**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Influence of a Carrington-like event on the atmospheric chemistry, temperature and dynamics" *by* M. Calisto et al.

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Answers to referee #1

We thank the referee for the valuable comments which are addressed below.

Comment 1: it is not clear how the ensemble simulations were generated: did the authors perturb the initial conditions or the forcing?

Authors reply: We added the following text to the manuscript: The ensemble members were generated by small (-0.1. . . +0.1%) perturbations of the CO_2 mixing ration during the first month of the model run.

Comment 2: The current simulations apply to equinox. Have the authors considered studying a similar event during boreal winter? Wouldn't one suspect that the residence time of NOx in the polar vortex be much longer during winter, extending to springtime?

Authors reply: We have added the following text in the discussion part of the manuscript: No, we have not considered other dates. However, we agree that the impact could be large if the event would occur during earlier winter.

Comment 3: The discussion in the Figures 2-6 does not explain what the "change" is, that is, compared to what? Also, the statistical significance at 95 % needs to include a mention of what statistics has been used (I assume a t-test), as well as how many degrees of freedom were assumed. I strongly suggest that the figures be redrafted using the same range in each axis, otherwise it is difficult to make simple visual inferences.

Authors reply: The Figures 2-6 show zonal mean ensemble responses calculated as a relative deviation of the experimental run from the reference run. The Student's T-test has been applied for the statistics, i.e. the TM_TEST function from IDL has been used. In general, the degrees of freedom are equal to N+M-2 where N, M is the number of groups members. In our case N=M=10 and df = 18. We have redrafted the figures in the manuscript.

Comment 4: The differences shown are zonal mean differences, which are interesting and important in their own, but do not include zonally asymmetric changes that instead average out in the zonal mean. It would be useful to know how important those are, like showing a simple difference of the zonal standard deviations.

Authors reply: We will produce maps with the zonally asymmetric changes.

The zonally changes due to the Carrington event to odd nitrogen is negligible compared to the latitudinal changes. The changes go up to a maximum of 5 % in the northern hemisphere and in the southern hemisphere (figure not shown).

Comment 5: Figure 6 is interesting but I suggest showing in addition also the differ-

ence of sea level pressure, which is more direct measure of the changes in AO.

Authors reply: We will add a plot showing the sea level pressure changes for the northern hemisphere. The changes in sea level pressure in the NH show an increase of more than 200 Pa (see Fig. XY). This increase in pressure can be used as a proxy for the changes in AO triggered by the Carrington event.

Answers to referee #2

We thank the referee for the valuable comments which are addressed below.

Specific comments: 1) p. 14753 (lines 25-27) and p. 14754 (lines 1-2): "This procedure suggests that there is hardly any penetration of the signal into the troposphere, which means that the event is unlikely to produce a nitrate spike in the ice core records. If correct, this depris us of using ice core nitrate as a potent indicator of such events (Wolff et al., 2012)." Comment: I agree that it does seem unlikely that such an event would produce a nitrate spike in the ice records. However, there has been discussion in the literature indicating that an increase of odd nitrogen in the stratosphere could be transported to the ground via either stratosphere/troposphere exchange or denitrification through polar stratospheric clouds (e.g. F. M. Vitt et al., J. Atmos. Solar-Terr. Phys., 62, 669-683, 2000). Perhaps the discussion in the paper should be modified slightly, given that direct production of odd nitrogen in the troposphere is not the only way to communicate a signal of nitrate to the ground.

Authors reply: We have added the following text to the paper: However, Vitt et al., (2000) suggest, that an increase of odd nitrogen in the stratosphere could be transported to the ground via either stratosphere/troposphere exchange or denitrification through polar stratospheric clouds. Therefore, it cannot be ruled out, that this solar proton event was able producing a nitrate spike in the ice core records even if this nitrate spike is made through stratosphere/troposphere exchange or denitrification and not by direct ionization in the troposphere.

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Interactive Discussion

Discussion Paper

Specific comments: 2) p. 14753, lines 20-21 and Figure 1: "The ionization rates taken from Rodger et al. (2008).." Comment: It wasn't clear how much the August 1972 ionization rates were increased to provide ion rates for the Carrington event. The discussion in this paper and the Rodger et al. (2008) study imply that it depends on the proton energy. It would be nice to have a couple of values indicated in the manuscript regarding this scaling. Something like "The proton flux at 30 MeV for the August 1972 period was scaled by X to derive the Carrington event proton flux at that energy." would be helpful. For example, Thomas et al. (2007) increased their computed October 1989 ionization rates by a factor of 6.5 to derive their Carrington event values.

