Review of “Protection without poison: Why tropical ozone maximizes in the interior of the atmosphere” by A. Match et al.

This study seeks to construct a minimal theory that would account for the ozone peak at ~26 km and provide a simple conceptual explanation of the peak location. This is accomplished by introducing a “Chapman+2 model” in which the Chapman reactions are augmented by two additional generalized “reactions” that account for ozone destruction by catalytic cycles and by transport, and subsequent analysis of the limit cases. The paper shows that these limit cases correspond to two conceptual explanations of the tropical ozone maximum, neither of which produces the maximum at the correct altitude. Instead, the authors show that the true maximum is located at the transition altitude between the two regimes in agreement with data.

Furthermore, the authors develop an explicit solution to ozone photochemistry under simplified assumptions (gray atmosphere) that produces an internal ozone peak at the correct altitude and in agreement with the proposed “regime transition paradigm”.

I think there is a pedagogical value in being able to provide an explicit formula for the tropical ozone profile, of the form $[O_3]$ = …, even if that formula (Eq. 28) is not exactly simple and probably(?) has to be evaluated numerically because it includes the Lambert W function. Still, it tells a consistent story of the proposed regime transition paradigm governing the location of the ozone peak. The paper is very clearly written. The math, while tedious in places, is explained in sufficient detail for the readers to follow the argument. I think this is a very nice contribution to stratospheric ozone science and clearly well within the scope of ACP.

I have only minor comments and suggestions, some more pedantic than the others.

Good work!
Kris Wargan

Minor and technical comments

L20-21. Do you need this distinction? There are other radiatively active (in the sense of having absorption spectra) gases that maximize “in the interior”, e.g. HNO_3 (at midlatitudes in this case). Also, I wouldn’t consider CH_4 well mixed. Other long-lived species, such as N_2O, CFCs etc. are neither well mixed nor confined to near the surface.

L50. MERRA-2 stands for “Modern Era Retrospective Analysis for Research and Applications”. Please, cite at least the MERRA-2 core paper, Gelaro et al., 2017, and maybe also the MERRA-2 ozone description and evaluation paper, Wargan et al., 2017. Is this a zonal average? Climatology? Over what latitudes and time? MERRA-2 ozone is at its best after ~2004 when MLS is assimilated.

L87. Typo: “which we then augment it with”. Drop the “it”

L102. It’s not entirely accurate to say that it’s “photolysis” that does the attenuation. Doesn’t scattering also play a role?

L120-127. Why is it OK to neglect the effects of mixing / leaky tropical pipe?

L130 / R5 & R6. Why is it justified to treat vertical advection as an effective chemical loss? In particular, L133-134 imply a linear dependence of the generalized loss on the concentrations of O and O_3. But advection by $\overline{\omega *}$ is proportional to the vertical gradient of the mixing ratio (equivalently, the appropriate term in the continuity equation), not to concentration. So, why does it work? This is actually discussed in Section 3.5.2 of Brasseur and Solomon 2005, but I think it should be briefly explained here too, especially that this approximate treatment of transport maybe responsible for some remaining inaccuracies of the Chapman+2 model, as later explained in the discussion of Fig. 3.

Eqs. 5 and 6. I hesitate to ask for more math;) However, it may be instructive to see the full set of continuity equations from which these are derived under a steady state assumption. It could be another short appendix. It's just a suggestion. A committed reader should be able to back out the full set of equations from 5 and 6.

L142. "implicit ... due to dependence on ozone aloft". So, not really quadratic in [O_3] to leading order as stated earlier in the same sentence? Or is the dependence of the photolysis rates on integrated ozone sub-quadratic/weak? Is any of this relevant to the calculations?

LL156-157. See my comment to L130 above.

L186. I would imagine that the diurnal cycle at those altitudes is pretty much negligible compared to the other factors.

L197-199 and 210. Nice!

L 202. "Chapman dynamics"? Or chemistry?

Eq 11. Shouldn't there be a factor of n_a in the numerator? Equation 5 has an n_a^3 in the prefactor and n_a^2 in the relevant term in the denominator.

L242. Typo: duplicate "regimes"

L247. As above. Is this a climatological mean tropical profile from MERRA-2? A profile for a specific day?

L250-251, Fig 5. I suggest cutting the figure off at the approximate altitude of the tropopause. Everything below that is irrelevant to this discussion.

L271 and below. Is "non-analytic" used in the mathematical sense of not having aTaylor expansion or in the loose sense (no closed formula)? In the latter case I think people say "non-analytical". I may be wrong.

L351, 354. Since Eq. 23 contains the Lamber W-function it may be questioned whether this qualifies as an analytical solution. I'm not sure if there's a closed formula for it even in this case where the argument is real and positive. As far as I know W is evaluated numerically using rather terrifying-looking approximations (e.g. https://mathworld.wolfram.com/LambertW-Function.html).

L390. The profiles in Fig. 7 look continuous to me. What am I missing?

L543. I think it's supposed to be Appendix C.

References

Gelaro, R., McCarty, W., Suárez, M. J., Todling, R., Molod, A., Takacs, L., et al. (2017). The Modern-Era Retrospective analysis for research and applications, version 2 (MERRA-2). Journal of Climate, 30(14), 5419–5454. https://doi.org/10.1175/JCLI-D-16-0758.1

Wargan, K., Labow, G., Frith, S., Pawson, S., Livesey, N., & Partyka, G. (2017). Evaluation of the ozone fields in NASA's MERRA-2 reanalysis. Journal of Climate, 30(8), 2961–2988. https://doi.org/10.1175/JCLI-D-16-0699.1