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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Middle atmospheric changes caused by the January and March 2012 solar proton events" *by* C. H. Jackman et al.

**Anonymous Referee #2**

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Major comments:

The authors use a 2D model to assess the impact of the SPEs on the atmospheric composition. This has limitations since the dynamical variability in such models is artificially low. For example, $NO_x$ descending in the polar vortex experiences excursions out of the polar night zone and is photochemically destroyed. Such transport aspects are not captured in 2D models in spite of eddy diffusion parameterizations. So a 2D model overestimates the amount of $NO_x$ descent.

Since the SPEs studied in this paper produced a significant amount of ionization directly in the upper stratosphere and lower mesosphere the transport limitations are not critical for short duration *in situ* impacts. However, the assessment of the longer



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term impact of the SPEs on the ozone will be negatively impacted. This is apparent in Figures 13, 15 and 16 where the persistence of the $NO_x$ anomaly in the 2D model is much higher than in the observations. A 3D model nudged with observed winds or even just the temperature (as the fast balance adjustment processes will pull the large-scale winds towards those of the observed state as well) would do a much better job capturing the observations.

The positive and negative anomaly superimposed on the SPEs $HO_2$ signal seen in the MLS data presented in Figure 6 indicates that there is an additional mode of dynamical variability above 65 km in SH polar region that the 2D model fails to capture.

The penetration of $NO_x$ anomalies into the stratosphere shown in Figures 17 and 18 and the associated ozone loss cannot be treated as realistic. Any quantification of medium-term impacts on the stratosphere using the 2D model is a dubious proposition. There is no indication in the manuscript why a 2D model was used as opposed to a chemistry GCM. The authors need to justify their choice of a 2D model for this study as they are comparing the results to observations. The chemistry analysis is not sufficiently novel by itself to merit publication. For example, there is no discussion of the lack of ion chemistry in producing the correct $HNO_3$ levels in the stratosphere. Discussion of chemistry model limitations should be included as well.

Minor comments:

Figures 1-4 should be combined into one four-panel figure to save space. There will not be any loss of readability.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23251, 2013.