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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of stratospheric water vapor trends: impact on stratospheric ozone chemistry" *by* A. Stenke and V. Grewe

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This paper analyses the impact of the water vapour trends in coupled chemistry climate simulations on ozone trends. One of the major conclusions is that an increase in water vapour enhances the PSCs over Antarctica and leads to higher ozone depletion whereas over the Arctic the chlorine chemistry is less affected by water vapour increases. It is actually the converse of what one would expect since any process that tips the Arctic in favour of ozone depletion could be significant. The results of the paper are therefore important, and, if correct, are worthy of publication in ACP.

General Comments

I have some doubts, though, whether the processes identified are those actually occurring in the atmosphere as the model appears to be poorly suited to address these problems. Firstly, the water vapour amounts are about 50% higher than observed (Figure 1), yet the climatology of the model (Hein et al., 2001) is reasonable. Models with low vertical resolution in the vicinity of the tropical tropopause can lead to large errors either due to difficulty in representing the sharp vertical gradient, or due to some other tropopause temperature bias. For example, the GFDL model AM2 (24 levels) has tropical water too high by about 30%. In moving to higher vertical resolution one typically gets much better results. For example, the 40 level GFDL model AM2 is reasonably accurate). Vertical resolution should not be a problem for the DLR model so I am mystified as to why the water vapour bias exists in the transient model run.

Secondly, according to HALOE data, below 10 hPa in the tropics the methane amount is reduced only by about one third. Thus the bulk of methane oxidation occurring in the atmosphere is above the model top boundary. Therefore I do not understand why the model shows an overall water vapour increase of the magnitude indicated. Indeed, in the atmosphere, $H_2O + 2CH_4$ is near uniform throughout the stratosphere at 6 ppmv whereas in the results of Hein et al. it increases from 7ppmv to 8 ppmv near the upper boundary.

The sensitivity of the polar processes to water vapour change need to be put particularly into atmospheric context. With virtual complete ozone destruction in polar regions in the atmosphere, within the region of PSCs, it is difficult to see how a near 40% further increase in ozone loss can be acheived by just a 1 ppmv water vapour increase, let alone any larger increase from a 5 ppmv perturbation. How does the current ozone hole simulation compare with observations? Of course a water vapour increase will increase the altitude range over which PSCs form, but the reader needs to see how this results quantitatively.

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There is a general lack of rigour in the use of language in many places, which I have pointed out below in a few cases. The text would benefit from a thorough rereading to ensure that ambiguities are eliminated from the final version.

Specific Recommendations

The abstract is loosely written and difficult to understand in places.
line 2: 'trend' should be increase.
line 13: The altitude at which the increase in OH is 5-10% is not given.
line 18-20: The sentence is not clear and a few words of clarification would help. Which is the significant factor — the increase in PSCs due to the increase in water vapour or the increase in denitrification for given PSC distribution?
line 22: '-3%' should be '3%'. line 22-23: Does the effect in the Arctic not show simply that the model has not represented the processes properly, perhaps due to insufficient PSCs for the unperturbed run?

Page 6562:
line 5: The error bar here may have little meaning. An up to date analysis (Randel et al., JAS, 61, 2133-2148, 2004) indicates that water vapour amounts have recently decreased so that the trends are now somewhat uncertain.
line 9: '-1.7' and '-5.5' should be '1.7' and '5.5' respectively.

Page 6565:
'OH-S' is poor notation, as the S could be confused with sulphur.

Page 6568-6570:
I find the summary of atmospheric chemistry confusing and could usefully be cut. Methane chemistry is complex leading to an expected 2 molecules of H_2O from each

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CH_4 molecule, so reaction R3 is incomplete. Br is mentioned, but is absent from the model. A reference to an appropriate textbook should suffice or alternately, the authors could refer to the appropriate reactions when the results are discussed later when the context becomes clear. One of the confusing issues is that the chemistry is pressure dependent, but this isn't acknowledged.

Page 6572:
The argument that ozone is enhanced during July may be plausible, but R12 to R14 do not give the full picture concerning O_3 change from the NO_x and HO_x cycles. Strictly, enhanced photolyis means that the product $J_{NO2}[NO_2]$ is higher. Is the argument that J_{NO2} is higher or NO_2 higher? If the former, what is the relevance of R12? If the latter, the NO_x ozone destruction cycle will counterbalance (to some degree) this extra ozone production.

Page 6575:
line 13: It is more usual to think of PSCs in the atmosphere covering the altitude range, say 15-25km and for temperatures below 195K (pressure dependent, of course). If the model uses lower temperatures then this should be indicated.

