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

Answers to referee #2

Comment 1: Page 656, first paragraph: you should mention (either here or in section 2) that above 18 km, the ionization rates by the Usoskin model are lower than the Heaps parameterization for all latitudes and solar cycle phases. Can you state which model agrees better with observations? Where does the apparent offset come from?

Authors reply: Usoskin's parameterization works well in the lower part below 30 km, where GCRs form the main source of ionization. The model was verified by comparison with available direct data sets (balloon and rocket-based measurements of the

degree of ionization) and other models (e.g., Bazilevskaya et al., Space Sci. Rev., 2008; Usoskin et al., Acta Geophys., 2009). It does, however, underestimate the ionization above ∼30 km since it neglects other non-CR sources of ionization, UVI and precipitating particles (higher up in the polar atmosphere). On the other hand, Heaps' parameterization is based on scarce empirical data and may contain large uncertainties. It is closer to the data in the upper polar atmosphere. As a summary, we believe that Usoskin's model produces more realistic results below 30 km, but is prone to underestimating the ionization above that altitude. We have added this explanation to section 2.

Comment 2: Page 656, line 19: CRIIs are not the only source of NOx in the polar winter stratosphere; observations have shown a quite considerable amount of NOx transported from the mesosphere or lower thermosphere into the stratosphere in many winters (Lopez-Puertas et al. [2006], Randall et al. [2009], Randall et al. [2007], Siskind et al. [2000], Seppaelae et al.,[2007]).

Authors reply: We have modified this paragraph according to the reviewer's suggestion (see Introduction).

Comment 3: Page 657, Lines 24 ff: Here and in other places where you emphasize the importance of GCRs for modeling of atmospheric trace gases, I wondered whether the changes you modeled are actually measurable.

Authors reply: To a large degree this question has been answered under comment No. 1. No changes have been made.

Would a model that includes GCRs actually agree better with measured trace gases (NOx, HNO3, ozone) than the same model without GCRs? Maybe you could make some statement about this.

Authors reply: It is a difficult question, because the agreement between simulated and observed atmospheric state depends on many other factors. We can say that if all other

processes were perfectly simulated and the observations were very accurate, then the model with GCR would be closer to the observations. The discussion of these aspects is out of the scope of this paper, therefore no changes have been made.

My impression is that this is not the case; in this sense, I would say the modeled impact of GCRs is not 'significant' as it is probably not verifiable. I may be wrong about this, but I think you should discuss this point somehow.

Authors reply: We can only state that we found statistically significant responses in some regions and for some quantities. We use "significance" in a statistical manner, referring to the Student's T-test. However, there are other definitions of significance, which can be discussed differently by different communities based on our results. A brief statement concerning this issue has been added at the end of the summary.

Comment 4: Page 658, SOCOL description: how are tropospheric source gases introduced into the model? Are tropospheric source gases introduced with a spatial dependency, e.g., considering spatial distribution of anthropogenic or biogenic sources? This is important for understanding the interhemispheric differences, especially in the troposphere, and should be stated clearly here. Also, does the model consider coupling to the ocean? Is the solar (radiation) cycle included in the model?

Authors reply: Mixing ratios as function of time of long lived well mixed gases (e.g., N_2O, CH_4, ODS) were prescribed in the planetary boundary layer with no spatial dependency, while the fluxes of NO_x and CO were prescribed using appropriate emission data sets. The time-dependent solar irradiance was also taken into account. The sea surface temperature and sea ice distribution were prescribed from the observation data. All applied boundary conditions except GCRs are identical to REF-B1 run described by Morgenstern et al. (2010). We have extended the description of the model set-up accordingly in Section 2.

Comment 5: Page 660, Lines 15 ff: Has the solar cycle dependency of the GCRs been considered in some way? I assume that in the model, a solar cycle dependency

of GCRs is implemented, but this appears to be not considered in the analysis of the results. Is there a signiïňĄcant difference between solar max and solar min in the results?

Authors reply: The solar cycle dependency has been considered, applying the actually observed variations in the cosmic ray flux for the considered period. In the paper we analyze only the difference between GCRs fully implemented and GCRs switched off as two extreme cases to establish the upper limit of the potential effects. The analysis of the difference between solar max and min is more complicated because the extraction of the GCR related signal would require time dependent statistical analysis (e.g., multiple regression analysis), which is difficult for our rather short (27 years) time series. A clarifying sentence has been added in the last paragraph of Section 2.

Comment 6: Page 661, line 25: Why use the 80% significance contour here, when everywhere else only 95% is shown?

Authors reply: This is merely to guide the reader's eye.

Comment 7: Page 661, Line 10ff: Is the inhomogeneity of anthropogenic source gases included in the model? Please add a statement in model description. (see also comment above)

Authors reply: Yes. See extended model set-up description in Section 2.

