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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An evaluation of the performance of chemistry transport models by comparison with research aircraft observations. Part 1: Concepts and overall model performance" *by* D. Brunner et al.

Anonymous Referee #2

Received and published: 14 July 2003

Dear Authors,

I have read your paper with enjoyment. You have clearly put an enormous amount of effort into aggregating a number of disparate sets of observations into a single, unified data set which will be of significant use to the modelling community. You have also provided a number of interesting examples as to how the data may be used to review model performance. A range of useful graphical techniques have been used both to display the spatial extent of the data and the model performance. The assessment of model performance is also reasonably comprehensive, and points the way forward for further model improvements.

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I have a similar concern to the other referee in that care must be taken in comparing the volume average 'instantaneous (what does this mean given that the semi-empirical atmospheric diffusion equation is time-averaged?) model predictions with a volume and time averaged aircraft observation. Again, the methodology for calculating S takes some account of this, as does, to some degree, the point-by-point output shown in Fig 10a. The paper would benefit from some additional discussion as to the merits and issues associated with undertaking such a comparison.

Small Issues. Given the importance of modelling vertical transport and dispersion processes, it is suggested that Table 1 list the methodologies used by each model (I'm assuming that they are not the same as used for horizontal transport).

Table 2- It is suggested that the emissions used by each model be placed explicitly in this table.

Considering the discussion on the inclusion of instrument noise into Ro, is instrument lag or response time also an issue which needs to be included?

Model biases- [mean_model-mean_obs]/mean_obs-> what is the average taken over? Also, the use of the normalised bias can lead to problems when the observed concnetration is small. Errors between observed and modelled can be amplified. Have concentration lower bounds been used to overcome this problem? If so , they should be stated. Have the authors considered using the fractional bias [2(mod-obs)/(mod+obs)]?

Why has the TM3 model been used to provide an observed NOx surrogate using observed NO and the predicted NOx to NO ratio? Also, I'm concerned that for very small NO concentrations (and large NOx:NO ratios) the scale factor would be very large-amplifying uncertainties in the NO measurements. Would an estimate of NO2 from NO and O3 and an assumption of steady-state be a useful alternative?

Technical trivia

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2.1. CTM and C_GCM already defined; HO2 should have subscript; adopt rather than adapt;

2.2 reference to Fig. 1a and b

3.4 Meridional distributions.... Is the discussion referring to Fig 10 about NO (as captioned) or NOx? Also the second reference to Fig 10a should be Fig 10b.

Fig 8. Nomenclature for bias should match that in the text Fig 9. Contours refer to the skill scores Fig 10. NO or NOx?

The paper needs to have some of the grammar improved to help with readability.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2499, 2003.