northern hemisphere. For the May storm a large relative decrease around 75 km in the southern hemisphere is detected, but not in June. The reason given is that the total flux of energetic particles is different. The authors suggest a small depletion about 90 km after May 10 in the SH is from a temperature increase rather than directly related to EPP at those heights and NOx production.

The authors thank the reviewer for the careful reading and the useful suggestions to improve the paper.

While the paper contains some new results, I do not think authors have reached substantial conclusions. We improved the conclusions to better emphasize the results of the paper.

They do not calculate the total ion pair production rates from these storms and show that the May storm is different from the June storm or that EPP occurred at a significant level in the stratosphere.   
We have added in section 2.3 and Figure 3 AtRIS (Atmospheric Radiation Interaction Simulator) computations of the ionization rates caused by the SPEs of May and June, and incorporated them into the discussion section.

They do not provide an explanation for the hemispheric asymmetry in the response except to state the seasons are different. We have added more explanation about the asymmetry in the response of the ozone in both hemisphere. Mainly the asymmetry in the sunlight availability changes both the background composition during quiet times and the life time of ozone depleting species created during the event. Both of them cause a different response of ozone following the events. The following figure shows the relative difference of NO from the ACE-FTS instrument for the event. It clearly shows that the evolution of NO is very different in both hemispheres. Which can then impact the response of ozone.



NH

SH

In addition, Figure 4 shows a larger increase (+30%) in ozone at 85 km than the discussed depletion (-10%) at 95 km, but it is not mentioned at all in the paper. We have added supplementary discussion about the rise of ozone at 85 km (see Figure 5 now). Despite the fact the ozone above 80 km rises one day before the peak of the event, those increased ozone levels decrease after May 11th going from +30% down to + 5% - +10% on May 17th.

The authors should endeavour to better connect the different ozone responses to the different storm characteristics, provide a more complete explanation of the hemispheric asymmetry and distinguish between the chemical and dynamical impacts on ozone. We have modified the text in both the results description and the discussion/conclusions section to describe the response of ozone relative to the variations of precipitating particle flux as well as to the ionization rates computed from GOES observations. Moreover, as stated above, we have added more explanations on the hemispheric asymmetry.

It is certainly unclear to me if the 10% reductions in ozone above 90 km are caused by EPP or simply dynamical variations (random variability or the seasonal cycle). By comparing the observations from 2024 with the 3 previous years at the same period (see Figure 7), the decrease in ozone at around 90 km following the May storm is a feature unique to the year 2024. However, we note that it is difficult to determine whether the decrease is only due to the effect of the storm or to random variability.

The paper really needs to show that the differences are statistically significant - perhaps comparisons to the previous May/June period would support the position that ozone is in fact responding to the particle inputs.

We have added comparison of the observations between May and June 2024 with the observations during the same periods of the three previous years (2023, 2022, 2021) (see Figure 7). For those periods, we discuss their respective geomagnetic activity and solar conditions (whether SPEs occurred). For the ozone, this comparison shows that the depletions observed following the events of May and June in the mesosphere are related to the SPEs. For the increase in the temperature above 80 km, it is also a unique feature of the year 2024 and does not seem to correspond to seasonal variability. Nonetheless, as for ozone at similar altitudes, we are not able with MLS data alone to determine whether it is caused by particle precipitation or by changes in the dynamics.

Below are minor comments that it would be good to address.

Title: The title does not reflect that this is a comparison of two storms.

We have added the June storm in the title.

Abstract:

The term "solar energetic proton (SEP) event" seems to mix up solar energetic particle (SEP) and solar proton event (SPE). I think he latter term is more appropriate here. We have changed the SEP events into solar proton events (SPE) everywhere in the text.

"and reaches a maximum of 60%" - it would be helpful to say what altitude. We have added the altitude of the maximum depletion.

Make it clear that the signals are only present in the southern hemisphere. We have made it clearer in the text as well as in the caption of the figures.

Introduction:

"predicted in 2025" - a reference would be good to add here. In fact, it's generally accepted that the peak in storms occurs on the declining phase. We added a reference for the prediction of the solar cycle 25, and we also emphasized that the statistics for occurrence and intensity are maximum during the descending phase but that they start rising when approaching the maximum of solar activity.

Correct the referencing style for inline reference - put them in parens. We have corrected the style of inline references by adding parentheses to them.

Be consistent in your definition of SEP - is it "solar energetic proton" (line 2) or "Solar Energetic Particle" line 18, with the definition repeated on 112. We changed the definition of SEP into Solar Proton Event (SPE) as suggested in a previous comment.

