

## ***Interactive comment on “The TOMCAT global chemical transport model: Description of chemical mechanism and model evaluation” by Sarah A. Monks et al.***

**Anonymous Referee #2**

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### **General comment**

The manuscript concerns the description and validation of the TOMCAT Chemistry Transport Model. One point which is not clear is what significant model improvements has been made, compared to recent versions used for intercomparison studies, that warrants a new benchmarking study at this point in time. There has been the addition of higher Volatile Organic Compounds and new Isoprene/Monoterpene chemistry, with no significant updates to the dynamics, microphysics or transport in the CTM. Currently the manuscript is written as such that the reader has no idea as to whether these chemistry updates improve the model performance, where a ‘before’ and ‘after’ simulation for a chosen year is not provided. Two arbitrary simulations years are

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presented (2000 and 2008), being run by different emissions and driven by different meteorology. Some of the validation is presented in a climatological sense, whereas some is presented for the simulation year in general, with some data being for different time periods. In general TOMCAT exhibits biases but the reasons given are not quantitative but rather speculative as the rigor of the analysis is not sufficient enough to provide concrete answers, with conclusions being used from previous studies presumably using an identical model version. One could exploit rarely used data such as from SAFARI, MINATROC and/or THESEO campaigns, which provides an unique snapshot across different chemical regimes if more data is necessary. In order to improve this manuscript I recommend a major revision following either (i) a focus on one year presumably the year 2000 using yearly specific measurements or (ii) do a sensitivity study with and without new chemistry for a chosen year to show effects and (hopefully) improvements.

### **Main comments**

Are the 31 levels having a higher resolution in the troposphere? How many layers describe the UTLS and Stratosphere?

Is the meteorology smoothed between 6 hourly updates or are step edges applied to e.g. H<sub>2</sub>O fields?

Emission inventories: In order to attain the most accurate simulations using a CTM requires time evolving emission input data which e.g. captures the development in the transport sector. In TOMCAT aircraft NO<sub>x</sub> emissions come from the QUANTIFY project for the fixed year of 2002 which will surely underestimate the contributions in aircraft NO<sub>x</sub> over the simulation period (year 2008). A valid reason of using these fixed emissions is not given where e.g. the MACC City inventory provides yearly specific aircraft NO<sub>x</sub> estimates.

Are any burning heights applied for the biomass burning emissions?

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Heterogeneous conversion: What is happening to the conversion of N<sub>2</sub>O<sub>5</sub> into HNO<sub>3</sub> on cloud surfaces? How do you calculate the available surface area density? Why is there only one heterogeneous reaction when HO<sub>2</sub> conversion on aerosols is also now considered important?

Photolysis rates: How old is the absorption spectral data and quantum yields employed?. Many changes to the recommendations have been made since the 1980's but no details are given as to what improvements have been made. Photolysis drives tropospheric chemistry therefore an accurate description of diurnal variability is needed to capture e.g. good NO<sub>2</sub> lifetimes. How are cloud and aerosol treated in terms of optical density through the column? Sun-spectrum used with any modifications due to the sun-cycles? As this is a benchmark paper it is not attractive that the reader has to trawl through the literature to get such details.

Given that the chemical scheme employed is for the description of tropospheric processes (e.g. no CFC's), how is the stratosphere constrained in order to get correct seasonality in the overhead O<sub>3</sub> column and thus actinic flux profile?

Observations:

Why use GOME-2 and not GOME O<sub>3</sub> data used for the year 2000 and 2008? Considering the current debate about the (potential) recovery of stratospheric O<sub>3</sub> it appears the wrong decade is being used for validation. Why use an O<sub>3</sub> sonde climatology from a completely different time period? This make little sense considering the choice of satellite data concerning O<sub>3</sub>. Both 2000 and 2008 annual means should be composed from the sondes and used to provide a better assessment of the vertical O<sub>3</sub> profiles as the reader is unable to assess the ability of TOMCAT to capture inter-annual variability in the distributions. Were O<sub>3</sub> measurements extracted on identical days for a valid comparison?

Validation:

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Why lump NO and NO<sub>2</sub> together?. Seasonality in NO<sub>2</sub> mixing ratios is a proxy for the performance of the photolysis routine which is masked by lumping both oxides together. Even if there is a persistent bias the seasonality should be captured in some way.

Have the satellite comparisons for e.g. NO<sub>2</sub> been done at the local overpass time for the days when measurements are available or in more ad-hoc way?

See comments above regarding O<sub>3</sub> measurement data.

Specific comments:

Pg 8, In 17: Providing seasonal means would be more informative for the reader, where significant changes should occur in the hemispheric zonal means. Pg 8, In 45: Please introduce a table with the O<sub>3</sub> diagnostics (Burden, Lifetime, Strat-Trop exchange and deposition) and then place in context using the new multi-model means. Also for the other gases such as CO and CH<sub>4</sub>, as information is scattered throughout the text. Pg 9, In 40: Indeed, it would be even more illuminating by providing a Table of global mean OH from various studies. Pg 13, In 32: Not surprising considering the mitigation practices over the last decade or two. This bias is potentially exaggerated by using non-yearly specific measurements. Many studies have focused on VOC emissions so can the authors state whether this is an artifact of CTM's in general? Pg 15, In 15: Looking at Figure 17 I can see discrepancies of >100 percent for some of the points. Better to discuss seasonality to identify which season has the largest bias given that PAN is temperature sensitive. Pg 15, In 15: "... may be too high ...". If a comparison has been made then surely it either is or isn't too high for this period. Pg 15, In 19: What is the relevance of GEOS-chem to this paper? What about the multitude of other CTM's? Figure 14 and 15: What is the motivation for lumping the measurements but not the model results? There are trends in surface observations related to emission trends. Is the bias larger for 2008 than 2000 i.e. does the deviance increase with time due to incorrect emission estimates?