Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-514-AC3, 2017
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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "NO_y production, ozone loss and changes in net radiative heating due to energetic particle precipitation in 2002–2010" *by* Miriam Sinnhuber et al.

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Received and published: 22 September 2017

Overall this is an interesting, well-conceived and well written paper that should be of solid interest for ACP readers. I have one general concern. Assuming it can be addressed, along with several more minor concerns, the paper should be suitable for publication.

We would like to thank the reviewer for this opinion, and for the careful review which certainly has made the paper more interesting.

As suggested, pressure is now given in hPa, not in Pa. Figures, tables and text have

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been redone accordingly. A bug in the zonal averaging of the 3dCTM data has been corrected. This has no impact on the conclusions, but leads to slightly higher NO_y (figures 2, 4, and 6, particularly after the SSW in early 2009) and ozone losses (former figure 9) in the Northern hemisphere. Results shown in former figures 7, table 2, and former figures 10 and 11 are not affected as those were averaged correctly. Following the suggestion of the reviewer, new figures have been included showing a comparison to MIPAS ozone observations, and a comparison to (modeled) net radiative heating rates. New references have been included: Natarajan et al. (2004); Randall et al. (2005); B-M. Sinnhuber et al. (2002); Damiani et al. (2016); Semeniuk et al. (2011).

The main concern is that one can't help but note that the authors put much more effort into validating their NOy calculations by comparing continuously with MIPAS. Section 3.3 is quite good in this regard. They do not do this with ozone; rather, they reference other work. However, this is unsatisfactory and the net effect is that their modeled ozone changes are less robust than their modeled NOy changes.

Thanks for pointing this out. We have added a comparison of model results with MIPAS ozone. A new subsection Sec. 4.1 *Comparison of modeled and observed ozone fields* is added at the beginning of Section 4. Figures comparing MIPAS ozone to model results from the EPP runs have been included, see figures Fig. 1 and Fig. 2.

Particularly for some of the strongest years, like 2003-2004, it would be extremely helpful and much more convincing just to show their calculated ozone for with and without EPP/SPE compared with some observations.

Different approaches to this have been tested. It turned out to be difficult to extract comparable results over a larger period of time and/or vertical range because of the strong dynamical variability of ozone, which is notably larger than the ozone variability due to EPP in most years. Two new figures (figures Fig. 3 and Fig. 4) were included comparing relative ozone anomalies, that is the (relative) difference between daily values and a multi-annual mean. The period 2006-2009 was chosen for the multi-annual

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mean because it is covered well by MIPAS observations, and is characterized by relatively low geomagnetic activity. Model results for both the EPP and the Base model scenarios are shown for the period 2002.5 to 2004.5, when significant differences between the model runs with and without EPP are observed in both hemispheres in the mesosphere and upper stratosphere, particularly after the SSW in the Northern hemisphere (EMAC). This has been added in the new subsection Sec. 4.2 *Comparison of modeled and observed ozone anomalies*.

Even simple comparisons- like for example, the ozone reduction discussed by Natarajan et al [2004, GRL] (and which remains, in my opinion, the most compelling case of stratospheric ozone reductions in response to these events, and which unfortunately, is not cited) would be better than nothing. Although it would be hoped that they could do more.

Thanks for pointing this out. A reference to Natarajan et al. (2004) and Randall et al. (2005) has been added to the introduction. The results of Natarajan et al. (2004) and Randall et al. (2005) as well as model results from (Semeniuk et al., 2011; Baumgaertner et al., 2011) are also discussed in comparison to the modeled ozone differences in Sec. 3.3 (previous Sec. 3.1).

Many previous works (Siskind, Funke, Jackman) gave the various contributions to NOy in both absolute numbers but also percent. Given the differing transport amongst the 3 models, it would be useful to get percent contributions. This would be especially helpful for the ozone and heating rate calculations. I had a very hard time deciding how significant these effects were.

