

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

W. Feng et al.

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We thank the reviewers for their comments and suggestions. Our replies are given below with an indication of what we will change for the final ACP paper.

Reply to Referee #1

> I find the paper well-written, thorough, interesting and enlightening and recommend publication to ACP subject to satisfactory dealing of the following issues:

1. The model ozone mid-latitude anomalies were constructed using the model output...

a) The model saves a variety of output files. These include zonal mean fields for all species on the 15th of the month (used in the paper). There is also a file with only a

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few fields, including 3-D global O_3 , saved every 2 days. In the past we have used these files to calculate anomalies and not seen a significant difference compared to the zonal mean. In the revised paper we can plot the O_3 figures using output from these files.

b) We will investigate the effect of using monthly v 2-daily fields and adjust the caption accordingly.

c) The deseasonalised model time-series in figure 3 are also derived from monthly snapshots (we will clarify the caption whether we keep this output or use the 2-daily fields). We will see if the higher frequency output improves the comparison but, when compared with ozone profiles, we find that 'daily' output overestimates O_3 in the lowermost stratosphere/upper troposphere around 100 hPa.

> 2. *The "accentuated" N.H. ozone decreases...*

The same data is plotted in both figures. By eye it does seem if the 1993 dip in the model deseasonalised curve is relatively deeper and broader than the observations and so would be expected to lead to a larger anomaly. We have not applied smoothing in the anomaly calculations. First, we calculate the monthly anomaly relative to 1980, i.e. $\text{anomaly} = (O_3 - \text{mean}(1980)) / \text{mean}(1980)$, then take the anomaly values and calculate the mean of 12 anomalies for each year. This method is also used for the forthcoming WMO (2006) assessment following a discussion among the authors on avoiding using data smoothing. For this paper it will better to be consistent with WMO (2006) than WMO (2002). We can add an explanation to the ACP manuscript.

> 3. *Does the modelled ozone feed back in the radiation scheme? Please clarify in the model description.*

Yes. For the heating rate calculation using the radiation scheme, we have used the simulated O_3 to calculate the solar/longwave radiation. This will be noted in the ACP

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paper.

Reply to Referee #3

> *The detailed study of the influence of different Br source gases*

In the ACP paper we will expand on the comparison with observations. On this point we will compare with HALOE CH₄ as a test of the long-term transport. Along with the reference to the modelled age of air (Chipperfield 2006) we are confident that the model Brewer-Dobson circulation is reasonable. It is worth re-iterating that we are not arguing that we can quantify the role of dynamics in mid-latitude ozone trends. On the contrary, we argue that, although the ERA-40 analyses can give a reasonable circulation with which to do chemical experiments, long-term variations in the analyses preclude using this dataset to derive long-term trends.

> *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...*

We will expand the discussion of the bromine experiments. We will think about changing the title. However, as the paper is not just about bromine, if we mention that we will need to also mention halogen trends etc and it will likely get unworkably long.

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

See response to specific comments.

2. Specific comments

> *Abstract, p6696, l3: ERA-40... analyses: please add: where the vertical advection in the θ domain is calculated from diabatic heating rates.*

The vertical advection in the θ domain is calculated from diabatic heating rates from 350 K upwards. This will be noted in the ACP paper.

> *Abstract, p6696, l8: ERA-40 analyses: One should be cautious in the abstract with statements which are not really proved in the paper (see comment on conclusions).*

There are a limited number of external forcings in the model. The chemical boundary conditions change only slowly and the aerosol loading is prescribed with known variations (e.g. Pinatubo increase). Clearly, the only external forcing which can drive such interannual variability is the meteorological analyses. Hadjinicolaou et al (2005) also performed a similar experiments with ERA-40 but with a parameterised ozone tracer (i.e. no other external forcing). Their model with parameterised ozone also showed a similar positive anomaly in NH column O₃ in the late 1980s.

> *Abstract, p6696, l10: some model fields: This statement seems to be rather unspecific and this topic is not really analysed in the paper itself.*

Specific examples shown in the paper are temperature (Fig. 9) and O₃ (Fig. 5). These will be mentioned more explicitly.

> *Sect. 1, p6696, l24, likely contributing processes: Just combine with the next sentence in order to avoid too general statements.*

We will try to combine these sentences. (Note that another Reviewer has also asked us to mention solar cycles and aerosol loading here and so the text will change anyway).

