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Atmospheric Chemistry and Physics Discussions

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.

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We would like to thank anonymous Referee 1 for the positive review of our manuscript. We answer the specific points made in the review below:

On top of precipitations regime, fertilizers have also been demonstrated to impact significantly nitrogen oxide emissions from soils, and can lead to strong increase in emissions (see Yienger and Levy, 1995, Jambert et al., 1997, Ganzeveld et al., 2002. . .). And yet, no mention is made about this impact in the paper. Could the authors add a few words about this? Would this impact be important for the African regions of interest in this particular study?

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Interactive Discussion

Discussion Paper

According to a recent study contrasting the use of fertilizers the agricultural practises adopted in Africa still result in only a modest application for crop production compared to regions in the US and China (Vitousek et al, 2009). This results in a net depletion of nutrients in the soil for the African case used, which includes nitrogen containing compounds, thus hindering crop yields. As the referee mentions, the emission inventory of Yienger and Levy (1995) does include some contribution to NO emission from fertilizers, where NO emissions are thought to be between 1-10 percent of the total nitrogen burden added. Moreover, this estimate of the contribution due to fertilizer use in Africa is likely to be on the low side for recent years considering that increases in use have occurred over the last decade. In contrast, the Lathiere et al (2006) dataset does not include the contribution due to fertilizers, nor does the RETRO dataset include contributions to NO from agricultural practises. In spite of this inconsistency, we assume that this term only has a minor effect in this region compared to the release of NO due to micro-biological activity as a result of both limited application and the relatively small conversion of nitrogen in fertilizer to NO. If policy-makers decide to prioritize the funding of fertilizer use as a means of tackling the problems of insufficient food production in Africa, then the additional contribution to NO emissions should be accounted for, which would require a global dataset for such emissions. We add some additional discussion in the introduction to clarify this point.

The authors mention the injection height for emissions from all sectors except transport. What about the injection height for transport and biomass burning emissions then, is it similar? Please give at least an order of magnitude for the different heights.

For transport we segregate the emissions thus; emissions from road transport only occur in the lowest layer of the model (first 50m), for ship emissions they are split evenly between the lowest two layers (first 100m) and for aircraft we adopt the vertical distribution provided with the input dataset from RETRO. We add additional text in the final version of the paper to clarify this point.

Apart from the use of the ORCHIDEE model to provide vegetation description, and

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the period of interest, the differences, or similarities, between the POET and the Lathière et al. (2006) inventories are not detailed. Could the author add a few words on this topic? Are these inventories based on the parameterisations for biogenic VOCs, do they both take into account the same impacts for NO emission from soils (pulse, biomass burning, fertilizers. . .)

We agree that we should clarify that the ORCHIDEE model in fact adopts the same biogenic emission scheme as that used in Guenther et al (1995) emission inventory (which is adopted in the POET emission dataset used in the BASE run). A main difference is that the input variables for the parameterizations come directly from ORCHIDEE in the Lathière et al (2006) inventory, meaning that there is some dynamic nature introduced with respect to climatic effects. Moreover, for isoprene and the monoterpenes the influence of (e.g.) leaf age is also included in the Lathière et al (2006) inventory which is missing in the original emission estimates in Guenther et al (1995). We include a brief summary of both the similarities and differences between both approaches in the final version of the paper.

In section 3, page 10376, line 2-7: Pay attention that the change in deforestation over the 1983-1995 period is not actually considered in the Lathière et al. (2006) inventory. Indeed, the same vegetation distribution, including crop distribution, is considered for the whole period, and the only variation in vegetation growth (LAI) is related to changes in climate and atmospheric CO2. Moreover, to my knowledge, crop distribution was considered as well in the work by Guenther et al. (1995).

We thank the referee for alerting us to this fact and change the text accordingly.

We also address all the specific comments made regarding errors and style.

Refs:

Guenther, A., Hewitt, C. N., Erickson, D., Fall, R., et al: A global model of natural volatile organic compound emissions, J. Geophys. Res., 100, 11447-11464, 1995

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Lathière, J., Hauglustaine, D.A., Friend, A.D., De Noblet-Ducondré, N., Viovy, N. and Folberth, G. A.: Impact of climate variability and land use changes on global biogenic volatile organic compound emissions, Atms. Chem. Phys., 6, 2129-2146, 2006, http://www.atmos-chem-phys.net/6/2129/2006/

Vitousek, P.M., Naylor, R., Crews, T., David, M. B., et al, Nutrient Imbalances in Agricultural development, Science, 324, June 2009.

Yienger, J. J., and Levy II, H.: Empirical model of global soil-biogenic NOx emissions, J. Geophys. Res., 100, 11447-11464, 1995.

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