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***Interactive comment on* “Satellite observations indicate substantial spatiotemporal variability in biomass burning NO_x emission factors for South America” by P. Castellanos et al.**

Anonymous Referee #2

Received and published: 24 November 2013

This paper uses comparisons of an atmospheric chemistry-transport model (TM5), driven by GFED3 biomass burning emissions data, with OMI NO₂ satellite observations to evaluate emission factors from biomass burning emissions over South America. The authors show that model-observation biases are consistent with spatially-varied, seasonally-varying NO_x emission factors that differ from the globally-average biome average emission factors. The authors conclude that this likely has implications for our current understanding of fire contributions to atmospheric NO_y chemistry, particularly in regions of agricultural fires, where their inferred emission factors are substantially larger than those assumed in current emissions data.

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The manuscript is well written, and the presented analysis appears very thorough and sound. The manuscript is certainly suitable for publication in ACP, without major changes.

There are a number of points for clarification that would improve the manuscript, which I list below.

General comments

The methodology used depends on the assumption that biases between TM5 and OMI-observed NO₂ result from emission errors. The case made for discounting errors in other aspects of the model NO₂ budget (e.g. chemistry) is not particularly strong. In particular, what is included in the model in terms of organic nitrate chemistry (e.g. PAN precursors, isoprene nitrates)? This is likely important in determining the model NO₂ lifetime in this region of enhanced biogenic emissions. e.g. a balance between HNO₃ production and PAN production likely has strong implications for the NO₂ distribution. An evaluation of model NO_y speciation would be useful in terms of stating or quantifying uncertainties in the NO₂ distribution. Huijnen et al., (2010) gives some detail of the TM5 chemistry, but only a limited evaluation in this region.

Similarly, in the discussion of the study by van Noije et al. (2006) (page 22768) and comparison of year 2000 GOME observations with TM4 simulations using 1997–2002 average GFED emissions, it is not clear why this demonstrates that the TM4 and TM5 chemistry and transport of NO_x over South America is reasonable. The inference appears to be that a 2x NO_x emission produces a 2x NO₂ column? This says nothing about the validity of the chemical scheme and model transport?

The model chemistry scheme given in Huijnen et al., (2010) for TM5 appears not to include acetone. Is this correct? How might this omission lead to biases in NO_y partitioning for this region?

Specific comments Page 22771, line 12-15: Discussion of sources of uncertainty in

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deriving OMI-constrained emission factors. Huijnen et al., (2010b) appear to show model biases in NO₂ somewhat larger than 30% over South America, the region of interest here.

Page 22777, line 9/10: Given that conclusions regarding increased emission factors from, agricultural burning is based on only a few pixels, is there any other evidence / literature to support this?

Fig. 5: It would be useful to also see spatial maps of the model-observation bias and how this changes spatially when using the new OMI-derived emissions versus GFED3. References Huijnen, V., et al., The global chemistry transport model TM5: description and evaluation of the tropospheric chemistry version 3.0, *Geosci. Model Develop.*, 3, 445–473, doi:10.5194/gmd-3-445-2010, 2010b.

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