Clim. Past Discuss., 9, C3421–C3423, 2014
www.clim-past-discuss.net/9/C3421/2014/

Climate of the Past
Discussions
Open Access

Interactive comment on "Impact of geomagnetic events on atmospheric chemistry and dynamics" *by* I. Suter et al.

I. Usoskin (Referee)

Ilya.Usoskin@oulu.fi

Received and published: 10 February 2014

The manuscript presents an interesting numerical model study of what would be the atmospheric/climatic impact of a geomagnetic excursion/reversal, when the intensity of the geomagnetic field, particularly its dipole component, is greatly reduced. This is of particular interest keeping in mind the myth widely discussed by laymen (warmed up by pseudo-scientific blockbusters) that such an event would be deadly dangerous for the life on Earth. As such, the present work clearly is of sufficient interest to warrant publication in CP. However, the work contains some inexactitudes and unclear points that should be clarified before the final acceptance. This calls for a moderate revision of the manuscript. This reviewer states that he/she is more familiar with geomagnetic and cosmic ray stuff but is quite ignorant about atmospheric chemistry.

The authors needs to clarify the terminology about the Laschamp event. It is usually not called a reversal, but rather an excursion. Although some data suggest that the z-component of the dipole might had become slightly negative for a short while, it returned back very soon without the reversal. The smallest dipole moment was about 1/8 of the present day value. The authors are requested to clarify the text accordingly.

When simulating the cosmic ray variability, the authors consider two cases: \phi=400 and 0 MV. This choice does not look obvious, as 400 MV corresponds to the solar cycle minimum periods of the modern epoch (see cited Usoskin et al., 2005), though the recent cycle minimum ca. 2009 had lower values of the modulation parameter. Zero-modulation is never reached in practice (even during the Muander minimum there was a weak residual modulation of \phi about 100 MV). This is fine as an extreme case but the authors should describe what conditions are represented by these phi values.

It is surprising that the authors do not apply an 11-yr cycle to the simulated cosmic ray modulation, considering instead a steady state case. Including the 11-yr cycle would be natural, as this reviewer believes.

The authors simulate cases with the greatly inclined dipole (45 and even 90 deg inclination). That's fine, but the longitude of the geomagnetic pole must be also shown and discussed. I guess, it would be quite a difference for the results, if the pole was located, e.g., over central Pacific and mid-Africa. Anyway, this should be specified. In addition, this reviewer assumes that the centered geomagnetic dipole model was applied, not an eccentric one. This also should be stated clearly.

Although the above inexactitudes may look crucial for a study a real event, they are not critical for the present hypothetical study. This reviewer does not request redoing any simulations, but only to state clearly what exactly was simulated.

The paper lacks a clear conclusion summary. The authors may want to summarize the results concisely.

Other comments are related to slight text polishing.

1) page 6606, line 1: "events" -> "excursions" 2) p.6606, line 10: after "due to enhanced ionization" add "by galactic cosmic rays" 3) p. 6607, l. 1: write "up to 10ˆ20 eV" 4) p.6607, l. 7-8, replace Potgieter, 1998 with a more recent review (Potgieter, Liv. Rev. Solar Phys., 2013). 5) p. 6607, l.8: "only a few" -> "several" (geomagnetic influence in fact may start already at 20 radii). 6) next line: remove "at least at lower latitudes". 7) p. 6607, l. 11 - see general comment above, about reversal. 8) p.6609, l.20: replace "ionization cascade" with "nucleonic-muon-electromagnetic cascade" 9) p.6610, l.4. what is the "hybrid sigma-p levels"? This sounds as a very particular jargon which needs to be explained. 10) p. 6610, l.8: "primitive" -> "basic"? 11) last paragraph of Sect. 3 - see general comments above. 12) p. 6618, l.25: "We find" -> "our simulation suggests" 13) p. 6619, l.23-25. This sentence is confusing and needs revision. First, the ion induced/mediated nucleation is not in its infancy. Both in-situ (e.g., Mironova et al., ACP, 2012) and in-vitro (Kirkby et al., Nature, 2011; Enghoff et al., GRL, 2011) studies suggest that the effect does exist but is very weak, observable only in extreme conditions. Another potential mechanism related to the global current circuit (ref. to works by Tinsley, Harrison, Yu) is not mentioned. 14) The last sentence needs to be removed. Promises are good but better just do the work.

Interactive comment on Clim. Past Discuss., 9, 6605, 2013.