Atmos. Chem. Phys. Discuss., 13, C7856–C7863, 2013
www.atmos-chem-phys-discuss.net/13/C7856/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Space-based observations of fire NO_x emission coefficients: a global biome-scale comparison" *by* A. K. Mebust and R. C. Cohen

Anonymous Referee #1

Received and published: 9 October 2013

General Comments:

This paper uses NO2 tropospheric column densities from the NASA Aura Ozone Monitoring Instrument (OMI) and fire radiative power (FRP) estimates from the NASA Aqua Moderate Resolution Imaging Spectroradiometer (MODIS) to determine mean NOx emission coefficients (ECs, g NO/MJ) for fires in six biomes (tropical forests, temperate forests, boreal forests, grasslands, shrublands, and agricultural land), as well as for several smaller-scale, geographically-contiguous "ecoregions." This is an extension of the authors' previous work (e.g. Mebust et al., 2011) to the global scale. The resulting mean NOx ECs are compared between biomes, between ecoregions within a biome,

and with other literature estimates of NOx ECs and emission factors (EFs, g NO/kg dry matter burned). The authors find that the mean ECs for the six biomes fall within a relatively narrow range, with the smallest biome-mean EC (for boreal forests) being 70% of the largest (for grasslands). They find that the majority of mean ecoregion ECs are also within or near this range – however, 24% of the mean ecoregion ECs are significantly different than the mean EC for the biome. For example, the two boreal forest ecoregions (North America and Asia) have mean ECs that differ by a factor of 2.

This is a well-written and well-organized paper that provides useful, original information on NOx emissions from biomass burning at a global scale. While the method has been previously published in ACP, the application to the global scale is a new and useful contribution. The methods are clearly outlined (although some clarification is needed in a few places) and the results support the interpretations and conclusions.

I do have a few concerns that I would like the authors to address in a revised draft. First, while the paper has a clear discussion of the difference between variability of ECs between fires and the standard error of the estimate of the mean EC for a biome or ecoregion, I think it is potentially misleading to only list the standard error in Table 2 and to use the standard error of the fit for the error bars in Figure 8 for comparison with other literature estimates. The variability of NOx ECs between fires is also important information, and should be presented more clearly. Second, the discussion of the uncertainties and biases in the OMI NO2 columns, the MODIS FRP, and the other assumptions in the method for calculating ECs in this paper is overly simplified and qualitative. While I don't expect the authors to repeat their detailed discussion of potential biases from Mebust et al. (2011) verbatim, it would be nice to include some of the quantitative estimates of biases and uncertainties from that paper here. Third, the authors occasionally use "fuel type" as if it were equivalent to "biome," which is confusing. I would suggest the authors use "biome" throughout, as "fuel type" implies a level of detail (fuel moisture, diameter, species, etc.) that is not present in either the biome or ecoregion analysis. Other comments are listed in detail below.

Overall, this is a good paper, and it should be accepted for final publication in ACP after revisions to address my comments as detailed below.

Major Comments:

1. Variability of ECs between fires:

I think that only listing the standard error of the fit in Table 2 (and in Figure 8) could be confusing for readers who might not have a full understanding of just how variable fires within a given biome or ecoregion can be. In order to model fires correctly (or to interpret model results) it is important to not just understand the mean emissions from the fire, but how much they can vary from event to event. I appreciated the discussion, starting at Line 3 of Page 21677, and Figure 2 showing the variability for all fires with high FRP (> 5000 MJ/s), but it is not clear if the factor-of-two geometric standard deviation given there is supposed to apply to all biomes and ecoregions, or if some are more variable than others. In addition, Figure 2 only has data for the high FRP events, and so it is not clear if that variability is consistent with the data in Table 2 and the supplemental tables.

I thus think it is important to list the natural variability (perhaps as geometric standard deviation) in Table 2 and the supplementary tables. I would also suggest that the variability, not the standard error of the fit, be used in Figure 8 as that is more consistent with the approach used in the literature reviews presented there for comparison (as noted by the authors in L1-4 of page 21684). And I would suggest adding a quantitative estimate of the variability in individual fire ECs to the conclusions (L13, Page 21686) and the abstract.

