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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of galactic cosmic rays on atmospheric composition and temperature" *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: The title of the manuscript could possibly changed to "Influence of Galactic Cosmic Rays on Atmospheric Composition and Dynamics" since the GCR influence on zonal wind is also discussed.

Authors reply: Done.

Comment 2: p. 660, lines12-18: Description of the model simulations, which are each 27-yr long (1976 to 2002). Comment: Do these model simulations have varying bound-

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ary conditions for the source gases (chlorofluorocarbons, nitrous oxide, methane, carbon dioxide, etc.) and sea surface temperatures? If so, how do these changing conditions influence the computed statistical significance of the results?

Authors reply: Our model is driven by time varying boundary conditions for the source gases, aerosol loading, solar irradiance and sea surface temperatures. All resulting changes in the atmospheric state are taken into account for the calculation of statistical significance. The computed statistical significance would probably be higher for the case with non-evolving boundary conditions, because the variability around the mean state is smaller in the absence of any changes in the prescribed fields. However, it is hard to say anything more definite because the natural variability can very well also depend on the state of the system. We have added more details concerning model set-up (end of Section 2).

Comment 3: p. 661, line 28 and p. 662, line 1: It appears that the GCR influence on HNO3 is only mentioned briefly here in regards to Figure 4. Perhaps another sentence should be added contrasting the GCR-caused HNO3 effect to the GCR-caused NOx effect.

Authors reply: Our parameterization of the ionization effects on the chemical composition of the atmosphere describes only the direct production of NOx and HOx as a function of ionization rates (see section 2). Therefore, the HNO3 build-up in our model directly results from the GCR induced NOx production. HNO3 production due to ion-ion recombination or positive hydrated ion reaction with N2O5 cannot be considered in the framework of our model, however it does not substantially limit the model capability, because HNO3 production caused by these processes is important mostly above 10 hPa where the ionization by GCRs is rather small (Kawa et al, 1995; Aikin et al., 1997). We have added proper explanation to the text (Section 3).

Comment 4: p. 664, lines 20-27 and p. 579, Fig. 9 caption: This is somewhat confusing for the reader. The text on p. 664 discusses a March monthly mean with use of the

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ionization rates from Usoskin et al. (2010) and a January monthly mean with use of the ionization rates from Heaps (1978), whereas the Fig. 9 caption only notes the January monthly mean plots. Comment: It seems like it would be best to show the same month when comparing monthly mean computations. Thus, a comparison of the two January monthly means (with the use of each separate GCR ionization rate input) sounds like a reasonable undertaking. Was the March monthly mean plotted for one model simulation and the January monthly mean plotted for the other model simulation? If so, please explain why. If not, please remove "March" from Figure 9.

Authors reply: We have eliminated this error from the caption of Figure 9. Figure 9 illustrates the annual mean responses obtained with both parameterizations and two months with maximal responses which are different for Heaps and Usoskin cases. We have explained it in the text more clearly.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 653, 2011.