

## ***Interactive comment on “Data assimilation of stratospheric constituents: a review” by W. A. Lahoz et al.***

**S. Eckermann**

stephen.eckermann@nrl.navy.mil

Received and published: 3 October 2007

### **1 Preamble**

As a comparative newcomer to this field I enjoyed reading this very nicely written and structured review of recent research in stratospheric constituent DA. I learnt quite a bit.

Since a review paper of this kind is probably directed to newcomers such as myself, I thought it might be helpful to the authors to supply a few personal comments: they should feel no obligation to respond to any of it directly, but I hope at least a few of the comments are of some use.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

## 2 General Comments

### P9563

In terms of main aims for assimilating ozone, what about improving NWP skill in the stratosphere? Better ozone should improve stratospheric shortwave heating rates (and more minor 9.6 micron ozone cooling) that directly affect forecasts of stratospheric temperature and winds. Prognostic ozone should ensure a more realistic balance and interaction among temperature, ozone and heating/cooling rates in NWP systems by, e.g., feeding prognostic ozone into radiation codes, temperatures into ozone photochemistry parameterization, and so on. This is actually addressed on P9579-9580 so it probably should at least be mentioned here?

### P9582

The authors review the linearized ozone photochemistry schemes described by eq. (3) under the now-common nomenclature of “Cariolle schemes,” citing them specifically as being due to the work of Cariolle and Déqué (1986) (P9566 L15-16).

My experience is that this description, absent some context, causes confusion, since when researchers naturally read the Cariolle and Déqué (1986) paper as the primary source for this approach, they find eq. (3) simply posited in that paper without citations to any prior work, nor any supporting photochemical arguments justifying use of this mathematical formulation. This in turn leads to a view that Cariolle schemes generally are a nonrigorous and mysterious “engineering” approach to the parameterization of gas-phase ozone photochemistry.

In fact, this is not the case. As reviewed in section 2.1 of McCormack et al. (2006), this equation has a long history and springs directly from a linearized expansion of the fundamental odd-oxygen photochemical production and loss rate equations. This was done initially for pure oxygen (Chapman) photochemistry (Lindzen and Goody, 1965), and subsequently extended to reactions involving nitrogen, hydrogen and chlo-

rine compounds (Blake and Lindzen, 1973; Stolarski and Douglass 1985). These analytical derivations show, for instance, that the  $-b^{-1}$  term in eq. (3) corresponds directly to the ozone photochemical lifetime discussed on Page 9581. A great deal of modeling and observational research during the 1980s validated most of the basic premises of eq. (3), in research on ozone-temperature correlations, planetary wave effects on ozone, and Kelvin wave constituent oscillations, among others (see McCormack et al., 2006).

Indeed, what seems likely is that eq. (3) was such a standard and well-validated relation during the mid-1980s that Cariolle and Déqué (1986) probably felt no need to explain the equation, simply viewing it as the standard ozone photochemical parameterization and their model-based approach as the next logical extension beyond analytical theory for deriving those photochemical coefficients.

Two decades later, this is no longer the case, and the origin of eq. (3) from decades of theoretical and observational research prior to the Cariolle and Déqué (1986) paper is no longer familiar to most readers. A review paper such as this is the ideal venue to make this clear, especially since the Cariolle approach is explicitly reviewed here. A supporting sentence or two is probably enough.

Finally, note that McCormack et al. (2006), rather than the current McCormack et al. (2004) reference, is the more up-to-date citation for our CHEM2D-OPP scheme.

### **P9594 L6-10**

Without chemistry, unless the degradation of the instrument is very sudden or very severe, rather than the more common slower mean drift (bias), it is not clear for constituent analysis that the effect will show up in either OmF or OmA statistics. Even if the drift is fast, the OmFs will only show a sudden “blip” before quickly returning to “normal” values even though a severe observation bias may still be there.

