Atmos. Chem. Phys. Discuss., 7, S99–S101, 2007
www.atmos-chem-phys-discuss.net/7/S99/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of model chemistry and data biases on stratospheric ozone assimilation" *by* L. Coy et al.

Anonymous Referee #2

Received and published: 30 January 2007

This is an interesting study of the influences of parameterized chemistry and data biases in a stratospheric ozone data assimilation system. It certainly merits publication, but there are a number of minor points that I'd want the authors to address before the paper is finalised in ACP.

I am curious as to why the authors chose the case of September / October 2002; the unique Southern Hemisphere major warming means that the results may be atypical. Or is it thought that this case might highlight issues better than more typical cases? Or is the intention to compare results with those of Geer et al (2006a)?

The coupling of the GMAO ozone assimilation with the NOGAPS-ALPHA model looks neat. Though it would be easier to understand if it was clearer from the outset how

the coupling frequency, analysis cycle and model timestep were related. For example, on p1071 we read that one advantage of the coupling used by GOATS is that “time interpolation of the meteorological analyses is not necessary”. But, it is only on p1076 that the authors mention a 6-h ozone analysis cycle - which would not require time interpolation in any case. As I recall, the GMAO ozone assimilation system can be (and has been) run with a short assimilation cycle, where the GOATS approach to coupling would be advantageous. Or do the authors use a 3D-FGAT type approach?

Two different sets of SBUV/2 data are used, one of which is referred to as “biased” and one “unbiased”. Please could the authors add some comments (or give a reference) that justifies this, compared to independent data - if possible. The reference data set (Fortuin and Kelder climatology) is based largely on SBUV data, but who’s to say that that data is not biased too? Figure 9 indicates that both of the SBUV/2 data sets seem biased at low levels, compared to the climatology.

The main results showing the effect of observation biases are well presented. The tests with the “adaptive” ozone parameterization were interesting, and demonstrate that this is not a straightforward fix to the problem. One could draw the conclusion that it would be desirable to bias-correct the observations before they are assimilated - though that begs the question of how that can practically be done.

Minor comments

p1072 - Would it be a good idea to initialise the ozone data using PV-based equivalent latitudes? That should reduce the spin-up time.

p1074 - The resolution of T79 seems very low compared to the operational NOGAPS T239 resolution. Since the NOGAPS model is only used for forecasting, the cost should not be a big issue. What is the resolution used for the ozone analysis grid?

p1083 - The ozone parameterization seems to me to make the ozone field rather bland, and unrealistically smooth (in Fig 12). I guess the parameterization can only represent

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the broad-scale effects of chemistry, by its nature. Does this smoothness also reflect the poor resolution of the ozone measurements as much as anything? Difference statistics will tend to favour over-smooth fields over more realistically structured fields where some of the features are slightly misplaced.

p1095 - I don't think it is worth including Appendix A; it only describes a trivial conversion between the different ways that hybrid coordinates are defined in the two models.

p1103 - The plots in this figure (2) are rather small. They could be bigger if they were arranged in a 2x2 square. The plots in Fig 4 are also rather small.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1067, 2007.