

## ***Interactive comment on “Satellite observations indicate substantial spatiotemporal variability in biomass burning NO<sub>x</sub> emission factors for South America” by P. Castellanos et al.***

**Anonymous Referee #1**

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This paper utilizes observations of NO<sub>2</sub> column densities measured by OMI, the TM5 model, and GFEDv3 dry matter consumption estimates to calculate emission factors for fires in South America during 2005. The authors take an idea developed by Lamsal et al. (2011), who identified a technique to use satellite NO<sub>2</sub> observations to update NO<sub>x</sub> emission inventories, and apply it in a novel way to constrain NO<sub>x</sub> emissions from fires with OMI NO<sub>2</sub> observations. By combining these constrained emissions and dry matter consumption estimates from GFEDv3, the authors derive emission factors for four different fire types - savanna, woodland, deforestation, and agriculture - over three different months. These emission factors are generally similar in magnitude to

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previous emission factors measured in situ for South America, and produce smaller estimates of total NO<sub>x</sub> fire emissions over South America than estimates derived using biome-specific global average emission factors. The authors identify seasonal patterns in emission factors for some biomes that are consistent with previously observed seasonal patterns (to the extent to which they exist) and provide new information in cases where seasonal behavior of NO<sub>x</sub> emission factors has not yet been studied (e.g. deforestation).

Overall: the paper brings together previous techniques and data and applies them in a novel way to expand knowledge of fire emissions of NO<sub>x</sub>. The paper is somewhat limited in temporal and spatial scope, given that it only considers observations for one year in South America; nevertheless, the work is interesting and represents an advancement in the field. I would recommend publication after the following comments are addressed.

General comments:

My major issue with this paper is with the discussion (or lack thereof) of potential biases. Many of the conclusions of this work rely on the quality the absolute values of the emission factors calculated - for example, they are directly compared to emission factors derived from in situ measurements. The emission factors measured in this work are derived from OMI NO<sub>2</sub> observations, the TM5 model, and GFED3 dry matter consumption estimates. However, there is limited consideration of how known or potential biases in these three elements (OMI, TM5, GFED3) might impact the final values. Furthermore, all three have been shown to have substantial biases (or potential biases) at the relevant spatial scales. Modeled NO<sub>2</sub> columns are subject to bias due to their low resolution in conjunction with nonlinear NO<sub>x</sub> chemistry (e.g. Valin et al., 2011). The instantaneous dilution of NO<sub>x</sub> throughout a large grid cell can cause the NO<sub>x</sub> lifetime to be under- or overestimated (depending on the concentration), which could result in a bias in  $\beta$  (a value used in this work and derived from TM5). Such a bias could have very important implications, but only minimal, non-quantitative discussions of inaccu-

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racy in the model chemistry are included. It has also been shown that it is possible, even likely, that satellite NO<sub>2</sub> retrievals exhibit biases over fires (e.g. Bousseres, 2013; Mebust et al., 2011). Given the clearly inaccurate NO<sub>2</sub> a priori profiles in the case of NO<sub>2</sub> retrievals over actively burning fires along with heavy aerosol loading, the possible existence of a bias needs to be discussed, and yet this issue is not once mentioned in the paper. Finally, potential biases in GFED are mostly ignored, with the exception of an underestimation in mass burned from agricultural fires. I am less familiar with the potential biases in GFED but undoubtedly there are at least a few. I am particularly interested in the possibility that there are systematic problems with the way GFED defines fuel type, and whether the derived fuel types in this work were compared to another land cover product or otherwise verified. Overall, the paper would strongly benefit from a thorough discussion of biases that might impact the derived emission factors (and relevant associated literature). Following on that discussion, new analyses should be added to the manuscript to show how the biases do or do not affect the conclusions.

On another note, this paper has a pretty limited scope in that it covers a single year and South America only. The authors do a good job of setting up the basis for a South America-focused analysis (starting P22761 L14), but I don't feel as though there is a good sense of why the analysis is restricted to a single year, especially since the requisite datasets are available over several years. The authors do mention that 2005 is a drought year (P22764 L6), but that alone does not justify using only a single year of data - in fact it makes it impossible to determine if the results shown here are generally true or are drought specific phenomena. I recognize that to do this analysis for additional year(s) would be a substantial amount of additional work, but I do think there needs to be a clearer, stronger argument as to why this would be outside the scope of this particular paper and how the reader should view the generality of the results presented herein.