Duplicate definitions of EPP on 31 and 44. The duplicate definition was removed.

Suggest: "Thus, \*a\* result of EPP is a decrease in" This sentence was part of a paragraph that was rephrased.

"However, strong evidence of SEP directly depleting stratospheric ozone are scarce" - I don't think this is true. See doi:10.5194/acp-11-9089-2011 where ozone is depleted immediately during the SPE down to 10 hPa (i.e., into the upper startosphere. Also the many papers led by Charlie Jackman (e.g., https://acd-ext.gsfc.nasa.gov/People/Jackman/Jackman\_2004.pdf). The evidence is strong, but large SPEs themselves are rare. We have expressed this fact in the text and added the reference.

Data:

Please include some discussion of the vertical resolution of the MLS data in the stratosphere and MLT. Also, averages are presented from 60-90˚ Does MLS measure all latitudes uniformly, or is a simple average of all profiles above 60˚? If the latter, does the sampling change over the period being analysed and what changes might that create? Also, make it clear you are using MLS temperatures - we only see them in the analysis in Figure 4.

We have added more information about the vertical resolution of the MLS instrument, which typically has a 2 km resolution in the stratosphere and 4 to 5 km in the MLT. For the averages, we indeed take a simple average over all profiles and the sampling of the MLS is not perfectly uniform in latitude. Between 60° and 82°, slightly more observations are made at higher latitude than at lower ones. The daily mean latitude coverage is -72,5° while the median is -72,9° so the mean ozone profiles are slightly biased by higher latitudes observations (if the sampling was uniform, the daily mean latitude would be -71°). Moreover, the daily coverage of the instrument in both latitude and longitude is not changing from day to day because of the helisynchronous orbit of the AURA satellite.

Results:

What do you mean by "but not always" - this is vague. Please state when, in this 6 month period, you have an SEP and no evidence of ozone depletion. + In regard to Figure 1 the authors state "Those variations on smaller time scales are not linked to geomagnetic storms illustrated by high peaks of geomagnetic activity in the bottom panel, in any of the ozone layers." To me it looks like they are. Especially in the SH, and stated by the authors on line 120.    
This paragraph was rephrased to make it clearer for the reader. In addition, we have added plain and dotted vertical line in Fig.1 that mark SPEs and geomagnetic storm respectively. In the text, we note that in the NH, the short-term variations at the beginning of the year do not appear to be caused by those events. While in the SH at the same period, there is not a large amount of ozone at very high altitude that can be influenced by particle precipitation.

"because they originate from the same region" - Can you provide some more evidence that the same active region erupted to create these events. More evidence was added to support this.

Figure 3 is just too small to distinguish the variations. Please make the Y scale much larger. The colours of the lines do not match the legend. We have increased the size of the y axis in the figure (now, Figure 4) and corrected the colours in the legend.

Can you provide evidence that the temperature variation is not just seasonal dynamical variability? Perhaps show 2022 and 2023 for comparison? As mentioned above, we have now added Figure 7 to compare the observations of the temperature between May and June in 2024 to the observations during the same period for the 3 previous years.

149/Figure 4: Is this NH or SH? Presumably the latter but it's not clear. Yes this was added to the caption of the figure. Same for Fig.5.

223: "In both cases, the spectrum of solar protons was hard enough to produce ionization in the upper stratosphere." This has not been shown and does not describe exactly how much might have been produced?

We have added computation of the ionization rates (Fig.3) during both events with the Atmospheric Radiation Interaction Simulator (AtRIS) with both Galactic Cosmic Ray (GCR) and energetic solar protons from GOES. The results of the simulation do show that the ionization rate increases significantly down to 30 km at least for 2 days during each event.

225: "This absence of response in stratospheric ozone could be explained by the season again." How, and why would this impact both hemispheres? i.e., both in winter and summer. Indeed, this part was confusing in its writing. We have removed it from the text, but added some explanations about the hemispheric asymmetry in the discussion.  
  
Conclusion:

"In the MLT region... the decrease in ozone are not linked to catalytic reactions with HOx and N Ox"  
If energetic electrons precipitated, they would produce HOx in the MLT (the MLT includes the region below the mesopause) and would therefore destroy ozone.   
214: "In the MLT region...unlike in the mesosphere ..." Since the M in MLT is mesosphere this statement does not make sense. Better to state the altitude ranges you are talking about explicitly. Yes, this was changed. The use of MLT was used with more care and we added the altitude range at several places in the text to make it more understandable.

No evidence has been provided that there was a change in the circulation or that it impacted O and H transport. We removed this part of the conclusion since it was too speculative.

Figure 1 caption - "ration" typo Corrected.