Review of "Ozone decrease observed in the upper atmosphere following the May 11th 2024 Mother’s day solar storm" by A. Winant et al.

The authors analyse MLS observation in early 2025 for examining the impact of geomagnetic storm on May-11 2024 on ozone in the middle atmosphere and MLT region. They find additional ozone depletion related to the event for the secondary and tertiary ozone layer in the Southern hemisphere. In addition, they study the solar proton event in June 2024 for comparison. None of these events caused long-lived ozone depletion in the stratosphere.

Through its worlwide observable auroras, the geomagnetic storms of May 2024 draw broad public attention. But also from a scientific perspective, the geomagnetic storm in May 2024 is interesting as it exhibited the second strongest Ap-index recorded, and ranks among the strongest storms in terms of the Dst-index. The paper is one of the first publications to study specifically the impact of this storm on the neutral atmosphere. The paper is timely and of general interest.

The paper is generally written clearly. The results are mostly presented concisely. There are some paragraphs which are not written carefully and which should be strongly improved.

Thank you for your in-depth reading and your comments that helped us to improve the paper. The corrections in the text are in red for the removed sentences and in blue for the added sentences. Our answers are given below.

I have a few comments to the scientific content:

1. The authors use two methods to derive the ozone changes due to the particle impact, comparing the time series after the event to a) a lowess filtered time-series and b) 5-day average before the event assumed to be a quiet period. The authors completely dismiss a comparison between the two methods, and just from the inspections of their figures, they seem to show quite a different behaviour. At the end, the autors prefer to draw their conclusions from the second approach (Fig. 4). The authors should discuss the different approaches.

We have added more explanations relate the results from the two methods both in the results as well as in the discussion and conclusion.

2. Inspecting Fig. 4 deltaO3, I am a bit puzzled by the persisting vertical structure of the signal seen at 80km (positive changes above, negative changes below). As this is just at the minimum between the secondary and tertiary ozone layer, a small vertical shift compared to the reference could also explain that pattern and the rather high values of the ozone change as the small reference goes into the denominator. So I doubt if the derived values are really significant. Absolute values would also clearer show the impact in the tertiary layer at 75 km.   
We have added the graphs with the absolute values in the figure for the May and June events. Moreover, for the large relative differences observed at 80 km, a sentence has been added to the text mentioning the effect that small values could have on the relative difference. In addition, the description of the observations in the secondary layer has been changed to highlight the observation at 84 km, where the relative difference is positive.

3. Fig. 4 bottom shows the temperature change in the SH. First, showing percentage change for the temperature is not really meaningful in my opinion and I would ask you to show the absolute changes. Perhaps I misunderstood, but the explanation you give for the apparent temperature increase seems to me wrong: as the radiative damping time at these altitude is a few days only, any temperature signal caused by diabatic heating would be lost after some days. Secondly, the additional NO would rather increase the cooling rate, at least at the top level. So, probably changes in the dynamics, i.e. a descent of air masses is the reaseon for the temperature increase. But it is essentialy impossible to differ between adiabatic heating related to start of the seasonal descent and dynamical changes related to the event without specific experiments with a model of the MLT region. From Jia et al., dynamical changes related to elevated Ap-levels could be expected at about 100km and not at 80 km, contrary to your analysis.   
As suggested, we added panels with the absolute temperature profiles to the figures. Secondly, we removed the sentence that was referring to the processes involved in the heating that we observed in May as it was to speculative. In the last section, we did not want to us the results from Jia et al. to explain the decrease of ozone between 70 km and 80 km, but rather in the secondary layer above 90 km. We have changed the text to make it more clear.

4. The main finding seems to be that a major geomagnetic storm of this strength does not automatically lead to significant changes of ozone in the stratosphere. The discusssion in the conclusion paragraph is in my opinion a bit unclear. Specifically, as for the EPP indirect effect NOy transported from the mesosphere into the stratosphere is important, at least a discussion of observations of NO (eg. ACE/FTS) for this event would be helpful.

We agree that observations of NO would be interesting for the discussion but the addition of new data from a instrument with which we have not worked yet is difficult to do within the given time. We have reformulated this paragraph to make it less speculative for the involvement of NOx in the stratosphere for this particular event.

Minor comments:

First I have two general remarks: Many of the references the authors give do not point to the original papers, in some cases reviews or papers based on the original literature. So please check the references in this respect. Some typesetting (no italics for chemical substances, braces for citations) must be corrected, as you should a consistent tempus.

