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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.

Anonymous Referee #2

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The authors investigate the impact of galactic cosmic rays (GCRs) on the chemical composition, temperature and dynamics of the troposphere and stratosphere using the chemistry-climate model SOCOL together with a state-of-the art parameterization of GCRs from the Usoskin scheme. The paper is generally well written. I found the method and topic generally sound and the results interesting, if small. However, I have quite a number of comments concerning the analysis and discussion of the results.

General comments:

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 ?

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]).

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. 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. 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.

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 ?

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 significant difference between solar max and solar min in the results?

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

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)

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 NOx is transported 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 stratospheric warmings).

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?

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

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.

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?

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)?

Page 664, line 21ff: Is there a significant difference in the observed surface tempera-

ture response between solar max and solar min?

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 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.

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?

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

Language / typos:

Introduction, line 19: ... into the polar regions.

Page 657, Line 10: ... cannot be performed with an 1D-model, but requires ...

Page 664, line 16: "PCS" -> "PSC"

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