

## ***Interactive comment on “Fire emission rates of NO<sub>x</sub> based on the empirical relationship between satellite-derived tropospheric NO<sub>2</sub> and fire radiative power” by S. F. Schreier et al.***

**Anonymous Referee #1**

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The paper "Fire emission rates of NO<sub>x</sub> based on the empirical relationship between satellite-derived tropospheric NO<sub>2</sub> and fire radiative power" by Schreier et al. presents a comprehensive study on the relation of fire radiative power and NO<sub>2</sub> columns densities, both derived from satellite measurements, for selected regions. Based on this empirical relationship, fire emission rates (FER) of NO<sub>x</sub> are derived. The paper is well written. I recommend publication in ACP after dealing with my concern and some minor comments below.

Major concern:

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The authors relate the measured enhancement of the NO<sub>2</sub> column density due to fires to the total NO<sub>x</sub> released. This requires, among other factors, a) tropospheric AMFs for the conversion of SCDs to VCDs, and b) the NO<sub>x</sub> lifetime for the relation between emissions and column densities. Both, tropospheric AMFs and the NO<sub>x</sub> lifetime, have large uncertainties, and can vary considerably on temporal and spatial scales. But the authors use simple assumptions (profile climatology for calculation of AMFs and one constant lifetime) in their study - due to the lack of better data.

The authors are well aware of these fundamental uncertainties, discuss them in depth, and also use them (i.e. regionally/temporally varying lifetimes or AMFs) as possible explanations for specific findings or discrepancies to bottom-up estimates. However, given the large uncertainties of both, tropospheric AMFs and NO<sub>x</sub> lifetime, which easily cover the variability range of FERs derived in this study, I am sceptic if it is actually possible or meaningful to derive FER estimates.

Thus, I do not agree e.g. with the second-to-last sentence of the abstract, as the authors have only presented biome-specific, diurnal, and regional differences of the empirical relation between NO<sub>2</sub> columns and FRP, but this could easily be explained by biome-specific, diurnal, and regional differences of AMFs (different profiles, different aerosols) or NO<sub>x</sub> lifetimes (different VOCs).

I still think that the study should be published on ACP for the well documented relation of NO<sub>2</sub> and FRP. But it could be that the scientific benefit of the observed regional differences might not be a regional changing FER, but a regional changing lifetime, which would not be less important!

I would like to ask the authors to take this change of perspective into account. Instead of waiting for better lifetime estimates to reduce FER uncertainties, one might as well take better bottom-up estimates of FER to investigate regional differences in plume chemistry. Thus I see that a different title like "The empirical relationship between satellite-derived tropospheric NO<sub>2</sub> and fire radiative power and possible implications

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for fire emission rates of NO<sub>x</sub>" would be more appropriate.

Minor comments:

28459/24: Please explain why one would expect a linear relationship between NO<sub>2</sub> and FRP. E.g. the NO<sub>x</sub> formation by the Zel'dovich mechanism is highly non-linear with T.

Equations 2-6: Please use subscripts for all indices!

28466/21: It is a bit weird that Equations 2-4 are presented and explained in detail, while in the end they are just neglected.

28470/24-26: Mistakable; "... megacities ... produce NO<sub>x</sub> ... except for SEA"!?

28474/11-14: I do not see this description matching the data presented in Figs. 5 and 6. The intercept varies regionally, but shows quite smooth patterns. The slopes, however, look quite noisy and have a very high variability of factor 10 on small scales (neighbouring pixels).

28485/16: "considerably"

28485/26: This outlook is in contradiction to 28467/4-6.

28485/27: Why have the boreal regions not been included in this study? How do you plan to deal with the low correlation coefficients (compare Fig. 2)?

28485/28-28486/2: This sounds like a simple and straightforward calculation; why has this not been included in the paper?

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