Page 6576:
line 20-23 and lines 26-27. The statistics are poorly presented such as to be ambiguous. In arithemetic 'x up to y' means $x < y$, but I don't think this is what is meant here. I suggest replacing the offending text in lines 20-21 by 'Tropospheric reductions do not exceed 1%.' and relacing the offending text in lines 22-23 by 'The most pronounced ozone reductions.....10%(3%)....' Similarly for lines 26-27. Incidentally this also changes the meaning to a more precise form, since there might arguably have been increases in some places.

Page 6577:
line 16: Does the surface UV calculated take into account cloud effects? The cloud corrected UV will be less sensitive to ozone than the clear sky UV because the shorter

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wavelengths in the direct beam will be preferentially reflected. In fact looking at the two figures 12 and 13, the annual mean ozone and UV changes are about the same in %, so it looks like a clear sky calculation. If this were the case, the comment in the text that the two results are correlated follows automatically from the calculation method, and would therefore be essentially misleading. I suggest that the authors remove this figure and associated comments unless cloud effects are considered. If cloud effects are being considered, a reference for the UV model needs to be supplied so that its quality can be judged.

Page 6578:
line 11: A comment needs to be made as to why H_2O increases affect the Antarctic but not the Arctic. Is this a model dependent feature, or a likely aspect of the atmosphere? Since all the other results agree essentially with previous work, in my view this is the most important conclusion of the paper, and needs developing.
line 24: The sentence 'The ozone reduction....' contains no verb. '-1%' should be '1%' and '-0.07' should be '0.07'.
line 25: WMO (1986) is now just a fond memory. Apart from which, it contains over 1000 pages, and I for one don't intend verifying the reference (and perhaps the authors don't know where this is said either, the reference just being part of folklore). Out of respect for the reader, the authors should refer to the page number or to any modern textbook.

Page 6578:
All the - signs should be removed from the numbers indicated. The fact that Dvortsov and Solomon didn't get all the signs right either, is no reason to make the same mistake!

Page 6580:
It is unclear what is meant by 'The cooling effect on stratospheric temperatures'. I suppose the authors mean the increasing GHG effect on stratospheric temperatures. This

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was actually first shown by Austin et al. (1992). Both that and shindell et al. (1998) are out of date and references instead should be made to WMO (2003), Chapter 3 or Austin et al. (ACP, 3, 1-27, 2003), but any statement has to be very carefully worded because the impact of increasing GHGs is still unclear. As it stands, even allowing for a reference change, stating that 'A prolonged increase in water vapour....' is simply scare-mongering. The authors provide no quantitative basis for a water vapour increase from observations (see Randel et al., 2004) and the source of the water vapour increase in the model is not explained. One could equally as likely say that the water vapour will drop (by extrapolating recent measurements) which will accelerate ozone recovery. This is the dilemma of the whole paper: how model weaknesses (biassed H_2O, low upper boundary), can allow one to make valid conclusions about the real world. In any case, according to the authors, water vapour increase has only a small impact on the Arctic.

Table 4:
Arctic, mid-latitudes, Antarctic, Tropics and spring are not specially defined. The latitude and time ranges need to be specified, and whether the figures are weighted for latitude.

Figure 2a:
The transient simulation (unreferenced, presumably being written up?) was stated as being from 1960-2000, but the figure shows the results clearly starting in about 1953.

Figure 6,8,9:
The axis labeling is a bit loose: the abscissa should have discrete points in time.

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General suggestions

The Conclusion section is not well structured, containing a sequence of discussion and conclusion aspects. It is also not always clear the direction in which the argument is proceeding. for example the paragraph on Page 6579 concerning supersonic aircraft at first seems irrelevant, but its significance becomes clear only in the last line of the paragraph. A hurried reader (or reviewer) may simply miss all this. My suggestion is to create a discussion section in which previous studies are examined and compared with the DLR model results.

Ozone in the spring Arctic and Antarctic need to be shown from the unperturbed model run in comparison with observations. Otherwise, the perturbed polar calculations will have little credibility.

I would like the authors to add a section to the paper explaining the impact of the model bias in tropical tropopause temperature and the impact of the low upper boundary on the results. These criticisms of the upper boundary have been in the community for several years now so the authors are presumably well aware of the issues.

I would like the authors to include a figure showing $H_2O + 2CH_4$ from the control simulation so that the realism of the model for simulating H_2O impacts can be properly judged.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6559, 2004.

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Interactive Comment