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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Past and future simulations of NO_2 from a coupled chemistry-climate model in comparison with observations" *by* H. Struthers et al.

H. Struthers et al.

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Point 1.

At no point in in the paper do we state that R5 (N2O + O(1D) -> 2NO) is the only or major loss process for N2O. However, the abstract has also been re-worded to clarify the distinction between N2O loss processes and NO2 change:

"Trends in NO2 derived from a 45 year integration of a chemistry-climate model (CCM) run have been compared with ground-based NO2 measurements at Lauder (45S) and Arrival Heights (78S). Observed trends in NO2 at both sites exceed the trends in N2O, the primary source gas for stratospheric NO2. This suggests that the processes driving the NO2 trend are not solely dictated by changes in N2O but are coupled to global atmospheric change, either chemically or dynamically or both. If CCMs are to accurately

estimate future changes in ozone, it is important that they comprehensively include all processes affecting NOx (NO+NO2) because NOx concentrations are an important factor affecting ozone concentrations. Comparison of measured and modelled NO2 trends is a sensitive test of the degree to which these processes are incorporated in ....."

Point 2.

One simplified arguement is as follows: NOy amount is controlled by transport and chemical production. In the lower and middle stratosphere the production rate is proportional to the N2O amount through the reaction R5. Therefore over long timescales, NOy will reach a near steady state with transport (v.grad) proportional to N2O amount. Transport is dominated by the vertical gradient, so assuming a constant scaling length for NOy implies that NOy should be approximately proportional to the product [O1D][N2O]. This relationship is evident at mid latitudes in the lower stratosphere from in the Plumb and Ko correlation diagrams.

The relationship (N2O vs NOy) has been studied by Olsen et al. 2001 JGR 106 D22, 28771-28784, the results of which were used by McLinden et al. 2001 JGR 106 D27, 787-793. McLinden et al. do not constrain their modelled NOy trend to be equal to the prescribed N2O trend but find that the two are in agreement. This reference has been added to the introduction.

Point 3.

The comments about UMETRAC are not well informed. UMETRAC is a state of the art model (see for example Austin et al., 2003) but as with all models can be improved. The model is certainly cheaper than it could be, but that does not alter the fact that a 40 year simulation requires some 70000 Cray T3E processor hours, depending on processor speed, a not insignificant amount. Other results with the model have not shown a particular defficiency in the vicinity of the subtropical barriers e.g. the presence of an ozone QBO is well marked (Butchart et al., 2003) despite the fact that ozone is

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influenced in part by the chemistry of long lived tracers. Some tracers such as CH4 are indeed specified but the biggest impact on methane is due to oxidation by OH rather than reaction with Cl. OH is approximately constant as the water vapour is fixed in these runs. The production of HCl through the stated reaction is, however, reasonably well represented as HCl is fully included and transported independently as one of the 15 transported species, as indeed are the other molecules mentioned by the referee. In short, the model has a substantial pedigree dating back to 1991 and the aspects that the referee refers to have been thoroughly researched. If the referee is interested in reading more he/she can refer to Austin and Butchart (2003) and earlier works referenced therein. The model does use a modern advection scheme but if you want to run at the spatial resolution of UMETRAC the cost is nonetheless very high. Further, few modern advection schemes are able to simulate the age of air correctly, a typical result is 30% underprediction. So, a free running model would in any case underestimate the amount of Cly particularly in the polar regions. We have been able to avoid this issue with our parameterisation of the long-lived species. Independently of the referee's suggestions we have been working on an improved model version which accepts the increased cost of advecting all long lived species explicitly. However, adjustments to the model results need first to be made to correct the low age of air issue.

Point 4.

The quoted stratospheric water vapour trend of 1% per year was based on the analysis of Rosenlof et al. 2001 GRL 28, 1195-1198, who study 10 independent water vapour data sets (including HALOE). We have re-worded the paragraph and added an additional reference (Randel et al. J Atmos. Sci. 61, 2133-2148, 2004).

"Stratospheric NO2 amounts have been shown to be sensitive to changes stratospheric water vapour (Fish,McLinden). Measured trends in stratospheric water vapour are uncertain (Sparc, Rosenlof,Randel). In this study, UMETRAC used a fixed climatology of stratospheric water vapour which may contribute to the discrepancy between modelled

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and observed NO2 trends. Because of the uncertainty in the observational record, no attempt to test the effect of changing stratospheric water vapour in the model has been considered to date."

Point 5.

Tables 1 and 2: the numbers are percentages of column amounts. Specification of this has been added to the table captions.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4545, 2004.