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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of African air pollution on regional and global tropospheric chemistry" *by* A. M. Aghedo et al.

Anonymous Referee #1

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This study presents an investigation of the relative importance of the four most important African sources of ozone precursors (biomass burning, lightning, biogenic, anthropogenic) to the tropospheric ozone budget over Africa itself and globally. To achieve this goal, the authors use the ECHAM model, a coupled global chemistry climate model. The subject is obviously of particular interest as Africa is known to contribute a significant amount to the global emissions of biomass burning, natural emissions from vegetation and lightning. Marufu et al. (2000) previously presented a budget assessment with another model, the TM3 model. Indeed, this latter study is well acknowledged in the paper by Aghebo et al. The study presented here draws new results and give another quantification of the relative importance of the different African sources to the global troposphere. For example, the conclusion is that 2.4%

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of the global tropospheric ozone is due to the biomass burning occurring over Africa. I would have thought that this source contributes more. A very interesting result and quite surprising to my knowledge is the contribution from the biogenic VOC, greater than the biomass burning products.

Such a topic is obviously interesting and nice enough to deserve a publication however I have major concerns with the paper in its present form. I encourage the authors to work again on the four following major points and to clarify the specific points I raise below :

I-) Major general concerns

1) Introduction: The goal is not very well presented here. I think it deserves more than the two lines sentence lines 7 to 9. That should be stated at the end after having exposed the rationale of the study, the state of the art and brief details concerning the different sources investigated here.

2) Section 2, as well as the entire study is based on Rast et al., 2006 which is in preparation. It is a shame ! I think it is difficult for readers to fully trust such an important study drawing quantitative results without having access to the reference study for the ECHAM model. I found very frustrating the type of sentence "...described in Rast et al., 2006" for key informations like the sensitivity tests, the reference experiment and the influence of the injection heights for some emissions for example. I would suggest if possible to include some results of the future publication in this one. End of section 2, although with a lesser importance, I would like to read more details instead of "...described in Roeckner et al., 2006 and Aghedo et al., 2006" (non available papers by now.) Just add a few sentences to justify your choice and make the following more robust.

3) Section 4.1: Model validation This is my major concern overall. The paragraph and the analysis coming along are definitely too short and not convincing. Such a global analysis on the influence of the African air pollution to the rest of the world deserves

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a thorough evaluation over the African continent. Before drawing the conclusions, it would be nice to check the “accuracy” of the model in reproducing the main characteristics of the ozone (at least) distribution over Africa. The authors use the MOZAIC data recorded over Africa and some other profiles over the other continents (US, South-America, Europe and Japan). Some profiles are presented in January and in July and the overall good agreement is claimed without a proper discussion. They have chosen not to show the seasonal variations. I think this information is always important to give because it really indicates the influence of the most important sources, like biomass burning for example. The authors should have used the Sauvage et al., 2005 (ACP), 2006 (JGR) papers to perform their model evaluation over Africa. These two recent studies make a use of the MOZAIC data available over Africa to draw the main characteristics of the ozone distribution over the continent. Besides, by combining the MOZAIC and the SHADOZ data, they have also presented a regional aspect of the ozone distribution over Africa and the adjacent oceans. I suggest to substantially modify this sub-section to make it a more solid evaluation. I think the Sauvage et al., 2005,2006 papers give the lines to compare with the model. For example, the Brazzaville profiles (available in the MOZAIC data base and not shown here) should be very interesting to show as this region experiences an influence of the northern biomass burning although it is in the southern hemisphere. Besides, is the model able to reproduce the South Atlantic Maximum throughout the year ? This would argue in favour of a good representation of the transport outside Africa, before quantifying it in the following sections.

4) Sections 4.3 and 4.4: As a general comment for the entire paper, I often miss important details. In these sections for example, I would have appreciated some details concerning the calculation of TOB. I am not very familiar with such calculations as many readers probably. Why is the tropopause set at 200 hPa? It is a little bit low in altitude for the tropical regions... especially to quantify the impact of lightning and biogenic components rapidly transported in the upper troposphere as it is shown. Table 3 needs also further explanations. How are calculated the columns 2 to 6 ? Please give

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also a sentence to explain the big difference between the sum of columns 2 to 6 and column 7 in Table 3. Still concerning Table 3 and comments going along in the text: Is there a real need for making a difference between biogenic VOC and biogenic CO, H2 and soil NOx.

II-) Specific points:

Introduction: Page 5800, Line 1 to 3: This sentence from Marufu et al. (2000) is surprising as the 16% contribution from biomass burning is twice the amount calculated in this study (from Table 5). Such a statement should be discussed below, maybe in the conclusion. TOB needs to be defined here. Page 5800, Line 24: This sentence is quite ambiguous with the above arguments stating that the lightning emissions should be close to the lower limit of a few Tg(N)/yr. Remove it or clarify its use.

Section 4.2 and 4.3: Page 5808, lines 18-20: The colour coding for Figure 3 does not make this comment very visible. Page 5808, last paragraph starting line 21: I think Figure 4 shows a marked seasonal variation on contrary of what is said in the text.

Section 4.5: Page 5815, lines 5-6: Overall this paragraph is very interesting. I would like to read more details on this particular statement. is it due to the particular monsoon dynamics or to the El-Nino events that occurred during the simulation period ? Please say more.

Section 4.6: The schultz et al., 2006 reference is missing in the list.

Table 1 and 2: The biomass burning emissions in Table 1 are quite different from Marufu et al., (2000). I think that deserves a brief comment. Besides, Table 2 shows that Africa contributes to about 44 % of the global biomass burning emissions (43% for CO, 42% for NMHC and 46% for NOx). Isn't it too much ? If it's true, how come the impact is only of 2.4 % ?

References: Sauvage B., V. Thouret, J- P. Cammas, F. Gheusi, G. Athier and P. Nédélec, Tropospheric ozone over Equatorial Africa: regional aspects from the

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Sauvage B., V. Thouret, A.M. Thompson, J.C. Witte, J- P. Cammas, P. Nédélec, and G. Athier, Enhanced View of the "Tropical Atlantic Ozone Paradox" and "Zonal Wave-one" from the In-situ MOZAIC and SHADOZ Data, J. Geophys. Res, January 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5797, 2006.