2. Quantitative details on potential biases and errors in the methods used

There are several points in the paper where I feel that I needed more information to evaluate the potential biases and errors in the method used to determine the NOx ECs:

L2-4, Page 21670: What is the lowest spatial resolution of the 40 pixels you use? Is

there any potential or observed bias between near-nadir pixels and the most off-nadir pixels you are using?

L4-7, Page 21670: Is this cloud-clearing criterion sufficient? Do the results change significantly if you adopt a stricter or looser value? Is there also a filter for cloud optical depth?

L3-4, Page 21671: Can you be quantitative about this potential low bias? Can it explain the factor of 2-5 differences seen in Mebust et al. (2011), or is it a more minor effect?

L1-6, Page 21672: I feel that this discussion of potential biases in the MODIS FRP is too short and too qualitative. I would include some of the more quantitative bias estimates from Mebust et al. (2011) here to give the reader a better understanding of the potential magnitude of the biases.

L8-10, Page 21672: Is this a valid assumption? How different were the data for the years 2009 and 2010? Why didn't you limit your analysis to 2005-2010? I think you need to justify this more.

L1-7, Page 21673: Why is there no discussion here about the uncertainty or bias in the wind fields? Again, a summary of the discussion from Mebust et al. (2011) could be included here.

L23-26, Page 21673: I'm not clear why this screening was applied, or how it might affect the results. Wouldn't eliminating large fire events bias the results? What percentage of the fire events were removed by this screening?

L8-14, Page 21684: Isn't this hypothesis inconsistent with the results from Mebust et al. (2011) referenced on L5-7 of Page 21671? In Mebust et al (2011) your EFs were factors of 2-5 lower than the reference values, implying either a underestimate of NOx emission or an overestimate of FRP, but here you are saying that for forests you are higher than the reference values, and suggest an underestimate of FRP as a potential explanation. I may just be confused, but I think you need to clarify what you think is

responsible for the differences in both cases.

3. “Fuel Type” versus “Biome”

At L21, Page 21666 and elsewhere, I don’t think you should use “fuel type-specific” here – I think “biome-specific” would capture your meaning better. I’d suggest similar changes wherever “fuel type” is used as a synonym for “biome,” to avoid confusion. Fuel type implies knowledge of the specific fuels (species, litter vs. grass vs. wood, average diameters, moisture content, etc.) that you don’t have available. Biome more accurately captures your meaning.

Other Comments:

L7, Page 21667: “empirically measured” – as opposed to theoretically measured? More seriously, I’d suggest a short discussion here how NOx EFs are calculated based on in situ observations (and giving the units of EFs, g NO/kg DM burned).

L19-20, Page 21667: I am not clear if you are arguing that, all other things being equal, NOx EFs are not necessarily correlated with MCE and fuel N content, or just that all other things are rarely equal, and so it is hard to observe the correlation?

L28, Page 21668: Do you really mean “simultaneous” estimations of energy and pollutant emissions, or near-simultaneous? How near? How far apart in space and time do the OMI NO2 and MODIS FRP observations need to be for your EC method to no longer work?

L5-7, Page 21671: As this paragraph conflates biases in both the OMI NO2 columns and the MODIS FRP, maybe it belongs more in Section 3?

L15-16, Page 21671: Please include the equatorial overpass time of Aqua, and be more specific on the “near-coincident” measurements – how near in space and time do they have to be?

L27-28, Page 21675: Were these three points unique in any way? Can you say why

they were outliers other than their impacts on the fit?

L2, Page 21679 and elsewhere: How do you explain the low Rˆ2 values of $\sim$0.1? Does this suggest your approach to assigning ecoregions is conflating two different fire types, or do you believe that there are cases where FRP is uncorrelated with NOx emissions?

L1-2, Page 21680: Why do the shrubs show higher FRP? They have more fuel loading than grasslands, but why are they higher than forests? Is this evidence for canopy shielding of FRP, or do the forests just burn more slowly, or what?