Specific comments:

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P22761 L8: This is not how I understood the conclusions of McMeeking et al. (2009). The molar ratio NO<sub>x</sub>/NH<sub>3</sub> (intended to account for fuel nitrogen) was loosely linearly fit with MCE, but with substantial variability and inconsistent in slope with previous work that examined NO<sub>x</sub>/NH<sub>3</sub> ratios as a function of MCE. That certainly does not indicate that fuel nitrogen and MCE together explain all of the variability in NO<sub>x</sub> emission factors.

P22761 L12: How does this MCE compare to typical fire MCEs? Are typical MCEs variable with fuel type? I know this comes up later but it is strange to bring up a number without providing any context for how it compares to a normal fire.

P22767 L11: I am concerned about the assumption that CO is an appropriate proxy for dry matter burned. First, I would disagree that the CO emission factor is constant to within 20%—there are ample observations (e.g. Yokelson et al. 2008) that indicate that CO emission factors (even specifically for “tropical” fires) can vary by a factor of 2 depending on the fire and the type of combustion. And even savanna fires vs. tropical forest fires (both types of fires analyzed in this work) have very different (i.e. >20%) CO emission factors in Akagi et al. (2011), which the authors cite as indicating variability is less than 20%. Further, in the discussion of observations of seasonal variability in NO<sub>x</sub> emissions factors the authors suggest that MCE effects explain some of the variability, but this would imply some amount of seasonal variability in CO emissions too as they are also dependent on MCE. So I would like to see some discussion of this effect. If MCE decreases across the season (one of the authors' propositions) then the CO emission factor would increase, which might help explain why TM5 under-predicts observed CO in October (and to a lesser extent, September).

P22767 L22: How is the model being temporally sampled? I'm assuming it's not a daily average - but is it a snapshot in time (from the timestep following the OMI overpass) or an average over some time period? Given the possibility for temporal differences in the OMI overpass (for example, two different swaths), does this mean that at times the model output is coming from different model times in different grid cells? How does this sampling strategy work with the 3-hr resolution of the GFED emissions? Why not

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take a 3 hour window of the model corresponding to the GFED emission resolution that overlaps with the OMI observations?

P22768 Paragraph 2: I'm not sure I get the argument here. The way I read the written summary of the other paper, it sounds like the model matches the observations, and when the fire emissions in the model are doubled, the column doubles - I don't understand why that indicates that the chemistry and transport is "reasonable". All it says is that increasing NO<sub>2</sub> emissions in the model increases the column at roughly the same rate - to say it in terms of the method described in the next section,  $\beta$  (over the whole region) is approximately 1. To me, that doesn't say anything about whether the chemistry is correct. (Here might be a good place to talk about potential chemistry biases.)

P22769 L1: This essentially describes why the possibility of NO<sub>2</sub> lifetime biases in the model needs to be discussed. If the lifetime is incorrect  $\beta$  will be incorrect.

P22769 L3: I would clarify that EOMI represents the fire emissions (not all emissions) of NO<sub>2</sub> derived from OMI, and that it is calculated assuming that the entire discrepancy between OMI and TM5 is from inaccurate fire emission factors and none of the discrepancy is from inaccurate biogenic/anthropogenic/lightning emissions or errors in GFED mass burned. (The authors might also want to discuss this assumption, whether it is valid, and how it affects the results.)

P22770 L20: Please include the number or percentage of cells that changed to agricultural fires after using this filter.

P22770 L24: Please provide a number after "few and sporadic".

P22771 L16: This is under normal NO<sub>2</sub> circumstances, not for heavily fire-influenced pixels. Also, how do the typical column densities in this analysis compare to this value ( $2 \times 10^{15}$ )? Keep in mind that in a 1x1 degree grid cell at this column density, approximately 50% of OMI pixels in that grid cell are higher than that.

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P22772 and on (Section 5): The authors make a lot of comparisons between different emission factors in this section, and it gets fairly confusing. It would be better to avoid putting the specific numbers directly into the text (since they are all in Table 1) and instead refer to the table and state the major point - e.g. "this value falls into the range of previous measurements and is slightly larger than the mean". Also, I'm a little concerned about the discussion of seasonal differences. The figure is not that compelling - it's not clear that the observed differences are statistically significant given the large error bars, and it looks like the entire "seasonal" difference is due to much higher values in July which suggests that there could simply be some substantial bias in that month. The authors should discuss the uncertainties and provide some sort of statistical assessment of how confident the monthly differences are.