We have added absolute values to the ozone differences and added some discussion about how these absolute values compare to observations (Natarajan et al., 2004; Randall et al., 2005) and other model studies (Reddmann et al., 2010). We did not add relative differences to NOy. These were tested for Figures 5 and 6, and it was found that the general conclusions do not change if relative differences are given. Absolute

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differences cover two orders of magnitudes (from 2 ppb to more than 100 ppb), and relative differences of more than 100 % therefore occur related to differences in the speed of the downwelling of the EPP NO_y signal. However, large percentage changes also occur in regions of very low NO_y, e.g., at the edge of the Antarctic ozone hole area where NO_y is depleted. For the heating rates, the net radiative heating rates of the Base runs of EMAC were added for comparison to the bottom panels of former figures Fig. 10 and 11 (now Figs. 14 and 15).

Continuing on the above thread, for Figure 11- the post SSW changes look interesting.

Comparisons with the net radiative heating rates of the EMAC model show that the largest changes to the heating rates, during spring in the upper stratosphere after large wintertime solar proton events or sudden stratospheric warmings, occur when the atmosphere approaches radiative equililbrium, i.e., net radiative heating rates are low (less than 0.5 K/day to 1–2.5 K/day), see lower panels of Figs. 5 and 6. Relative changes can approach 100 %, and potentially have a bigger impact than the changes during midwinter, when the net radiative heating rates are higher. During midwinter, relative changes are in the range of 2–10 % of the net value only. A more detailed discussion of this has been added to the text.

Again, along the lines of my comments above, it would be much more compelling if they could do some comparison with observations. For example, show an average of the 3 post-SSW year temperatures compared with non-SSW years and then with their model. Given how comprehensive their NOy model-data comparisons were, the lack of such comparisons for ozone and heating/temperature changes are more apparent.

This is a good suggestion; however, it is not possible to carry it out with the model scenarios as set up here. All model runs shown in this paper are done with a specified dynamics setup, meaning that in the stratosphere and troposphere, temperatures and wind-fields are relaxed to observations. This is done in the same way for the EPP and Base model runs, so stratospheric temperatures are almost identical. This model

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setup was chosen to ensure that the interannual variation of ozone and NOy agree with observations of specific years as well as possible. For a study of the impact of the heating rates on stratospheric temperatures, follow-up studies with free-running models are needed. These would be of great interest obviously, but are out of the scope of this paper.

Ultimately, with the uncertainty both in relative contribution and as well as validation, I find the last several lines (12-15 on page 32) to be too speculative in my opinion and should be removed. I only see a significant blob of color on the EMAC plot and only for one spring. Almost nothing in 2006 and 2009.

We agree that this should have been formulated (and now is formulated) more carefully:

The indirect effect could contribute to the reformation of a strong and long-lasting vortex in late winter and spring after sudden stratospheric warmings in years with high geomagnetic activity, as e.g. in winter 2003–2004.

Though this is observed only in the EMAC model, mentioning it seems justified to us because the impact of sudden stratospheric warmings on both NOy and ozone are better represented by EMAC than by the other two models.

Intro: I believe SPEs have been known to be sufficiently energetic to directly ionize the lower stratosphere. The text keeps saying "upper".

A note in the introduction was added that protons can penetrate down to the lower stratosphere in events with particularly hard energy spectra.

English grammar: line 28 page 5 "prevents .. from propagating"

Changed.

Figure 2- they should zoom in on the vertical axis- there is no reason to show 5 orders of magnitude when 3 will cover the range.

Done.

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Line 9, page 6; line 1 page 10 and many places elsewhere. Could the authors please use hPa rather than Pa? Unless ACP has a preference, I believe more people in aeronomy intuitively think in terms of hPa.

Done.

Related to the above- line 29, page 12. Doesn't > 100 pa refer to the stratosphere? The text says mesosphere.

The sign was reversed, it now says $\leq$ 1 hPa.

Figure 8- why are there apparent vertical discontinuities in the ozone change? For example, top panel, beginning of 2005 where the colors go from dark green to blue instantly over a wide range of values.

This is the direct impact of solar proton events, e.g., in October 2003, January 2005, and December 2006. In the model results, SPEs deplete ozone in the whole vertical range from about 10 hPa (30 km) up to the mesopause; however, it should be pointed out that these changes related to strong solar proton events in the mid-to upper stratosphere are a few percent only, probably too low to be observable. A bullet point discussing the solar proton event impact in more detail has been added to the discussion.

Referencing: I believe Randall et al 2007 first coined the phrase "EPP-IE". They should be cited on line 8 of page 3. They already are cited elsewhere.