Authors reply: The ionization rates for the Carrington event are taken from the work of Rodger et al. (2008), who combined a spectral form of the August 1972 SPE (their Fig. 1) with the time behaviour of the > 30 MeV fluxes (their figure 2, top). This means, that at each time step, the spectrum was scaled to match the estimated >30 MeV flux at that time. Ionization rates were then calculated using this time series of spectra. Rodger et al. did not provide spectra or ionization rates for the real August 1972 SPE, only the spectral form was used. Therefore, we cannot provide a scaling number that would relate the ionization rates used in this work to those of the real August 1972 SPE. However, we can compare the Carrington event ionization rates to other SPEs, and as an example we have chosen the October 2003 SPE, which is one of the largest SPEs recorded and for which we have the ionization rates readily available (they have been published, e.g., in Verronen et al. JGR, 2005). We integrated the ionization rates over the duration of the event, so that we can compare the total number of ion pairs produced at each altitude. We find that at 35-60 km the total rates of the Carrington event are 4 - 4.5 times higher than those of October 2003. This number is similar to the scaling factor used by Thomas et al. (2007), although they used a different SPE. Above 80 km and below 30 km, Carrington rates are smaller than those of October 2003, which is due to the different spectral form of the two events. A shorter explanation has been added to the manuscript.

Specific comments: 3) Figure 1: Ionization rates Comment: The ionization rates presented in Figure 1 appear to be somewhat similar values as those presented in Figure 4 of D. W. Rusch

et al. (Planet. Space Sci., 29, 767-774, 1981) for the August 1972 event. Rusch et al. (1981) show a peak ionization rate of >40,000 cmˆ-3 sˆ-1 whereas this paper appears to have a peak rate of >25,000 cmˆ-3 sˆ-1 (if I am reading the contours correctly). It does appear that the peak ionization rate is slightly higher in altitude in this work than in the Rusch et al. (1981) paper. Some mention of this previous study would be of use here.

Authors reply: We have added the following text to the manuscript: The comparison with the ionization rates of Rusch et al. (1981) is not straight forward, because different atmospheres, ionization rate calculation methods, and proton spectral forms were almost certainly used. These differences are likely the reason why the ionization peak of Rusch et al. is at a lower altitude than ours. The peak ionization rates are of the same order of magnitude, but our Fig. 1 displays daily average values that are naturally smaller than those given in a finer time resolution by Rusch et al. in their Fig. 4. We checked our ionization rates, which originally were calculated with a 5-minute resolution. In those 5-min data, the peak ionization rates reach 100,000 cm^{-3} s^{-1} around 50 km and are therefore about 2.5 times higher than the peak values given by Rusch et al. for the August 1972 SPE.

Suggested technical corrections:

1) p. 14751, line 23: Change "as reference event" to "as a reference event"

2) p. 14752, lines 2-5: Remove sentence "The energy deposition and ionization rates (IR) due to primary protons and secondary electrons with respect to altitude were then calculated using a method utilizing energy-range relationships for protons (Verronen et al., 2005)." Comment: It seems like this sentence is mostly a restatement of the

previous sentence in the paper: "In order to model the Carrington Event, we adopted the altitude dependent ionization rates (IR) from Rodger et al. (2008), which were calculated using a method utilizing energy-range relationships for protons (Verronen et al., 2005)."

3) p. 14754, line 1: Change "depris" to "deprives"

Authors reply: Will be changed.

Answers to referee #3

We thank the referee for the valuable comments which are addressed below.

Comment 1: It seems that presented effects for the middle atmosphere look very similar to other strong and calculated observed SPEs in spite of increased proton fluxes used by the authors model runs. (such similarity needs special comments inside presented paper).

Authors reply: The authors have used Jackman et al. (2007) to make a comparison with another large SPE, therefore the asked special comment is already given inside the paper. Therefore, no changes were made to the manuscript.

Comment 2: It seems that calculated changes of zonal wind (increased) need additional attention. As was published before (Krivolutsky et al, ASR'2006), and devoted to strong SPE of July 2000, such difference may depend on the sign of basic zonal wind. When the absolute values of zonal wind were used in mentioned paper the difference became negative.

Authors reply: Figure 6 in our paper is showing the difference between the experimental run and the reference run. The absolute values are not shown. It is not clear to us what the reviewer is asking here.

Comment 3: It looks important to find rather strong temperature changes in the lower atmosphere (different for Northern and Southern polar regions). It would be well to have more detailed explanation related to this result.

Authors reply: We have added an additional picture for the sea level pressure in the manuscript for the NH to better explain the changes in the Arctic Oscillation which in turn result in surface air temperature changes.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14747, 2012.