> *Sect. 1, p6696, l26, dynamical changes: Please just list the most important.*

Dynamical changes (i.e. tropopause height changes, changes in the meridional cir-

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culation) are considered to be important in certain circumstances (NH mid-latitude winter/spring) and so they are worth mentioning.

> *Sect.1, p6697, l1, 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.*

It means that no single study has yet included all important processes in a complete and detailed way. We will add the word 'yet'.

> *Sect.1, p6697,l2, Changes in observed variations...*

We mean that what might be considered as a 'trend' over a short timescale, may appear more as (long-term) 'variability' when looked at over a longer timescale. For example the low column ozone values of 1993 now appear as a transient negative anomaly against a much smaller long-term trend. We will change 'changes' to 'temporal changes'.

> *Sect.1, p6698, l16, This approach...*

Both run D and run A are shown in Figure 1 (and run A is better). We will add some more information of Bry differences between run D and A.

> *Sect.1, p6698, l26, Salawitch (2005)..*

We will add more discussion with respect to Salawitch et al (2005). (See also reply to D. Weisenstein).

> *Sect.2, p6699, l4, 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?

We will rephrase this.

> *Sect.2,p6699, I9, better: I would prefer to avoid such general judgements (which can be found also at other places in the text).*

This sentence is based on the conclusions from Chipperfield (2006). We will add text to make the sentence read: 'better representation of vertical transport and age-of-air even with ERA-40 analyses than using vertical winds derived from the analyses (Chipperfield, 2006).'

> *Sect.2, p6699, 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.*

We will add references to indicate the ERA-40 problems.

>

Sect.3, p6700, I1, 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.

We will leave this as Section 3 as we will be expanding the Bry discussion (e.g. Run A v Run D) anyway.

> *Sect.3, p6700, I14, (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.*

That is a reference for the DOAS instrument. We are showing the full profiles here. There is a further Dorf et al (JGR, to be submitted) that we will reference in the ACP paper.

> *Sect.3, p6700, 119, is due to can be explained by*

OK.

> *Sect.3, p6701, 11, whole paragraph suffers from the fact that cited paper is unavailable at the time when the report was written.*

OK, this should change for the ACP paper.

> *Sect.3, p6701, 18, Clearly, the model run C: As model results are shown [for] run C and not for run D, is it possible to show also Bry for run C?*

The reviewer actually wants a comparison of runs A and D for Bry (see above). Including run C as well will make the plot very busy and the removal of the Bry compared to run A really does just have the expected scaling effect.

> *Sect.3.2, p6701, 112, column: Is the mean column area weighted?*

Yes. This will be noted.

> *Sect.3.2, p6702, 16, are larger for all runs: How much larger? Is the really true in the SH despite the higher ozone loss? At least inspecting Fig.2 in Chipperfield 2003, this seems not to be obvious.*

We will add a line from Chipperfield (2003) for a direct comparison.

> *Sect.3.2, p6702, 19, overestimated: What are the reasons for this overestimation..*

The model gives more ozone in the lowermost stratosphere around 350 K compared with ozone profiles. That is the main reason for this overestimation. Including the additional bromine does not resolve this problem. This will be noted.

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> *Sect.3.2, p6702, I10, optimum.. .*

We will change to 'most realistic'.

> *Sect.3.2, p6702, I11, might tend...: Is this a strong effect? How much younger is the model compared to observations?*

We will add in an explicit reference to Chipperfield (2006). At 20 km the relative discrepancy in age compared to the tropics for this version of the model reached about 1 year at high latitudes.

> *sect.3.3, p6702, I20, TOMS/SBUV dataset: Why different datasets are used (sect.3.2)?*

Section 3.2 uses the TOMS data as used/supplied by P. Newman who performs this analysis.

> *Sect.3.3, p6702, I27, overestimated..*

The simple reason is that the model overestimates O3 in the lowermost stratosphere where now it is fully calculated rather than being near the constrained bottom boundary.

> *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?*

It is not clear what is meant by 'former runs' - we don't have access to the Hadjinicolaou et al profiles. We have described the profile comparisons. In the ACP paper we will show HALOE O3 comparisons anyway. If space permits we can include other profiles.

> *Sect.3.3, p6703, I9, reproduced: Obvious only in the NH.*

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We will add: 'especially before the early 1990s in the NH'.

> *Sect.3.3, p6703, I11, changing analyses: What exactly do you mean?*

We mean changing ECMWF analyses from ERA40 to operational analyses (e.g. see Fig 4. caption).

