

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.

Anonymous Referee #2

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This manuscript contrasts two biogenic emission inventories to examine the impact of biogenic emissions on tropospheric ozone and its precursors over Africa. The chemical transport model results are compared to observations. The authors find that biogenic NO emissions are responsible for the production of a significant fraction of ozone. Biogenic C1-C3 VOCs have a more minor effect on tropospheric composition.

The paper appears to be mostly a sensitivity study where two emissions inventories are interchanged to examine their impact on model performance. The paper is too long and contains too many figures, the discussions of the differences are often convoluted and lack clarity. I was left with no clear message as to whether one inventory is better than

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the other and why. I suggest that the authors substantially reduce the paper length and number of figures+tables and focus on their key findings.

The manuscript and the different sensitivity simulations are difficult to sort out because there is a lack of consistency in referring to the different simulations. BASE is sometimes referred to as POET, the Tables and figures are not annotated in a way that makes it easy for the reader to figure out which simulations are shown (for example Table 2 and 3, Figure 4... see detailed comments below).

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

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?

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.

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Overall, I recommend that the paper be more focused in its discussion. In particular, sections 3 and 4 are too long and contain too many figures. They are simple modeling exercises comparing 4 different simulations. The reader doesn't need to see all the details, and the authors should only highlight the key differences. Too often the authors focus on minute differences (such as in Tables 3-6). After reading them I was left with a very confusing picture of what these key point were...

Section 5. Here again the discussion of the comparison between model and observations is too long and this should be streamlined.

All the figure legends should be carefully examined and rewritten to clarify what is shown.

Detailed comments.

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

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. Also, see point above about model description. If the BASE inventory is based on Yienger and Levy, what is the LATH simulation based on?

3. Page 10378. This whole page is confusing. The discussion of the differences between 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 troposphere? 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.

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

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labelled.

5. Figure 7. The figure caption here (and for many other figures) does not explain what the figure shows "As for Fig. 4 except for the sensitivity test NOSOIL ..." is not good enough. Please be more specific.

6. Page 10380 "5-45% 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%. Instead of a range, it would be more useful to give mean differences (either seasonal or annual) over different regions.

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.

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).

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.

11. Page 10387 "the model values tend to be over-estimated by ~50%..." what does that mean?

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).

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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).

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

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