

Interactive comment on “Mid-latitude ozone changes: studies with a 3-D CTM forced by ERA-40 analyses” by W. Feng et al.

Anonymous Referee #3

Received and published: 22 August 2006

1 General comments

The paper is an update and an extension of previous publications on long-term simulations using the SLIMCAT model. More specifically, using ERA-40 analyses the impact of increasing halogens and additional bromine from short lived source gases is examined now for the period 1980-2004. The importance of the latter species has been realized during the last years. CTMs serve as sophisticated evaluation test beds for climate chemistry models (CCMs). Comparing state-of-the-art CTMs as SLIMCAT which use the newest modules describing chemistry and micro physics and comparing most recent observation data sets is therefore of highest relevance for forecasting which development the ozone layer will take.

Interactive
Comment

CTMs depend strongly on the quality of the meteorological analyses used. Previous studies have shown that using ERA-40 winds results in a too strong residual circulation affecting the chemical state in the stratosphere. Using a special configuration this SLIMCAT version avoids this deficit and could therefore be used to run simulation up to 2004, which is important when analysing the expected overturn of the CI burden and its consequences for ozone chemistry.

The detailed study of the influence of different Br source gases and the comparison of model runs with and without time dependent halogen yield rather direct results, as the authors themselves state in the conclusions, confirming previous studies. However, the role of dynamics for determining the ozone trend in midlatitudes and the question how the description of stratospheric transport and the use of analyses influence the results, remains obscure. Especially in that context, many statements are weak. I would like to ask the authors if it is not possible to substantiate their hypotheses, for example by comparing the model with observations not just for ozone but also for other long-lived tracers. For example, it would be important to know if the model deficits are caused by problems in describing the transport of polar depleted air masses to mid latitudes or in the description of the residual circulation.

Regarding the bromine sensitivity runs the impact of the new approach by including short-lived bromine source gases (and not just scaling CH₃Br) is not analysed in too much depth. Probably this could be improved easily by the authors and the bromine topic could then be reflected in the title. From the title the paper seems to focus on forcing aspects which is not really true.

The description of the experiments and comparisons are in general sufficiently complete and precise, but need some extension in some cases (see specific comments).

In summary, the paper should be published in ACP after the revisions outlined above and specific corrections have been included.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

2 Specific comments

Abstract, p6696, 13: ERA-40 ... analyses please add: where the vertical advection in the θ domain is calculated from diabatic heating rates.

Abstract, p6696, 18: ERA-40 analyses One should be cautious in the abstract with statements which are not really proved in the paper (see comment on conclusions).

Abstract, p6696, 110, some model fields This statement seems to be rather unspecific and this topic is not really analysed in the paper itself.

sect. 1, p6696, 124, likely contributing processes Just combine with the next sentence in order to avoid too general statements.

sect. 1, p6696, 126, dynamical changes Please just list the most important.

sect. 1, p6697, 11, different processes have not brought together In view that a CTM will not be able to study dynamical processes as such, and that on the other hand some CCM studies incorporate sophisticated chemical and microphysical modules, this remark is somewhat misleading.

sect. 1, p6697, 12, Changes in the observed variations It not clear what exactly is meant: changes of variability?

sect. 1, p6698, 116, This approach Here the authors trigger the reader to expect the paper to answer this question. Indeed, there is a run D, which could be compared to run A. But thus is not done in the paper in detail. I couldn't find a graphical comparison of these runs nor a thorough discussion. You could leave run D out or you could give this comparison a focus which would be very valuable.

sect. 1, p6698, 126, Salawitch (2005) From here one would expect a more detailed comparison of the present approach and the one of Salawitch in the course of in the paper, which would be an interesting add-on.