Thanks for pointing this out, the reference to Randall et al. (2007) has been added at this point.

Siskind references get the first initials wrong. In one place its DR in another its DW. Should be DE

Changed.

Same problem with Fleimng- its EK in the reference with Jackman. EL in other places.

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Changed to the correct (EL).

Matthes paper is 2017 as it has appeared now.

Changed.

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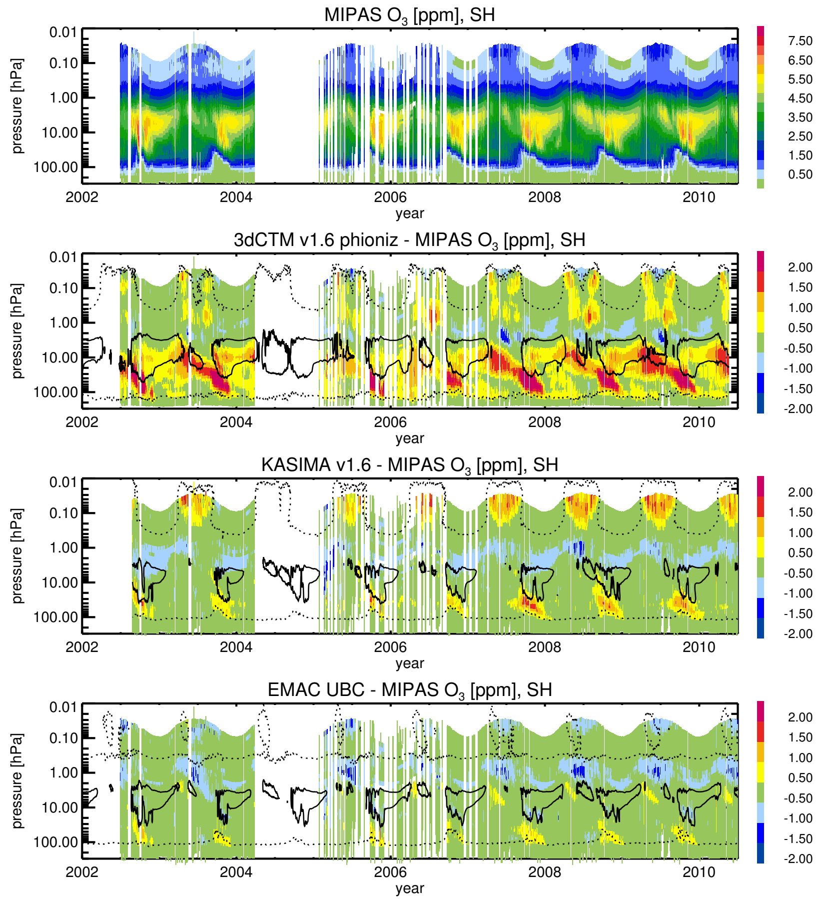
MIPAS O$_3$ [ppm], SH
3dCTM v1.6 phioniz - MIPAS O$_3$ [ppm], SH
KASIMA v1.6 - MIPAS O$_3$ [ppm], SH
EMAC UBC - MIPAS O$_3$ [ppm], SH
pressure [hPa]
year
0.01
0.10
1.00
10.00
100.00
2002
2004
2006
2008
2010
7.50
6.50
5.50
4.50
3.50
2.50
1.50
0.50
2.00
1.50
1.00
0.50
-0.50
-1.00
-1.50
-2.00

Fig. 1.

