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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A three-dimensional model study of long-term mid-high latitude lower stratosphere ozone changes" *by* M. P. Chipperfield

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Referee 1.

General.

More discussion of Run C has been included in Section 3.3. However, the effect of aerosol chemistry on mid-latitude O3 and e.g. the Pinatubo effect, has already been discussed in Chipperfield [1999] in CTM studies using UKMO analyses. This reference has therefore been cited.

Specific Comments.

Section 2.1 / page 1086 / line 15: The 2D model results are interpolated in latitude and applied as a geographical-latitude zonal mean. The lower/upper boundary fields are updated every month with new 2D model values for that month. The shorter lived species are also overwritten, but their values adjust quickly anyway to reflect the local conditions in the 3D model. (ClO is actually part of the model ClOx family). A part sentence has been added in the 2D model description.

Section 2.2 / page 1087 / line 13: The 3D model was initialised from the 2D model fields using theta as the vertical coordinate (as opposed to pressure). Therefore, differences in the temperatures profiles between the 2D model (which calculates its own temperature but also uses theta as a vertical coordinate) and the analyses will cause a shift in the vertical profile and hence a change in the column. The 2D model T fields are too warm which results in a 2D O3 value at a certain pressure being mapped to a lower pressure in the 3D model producing a smaller column. Along with the tropospheric contribution this should account for the 2D/3D model differences in Figure 3. The 3D model with the realistic temperatures obviously gives a better simulation - which is the main focus of this paper. Some text has been added to Sections 2.2 and 3.3.

Section 3.1: Output for the full duration of Run A has been added to Figure 1b. Two sentences discussing the fact that the 3D model run stops around the time of the 2D model peak have been added.

Section 3.2 For the observations, March 1993 is the last of the Nimbus 7 points. March 1994 is the only point from Meteor 3 TOMS. However, this change in observations is unlikely to explain such large differences. Previous versions of the model forced by UKMO analyses did capture this relative variation in the early 1990s. Therefore, the explanation of why the model does not capture the variability in these runs is due to the analyses, but the situation is not as clear as implied in the first draft - i.e. that the change from ERA15 marks then end of the good agreement. As a whole the ERA15 period shows good agreement but individual years can disagree. (For example although the assimilation model may be the same the data input can change). The text in Section 3.2 has been changed to reflect this greater uncertainty.

Section 3.3 Run D has been removed from Figures 3c and 3d. The initial calculation of

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the % difference since 1980 does produce values which are zero in 1980. Then, this model line is smoothed with a 2-year running mean. Depending on the model values in adjacent years, this smoothing can cause a departure from zero. A note has been added to the caption.

Section 3.4 The model quantities in Figure 5 are the same as Figure 4. For the model it is the anomaly in the partial column.

Section 3.5 The solar cycle was not included in the model itself (i.e. as a parameterisation). Rather the estimated impact of the solar cycle (based on observations) was added to the model results for a non-solar cycle run.

To my knowledge the altitude profile of observed solar cycle has not been well determined. WMO [2003] discusses results that suggest an observed variation in the upper stratosphere from 35-45km. To get a large signal in the mid-latitude column there is also likely to be changes lower down. However, as models fail to reproduce the observed column signal it is not possible to use that information.

Logan (2003) discussed the differences between European and Canadian sonde stations. She argues there is very little difference between central European stations (e.g. Hohenpeissenberg, Payerne) and so it is reasonable to compare model output from Jungfraujoch with Hohenpeissenberg.

Model results are always potentially dependent on resolution. A sentence stating this has been added to section 5.

Section 3.6 This is a fair point about the years chosen for the profile comparison. To avoid problems with the changes in analyses and make the section more comparable to Section 3.7 I have change the 2nd pair of years to 1991/92, redone the plots and updated the discussion.

Section 3.7 The effect of Pinatubo on column O3 can be seen in Figures 3 and 4. The discussion of 'other factors' has been expanded.

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Minor Comments

Abstract / line 8: Text added.