> *Sect.3.3, p6703, I13, significant change: What do you mean [by] significant? With respect to the interannual variability the last two years of the simulation are much less conspicuous than the mid 80s.*

We meant that the SH anomaly appears to turn down sharply. In the ACP paper we will change 'significant' to 'noticeable' to avoid any misinterpretation.

> *sect.3.3,p6703,I15: Why is run D not discussed?*

Run D is a particular model experiment and comparison with this is useful for understanding the impact of treating Bry with certain approximations. Comparison of Runs D and A are relevant for the Bry comparisons (Section 3.1). However, the results are not that useful for understanding the impact of Bry from short-lived species - for this comparison of runs A and C are most relevant.

> *Sect.3.3, p6703: You argue..*

We argue that including the lowermost stratosphere causes an overestimation of column O₃ in general. We also argue that the ERA-40 circulation is responsible for the relatively larger overestimation in the late 1980s (see also comment above on the Hadjinicolaou et al study). Circulation changes do also contribute to the post-Pinatubo low - it is not all aerosol (see Chipperfield (2003) and Hadjinicolaou et al (1997)). The positive 1980s anomaly is not due to the modelled aerosol field

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

That is beyond the scope of this paper. We expect that such a comparison may be included in Chapter 3 in WMO (2006).

> *Sect.3.4, p6704, I15: What is the role of the QBO?*

ERA-40 analyses do have a QBO but we have not analysed that here. In the ACP paper we will compare with HALOE CH₄ data and if this reveals anything related to the QBO we will comment on that.

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

This will be expanded. NO₂ is not expected to be important for controlling O₃ at 40 km (more important lower down).

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

This is a comment we are making based on our Figure 9. A 10 K change in the smoothed average temperature over 2 years is surely unrealistic?

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

A Mixing ratios of bromine are multiplied by a factor alpha (60 is used).

> *sect.4,p6707,I1, whole paragraph..*

We will add some text to mention that we are using the analyses in a particular way and that resolution of the runs done here is relatively coarse. For the comment about the model domain and ERA-40, as we say above the spurious long-term variability can

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be seen in other studies.

> *Fig.2, caption, updated from: What does updated mean?*

It means that we show more years of observations than shown in the original Newman et al (1997) paper. The word is often used in this way on figure captions.

Reply to Referee #4

> *The paper is well written and refers to recent publications in the field of the ozone recovery study..*

The paper is more than simply an update of Chipperfield (2003). The information on the role of bromine is all new. The longer time series of runs allow us to look at the cause of the increase in mid-latitude column O₃, which was not possible before. Some model comparisons do appear 'worse'. However, a significant factor in that is due to the use of ERA-40 analyses rather than ERA-15. As ERA-40 extend over a much greater altitude range and over a longer time period it is still reasonable to use them and see how they perform.

> *General comments: 1. Feng et al. (2006) vs. Chipperfield (2003)...*

The global circulation in this formulation of the model is reasonable (i.e. good age of air calculation, tape recorder signal etc - see Chipperfield (2006)). To repeat: the Chipperfield (2003) runs used a lower boundary of 350 K and an imposed O₃ mixing ratio there. The lower stratosphere was constrained to be realistic while now O₃ there is calculated. It can happen that when the domain of a model is extended, and more processes/parameters become calculated that previously unknown discrepancies come to light. More discussion will be added in the ACP paper.

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> *P.6702,L6: I would add "by 10-20 DU" after "for all runs".*

We will add this.

> *Figure 2 and 3a and b: I would overplot the output of Chipperfield (2003) run A. That will ease the comparisons and the discussions.*

We will overplot results from Chipperfield (2003).

> *2. Measurements vs Model The calculated deseasonalised O3 time evolution as presented in the manuscript gives very intriguing results compared to measurements (Figure 3 and 4) at mid-latitudes..*

This difference, as we say in the paper, is due to differences in the ERA-15 v ERA-40 data which is use to force the model. The ERA-40 data appears to have spurious long-term variability and this causes the positive O3 anomaly in the late 1980s. (There will be changes in data which are assimilated and so reanalyses cannot be taken to be homogeneous datasets). An important message of the paper is therefore that analyses such as this cannot be used for long-term dynamical trends studies.

