

First of all we would like to thank the reviewer for the valuable comments.

Major concern:

The authors relate the measured enhancement of the NO₂ column density due to fires to the total NO_x released. This requires, among other factors, a) tropospheric AMFs for the conversion of SCDs to VCDs, and b) the NO_x lifetime for the relation between emissions and column densities. Both, tropospheric AMFs and the NO_x 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_x 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.

We are aware of the uncertainties in the data and have discussed them in detail.

We have also pointed out that these uncertainties are reduced considerably due to the use of monthly means. In addition, we have averaged all single (monthly) data points over the given FRP interval in the approach, and thus, expect further reduction of uncertainties, especially regarding the regional variability.

As a result, we obtained promising slopes of the linear relationship between Pf and FRP, which we refer to as FERs of NO_x. Nevertheless, we agree that the uncertainties in the approach could still be large enough to cover the variability range of the FERs rates, and thus, added the following sentence to the discussions:

“Moreover, the uncertainties in the air mass factors (AMFs) and NO_x lifetime could cover the variability range of FERs derived in this study.”

As we expect further uncertainties for the conversion of FERs into EFs of NO_x, we have decided to omit the presentation of EFs in this manuscript and address this issue briefly in the discussions instead.

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 NO₂ columns and FRP, but this could easily be explained by biome-specific, diurnal, and regional differences of AMFs (different profiles, different aerosols) or NO_x lifetimes (different VOCs).

We agree that the second-to-last sentence might be formulated too optimistic for the abstract. Thus, we have changed the sentence to:

“This analysis demonstrates that the strong empirical relationship between TVC NO₂ and FRP and the following simplified assumptions are a useful tool for the characterization of NO_x emission rates from vegetation fires in the tropics and subtropics.”

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 NO₂ and fire radiative power and possible implications for fire emission rates of NO_x" would be more appropriate.

We have changed the title as suggested.

Minor comments:

28459/24: Please explain why one would expect a linear relationship between NO₂ and FRP. E.g. the NO_x formation by the Zel'dovich mechanism is highly non-linear with T.

The assumption made here (and in other studies) is that the fire radiative power is mainly related to the amount of fuel burned and not the temperature of the individual fire. In other words, FRP is actually related to biomass combustion rate, and thus, area size of the fire. It is specifically designed to depend as little as possible on fire temperature for a given combustion rate. Typical fire temperatures come into play via the typical fire types (and thus emission factors) of the different land cover types. The amount of NO_x emitted in turn is assumed to be linearly related to the amount of fuel burned. If the temperatures of fires vary strongly, this should introduce uncertainties and possibly also deviations from the linear relationship. Therefore NO₂ is linearly related to FRP for specific land cover types as a first order approximation.

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.

The intention of the detailed explanation and presentation of equations 2-4 was to show the principles of NO₂ loss as a function of time and that it is rather difficult to get detailed information about these terms. As a consequence, we choose a more simple approach by assuming a constant lifetime of NO_x, which can be found in the literature. We therefore decided to keep this summary in the revised paper.

28470/24-26: Mistakable; "... megacities ... produce NO_x ... except for SEA"!?

We agree that the formulation of the sentence is misleading and changed it to:

“The selected regions are generally far away from megacities, which produce significant amounts of NO_x by high temperature combustion processes. However, the Greater Bangkok Area located in Southeast Asia (SEA) is an exceptional case due to its emissions from traffic.”

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).

We partly agree with this comment. The general pattern of the slopes is rather smooth. There are some pixels that are higher by a factor of 10 in comparison to the neighboring pixels. However, the relative number of these pixels is very low. In the revised manuscript we now write:

“The spatial distribution of the y-intercepts (Fig. 5) and gradients (Fig. 6) is generally smooth and shows some regional variation, indicating that a robust link exists between TVC NO₂ and FRP. The higher (lower) gradients indicate that lower (higher) values of FRP are necessary for reaching a specific NO₂ level. There are few pixels with unexpected high gradients, which, however, will not affect our analysis as the relative number of these outliers is very low.”

28485/16: "considerably"

Corrected.

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

We agree with this comment, and thus, in the revised paper omit the following part of the sentence: “more accurate lifetimes for NO₂ based on model results,”

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)?

We didn't take the boreal regions into account for this manuscript for several reasons. First, this manuscript was intended to show the general approach, which seems to work best for the tropical and subtropical regions and biomes. Second, the reference sector method, which is used for the stratospheric correction, is affected by larger uncertainties in the boreal regions. As this manuscript has a rather extensive length, we think that it is preferable to focus on boreal regions in a subsequent work. Although the correlation coefficients are lower in the boreal regions, we find that our approach by using all pixels with $r > 0.3$ still yields an appropriate number of pixels for the analysis to present interesting results.

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

As mentioned before, the extent of the manuscript is already quite large. The acquisition of the FRP data from a geostationary satellite for the representation of the diurnal pattern of fires will be a next step towards the estimation of NO_x emissions from vegetation fires.