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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fire emission rates of NO_x based on the empirical relationship between satellite-derived tropospheric NO_2 and fire radiative power" *by* S. F. Schreier et al.

Anonymous Referee #2

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Schreier and colleagues have overlaid FRP observations with NO2 concentration measurements and analyzed differences in slopes between the two quantities for regions and time periods when fires dominate the NO2 concentrations. This revealed a number of interesting insights into fire NO2 emission ratios. The authors have carefully addressed earlier concerns about their methods and lack of new scientific insights and have done a good job in describing their findings and related uncertainties. I still have one major issue with the approach but do feel the work now deserves to be published after addressing the following fundamental issue.

My main concern is related to use of MODIS FRP observations. Without cloud cover,

fires are observed four times a day. The chances that the satellite observes a slowly moving or stationary fire are much higher than observing a fast moving savanna fire. This is reflected for example in the amount of area burned per fire observed, which increases with decreasing vegetation cover (see for example Figure 4 in Giglio et al., 2006, doi:10.5194/acp-6-957-2006). In other words, one unit of MODIS FRP observed in a grassland (usually moving fast) is not the same as one unit of MODIS FRP observed in a woodland or forest (moving more slowly) with regard to biomass burned.

This means that the findings presented in the paper are valid only when using MODIS FRP observations. If the same analysis would be repeated with geostationary derived FRP or another orbiting satellite with for example another overpass time the results would almost certainly change. I would therefore stress the authors to make clear to the audience that their results are sensor specific, and refrain from presenting emission factors (as opposed to emission ratios) as done in Table 4 as these are probably unreliable. I do understand this is to some degree accounted for using biome-specific conversion factors (page 28478) but these cannot account for the fine detail you present in Table 4 as they are averages over worldwide biomes.

Minor comments: - Please save Figure 10 and 11 as jpg or another format, right now the whole file is 15Mb and slower computers have difficulty because they have to render all the data points in the two figures

- Introduction: "Although GFASv1.0 is based on a different approach, average annual emissions of NOx from vegetation fires are in good agreement ($\pm$50 %) with the widely used Global Fire Emission Database (GFEDv3.1)." Yes, but later you mention these two data sets are linked using conversion factors, so there is little confidence to be gained from the agreement between the two datasets.

- 28470: "In some years, more than 10 % of the total continental area is burned in Africa". Earlier you mentioned most of the global burned area is from Africa, please reconcile

- Summary: "In conclusion, the FERs of NOx derived for different types of vegetation form the foundation of future efforts aimed at a new top-down based method for estimating global NOx emissions from vegetation fires". Yes, but please do look carefully into issues with regard to FRP.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28453, 2013.