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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric changes caused by galactic cosmic rays over the period 1960–2010" *by* C. H. Jackman et al.

Anonymous Referee #2

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General comments

The manuscript describes the atmospheric changes caused by the influence of Galactic Cosmic Rays (GCR) on the production of nitrogen and hydrogen oxides. The authors analyze the response of the chemical composition to GCR simulated with two global chemical models using several scenarios of the boundary conditions. The subject of the manuscript is relevant to the ACP scope. The paper is well written and structured. The reference list includes most of the previous publications on this subject. All figures and tables are of good quality. The results contains already known information about the influence of GCR on the chemical composition as well as some new results concerning the influence of atmospheric state (e.g., chlorine and stratospheric aerosol loading) on the global mean total column ozone response to GCR. I

think the publication of the manuscript can be recommended. However, there are several issues in the manuscript (see specific) comments and some moderate revisions would be necessary before the publication.

Specific comments:

1. The response of the chemical composition to GCR obtained with the exploited models agrees well with the results published by Calisto et al. (2011), however it is heavily underestimated in comparison with the results of Semeniuk et al. (2011). This disagreement was briefly discussed by Mironiova et al. (2015, 10.1007/s11214-015-0185-4) and I think it should be also discussed in the paper, because it is important for the community.

2. Comparison of the main results against Calisto et al. (2011) and Semeniuk et al. (2011) requires some comparison of the applied ionization rates, because the difference between NAIRAS and Usoskin et al. (2010) calculations should be well characterized.

3. The choice of the models is not justified. I do not understand why the models with prescribed dynamics/transport were chosen. If some influence of GCR on ozone concentration is expected than this model choice hampers the possibility to study subsequent effects of GCR on temperature, circulation and climate.

4. Analyzing the results of sensitivity studies with their 2-D model the authors consider only global/annual mean total column ozone (GAMTCO). I think it is not a good choice because in the tropical area which contributes a lot to global mean value the influence of GCR is very small due to high cutoff rigidity. Therefore the magnitude of the GAMTCO changes caused by GCR is very small. It can be even considered negligible, because it is smaller than the measurement uncertainties. Would it be the same if the authors look at the higher latitude zones where the ionization by GCR is more pronounced.

Minor/technical issues:

1. page 33935, line 26: It reads like GCR produce constituents w/o ionization. I suggest reformulate, because NOx, HOx production is the results of ionization.

2. page 33937, lines 5-19: Are lightning and aircraft emissions included in WACCM? The authors said they are included in 2-D GSFC.

3. Section 4.2: How good is representation of tropospheric chemistry in 2-D environment? The chemistry is non linear, but it is necessary to use zonal mean fields. It would be interesting to compare OH distribution from the two applied models.

4. page 33940, lines 22-23: Not proper explanation. I think NMHC and VOC's included in the both models also play important role.

5. page 33943, second paragraph: In Figure 7(upper panel) the increase of tropospheric ozone is explained by CH4 increase. Why it is not the case for Figure 7 (lower panel). It would be interesting to explain.

6. page 33945, line 21: I think "intensity" should be added after "reactions"

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33931, 2015.