> *The sensitivity of ozone change to parameters like Cly, ClO, CH4 and temperature is presented...*

In the revised ACP paper we will include comparisons with HALOE data, which offers the longest time series of data with which to compare with the model. The discrepancies between the HALOE data and new datasets from AURA and ACE are worthy of many papers in their own right and so for these datasets it will be better for us to cite papers such as Froidevaux et al. who discuss differences with HALOE.

> *3. Other parameters influencing ozone long-term evolution Great emphases have been given to the influence of halogen and bromine...*

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We can add a mention of the solar cycle and estimate its effect on the long-term evolution of the anomaly (as in Chipperfield (2003) based on observed variations). The effect of Pinatubo has been investigated elsewhere and we do not repeat that here. In our case the runs done with ERA-15 winds are still relevant here.

> *4. High vs. Mid-latitude Results* The manuscript title focuses on "Mid-latitude ozone change..." although one third of the paper is dedicated to high latitude ozone change..

With the other changes to the paper the proportion devoted to high latitudes will decrease. We have included that as interannual variability in Arctic column ozone is large and it is useful to see how well these ERA-40 winds capture this, and what the impact is on modelled O3 loss. This 'Newman' plot gives an indication of that in a concise way.

> *The calculated amount of BrO vertical profile is compared with measurements performed at Kiruna (67N). The agreement is indeed very good...*

Comparisons of BrO at these sites will be contained on a forthcoming paper Dorf et al. We will cite this paper.

Specific comments

> *"Volcanic loading" (p6696, l21) is mentioned in the abstract although no real studies have been performed, shown or even discussed in the manuscript.*

This comment is a bit strange. The term 'volcanic loading' is mentioned in the abstract specifically in the context of the impact of additional bromine. This is seen in Figure 4 and mentioned in section 3.3 where the anomaly from run C is very similar to run A except when the aerosol loading is high.

> *It is stated (p6701, l6) that "run A has a larger mixing ratio of Br below 25 km*

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(compared to run D)." But this impossible to check in Figure 1.

Here Br meant 'bromine', which is confusing at best. We will change that to Bry in the ACP paper.

Reply to D. Weisentein

> *The first paragraph of the introduction should mention aerosols and solar cycles, as well as "chemical loss due to increased halogens and dynamical changes" as processes contributing to mid-latitude ozone change.*

We will mention aerosols and solar cycles in the ACP paper.

> *Section 2.1, 2nd paragraph describes the modelled bromine source gases. Combining H1211 and H1301 emissions leads to some skewing of the odd bromine profile, since these two gases have rather different lifetimes. Likewise for combining the very short lived bromine species (VSLs) into CH₂Br₂, as CHBr₃ has a different lifetime (though the effect should be restricted to the troposphere in this case). These are minor impacts and don't detract from the paper, but including two halons and two VSLs would make the modelling cleaner.*

OK, but within a 3-D model there are always constraints on computer resources (CPU time, memory) in the first place and then again if one has to consider rerunning a whole series of experiments. We agree that these are minor impacts and therefore do not justify redoing all of the 3-D model runs.

> *Section 2.1, last paragraph...*

The Table is correct. We will correct the text to also indicate that run C did not have the halon.

> *Section 3.3, first paragraph should have an explanation of how the data in Figure 3 was deseasonalised ..*

Our analysis is consistent with what was done for the upcoming WMO (2006) Assessment. The WMO approach changed since 2002 to avoid data smoothing. Therefore we have not applied a smoothing technique in the anomalies calculation. First, we calculate anomaly calculation relative to 1980, i.e. $\text{anomaly} = (\text{O3} - \text{mean}(1980)) / \text{mean}(1980)$, then take the anomaly value and calculate the mean of 12 month anomaly for each year. We will add an explanation in ACP paper.

> *Section 4, the conclusion should state that this 3-D CTM agrees with the Salawitch et al. (2005) paper on the importance of VSL bromine species to the ozone trend primarily during times of elevated aerosol loading. Despite the fact that the Salawitch paper ignored VSL species and simply added 4-8 pptv of bromine throughout the stratosphere.*

We will add this comment wrt the Salawitch paper.

> *This paper is well-written and should be published in ACP. However, it should be noted that there are several aspects of the historical ozone trend which are not addressed. These include interannual variability (there is no calculation with annually-repeating temperature and circulation for comparison), solar cycles (apparently not included), and Pinatubo effects (included but no calculation without, so no conclusions possible).*

We will add some text to note that other factors affect ozone.

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

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