

Interactive comment on “Chemistry-transport modeling of the satellite observed distribution of tropical tropospheric ozone” by W. Peters et al.

L. Horowitz (Referee)

lwh@gfdl.noaa.gov

Received and published: 24 January 2002

General comments:

This paper describes a comparison of long-term satellite-based observations of tropical tropospheric ozone column (TTOC) with a chemical transport model driven with assimilated winds. This study represents one of the first uses of this extensive dataset for model evaluation. Several useful measures of model-measurement discrepancy are proposed. The authors find certain systematic biases in their model simulation. They perform several sensitivity studies to assess the influence of meteorology and NO_x sources on TTOC.

This paper is quite original and presents several important findings. It also suggests interesting future directions for utilizing satellite observations in tropospheric modelling

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studies.

Specific comments:

Abstract.

The improvements shown by increasing the lightning NO_x were the most significant of all the sensitivity studies. This should be mentioned in the Abstract.

Section 2. Model description.

What is the resolution of the ECMWF dataset used? (T106?)

Section 2.1. Biomass burning emissions.

Does EDGAR include all biomass burning sources, or only "anthropogenic" burning?

On what spatial scale is the scaling for the alternate fire calendar done (gridbox, continent)?

Section 2.4. Labeled tracers.

How do you define the tropopause for purposes of calculating STE?

How good is the assumption of being NO_x-controlled? Does one need to consider the source of HO_x (as well as NO_x)? Also, comment on (non)linearity of ozone chemistry.

Section 3. Observations.

Does the "noise" in the observations represent errors in the observations (or technique) and/or (real) observed variability?

Is the MR "a-priori zonal distribution" calculated/applied in the troposphere, stratosphere, or both?

You should mention the large differences found between various TOMS-based T_{TOC} retrieval methods.

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Section 4. Measure of error.

You mention that the discrepancies are largest over the Atlantic ocean. This is certainly correct in absolute terms (DU). Comment on the relative (%) discrepancies.

How do the meridional variations in the model compare with the observations?

Section 4.1. Zonal TTOC distribution.

Since you focus on the Atlantic mismatch, you should discuss how reliable the observations of this feature are. For instance, do other satellite-based methods get the same feature? Same magnitude? Also, have previous model studies successfully simulated it?

You should explain the sensitivity of TTOC to tropopause height. Also, again, how do you define tropopause height in the model?

In Eq. (1), should the averaging be inside the square root sign? Otherwise, it is not an RMS deviation (but an l1-norm).

Section 5. Comparison of TTOC.

The overestimates over the Pacific Ocean in DJF are not always "slight." They often exceed one-sigma.

It would be interesting to see the interannual component of epsilon-2 as well as the seasonal cycle. Currently, you don't really show any tests of the patterns of interannual variability.

Section 6. Influence of transport.

Have you determined which particular meteorology factors are the most important contributors to the results of Figure 5 (i.e., lightning NO_x/ convection vs. large-scale transport)?

Section 7. Role of photochemistry.

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Explain the distribution of the ozone contribution from lightning NO_x. How zonally asymmetric is the lightning NO_x source? What about land-ocean differences? Is the zonal asymmetry of the contribution to ozone stronger or weaker than the lightning NO_x source itself (i.e., is it affected by transport)?

The strongest contributors to the zonal asymmetry are both terrestrial surface sources, concentrated in the tropics. Would this have been expected?

Is the A-M-J peak in the lightning influence a general result, or is it strongly model dependent?

Section 8. Sensitivity analysis.

What fraction of the soil emissions occur in the tropics?

Why do you assume that the error in a component is only in its magnitude, not in its seasonal or spatial pattern?

Section 8.1. Strategy.

You should refer to the longitude regions (loosely) as "Atlantic" and "Pacific" in the text.

While you extensively describe the sensitivity studies from Table 2 that resulted in improvements, the other simulations are not discussed at all. A brief discussion of these other simulations and their results would be quite interesting. Also, could you add the actual epsilon values for these other simulations (if possible).