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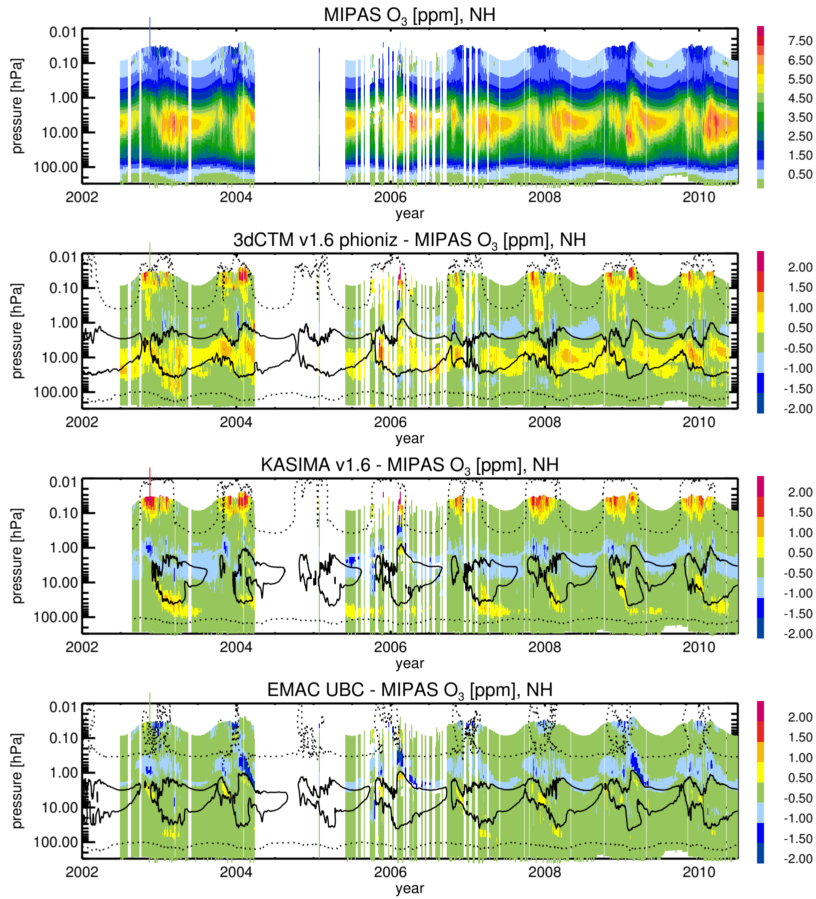

Fig. 2.

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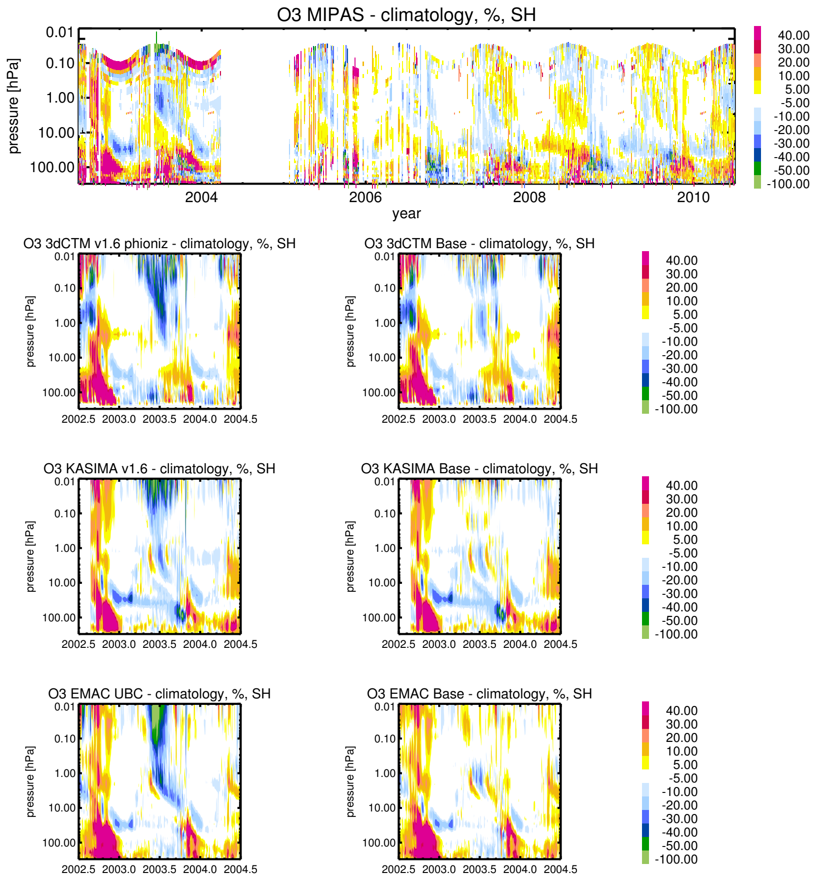
O3 MIPAS - climatology, %, SH
pressure [hPa]
year
O3 3dCTM v1.6 phioniz - climatology, %, SH
O3 3dCTM Base - climatology, %, SH
O3 KASIMA v1.6 - climatology, %, SH
O3 KASIMA Base - climatology, %, SH
O3 EMAC UBC - climatology, %, SH
O3 EMAC Base - climatology, %, SH

Fig. 3.