Comment 8: Page 661, Line 10ff: Other explanation for the stronger impact on the SH polar lowermost stratosphere / upper troposphere: due to the more stable polar vortices and weaker exchange with mid-latitudes during polar winter, GCR induced NO_x is trans- ported further down into the atmosphere in the SH polar winter than in the NH polar winter, where the signal is frequently mixed to mid-latitudes (especially during strato- spheric warmings).

Authors reply: While the other explanation offered by the reviewer might add to the overall effect, we do not think that this can be an important pathway. GCR-induced

NOx in the LS at winter time will be rapidly converted into HNO3 and therefore not survive the transport time downward as NOx species. Only after photolysis of HNO3 (which is slow in winter time) could this reactive nitrogen add to the changes in the polar tropospheric ozone chemistry. Therefore, we think that the explanation originally provided is likely playing the main role.

Comment 9: Page 662, Line 6 ff: the explanation how additional ozone loss occurs in the NH polar winter sounds plausible, but please also explain why this does not occur in the SH? Because there, chlorine activation is already complete because of the greater abundance of water ice clouds? Or because ozone is already practically zero?

Authors reply: SH is closer to saturation (more available Cly due to stronger downward transport in polar vortex, lower temperatures, more PSCs, very low ozone), therefore the GCR effects are not observed. We have added this explanation to the text.

Comment 10: Page 664, first paragraph: "a proper description of the ionization rate in the upper troposphere ... is required for a correct simulation of atmospheric composition": see my comment to page 657: I wonder whether the modeled changes are actually measurable (and verifiable).

Authors reply: See answer to comment 3 concerning statistical significance. Note further that a discussion of accuracy and precision of the observational data is out of the scope of this paper.

On the other hand, I found the impact on surface temperatures of what is actually quite a small ozone change quite large.

Authors reply: Presumable the reviewer means the NH LS. The ozone depletion (up to 6%) is actually not so small.

Comment 11: Page 664, line 10 ff: .... shows the importance of the Usoskin scheme for correctly describing ... ozone production in the southern hemisphere troposphere ... as far as I see, the differences are below +-1%, so I find this statement a bit exaggerated.

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Authors reply: We have rephrased this sentence.

Comment 12: Page 664, line 21ff: why show March for the Usoskin ionization rates, and January for the Heaps ionization rates? Actually, considering how very similar the surface T patterns look for March/Usoskin and January/Heaps, are you sure those are not both for the same month?

Authors reply: This was badly explained, as also Referee 1 noted. Figure 9 illustrates the annual mean responses obtained with both parameterizations and two months with maximal responses which are different for Heaps (January) and Usoskin (March) cases. We have improved the text accordingly in Section 3.

Comment 13: Page 664, line 21ff: does the model include ocean coupling, or are SSTs prescribed? If sea surface temperatures are prescribed, how would that affect the observed surface temperatures (over the ocean)?

Authors reply: We applied prescribed time evolving SST from observations. Consequently, as we can see in the surface air temperature plots, there is no statistically significant change over the ocean.

Comment 14: Page 664, line 21ff: Is there a signiïňA̧cant difference in the observed surface temperature response between solar max and solar min?

Authors reply: We did not analyze the difference between solar max and min because the extraction of the solar signal requires time dependent statistical analysis (e.g., multiple regression analysis), which is difficult for our rather short (27 years) time series.

Comment 15: Page 665, Line 2: A very similar pattern of surface temperature responses during high winter has been observed in ECMWF ERA 40 temperatures correlated to the geomagnetic activity index Ap (Seppaelae et al, JGR, 2009), and in model results of the ECHAM5/Messy model as a response to NOx produced by geomagnetic activity in the upper mesosphere (Baumgaertner et al, ACPD, 10, 30171-30203, 2010). In the latter case the surface temperature response is also interpreted as a modulation

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of the NAM index propagating down from the lower stratosphere. Though the source of the stratospheric NOx / ozone loss is different (or thought to be different) in the Seppaelae and Baumgaertner investigations, the observed response of the troposphere to a stratospheric forcing seems to be very similar to your investigation, and you should discuss these here.

Authors reply: We have added a short discussion of this issue to the text.

Comment 16: Page 665, Line 5: See also summary (page 666): why is the impact on surface temperatures so much lower in the Southern hemisphere? And, is the impact also mostly restricted to mid-winter, as in the Seppaelae and Baumgaertner investigations?

Authors reply: The simulated ozone response is rather small over the SH, therefore smaller response of the surface air temperature can be expected. Moreover, the ocean with prescribed temperature which dominates in the SH can suppress the response. Indeed, the main response is in mid-winter (as stated in the paper).

Comment 17: Page 667, line 12: "agree largely" ... well, qualitatively, but it seems that the Usoskin is systematically lower above 18 km.

Authors reply: The reason that the parameterization of Usoskin is lower than the Heaps parameterization is explained in our reply to comment 1.

Language / typos: Will be changed.

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