The lifetime of NO_x depends not only on its avoiding dry deposition, but also (primarily) avoiding oxidation (to HNO₃).

When you distribute the biomass burning NO_x over the lower 6 km, there is an improvement in both the Atlantic (where the model was underestimating ozone) and the Pacific (where the model was overestimating ozone). Why is this?

You describe the seasonal changes in the convective transport of NO_x when using

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the alternate fire calendar. Do these changes correspond directly to the changes in biomass burning emissions? Is the increase in epsilon-1 (from 0.84 to 0.85) really "significant."

Is the 2 DU increase in TTOC when biomass burning emissions are doubled an annual average (or for September)?

While doubling the lightning NO_x source would still be within the IPCC estimates, it would be outside the range used in most (recent) CTMs. The results of this change are shown in Table 2 (not Table 3).

Could the tropical/extratropical distribution of lightning NO_x in your model be "incorrect"? How would this affect your conclusions here?

By the criteria you set out in this Section, the doubling of lightning NO_x is clearly the best improvement from any of your sensitivity runs. It is hard to justify ignoring it because of extratropical ozone, when lightning has a largely tropical influence, and you may potentially have STE problems confounding results in the extratropics.

Section 9. Vertical ozone profiles.

By "additional biomass burning NO_x emissions" do you mean "doubled biomass burning NO_x emissions"?

I am not sure what you mean by "the large scales which are dominated by transport." Aren't the small-scale (vertical) features in the sonde observations influenced by transport?

In these comparisons, do you use model output from the particular time of day of the observations?

Please comment on the relative vertical resolution of the model and observations.

Do the vertical profiles agree better with the sondes in seasonal multi- year comparisons?

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Could missing details in the the vertical profiles indicate problems with vertical transport in the model (or perhaps inadequate vertical resolution)?

Section 10. Discussion.

Paragraph 2: The -5 DU offset brings model and measurements to (almost) within one-sigma over the Atlantic. But it also improves agreement over the Pacific. Why?

Paragraph 3: Are you implying that the observations (or at least this particular retrieval method) must be wrong, or just that no "plausible" change to the model can bring it into agreement with the observations?

Paragraph 3: What do you mean by "forcing"? (Supply of NO_x to the free troposphere?)

Paragraph 3: Dry deposition should not be responsible for "damping" the response of ozone. If deposition were the only sink, shouldn't ozone concentration increase linearly with O₃ production (first-order loss)?

Paragraph 4: Are you talking about linearizing the chemistry, or pre- calculating a lookup table? (They are different.) The model used in the Galanter et al. (2000) study does not include feedbacks between NO_x and OH. The NO_x chemistry is calculated with a fixed, non-interactive OH field. This probably explains the greater sensitivity to forcings. I don't think that the issue is linearizing the chemistry, but rather the non- interactivity. See also Moxim and Levy (2000) for further discussion of the Atlantic ozone maximum using the same model.

Paragraph 5: You mention that zonal gradients of TTOC are largely determined by transport. There may also be a contribution from lightning (which also changes with the meteorology).

Paragraph 6: Is the inadequate representation of biomass burning plumes due to insufficient vertical resolution in the model or, e.g., problems with subgrid parameterizations of vertical transport?

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Paragraph 7: Even if some combination of the tunings mentioned got the model within one-sigma of the observations (on average), wouldn't a systematic bias (Atlantic-Pacific) still remain?

Paragraph 7: Concerning the damped response of ozone in the model, doesn't this mean that there is a *serious* disagreement between the model and these observations. Given the damping, a very large change in model parameters would be necessary to remedy this disagreement. In this sense, perhaps ozone is a *good* tracer for evaluating model parameters.

Section 11. Conclusions.

You mention that it is necessary to further reduce the uncertainty in the TTOC data to aid in model evaluation. Assuming you believe the current observations and uncertainty estimates, you have shown that they pose a sufficient challenge to models now, even without decreasing the error estimates of the observations at all. (Of course, it would always be useful to have better observations, anyway.)

Interactive comment on Atmos. Chem. Phys. Discuss., 1, 337, 2001.

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