**Atmospheric
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Interactive
Comment

Interactive comment on "The influence of biogenic emissions from Africa on tropical tropospheric ozone during 2006: a global modeling study" *by* J. E. Williams et al.

J E Williams

williams@knmi.nl

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We would like to thank anonymous Referee 2 for the generally positive review of our manuscript. The main criticism concerns the length of the paper and the number of Figures which are included, making the main conclusions rather difficult to find. In light of this we will shorten the paper by adopting some of the suggestions mentioned by the referee.

Model description. The different simulations and emission inventories are not clearly described in section 2. For example, on page 10374 it would make more sense to first describe what the BASE and LATH simulations have in common, and then describe

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what species differ. In the LATH simulation are the new biogenic emissions for Africa only or for the whole globe?

We re-write this section as follows: "In order to investigate the influence of the different biogenic inventories we define a number of different experiments where we vary the biogenic emission fluxes applied in the model. For the BASE (baseline) simulation we use the biogenic emission files supplied from the POET project (Granier et al., 2005), which provide values for biogenic NO, carbon monoxide (CO), methanol (CH_3OH), ethene (C_2H_4), ethane (C_2H_6), propane (C_3H_8), propene (C_3H_6), acetone, isoprene and the monoterpenes, which are all applied globally. For the LATH simulation we use the 12 year average from between 1983–1995 as provided by Lathière et al. (2006). This replaces the emission fluxes for NO, CH_3OH, acetone, isoprene and the monoterpenes compared to the BASE simulation and provides additional emission estimates for acetaldehyde (CH_3CHO), acetic acid (CH_3COOH), and formaldehyde (HCHO), where the emissions used for all the other BVOC species are identical to the BASE simulation. Again, the Lathière et al. (2006) emission estimates are applied globally."

It would be also useful for the reader to have some explanation of the differences in methodologies for the derivation of the inventories for NO,CH3OH, acetone, isoprene and monoterpenes. What are the emissions inventories based on? This would help in understanding some of the differences. For example,why is the seasonality in biogenic NO emissions so much stronger in Guinea and S.Africa for the LATH inventory?

Although we aim at reducing the length of the paper in line with the referees main criticism, we will introduce a paragraph which gives brief details as to the methodologies adopted by both inventories. For details of this please see the response to the review by Referee 1. However, we feel that differentiating the reasons for differences in seasonality between the two datasets in not within the scope of this paper and should be conducted by the community responsible for providing such datasets for use in global models.

Sensitivity simulations. For the NOSOIL simulation, was that conducted for the BASE or LATH simulation? This is not clearly stated in the text. Similarly for the NOBIO simulation: which simulation was that conducted for? I assume that the results might be different depending on what simulation is used to start with. From Table 2 I inferred that it is the LATH simulation that is used for these sensitivity studies. Why choose this one and not the BASE?

We will provide more clarity in the text concerning the use of the LATH inventory for both the NOSOIL and NOBIO sensitivity simulations. We do not use the BASE simulation as we feel that the model performance is somewhat better using the Lathière et al. (2006) inventory, a conclusion which we will be given more weight in the final version. Considering that both emission inventories essentially use the same parameterizations, this is most likely due to the dynamic input variables used in the derivation of the Lathière et al. (2006) inventory. Moreover, more BVOC emission are provided by the Lathière et al. (2006) inventory.

One of the interesting part of the result is the effect of biogenic emissions on tropospheric ozone (NOSOIL), yet the results are buried in the discussion of the seasonal differences between the LATH and BASE simulations. I suggest having a specific section (or subsection) discussing this.

We will segregate the sensitivity studies into an additional sub-section as requested by the referee.

Detailed comments:

1. Abstract. Line 3. "composition of the tropopause". Do you mean troposphere?

Now Amended.

2. Page 10375. Here and on Figure 1, there is a "Yienger Levy" inventory is mentioned. Does that refer to the BASE scenario. Please be consistent in the text and keep the same names when referreing to simulations. If the BASE inventory is based on Yienger

and Levy, what is the LATH simulation based on?

We provide more clarity, thus: "Moreover, the differences in the annually integrated fluxes between the BASE simulation, which adopts the emission inventory of Yienger and Levy (1995), and LATH simulation, which adopts the emission inventory of Lathière et al. (2006) are relatively small for all regions, although more seasonality is exhibited by the inventory of Lathière et al. (2006) as show in Fig. 1."

3. Page 10378. This whole page is confusing. The discussion of the differences be-tween the two model simulations displayed on Figure 4 should be rewritten. Why does the LATH simulation yields lower O3 than the BASE simulation for most of the tropo-sphere? Why is LATH higher than BASE for other regions? The discussion goes back and forth discussion issues with the TM4 simulation and with the different inventories making it all very confusing, but it doesn't explain the basic features shown on Figure 4.