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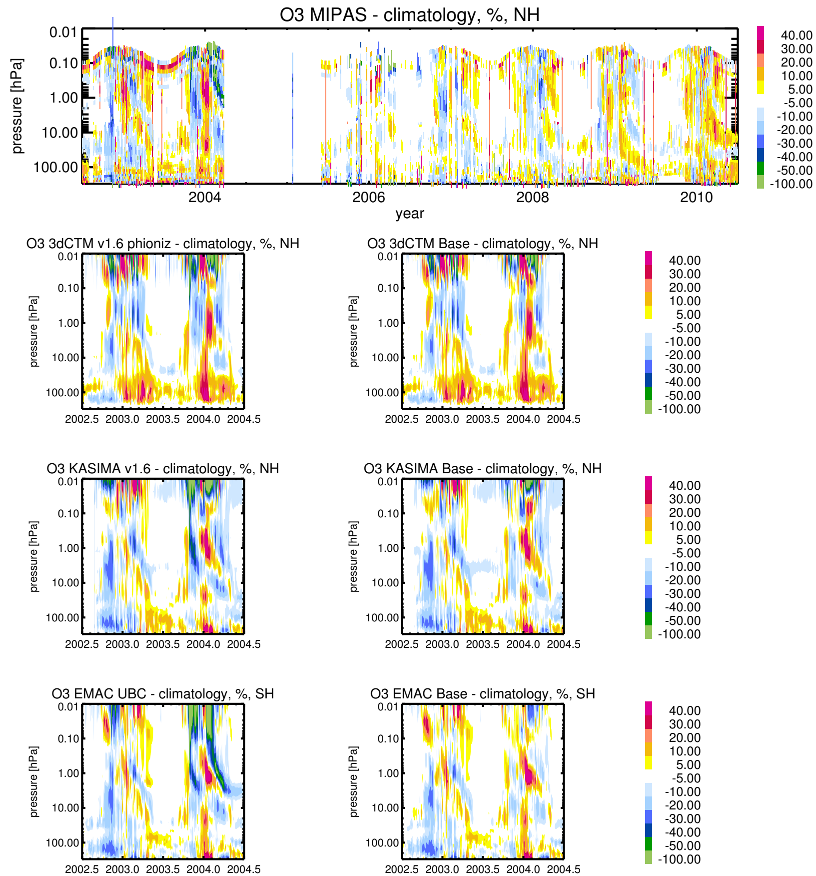

Fig. 4.

Interactive comment

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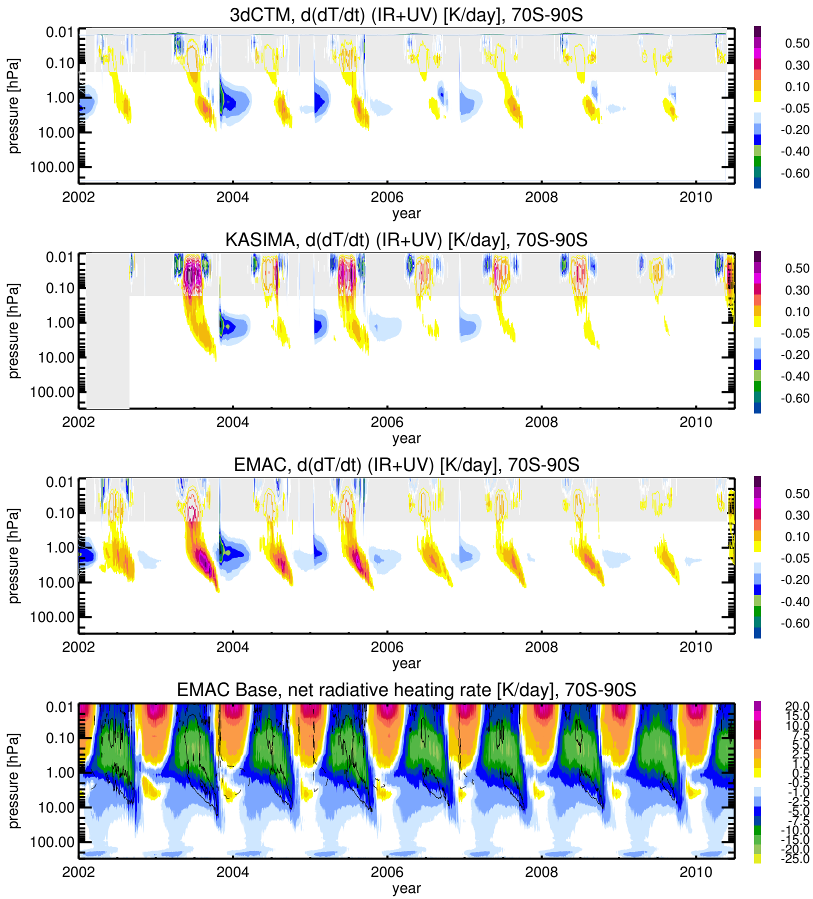