P22773 L11: The findings might support this, but they do not prove it. There are other possible explanations, including fuel nitrogen effects, and these should be discussed or at least mentioned.

P22773 L20: Again, it is not proven that the MCE is exceptionally low in this August, although though the emission factor is. Clarify the uncertainty around this statement.

P22774 L7: I would not use the word "likely" given the several problems identified with the assessment of agricultural fires - including the identified low bias in GFED, the small number of analyzed cells which implies that the values are highly uncertain, and the fact that the agriculturally influenced cells likely are influenced by a larger fraction of anthropogenic and biogenic emissions and thus the authors' assumption that the model correctly captures these other types of emissions induces more uncertainty. These factors should also be mentioned.

P22774 L11: I cannot see how this analysis could simultaneously suggest that agricultural emission factors should be doubled and GFED dry matter consumption should be also increased. The author's values for emission factors are based on GFED dry matter consumption so any low bias in GFED dry matter will automatically produce a

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higher emission factor. If GFED is biased low by 55% then the emission factor that was derived from that will be biased high in response.

P22774 L20: How does this analysis differ for SCIAMACHY vs. OMI? Specifically, how did you sample the model in time given that the SCIAMACHY overpass is at a different time of day?

P22776 L4: It is certainly possible that these discrepancies are due to within-biome variability in emission factors, but it is also possible (and I would argue likely at least in part) that they are due to sources of error or uncertainty in the analysis, including all those I've already mentioned or others (for example, uncertainties in scaling 3hr emissions).

P22776 Section 6: Please review the conclusions to make sure that they reflect any changes made - for example, statements on agricultural emission factors in conjunction with GFED underestimates.

P22788 (Figure 1): This figure strongly suggests the possibility for both model and observational biases. It looks like it is nearly uniformly true that in high NO<sub>2</sub> areas, the model overestimates the observations, and in low NO<sub>2</sub> areas the model underestimates the observations. Given this consistency, I would hesitate to call these process-based (i.e. differences in emission factors) without a thorough analysis of the effects of biases both in OMI and in the model itself.

P22791 (Figure 4): I don't understand - why does biomass burning in July only either stay exactly the same or increase by 30-40%? That discontinuity, to me, suggests some sort of computational error or similar problem.

P22793 (Figure 6): This figure isn't very compelling. I know that the RMSE decreases when using OMI derived emissions in many of the cases, but visually they look nearly identical (OMI derived vs. initial). Rather than the figure you could just include the RMSE values in a table.

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Technical/stylistic comments:

P22759 L4: I understand that deforestation and agricultural fires are important in this region and that is why fire is discussed as a "tool" but it seems incomplete to not at least mention wild fires in addition to intentional ones.

P22760 Eq. 1: Notation in this equation is inconsistent - in one case B is superscripted and in the other it is contained in parentheses. Use a consistent notation. Also, the B and subsequent list of fire types needs to be further separated from the rest of the equation - it looks like it is a variable being multiplied.

P22760 L10: This sentence is awkward and could be reworded.

P22760 L15: I would use "scaling" or "conversion" rather than "partitioning".

P22760 L20: The description of prompt NO<sub>x</sub> is unnecessarily long. I would just say that "Laboratory studies indicate that emitted nitrogen-containing chemical species are accounted for by volatilized nitrogen from the fuel (refs)".

P22761 L24 & P22764 L6: These two semicolons should be commas.

P22762 through the end of the introduction: This section is really long and hard to read because it is so detailed. I understand the desire to explain fire characteristics for each type of fire, but there should be more focus on the details that are important to your later discussion and conclusions. I would suggest pulling out the specific values that are already included in your table (Table 1) unless there is a very compelling reason to include them (such as they are necessary to make a comparison point), and shortening the in-text discussion of each fire type as much as possible.

P22762 L24: These 2 paragraphs on RSC interrupt your description of the four fire types and thus it seems out of place. It would be helpful to move this up somewhere to a section where you are talking about MCE.

P22764 L13: You don't need this sentence - don't need to justify your choice to validate

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using an independent dataset.

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