- sect. 2, p6699, 14, perform well ...** Leave out or say perhaps: reasonable agreement between observations and the model has been found in many cases. In view of the problems of this long-term study, do you mean more specifically seasonal studies?
- sect. 2, p6699, 19, better** I would prefer to avoid such general judgements (which can be found also at other places in the text).
- sect. 2, p6699, 19, even with ERA-40** The authors should give some hint for the reader that there has been some problems using ERA-40 and what the problems are.
- sect. 3, p6700, 11, whole paragraph** What is the meaning of this paragraph? It seems to be a reasoning for the chosen model setup, and could be therefore part of section 2.
- sect. 3, p6700, 114, (e.g. Dorf ...)** Is that meant as a reference? The profile shown here extends down to the surface, in the cited paper profiles above 15 km are shown.
- sect. 3, p6700, 119, is due to** can be explained by
- sect. 3, p6701, 11, whole paragraph** Suffers from the fact that cited paper is unavailable at the time when the report was written.
- sect. 3, p6701, 18, Clearly, the model run C** As model results are shown of run C and not for run D, is it possible to show also Bry for run C?
- sect. 3.2, p6701, 112, column** Is the mean column area weighted?
- sect. 3.2, p6702, 16, are larger for all runs** How much larger? Is that really true in the SH despite the higher ozone loss? At least inspecting Fig. 2 in Chipperfield 2003, this seems not to be obvious.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- sect. 3.2, p6702, I9, overestimated** What are the reasons for this overestimation? In respect of the influence of Br chemistry, this overestimation should be discussed in more detail.
- sect. 3.2, p6702, I10, optimum** This is a very strong statement. Perhaps you could say, that for several tested configurations of the SLIMCAT model, the formulation using heating rates and σ/θ levels yielded the most realistic results.
- sect. 3.2, p6702, I11, might tend ...** Is this a strong effect? How much younger is the model compared to observations?
- sect. 3.3, p6702, I20, TOMS/SBUV dataset** Why different datasets are used (sect. 3.2) ?
- sect. 3.3, p6702, I27, overestimated** Can you give a reason for the overestimation? The smaller overestimation in the SH: is that caused by the higher ozone loss in the SH? Comparing with Chipperfield 2003, the post-Pinatubo period is now much more pronounced. Can you comment on that?
- sect. 3.3, p6703, I8, because ... full** The statements here and at other places regarding the influence of dynamics should be substantiated by comparisons with long-lived tracers. How do the ozone profiles compare with the former runs?
- sect. 3.3, p6703, I9, reproduced** Obvious only in the NH.
- sect. 3.3, p6703, I11, changing analyses** What exactly do you mean?
- sect. 3.3, p6703, I13, significant change** What do you mean with significant? With respect to the interannual variability the last two years of the simulation are much less conspicuous than the mid 80s.
- sect. 3.3, p6703, I15** Why is run D not discussed?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

sect. 3.3, p6703 You argue, that much of the positive deviation of ozone in the late 80s is related to the inclusion of the lowermost stratosphere. Is this related to the relative underestimation during the Pinatubo period and, at the end, could both deviations be related to the handling of aerosols in the model?

sect. 3.3, p6703, I15, whole paragraph A comparison with 2D models would be interesting (see introduction).

sect. 3.4, p6704, I15 What is the role of the QBO?

sect. 3.4, p6704, I18, CH4/Cl_y This needs more explanation or references. What about the influence of NO₂?

sect. 3.4, p6704, I23, unrealistic ... Please give a reference for that finding.

sect. 3.5, p6705, I13, EESC Which scaling for bromine has been used?

sect. 4, p6707, I1, whole paragraph First, you should note here again, that you are using a special configuration which also could influence your findings; you are not using the analyses as they are. Second, as the setup of the model has changed (by extending the model domain and probably some chemistry modules) you cannot really rule out other influences without making specific sensitivity runs. You may have done them, but they are not mentioned in the text. In addition, the influence of the rather coarse horizontal and vertical resolution cannot be completely neglected.

Fig. 2, caption, updated from what does updated mean?

3 Technical corrections

Abst, p6696, I16, not increase to due to

S2622

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Abst, p6696, I17, changes to changes of

sect. 3, p6700, I16, stratosphere

sect. 3.3, p6703, I19, decreased decrease

sect. 3.4, p6703, I22, ... 3 ... change to: three

sect. 3.4, p6704, I21, at 40 km and 20 km

sect. 3.5, p6705, I22, selected runs change to: run A-B and A-C

Figs, legends the denotation of the model runs in the legend should be the same in every figure, as should be the colors of the corresponding lines

Figs, O₃, O₃, ozone the labeling in the figures should be the same

Figs, labels in some figures difficult to read and should be enlarged

Fig. 4, lines colour (esp. dotted) lines difficult to discriminate, black line dominates

Fig. 5, green line please give a hint that lines coincide

Fig. 5, denotation please denote a-d as the order is different to Fig. 4

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6695, 2006.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)