Review of *Simulation of Atmospheric Ozone Layer Formation and Study on the Influence of Equatorial Atmospheric Vertical Motion on the Ozone Layer* submitted to ACP

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The submitted paper identifies a timely and fundamental research question: how does the basic state of the ozone layer respond to variations in vertical velocity. This question deserves careful attention, but the treatment in the submitted paper is incomplete, rendering it difficult to review. Some of the claims in the paper would be interesting if true, but are insufficiently supported. Some of the results in the paper seem highly implausible.

Briefly: the 1D photochemical model is a productive tool for exploring these questions, but a description that allows its results to be appropriately understood is incomplete. Questions that need to be answered include:

- How is radiative transfer calculated? Is it with simple optical depth functions or a more sophisticated treatment that includes scattering? Is an established model used?
- What is the range of wavelengths for the photolysis calculation?
- Are all chemical species advected or just ozone? What numerical treatment is used for advection?
- What photochemical regime is intended to be represented by the model: the stratospheric photochemical regime of catalytic ozone destruction? The mesospheric regime? The tropospheric regime? All of the above? The paper claims to use approximately 100 gas-phase chemical reactions (line 141) but I could only see 34 reactions in Table 4-1. Table 4-1 appears to have serious issues. There is a duplicate reaction (O + O2 + M), and there is a reaction that I am not familiar with and that I could not immediately locate in the NASA JPL manual from which it ostensibly originated (2 O3 $\rightarrow$ 3 O2), and at least one crucial reaction for NOx photochemistry in the troposphere appears to be missing (HO2 + NO $\rightarrow$ NO2 + OH) calling into question the applicability to the troposphere. The treatment of third bodies is confusing (sometimes listed as M and sometimes listed as a specific constituent on the left-hand-side

and right-hand-side of the reaction), and it would not surprise me if there are other anomalies in Table 4-1.

- There are initial profiles of precursor chemicals relevant for ozone photochemistry (CH4, CO, N2O, and H2O). Under vertical advection are these constituents gradually lifted out of the atmosphere? Is there a compositional lower boundary condition?

The vertical velocity profiles used are questionable. The value chosen for the stratosphere is 1-2 orders of magnitude larger than what is inferred from reanalyses, which is order 0.3 mm s^{-1} (e.g., Abalos et al., 2015), whereas the paper uses 0.3 - 2 cm/s (Line 124).

The zero motion limit predicts ozone that is an order of magnitude larger than what is observed. I appreciate that in observations, the effects of vertical velocity are already relevant, but previous studies of photochemical equilibrium have found much more realistic estimates for the total column ozone. Extraordinary care in describing the model and why it leads to these large values is required to substantiate this dubious finding.

The idea that photochemical equilibrium can involve very slow accumulation of ozone is one that I find plausible, so I do believe that weak vertical motion could lead to large reductions of ozone compared to zero motion, but owing to issues noted above, I cannot meaningfully evaluate the specific numbers reported in the submitted paper.

It is claimed that ozone exhibits internal oscillatory behavior (Line 275). In the face of such a counterintuitive claim, a compelling explanation of how such behavior emerges in the model under analysis is required. No such physical explanation is offered. The prospect that such an oscillation is a numerical artifact was not ruled out. I quite doubt that such an oscillation is a meaningful behavior of the actual photochemical system or any credible model thereof. The Chapman Cycle does not exhibit oscillatory behavior at a given altitude. In trying to imagine how oscillatory behavior could emerge, it might be that it emerges as a dynamic of the column as a whole, yet this would seem to require, at a minimum, that there is some process that is moving information upward and not just downward (as results from photolysis). In the absence of vertical motion, there is no such process moving information upward. The paper does not offer any physical explanation or justification for the internal oscillation, thereby falling far short of the evidentiary standards of ACP.

To conclude: this paper poses an interesting question that merits theoretical attention from the community, but its extraordinary claims are not explained by physical argumentation and are only supported by an incompletely documented model that is driven by implausible parameters and, when relevant, disagrees by an order of magnitude with prior modeling efforts.

References

Abalos, M., B. Legras, F. Ploeger, and W. J. Randel, 2015: Evaluating the advective Brewer-Dobson circulation in three reanalyses for the period 1979-2012. *Journal of Geophysical Research: Atmospheres*, 7534–7554, doi:10.1002/2015JD023182.