Fig. 5.

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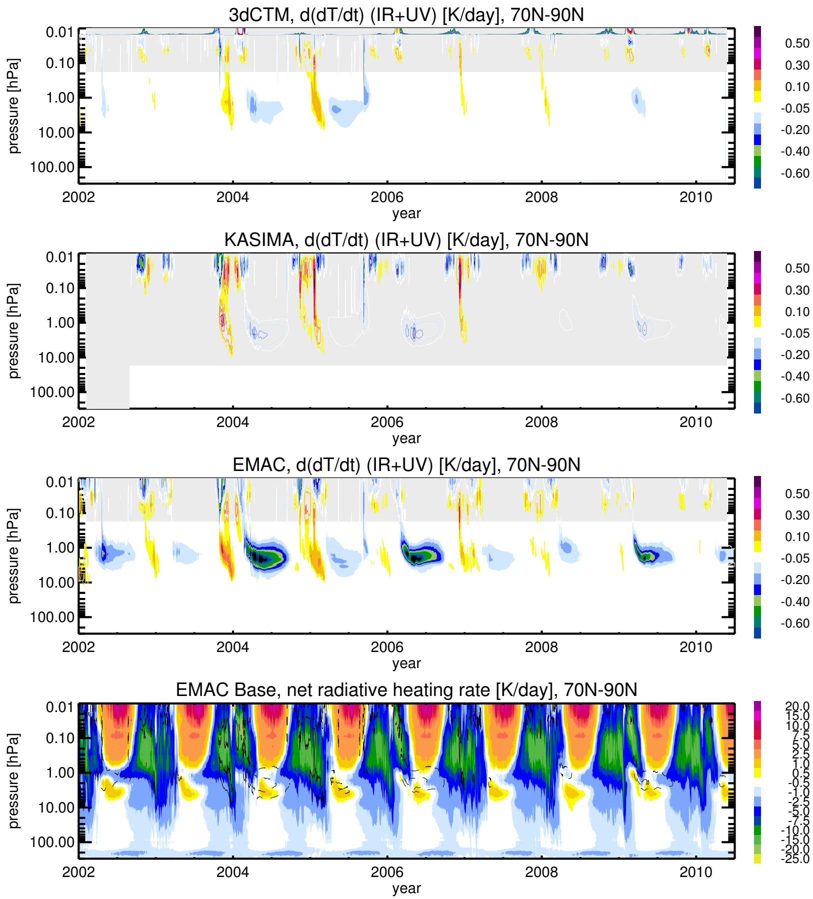
3dCTM, d(dT/dt) (IR+UV) [K/day], 70N-90N
KASIMA, d(dT/dt) (IR+UV) [K/day], 70N-90N
EMAC, d(dT/dt) (IR+UV) [K/day], 70N-90N
EMAC Base, net radiative heating rate [K/day], 70N-90N
pressure [hPa]
year
0.01
0.10
1.00
10.00
100.00
2002
2004
2006
2008
2010
0.50
0.30
0.10
-0.05
-0.20
-0.40
-0.60

Fig. 6.

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