This is because, without chemistry, the model must rely on the input constituent data as being accurate, since it simply pushes those fields around passively and cannot

change the mixing ratios. Mean drifts in the input constituent observations, then, will be incorporated into their forecasts and analyses so that the OmFs and OmAs will be about the same, since all will include these drifts. With chemistry, however, the forecast will pull the biased constituent mixing ratios back towards a more typical state: e.g., for ozone, back to the equilibrium reference state  $\chi_0$  in eq. (3). This will yield a large and persistent OmF that reflects this instrument bias, and seems to be a positive aspect of including chemistry in constituent DA generally even when its effects in the presence of unbiased data seem small. This general issue is discussed in more depth by Coy et al. (2007): e.g., the discussion on p2932.

**P9626:** While obviously not a full NWP-based approach, the Coy et al. (2007) GOATS work was not a strict CTM-based approach either. While its fine here in Table 3, our GOATS study seems to us to fit somewhere between the NWP-based and CTM-based approach in Tables 2 and 3, respectively: indeed, it was a first step towards developing a full NWP-based approach using our 3D-VAR system. As shown in Fig. 1 of Coy et al. (2007), the full NWP model was run at each time step but did not update the meteorological analysis, however the ozone assimilation was used within the NWP model and was fed directly into the model's radiation code, so there was a direct interaction between ozone assimilation and NWP model that is not inherent in the standard CTM-based approach.

### 3 Minor Typos/Suggestions

**P9573 L26:** spell out the acronym “NMC”

**P9574 L24:** Global Modeling *and* Assimilation Office

**P9576 L19:** better to say “winds, temperatures and mass fields” here? Also, is it also worth pointing out here that NWP dynamical cores must solve for specific humidity, since they formulate the Navier Stokes equations with moisture terms included? e.g.,

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

using virtual potential temperature as the prognostic thermodynamic variable. This immediately requires tropospheric humidity DA, which yields mature humidity DA code in most operational NWP systems that additional stratospheric humidity DA efforts must leverage off of and not unduly interfere with.

**P9577 L12:** is discussion of CMAM as an NWP-based approach potentially a little confusing here?

**P9584 L28:** what about AIRS on Aqua as well?

**P9586 L4:** here it is stated that methane chemistry can be neglected, whereas on P9598 the discussion here describes how a parameterization of methane oxidation is needed to get the water vapor and methane right in BASCOE and ECMWF.

**P9587 L23 et seq.:** worth here alluding to some of the prior discussion on these techniques in section 2.2?

**P9594 L9:** use

**P9595 L7:** which are: L27: insert full stop.

**P9599 L3:** are model generated: L12: describe

**P9600 L2:** doesn't this statement clash with the earlier statement on P9583 L14 that the chemistry scheme has effects only in the mesosphere and little/no effect on the quality of stratospheric ozone analysis? What else but chemistry does improved modeling buy you in the CTM-based approach in which the wind and temperature fields are prespecified?

**P9602 L15:** affect the *meteorological* analyses?

**P9602-P9603:** I was a bit surprised the NCEP ozone DA-based monitoring and forecasting weren't described here. They now use CHEM2D-OPP chemistry (McCormack et al., 2006) and (I believe) now assimilate OMI data operationally.

**9603 L10:** what about low ozone meteorological "minihole events" in the Arctic, which are severe and relatively common in and around the U.K., southern Sweden, etc.?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

## References

- [Blake and Lindzen(1973)] Blake, D. and Lindzen, R. S.: Effect of photochemical models on calculated equilibria and cooling rates in the stratosphere, *Mon. Wea. Rev.*, 101, 783–802, 1973.
- [Lindzen and Goody(1965)] Lindzen, R. S. and Goody, R.: Radiative and photochemical processes in mesospheric dynamics: Part I, models for radiative and photochemical processes, *J. Atmos. Sci.*, 22, 341–348, 1965.
- [McCormack et al.(2006)] McCormack, J. P., S. D. Eckermann, D. E. Siskind, and T. J. McGee, CHEM2D-OPP: A new linearized gas-phase ozone photochemistry parameterization for high-altitude NWP and climate models, *Atmos. Chem. Phys.*, 6, 4943-4972, 2006.
- [Stolarski and Douglass(1985)] Stolarski, R. S. and Douglass, A. R.: Parameterization of the photochemistry of stratospheric ozone including catalytic loss processes, *J. Geophys. Res.*, 90, 10 709–10 718, 1985.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper