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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

Interactive comment on "Biomass burning related ozone damage on vegetation over the Amazon forest" *by* F. Pacifico et al.

F. Pacifico et al.

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Response to Anonymous Referee #1 (acpd-14-C8024-2014)

The word "Sensitivity" probably should appear in the title R: We have replaced the title with: 'Biomass burning related ozone damage on vegetation over the Amazon forest: A model sensitivity study.'.

Other comments: 1. Has visible leaf damage to tropical trees ever been reported during/after the biomass burning season? R: To our knowledge there are not published data of field experiments of ozone damage on plants for the Amazon forest.

2. Much of the paper is devoted to understanding the model's over prediction of surface

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Interactive Discussion

Discussion Paper

ozone at 2 sites in the Amazon. The overprediction is a problem since the damage depends on the absolute magnitude of the ozone concentration. The authors' honesty is appreciated. Connected to this issue, that is not yet discussed in the paper but should be, how well does the HadGEM2 model simulate the meteorology over the Amazon? Please include discussion of this validation, what does the model surface temperature, precipitation etc. over Amazon look like compared with observations? Does the model ozone bias occur everywhere in the lower troposphere? Or does the model do a better job of ozone simulation in heavily polluted regions? R: We have included the model evaluation for HadGEM2 meteorology and tropospheric ozone in the supplementary material.

3. Can you use satellite data of tropospheric ozone and NOx to evaluate the model's chemical performance over the Amazon further? R: Unfortunately, we could not find reliable satellite date of tropospheric ozone and NOx for the Amazon region.

4. "The decade-mean CO2 atmospheric mixing ratio was 368 ppm". How sensitive are your results to this assumption i.e prescribed not dynamic CO2 levels? I imagine the atmospheric CO2 levels near the tropical leaves will be quite variable. R: There are large variations on the diurnal cycle of CO2 as well (respiration fluxes building up during the night, stable shallow boundary layer / night-time inversions). Further investigation will be needed, as current model simulations do not take into account of dynamic CO2 levels. Indeed, all global vegetation models are run using annual global CO2 concentration. Between 2000 and 2009 CO2 concentrations increase by ca. 18ppm. Adopting a Beta factor approach, and assuming a beta factor of 0.60 from Free Air Carbon Enrichment experiments, Norby et al., 2005, this translates into an expected modest increase of ca. 2% in NPP over the decade.

5. The simulations aren't fully coupled such that the loss of forest leaves due the fires does not manifest as a change in the dry deposition of the ozone (and BVOC emission), correct? How does this lack of full coupling influence your results? Is it possible that the observed ozone cycles at the 2 sites might be related to the change

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in deposition (decreased ozone loss) over the season, rather than localized production from fire emissions? R: As said in the Model Description (page 19961, line 5) and in the Discussion and Conclusions, the model does not include an interactive fire scheme. Leaf area is lost due to fires and, since stomatal conductance is a major sink for ozone, this may affect ozone deposition. However, the leaf area lost through fire is marginal in comparison to the total leaf area over the Amazon and should thus not affect the ozone concentration. Moreover, most fires occur in pasture areas, and not over forest areas. Second, ozone production in Amazonia is limited by NOx. In the dry season, NOx concentrations can rise by a factor of 5 in comparison to the wet season (e.g., Kirkman et al., 2002), and an increase of the same order is expected for the chemical production of ozone. It is true that the canopy resistance decreases in the dry season (Rummel et al., 2007), but not due to the loss of leaves by fire. It decreases mostly because specific humidity deficit increases in the dry season, resulting in closed stomata.

6. It is intriguing that the authors included domestic biofuel emissions into their analysis. Can they offer any reason why to do this? Isn't domestic biofuel a separate activity altogether? What are the emissions totals for each source in the region? R: Domestic biofuel emissions include sources like fire wood burning, which is why we include domestic biofuel sources of biomass burning, please see page 19963 line 1. At page 19963 line 2 we have added: 'The dominant fire types in South America are from deforestation and degradation fires in an arc around Amazonia, with some regional hotspots of agricultural burning (see Figure 13 in Van der Werf et al., 2010). Between 2001 and 2009 the percentage contribution to annual fire emissions from fire types (deforestation and degradation, grassland and savanna, woodland, forest, agriculture) are (59%, 22%, 10%, 8%, 2%) over Southern Hemisphere South America (Figure 13 van der Werf et al., 2010), with minor differences between this dataset (GFedv3) and the earlier Gfedv2 in this region (see Fig. 16 in Van der Werf et al., 2010).'

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C10030/2014/acpd-14-C10030-2014-

supplement.zip

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