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Atmospheric Chemistry and Physics Discussions

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

L. Coy et al.

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Authors' reply to Reviewer 2 comments

Reviewer 2 has made a number of insightful comments. We reply to each in turn:

1. 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)?

There were several reasons why we chose this time period:

- This event has been extensively studied using a range of global models and as-

similation systems, such as Geer et al. (2006a) (see also the March 2005 special issue of The Journal of the Atmospheric Sciences) as well as with our GCM (Allen et al., 2006). Thus, as the reviewer states, it does provide a convenient reference point for comparison with other similarly motivated studies.

- Biggest challenges to stratospheric predictability appear to involve forecasting major warmings like this one (e.g., Lahoz, W. A., Quart. J. Roy. Meteor. Soc., 125, 2205-2238,1999; Simmons, A., et al., J. Atmos. Sci., 62, 668-689, 2005). Thus, this event provides a useful case study in which both dynamics and chemistry within the model are likely to be important for the +6 hour ozone forecasts and thus for the quality and performance of our assimilated ozone product.

- Our chemistry scheme currently lacks a heterogeneous loss term. The disturbed 2002 winter period means that, given warmer temperatures, heterogeneous ozone loss is probably not as significant and thus a better fit for our CHEM2D-OPP chemistry as currently formulated.

- On the practical side, our previous work during this time period (Allen et al., 2006) meant that we had ready a data base of appropriate meteorological fields from this time period.

2. 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?

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Figure 1 gives the overview of our assimilation cycle. The coupling frequency and analysis cycle are both 6 hours. The GCM's time steps are much smaller (5 minutes). The analysis groups observations from a 6-hour data window, so this is not a 3D-FGAT approach. We chose a 6-hour analysis cycle because those were times when the operational meteorological analyses were available to re-initialize the GOATS meteorological fields. The time interpolation statement was made in comparing GOATS to an off-line transport model, where analysis winds (typically available only every 6-hours) have to be interpolated to the transport model's smaller time steps. By running the full GCM, GOATS avoids the need for such time interpolation.

3. 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.

We were not trying to make an absolute judgment in this paper. "Biased" was intended as shorthand for "biased high with respect to our model ozone climatology" (based on Fortuin and Kelder) and with respect to more recent versions of the SBUV/2 ozone observations." This shorthand allowed us to easily distinguish between the two ozone data sets in figure captions, figure labels, and in the text. We will try to make this clearer in revision.

Having said this, the absolute biases in the Fortuin and Kelder climatology appear to be generally small: see, for example, Figs. 15-17 from the ASSET intecomparison of ozone analyses from many different systems and instruments (Geer et al., Atmos. Chem. Phys., 6, 5445-5474, 2006).

4. The main results showing the effect of observation biases are well pre-

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sented. 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.

We agree. In our discussion section (page 25 line 20) we concluded that off-line bias monitoring and correction offer the best solution. How to remove bias off-line is a good question that was outside the scope of this paper. Both model and observations can contain biases and obtaining good agreement between them is an important topic in data assimilation. While our GOATS experiments did not tackle the bias removal problem, they did show that the system was capable of running with the biased observations without generating any serious artifacts in the ozone analysis, other than the ozone fields being biased high. Presumably this was because the SBUV/2 observations were nearly globally distributed and thus GOATS was able to globally adjust to the input data bias.

Minor comments

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

A more realistic ozone distribution (such as from PV-based algorithms or another assimilation system) would reduce spin-up time. We used a simple zonal averaged ozone initial condition so that our initial ozone field would be independent of other ozone analyses. A PV-based initial ozone field would have made our initial ozone dependent on a particular set of meteorological fields and we wanted the flexibility of having the same initial ozone field, even if we used other meteorological fields. In this paper, however, we only used one set of meteorological fields. Once GOATS has integrated past this spin-up period the assimilation is independent of the initial ozone fields, however they were specified.

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2. 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?

The reviewer is correct in noting that GOATS can be run with NOGAPS-ALPHA at either T79 or T239 spectral resolution. We chose T79 spectral resolution in this study to make the GOATS runs computationally tractable and the output of manageable size, thereby permitting scientific analysis of output from a suite of different assimilation experiments (e.g., biased versus unbiased data, standard versus adaptive chemistry, etc.). Our major conclusions are all fairly general results that should hold regardless of the spectral resolution of the GCM.

The ozone analysis grid resolution was 1.5 x 1.5 degrees (240 longitudes by 121 latitudes). This nearly matches the NOGAPS T79 resolution (240 x 120 on the quadratic Gaussian grid). This information will be added to the GOATS description section in revision.

3. 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 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.

At higher altitudes where photochemistry dominates, ozone variability is controlled by the local temperature rather than by advection across mean gradients. Thus, in high-altitude regions where small-scale dynamical variability and mean ozone gradients are large but the temperature variability is relatively small, photochemistry would be expected to yield a smoother-looking ozone field relative to a purely dynamically-

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controlled simulation. The standard deviations of the O-F statistics in the tropics (Fig. 11) and southern polar cap (Fig. 13) suggest that the smoother ozone fields with photochemistry are more realistic at that altitude than those without chemistry, for this case at least. In the tropics the observations are fairly uniform as chemistry is starting to dominate the ozone evolution at higher altitudes. In the southern polar cap, GOATS runs both with and without chemistry fields seem to have the same O-Fs, implying that both fields, though different, agree with the observations just as well. In this case the reviewer's point may be valid, as the sharper fields may be slightly misplaced with respect to the observations, thus lowering their O-F statistics to the small values as unrealistically smoother fields. More than the simple statistics presented in this paper would have to be looked at to determine if high-altitude analyzed ozone is being excessively smoothed by the chemistry. We hope to look further into this issue in future work.

4. 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.

Agreed. We will remove Appendix A and the reference to it in revision.

5. 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.

Our previous experience with ACPD suggests the figures will be larger when published in final ACP format by making them double-column figures that extend across the full width of the A4 page. The compact ACPD format does not allow for this.

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