This is principally due to the enhanced sequestration of reactive nitrogen into longer-lived nitrogen reservoir species as explained in Sect. 5.. We introduce an explicit link to this section in the re-drafted description of Figure 4, thus;

"Figure 4 shows .... when compared to the BASE simulation. For the lower tropo-sphere the highest [O3] occurs between ± 10 deg latitude during the seasons DJF and JJA, where there is a shift from the Northern Hemisphere (hereafter referred to as NH) to the Southern Hemisphere (hereafter referred to as SH) in the course of the year. The maximum [O3] values of 100 ppbv occur near regions influenced by intense biomass burning activity. Imposed on this are the effects of the regional differences in the soil NOx emission inventories as shown in Fig. 1, with maximum differences occurring in southern Africa during season DJF (c.f. Fig 4b). Moreover, for periods where there are similar regional biogenic NOx emissions (e.g. southern Africa during MAM) there is also a signature present from the higher isoprene emissions present in the BASE run. In general, the LATH run has a lower tropical tropospheric ozone burden as a result of

a re-distribution in the speciation of reactive nitrogen between reservoir compounds, which we describe in more detail directly below and later in Sect. 5. These decreases in tropospheric [O3] extend out over the tropical Atlantic Ocean and induce changes in the tropical tropospheric ozone column (TTOC) simulated in the model, which is defined as the integrated ozone column below 200 hPa as used in Valks et al. (2003). Moreover, a signature of the enhanced seasonality present in the soil NOx emissions in Lathière et al. (2006) emission inventory can also be seen in the differences in the TTOC as far as South America (e.g.) increases of 10

4.Table 2. Each line should be labelled. It took me quite a while to understand this Table as there are 5 lines for NOx (and 3-4 for the other species). None of the lines are labelled.

We now introduce specific labelling into the Table 2 to aid clarity in the final version of the paper.

5.Figure 7. The figure caption here (and for many other figures) does not explain what the figure shows. Please be more specific.

We provide more verbose figures legends for Figures 5 and 6 in the final version of the paper.

6.Pge 10380 5-45 percent of the lower tropospheric ozone in the LATH run... where do these numbers come from? The lowest number I see on the figure is 1 percent. Instead of a range, it would be more useful to give mean differences (either seasonal or annual) over different regions.

In the new sub-section which deals with the NOSOIL sensitivity study we change the text to the following: "The highest biogenic NO emissions occur between 20S-20N (see Fig. 1), approx. 2–45 percent of the tropospheric ozone below 200hPa is formed as a direct result of NO which is emitted from soils." Figure 7 already provides regional seasonal differences for the chosen 2-D transect.

7.Figures 8 and 9 "support the conclusions drawn from analyzing the 2-D transects" in the authors' words. So I suggest eliminating these 2 figures as they do not bring new information.

Although we wished to demonstrate the global influence of the emission of NO from Africa on the tropical marine boundary layer over the Atlantic by introducing these figures we remove them from the final version of the paper in the interests of brevity in line with the referees suggestions.

8.Page 10382 line 13 "for all longitudes" please specify what the range of longitude is. Is it over Africa or global?

9.Table 3. The title should give the region considered (lat/lon ranges).

Now Amended.

10.Tables 3-6. The changes between the BASE and LATH simulations and for the sensitivity simulations are all fairly small. I am not sure what is gained by these tables. I suggest eliminating them.

We disagree with the referee on this point as these tables provide information regarding the performance of the TM4 model after making the numerous changes to both the model configuration (enhanced vertical levels) and the chemical reaction data, where the lifetimes of both CH4 and CO are an established method of gauging the performance of a CTM. For instance if the lifetime of CH4 is of the order of a few years, or lifetime of O3 only a few days, the chemical reactivity of the chemical mechanism would be too high which would not give confidence concerning the CTM. Table 6 is necessary in order to quantify the importance of biogenic emissions from Africa in a global context without including additional figures. In the context of the AMMA project, where this manuscript is associated with the special ACP issue for this project, this was considered to be an important objective.

11.Page 10387 " the model values tend to be over-estimated by approx. 50 percent ..."

what does that mean?

Compared to the MOZAIC profiles the model has higher surface ozone in the seasonal average. We change this to "When compared to the MOZAIC seasonal averages the model tends to over-estimate surface ozone by approx 50 percent throughout the entire year," in the final version of the manuscript.

12.Page 10388. Discussion of figures 11 and 12. To explain some of the differences between modeled and observed ozone in figure 11, the authors mention two possibilities: problems with biogenic NO emissions or with the convection. They argue that they can use the results of the convective tracer simulation so separate emissions vs convection problems. I don't see how they can really do this. In order to really separate the 2 they would need to compare the model to observations of passive tracers with known emissions (like radon, or CO).

An individual passive tracer has been included for each of the designated regions which are defined in the study and given a lifetime of 20 days. Therefore, no interference occurs from the regions outside of Africa. For species such as CO, tagged studies of regional CO emissions within Africa have shown that over 50 percent occurs from regions outside the tropics via the long-range transport of air masses. This complicates the use of this species to assess whether the convection from directly below the MOZAIC flight path is sufficient enough. To the authors knowledge no corresponding Radon measurements exist in the UT for the period chosen for this study, and again, there are sources of Radon outside of the African continent.

13.Figure 15. I suggest eliminating the 1 sigma values for the models simulations as they make reading the figures very difficult (all the error bars overlap and hide the means).

The error bars will be removed from the plot and some indication of the magnitude provided in the updated Figure legends.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10367, 2009.

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