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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An evaluation of the simulation of the edge of the Antarctic vortex by chemistry-climate models" *by* H. Struthers et al.

H. Struthers et al.

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Response to review comments Anonymous Referee's

General comments, both reviews.

The review comments show that the original manuscript lacked clarity, particularly with respect to the aims and motivations of this work.

This is typified by the comment made by reviewer #2. 'All the models have increasing CFCs as part of their boundary condition and the increasing ozone hole size seen is simply a response to increasing Cl.' The main point of this work is that if the 220 DU definition of the ozone hole is used, then the ozone hole size in CCMs is not simply a response to increasing Cl but is greatly affected by model biases. Previous model intercomparisons have used the 220 DU contour to define the ozone hole threshold.

We contend this may misrepresent the horizontal extent of ozone depletion simulated in CCMs in response to changes in Cl (and Br). As we show here, comparisons of ozone hole area between observations and models can be performed in a consistent way which provides greater information about the fidelity of the models.

This work is not an attempt to study in detail the interaction between dynamics and chemistry at the vortex edge nor is this a study aimed at understanding the details of the interannual variation of horizontal extent of severe Antarctic ozone depletion and its coupling to dynamic variability.

Rather, here we simply compare the large scale mean state of the zonal mean total column ozone in observations and CCMs (focusing on the period of high halogen loading) and compare the features of the ozone fields with a measure of the strength of meridional mixing. We study how the mean total column ozone and CCMs evolved over the 1980 - 2000 period in response to changes in stratospheric halogen concentrations i.e. over interdecadal timescales and discuss two definitions of the ozone hole area that may be useful in future observation/model comparisons.

For all their success, as a group CCMs have not fared particularly well in their attempts to simulate the size of the Antarctic ozone hole and, with the upcoming SPARC special report on CCM evaluation it is timely to consider how best to perform observation/model ozone hole comparisons.

The paper, in particular the introduction and conclusions have been extensively rewritten to better clarify the important points driving the analysis described in this work. Section 5.2 and Figure 6 have been removed and Figure 7 has been altered. We have also worked to improve the writing style as suggested by reviewer #2.

We do not attempt to complete a 'process oriented evaluation' in this paper. Particularly for the kappa diagnostic, there are many potential reasons why the shape of the kappa curves differ between models and measurements (wave mean flow + unresolved gravity waves) as explained in the conclusions of the original manuscript. The

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representation of these processes in CCMs is rather model specific and it seems there is little point in attempting to diagnose specific processes on a model by model basis when the conclusions cannot be easily generalized.

Both reviewers criticize the use of the 550K potential temperature surface for the calculation of kappa. The 550K surface was chosen for this analysis to be backwardly compatible with the previous study (Bodeker et al., 2002). In that study this level was selected to be close to the 520K level used by Randel and Wu (1995) in a similar analysis. The 520K level could not be used here since model output were not available on this level and so the closest isentropic level at 550K was used. Earlier analysis in preparation for the study presented in Bodeker et al. 2002 showed that the results change negligibly if the 450K isentrope rather than the 550K isentrope is used.

Although it would be preferable to study the whole ozone hole season in the observations and models we only have October model output available on a daily basis therefore we are restricted to this month. We do not expect this to greatly affect the conclusions from this work.

Because of the extensive changes to the manuscript, many of the specific point raised by the reviewers are not relevant to the new version of the paper. Specific points relevant to the new manuscript:

Reviewer #2

p. 20161, l. 22. 'validated' replaced with 'evaluated'

p. 20163. Even for column quantities the equivalent latitude transform is well defined and unique for each theta surface but a transformed column quantity should be referenced with respect to the theta surface used for the transform. e.g. total column ozone transformed using the 550K PV. A comment in section 4, 'Observed dynamical containment of the ozone hole' has been added explaining this:

'Note that the equivalent latitude mapping of the total column ozone depends on the

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theta surface of the PV used. For brevity in the rest of the paper, the total column ozone transformed to equivalent latitude using the 550K PV will be simply referred to as the total column ozone.'

p. 20165, lines 1-9. The word 'correlation' was used in the original manuscript to mean that there is a relationship between the ozone gradients and kappa. It was not meant to imply there was any temporal correlation. We have removed the word 'correlation' when referring to the ozone gradient/kappa relationship.

p. 20169, lines 12-16.

See previous comment.

p. 20170, lines 8-15.

Discussion of the quantification of ozone transport has been removed.

p. 20173, lines 5-7.

Only one model (ECHAM4CHEM) provided model projections past 2000 (out to 2020). The wording explaining this has been modified for clarity and the time axis of Figure 7 extend to the end of the ECHAM4CHEM output (2020).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 20155, 2008.

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