Atmos. Chem. Phys. Discuss., 10, C15074–C15081, 2011
www.atmos-chem-phys-discuss.net/10/C15074/2011/

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.

A. J. G. Baumgaertner et al.

andreas.baumgaertner@dlr.de

Received and published: 21 April 2011

We thank the referee for his valuable comments, which are addressed below.

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 tran-

sient 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.

We added: "Note that the ice free oceans generally show very small anomalies, which is expected given that the SSTs, which have a large effect on the SAT, are repeated from year to year."

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.

We have followed the suggestions as detailed below.

Specific comments:

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

We removed the paragraph.

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

ACPD

10, C15074–C15081, 2011

EPP-NOx and not just internal variability?

Fig. 12 is indeed only based on a single winter. However, it is just meant as an example that the NAM anomaly can propagate to the surface in the model as expected from observations. It could be attempted to depict this for all simulation years by a clever analysis, but this would make it more difficult to see the NAM downward propagation because the behaviour is of course different every year. The statistical analysis using all years is provided in Figure 11. We have changed the wording to "where we show a single exemplary winter from both simulations".

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?

Fig 1 and Fig 2 of this reply show the NOX in the S-noEPP simulation and the difference, respectively, as requested. Regions outside the "tongue" appear to be not affected by the EPP-NOx. This is now indicated in the revised manuscript and these additional figures are also shown.

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?

ACPD
10, C15074–C15081, 2011

Using only single months is unfortunately not feasible because of the limited number of years available – then the variability is too large. However, a separate study investigating this issue using a different model is currently conducted by Randall et al (2010). Unfortunately the individual heating/cooling terms are currently not available from the model, so this analysis will be further work using a model version with extended diagnostic capabilities.

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?

In an attempt to answer this we included the standard deviations (calculated from the individual years) in the histogram, see Fig. 3 in this reply. From this, the result seems to be very robust. We repeated the analysis for only those years where no warming occurred (grey lines), but still got a similar result. This will be discussed and shown in the revised manuscript.

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?

At the surface, there is no correlation between 1942 and 1970 (although in the stratosphere they claim there is). We have clarified this in the manuscript: "_only_ since about 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?

Changed to "ozone depleting substances"

ACPD
10, C15074–C15081, 2011

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

The magnitude of the variations changes with height (because of the vertical increase of geopotential height), therefore the data was normalized with respect to the standard deviation at every level separately.

References:

Randall, C E, E D Peck, L A Holt, V Harvey, D R Marsh, X Fang, C H Jackman, M J Mills, S M Bailey, 2010, Atmospheric Coupling via Energetic Particle Precipitation, AGU Fall meeting, SA31B-1730.