L2  SEP: you are using the abbreviation SEP here as solar energetic proton event, in L18 as Solar Energetic particle event, and in L112 just as solar energetic particles. Please be consistent.   
Indeed the labeling was not consistent, this has been changed. Every occurrence is now labeled as ‘Solar Energetic Particle’ event.

L4  AURA/MLS instrument :Added

L6  and at other places: O3 no italics: We changed for all occurrences of O3 and other chemical species.

L6  upper part: perhaps MLT? : Changed

L12 please have a consistent tempus in this paragraph: The tempus was changed to be more consistent.

L15 large perturbations : reference : the sentence before was slightly modified and a reference was added.

L16 deacrease : Corrected

L16 reference here and at many othe places in braces: Braces added to all references.

L17 coronal mass ejections (CMEs): The typo was corrected

L21 references : References were added to the text.

L22 typo Dst : We corrected the typo

L27 latitudes : corrected

L33 NOx is? : changed to Nitrogen oxides NOx are  
L34 define lower thermosphere: we added (90 to 100 km)  
L35 Those species: sentence is wrong. Only NOx is long-lived in the winter mesosphere, and in the stratosphere only in the reservoire gases as NOy. HOx is shortlived everywhere.   
L35 In the presence ...: not correct, as in the MLT region there is no polar vortex as its is found in the stratosphere. Here elevated stratopause events are important.  
L41 Thus ... not generally true (self healing effct). Generally, many references in this paragraph are erratic as the presentation of the processes. Please rewrite.

Answer for L33, 34, 35, 41: As asked by the reviewer, we heavily redacted this paragraph of the introduction for more clarity and added references to original papers.

L60 reference : We added the reference: Schwartz, M.: MLS/Aura Level 2 Ozone (O3) Mixing Ratio V005, https://doi.org/10.5067/AURA/MLS/DATA2516, 2021

L63 coverage is only to 82° (L60)? : Yes, thank you for bringing this to our attention, the orbit of the MLS satellite limits its observations to 82° of latitudes in each hemisphere. We changed the 90° in the text to 82°. The 90° comes from the latitude filtering used in the code that is set to 90°.

L81 is this your strategy?: This is the approach we use. Changed in the text.

L86 reference to the lowess method? exact parameters? : The reference to the original paper was added with the value of the ‘frac’ (f = 0.25) parameter used to smooth the data.

L106 completely removed? : This was changed into : ‘depleted from around 2.5 ppmv to below 1 ppmv’

L112 Delete or rewrite this paragraph. How much ozone change do you expect from the particle flux and energy in the middle stratosphere? As suggested, we deleted this paragraph from the text.

Fig.1 improve color coding for less barrier : Figure 1 was changed to reduce the visibility of the grid. This was done for all images.

L127 because .... delete. Depends on the lifetime of the active region : The sentence was slightly changed, and we have added a reference that shows that the region that caused the SEP of May was still active in June after it had done one full rotation around the Sun.

Fig.3 lines are hard to differ. : The plot was updated with higher contrast to help better differ the different lines.

L155 computed with O3 vmr??? : As we have changed the figure that text was also changed.

Fig.4 SH? : Indeed, it is in the southern hemisphere. We added to the caption of Fig.4 and Fig.5

L156 whats some ozone? This sentence was removed since it was too vague and the exact ozone variations are described below.

L178 here EPP is EE precipitation? : Here we mean both protons and electrons. Changes to energetic particle precipitation in the text.

L177 please show that quantitatively   
L178 Joule : Done  
L179 energetic particles loose their energy by inelastic collisions (causing partly ionization), ending up in some heat, excitation, radiative loss. Joule heating is caused by ion drag and not directly related to the EPP.

Answer to L177, L178, L179: This has to do with the scientific comment #3. This part of the text was revised.

L183 slow ozone depletion??: We clarified this sentence in the text.

L209 is possible?: We corrected the tempus

L222 This paragraph is not written carefully and somwhat speculative whithout enlightening. What is a quick response, what is hard enough, and how much ionization do you need to see an effect, how do you define upper stratosphere (60km??), etc.. Yes, the season is not favourable for an impact in the SH stratosphere because of the too short lifetime of NOx in the still sunlit mesosphere. Further analysis, for example regarding the descent, needs more observation of long-lived tracers and very specifically of NOy, or just applyication of an atmospheric chemistry model like WACCM.

We strongly reformulated this paragraph to remove the speculations on the effects of NOx for the events in question and keep our conclusions to the observations.