Section 1 / page 1085 / line 21: Text changed (though it was the ACP editors who changed it originally).

Section 2 / page 1085 / line 21: Text changed to '1979-1998'.

Section 2 / page 1085 / line 26: Text added.

Section 3.5 / page 1091 / line 28: Text changed to 'lower stratosphere (LS)'.

References / page 1098 / line 13: Comma added.

Referee 2.

General Comments

1. A discussion has been added in Section 3.3 and a new reference to Braesicke and Pyle [2003] has been added.

2. More discussion of C (and D) have been added (see response to other comments). Also a citation to Chipperfield [1999] has been added.

Specific Comments

1. Page 1083, lines 20-25. A reference to WMO [2003] has been added information from that reference about the relative importance of the Upper Stratosphere has been added. Another sentence has been added in the model description mentioning the neglect of the upper stratosphere.

2. Page 1084, lines 23-26. This is true but it is very difficult to estimate this error a priori. The runs presented here are the first to have used full chemistry in a 3D CTM and there are other with parameterised chemistry. An understanding of the ability of the analyses to capture the long-term variability will have to be based on the experience of many such studies. I have added a sentence to the end of this paragraph: 'However,

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the ability of CTMs forced be meteorological analyses over longer (decadel) timescales remains to be established.'

3. Page 1088, lines 2-4. Yes, aerosol data in the period 1995 - 1998 (the end of the runs) is now available. However, the range of 3D runs presented here are computationally expensive and it would be very time consuming to repeat them. Given the discussion about the changes of analyses in the mid 1990's affecting the comparisons and calculated trends the use of fixed aerosol fields post 1995 in runs A and B would not affect the main conclusions. This would affect the comparison of run C and run A and in the new version a sentence has been added to note this limitation.

4. Section 3.2 The passive O3 lines have been left in the figure for comparison with previous similar plots for the 1990s (which are referred to). A description of the passive lines has been added to the figure caption.

5. The 'anomaly plot' in Figure 4 shows the % difference of O3 compared to the mean for the same month of 1980 and smoothed over 2 years. The 'deseasonalised plot' in Figure 3 shows the difference compared to the monthly mean over the whole run (with the annual mean over the whole run added back to put it on the same scale as the total column values) and smoothed over 1 year. Observations show the 1985 dip is restricted to 1 year and so longer smoothing would wash out the signal. I have done a test for different soothing periods for Figure 3 and using two years effectively removes the bump. I have added information about the length of smoothing in Figure 3 to the caption and added a sentence in the discussion of Figure 4.

6. The plots have been redrawn and the scale expanded.

7. This is a repeat of a statement contained in the (to-be-published) Logan (2003) reference.

8. Yes. The text has been corrected.

9. Yes, the profile changes (difference between two time periods) does change with the

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dates chosen. As the O3 column is dominated by the LS the changes here will follow the column variation. This shows the problem in extracting a 'trend' from a profile time series. This is the reason why Figure 7 shows both a profile change between two times (which is complicated by dynamical variability etc) and the difference between two model runs with different halogens. For the model results, the difference between two experiments with different halogen loading will give a cleaner, direct indication of the 'chemical' change. A sentence has been added to the end of section 3.6

10. The information that I have about Hadjinicoloau et al (2002) is based on their published results, which are restricted to the NH. They make definite statements about their results showing that dynamics has contributed to the NH O3 trends. The results of my paper argue for more caution in trying to use a CTM to diagnose dynamical trends especially outside of e.g. the ERA 15 period. (However, I do think that CTMs can be used to diagnose the chemical signal by performing different expts etc).

I think the 'differences' between Hadjinicoloau et al (2002) and the 'constant halogen' runs in my paper is one of interpretation. They presented their column O3 comparisons (their Fig 4) as a model anomaly versus observed anomaly (rather than e.g. a % change since 1980) which tends to mask the large divergence in the 1980s. Also their 'best' trend fit was between 1998 and 1979, which is somewhat arbitrary.

The text has been re-written to cover these points.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1081, 2003.