L9-11, Page 21683: Why do the shrub fires emit more NOx per unit energy? Higher fuel N content? Different combustion conditions?

Typos and Minor Edits:

L17, Page 21666: “mean fire emissions of NOx per unit energy” would be clearer.

L18-21, Page 21666: These two phrases appear to say the same thing, possibly because it’s not clear what “this range” in the second phrase refers to. Did you mean the range of biome mean ECs (0.250-0.362 g NO/MJ)?

L26, Page 21666: I understand the focus of this paper is NOx, but since you mention aerosols as a direct climate impact, I think you should also mention their indirect impacts via clouds.

L2, Page 21667: The US EPA air quality standard for NO2 is pretty high (100 ppb for 1-hour, 53 ppb annual average), so I think it’s not quite right to say the NO2 from fires has a direct health impact – if you were breathing smoke at that concentration, your health would likely be impacted, but I think NO2 exposure would be the least of your worries. I’d leave it at O3 and aerosols.

L5, Page 21667: remove comma after “emissions.”

L9-10, Page 21667: I’m not sure it’s that they are “simpler to measure or estimate”, it’s more that you can measure burn area, and by combining that with an estimate of fuel

loading and combustion completeness you can get an estimate of biomass burned. I'd suggest rephrasing this.

L12, Page 21668: "span the globe, have full annual coverage" is not strictly true if we interpret it to mean global, continuous coverage. Can you rephrase this to be more narrow and accurate, like "high spatial and temporal coverage" or "daily observations at high spatial resolution?"

L13-15, Page 21669: I don't see what these two sentences add to the paper, so I'd suggest cutting them.

L22, Page 21670: "few NOx sources" – did you mean few anthropogenic sources, few continuous sources, few large sources, or what?

L15, Page 21672: Add refs for "common EF schemes"

L13, Page 21673: Coincident or nearly coincident?

L23, Page 21675: Add ref for bootstrap method.

L1-2, Page 21676: Seems like this sentence fits better after Line 25 of page 21674.

L4-5, Page 21678: You might consider rewording this, as it requires several readings to make sense of right now (I thought it was a typo on my first read through). Perhaps start by saying that 3 forest ecoregions spanned two biomes, and so you end up with 45 ECs for the 42 ecoregions.

L12-13, Page 21678: I know what you are saying, but the phrase "statistically significantly different" is very awkward (and statistically is misspelled the second time). I think, since you say "at the 0.05 level" later, you can cut the word "statistically" in both places.

L18-24, Page 21680: "These ECs" on L18 – it is not clear to me if you are referring to the biome or ecoregion ECs, or both. It is also not clear what "range" you are referring to on L19. Finally, on L24, please clarify which EC you mean when you say "within the

standard error of the EC."

L1-2, Page 21681: I think it would be clearer if you used the phrase "standard error of the fit" or "standard error of the mean EC" instead of just "standard error" both here and throughout the paper.

L9, Page 21681: I'd suggest adding "if the mean is used" to the end of this sentence after "for these specific regions."

L16, Page 21682: I'd suggest adding "using the updated method" to the end of this sentence after "indicated above." This would make clear that you are not just reproducing a plot from Mebust et al. (2011), but that this is a revised analysis.

L24, Page 21684: Remove "e.g."

Table 2: In either the caption or as a footnote to the table, make clear that the uncertainty in the second column is the standard error of the fit for the mean EC, NOT the variability between fires, and add an estimate of the variability between fires to the table.

Figure 1: Describe what is meant by "other" in the figure caption, as you do in the text.

Figure 2: In both the caption and on the x-axis of the figure, make clear these are ECs of NOx expressed as g NO. Right now if this figure were cut from the paper it would not be clear what species is being estimated.

Figures 3-6: In the captions, where you say "range of mean biome ECs", you mean the standard error of the fit for the mean biome EC, correct? If so, I'd use that more specific language to avoid confusion between standard error of the mean and variability between fires.

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