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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Geomagnetic activity related NO_x enhancements and polar surface air temperature variability in a chemistry climate model: modulation of the NAM index" *by* A. J. G. Baumgaertner et al.

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In their manuscript Baumgaertner et al. show the influence of NOx enhancements due to geomagnetic activity on polar surface temperatures in a chemistry climate model. Their current model calculations support similar findings derived from meteorological reanalyses (by Seppala et al., 2009) and thus help to better understand the mechanisms involved. The paper is in general well written and I recommend publication in Atmos. Chem. Phys. after clarification of a few (mostly minor) points.

General comments:

1. One of the central results of this study is the impact on polar surface air temperatures. However, the model uses prescribed sea surface temperatures (SSTs), and I expect that the SSTs will have a large impact on the surface air temperatures. The authors already consider the potential impact of SSTs by a statistical test of the transient simulation (Fig. 2) and by performing the sensitivity runs with identical SSTs. This clearly shows that their results are robust over the continents and the ice covered oceans, but there is (not surprisingly I guess) almost no temperature effect over the ice free oceans where SSTs are prescribed. I suggest that the effect of fixed SSTs is discussed in a bit more detail and a possible caveat is added to the paper.

2. The good agreement between modelled and observed polar surface air temperatures is an exciting result from the current study and gives some confidence that the model can be used to investigate in greater detail the mechanisms of the coupling involved. I encourage the authors to expand their discussion on the mechanism (Section 3.3) and to investigate some aspects of the coupling in a bit more detail. Some specific suggestions are given below.

Specific comments:

3. The first paragraph of Section 3.3 is redundant and can be shortened or completely removed.

4. Do I understand this correctly that the investigation of NAM anomalies (page 30183, lines 25 and following; Fig. 12) is based on a single winter? How meaningful is this comparison? How can you be sure that the differences in the NAM index are due to EPP-NOx and not just internal variability?

5. Fig. 6: Can you show NOx for the no-EPP case (and/or the difference between the two simulations) as well? Clearly the large tongue of enhanced NOX is due to EPP, but by how much are the other areas influenced by EPP-NOx?

6. Figs. 8 and 9: It would be interesting to see the ozone and temperature differences not only for DJF but as a function of time over the winter; e.g., by showing three panels for December, January and February individually. This would hopefully help to better understand the mechanism of the coupling: How (where and when) does the ozone respond to the NOx increases; how (where and when) does the temperature respond to the ozone changes? From Figs. 8 and 9 it appears that the largest cooling occurs at lower altitudes than the largest ozone reductions; this is attributed to changes in circulation. Do you have any diagnostics in the model that can further support this? What would be the expected temperature response to radiation only?

7. Fig. 11: The shift in the NAM index histogram is a very interesting result. Can you quantify to what degree this is statistically significant?

Minor comments / Technical corrections:

p. 30174, l. 1: correlation between Ap and NAO "...since about 1970". What do you mean here? There is no correlation before 1970 or there are no data before 1970?

p. 30179, l. 1: Maybe I'm a bit picky here but, CCl4 is not a chlorofluorocarbon and CH3Br is not a halon. Maybe just call these ozone depleting substances?

Fig. 10: What does "normalized" mean here? "Normalized" with respect to the standard deviation?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30171, 2010.