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

ACPD

10, C15074–C15081, 2011

Interactive Comment

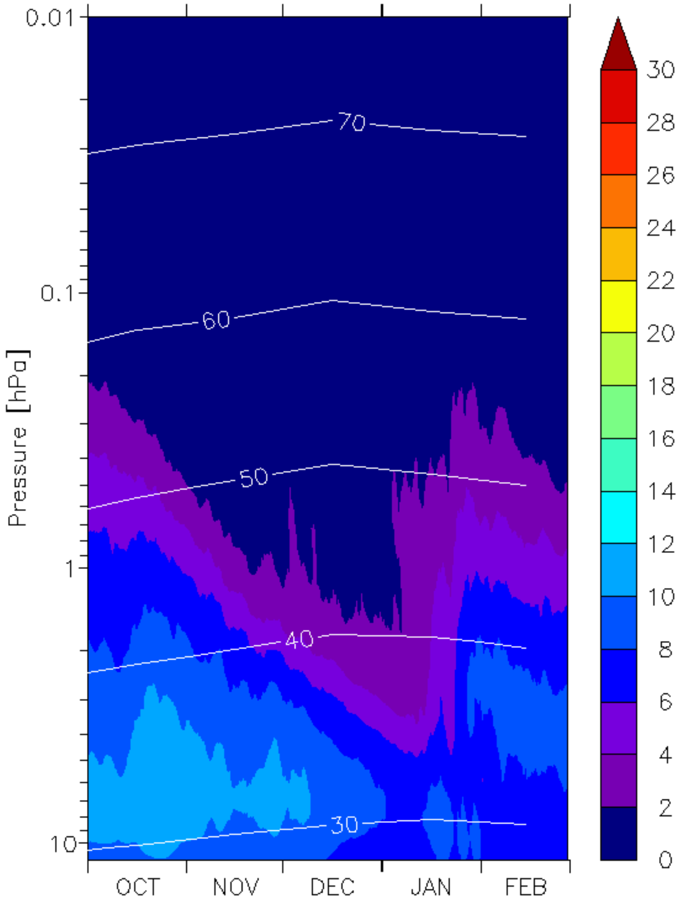

Fig. 1. NOx mixing ratio (ppbv) poleward of 60N in the simulation S-noEPP

ACPD
10, C15074–C15081, 2011

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

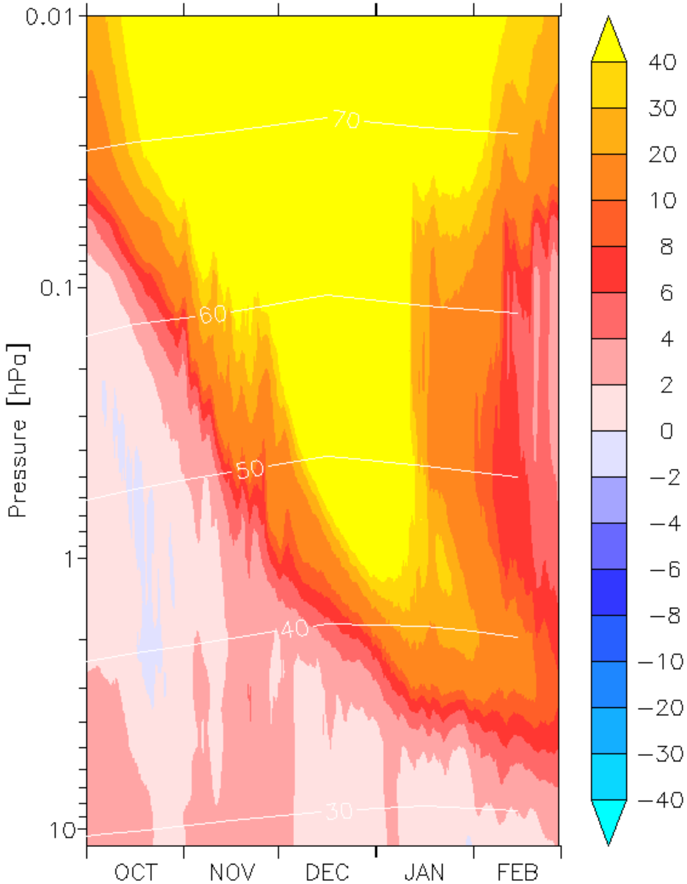

Fig. 2. NOx difference between S-EPP and S-noEPP poleward of 60N (ppbv)

ACPD
10, C15074–C15081, 2011

Interactive Comment

ACPD
10, C15074–C15081, 2011

Interactive Comment

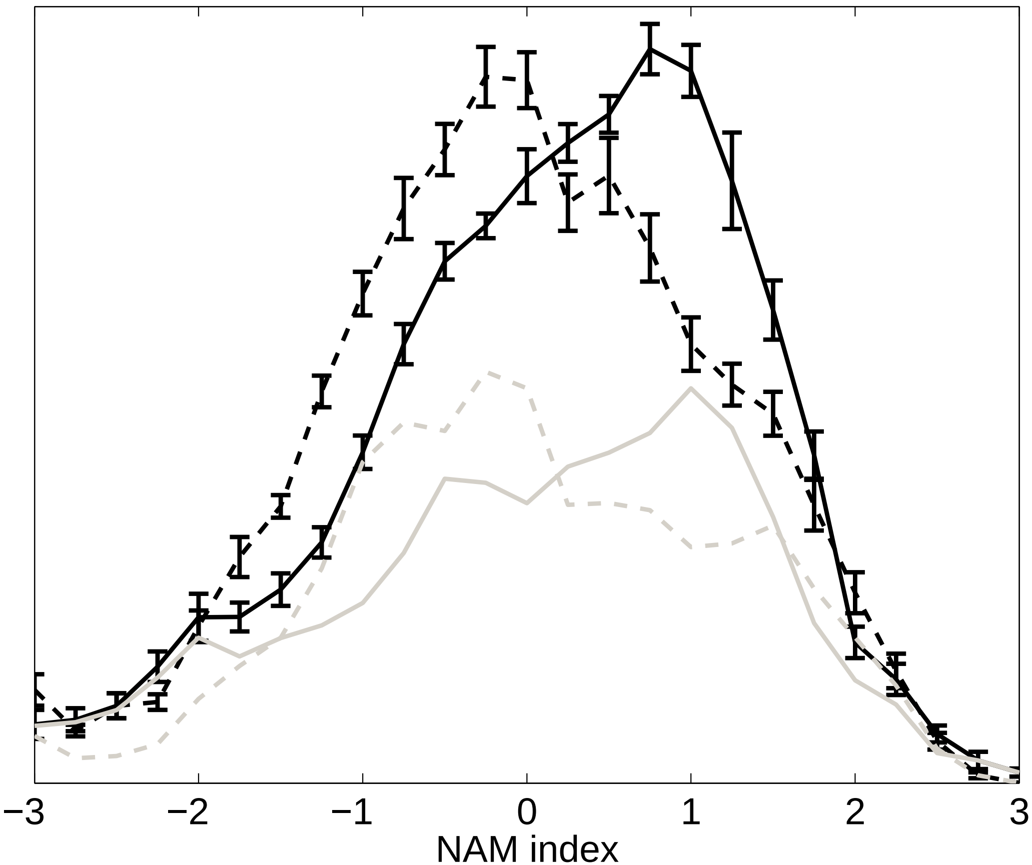

Fig. 3. Solid black: S-EPP, dashed black: S-noEPP. Error bars: standard deviation calculated from the individual years. Grey lines: analogous results for only those years where no SSWs occurred.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper