

Interactive comment on “Uncertainties and assessments of chemistry-climate models of the stratosphere” by J. Austin et al.

Anonymous Referee #2

Received and published: 29 August 2002

This is an interesting and well written paper that presents an intercomparison of numerous global chemistry-climate models with an emphasis on producing accurate stratospheric ozone. The paper is relevant since it highlights several shortcomings in the ability of models to reproduce even the current state of the atmosphere, let alone future scenarios. I recommend acceptance after consideration of the following comments.

Specific Comments:

1) The authors suggest that comparing results from different models is preferable to ensemble runs "since a given model will tend to have systematic errors." (p1039.27) The implication is that systematic errors introduced by the choice of a particular parameterization and/or model parameter will be removed by sampling several models. This may or may not be true, depending on the model. The correct way to test this would be with

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ensemble runs of the same model only changing one model parameter. Even then, the non-linear interaction of changing more than one model component would need to be explored. Therefore, comparison between a selection of models and observations is useful primarily as an assessment of model performance. However, it is difficult to attribute model/data and model/model differences to choices of model parameters and parameterizations (e.g. resolution and gwd scheme). Unfortunately the authors on several occasions attempt to link differences in model predictions to a choice in model configuration, where the connection is far from certain. For example, the connection made between heat flux and model resolution (p1052.10) is quite speculative. Similarly the effects of upper boundary placement on the residual circulation cannot be determined by model comparisons. Perhaps discussions of these connections could be moved to the summary/discussion section.

2) The large variability in CMAM heat fluxes calls into question the use of β as a diagnostic for comparing models. Since removal of one data point can dramatically affect the slope of the regression line, the uncertainty in this term must be large. Can error estimates be reported for values of beta in Table 3 - this would give confidence that differences in β between models is significant.

3) The presentation of modeled water vapor increases is very limited (just one paragraph). Perhaps the modeled rates for water vapor and tropical tropopause temperature could be presented in a table, along with a discussion of differences in how water is handled in each model?

4) The abstract implies full Antarctic ozone recovery is predicted by 2050. In reality, this is based on just 2 models and has considerable uncertainty. If anything, the paper convinces the reader that current models fail to reproduce the basic atmospheric state, and call into question such predictions.

5) Is the highly-variable 'minimum daily ozone' the best diagnostic for ozone depletion in the Arctic? (p1063) The discussion implies that there is too much inter-annual vari-

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ability to determine an Arctic ozone trend. In addition, the large scatter requires the use of a Gaussian filter (the FWHM of which is not specified). Perhaps an area average would decrease the noise level. To ease comparison between hemispheres, it would be preferable that Fig. 10 (upper panel) be in the same format as Fig. 9 (i.e. separation of transient and timeslice runs).

6) Could the authors define what is meant by "pattern correlation" (p1048.21). Is this simply a correlation between all (area-weighted) points?

Technical Corrections:

p1068.20 should MA-ECHAM be MAECHAM/CHEM?

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 1035, 2002.

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