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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Field measurements of trace gases and aerosols emitted by peat fires in Central Kalimantan, Indonesia during the 2015 El Niño" *by* Chelsea E. Stockwell et al.

Chelsea E. Stockwell et al.

bob.yokelson@umontana.edu

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Response to Referee #3

We thank the Referee for their encouraging assessment and constructive suggestions, which will improve the paper. The Referee comments are reproduced below followed by our detailed response.

Anonymous Referee #3

This paper presents results of a field campaign measuring emissions from burning Indonesian peat in-situ, during the intense burning during the intense 2015 El Nino event. These very challenging measurements were collected with mobile sampling set

up incorporating FTIR to measure a range of gas phase species, several photoacoustic extinctiometers to measure aerosol light absorption and scattering at two wavelengths and filter and canister samplers to collected integrated samples of condensed- and gas-phase species, respectively. 35 separate plumes were measured with different combinations of instruments, resulting in measurements constraining both the central tendency and the (relatively large) variability in emission factors resulting from combustion of this fuel.

As evidenced by the startlingly high PM concentrations (>3000 ug/m3) observed in nearby cities, peat combustion can and does make an enormous contribution to loadings of atmospheric aerosols and a wide range of gas-phases species. Considering that the only extant emission factors come from a handful of laboratory burns, which a) do not capture the potential variability in emissions and, ,b) may not recreate combustion conditions observed when the fuel is in place, there is great value in the results of this study. The measurements of gas and aerosol species and aerosol optical properties appear to be carefully conducted and are well documented in this manuscript, and I particularly applaud attention to uncertainty in measurements and the resulting propagated uncertainty in EFs (though echo the other referees' comments concerning their presentation in tables). A rather extensive effort is made to compare the results with those measured in earlier lab measurements, showing general consistency but some very significant differences. The resulting emission factors will be of great use for emission inventory development and chemical transport modeling to understand the impacts of the dramatic land use transformation taking place in this region. Therefore, the manuscript is certainly suitable for publication in ACP. Below I list several points of clarification that would enhance the readability and utility of the manuscript.

R3.1: One general comment is that there is a relatively large number of portions of the text that seem overly detailed and make the paper harder to read than it might otherwise be. Ideally, these could be moved to an online supplemental section. While I understand the use of a spreadsheet for the supplement in this case, perhaps a second

file could be used or some of these details moved to aid readers. Examples include: P. 6, L1-25 (description of sampling sites), P. 8, L22-28 (description of PAX operating principle), P. 10, L20-28 (description of alternate data reduction approaches).

Authors: There are two schools of thought on whether to include details in the main paper, which lengthens it, or in a supplement where they may be ignored. The Referees have done an excellent job of reading the entire paper in detail, and while we agree with the Referee, the suggested supplement content would be a bit fragmented and have the unwieldy task of containing both text and spreadsheets. Thus, we prefer to skip this suggestion.

Minor Points:

R3.2: P9, L23-24 – This description is not clear and imprecise; for example, what is the standard deviation of smoke PM? I think I understand this to be the standard deviation of PM mass concentration, but since this is from a single filter, how is a standard deviation determined? And why is 10% of PM concentration used?

Authors: The standard deviation corresponds to the standard deviation of three replicate measurements of filter pre- and post-weights. The inclusion of 10% of the PM mass is a conservative estimate of the analytical uncertainty in this measurement. The reasoning is, the standard deviations of the filter weights and background PM are very small (ug). For filters with high mass loadings, the absolute error estimated on standard deviations alone would be very small ($<$0.1%), so we add in this relative error.

To clarify, we changed this text to read: "Uncertainty in the excess mass in the smoke plumes was propagated using the standard deviation of triplicate measurements of pre- and post-sampling filter weights, the standard deviation of background PM masses, and 10 % of the PM mass concentration, which is a conservative estimate of the analytical error associated with this measurement."

R3.3: P12, L3-4 – this is imprecise, BC does not absorb light 'proportional to fre-

quency'.

Authors: To a first approximation sp2 hybridized carbon (including BC) absorbs light proportional to frequency (e.g. https://en.wikipedia.org/wiki/Aethalometer), which is mathematically equivalent to the more common statement that BC has an AAE of one (Bond and Bergstrom, 2006).

R3.4: P12, L25 – Unclear. What is meant by 'less close', and what do you mean 'not well-designed for comparison'?

Authors: This text has been deleted as explained in the response to Referee #2.

R3.5: P13, L11-12- While I get what you're saying, this is also imprecise. It doesn't necessarily follow that fires with both smoldering and flaming will have a linkage between MCE and EFs. I think what you mean is that in cases where you have a wider range of MCEs you tend to see a (anti) correlation between MCE and EFs, but the small range of low MCEs observed here means you don't see such a trend.

Authors: Good point. The MCE range is small, but more importantly, it is shifted below the range where it would help indicate the mix of flaming and smoldering, which have different products. We have changed the text as follows.

Old text: "For most biomass fires there is both flaming and smoldering and so EFs correlate with MCE, but these fires burned by smoldering only with no high MCE values (e.g. $>$0.9) and little or no correlation of EFs with MCE."

New text: "For most biomass fires there is both flaming and smoldering combustion and EFs for flaming compounds are observed to correlate with MCE while EFs for smoldering compounds (most NMOGs) tend to be anti-correlated with MCE (Burling et al., 2011). However, these fires burned by smoldering only with no high MCE values (e.g. $>$0.9) and little or no dependence of EFs on MCE was observed."

R3.6: P13, L13-14 – Awkward sentence, what would such characteristics be?

Authors: As noted in our response to the other Referees, we did not note a clear dependence of EFs on any peat characteristic. We also did not have very good capabilities to characterize the peat or map the peat types, so rather than discuss this in detail we have just deleted the text.

R3.7: P13, L18-19 – This evolution in the absorption in this plume seems to be also linked with CO emissions, with Bap(405nm) and CO very tightly correlated in the early stage of the burn, and much less so later on. Were stages of combustion typically seen? Is there any somewhat consistent trajectory in emissions during a burn? I assume that because you measured a relatively large number of plumes that you captured emissions from a range of stages, but it would be interesting to learn of any consistency to give some insights into how laboratory tests can be more representative of combustion observed in large fuel beds in-situ.

Authors: Good point. Babs-405 and CO are actually tightly correlated throughout the sampling, but the ratio of Babs-405 to CO decreases towards the end, which is consistent with an increase in the glowing to pyrolysis ratio. We're not sure what the most representative mix of these processes is, but hope we captured it with our sample size. To acknowledge this signature of the types of smoldering we added text at P13, L21: "B_{abs} at 405 and CO remain correlated, but the ratio of B_{abs} at 405 to CO decreases towards the end of the 5 Nov data, which is consistent with an increase in the glowing/pyrolysis ratio (Yokelson et al., 1997). Variation in the mix of these smoldering processes likely causes some of the variation in EFs."

R3.8: P14, L3-4 – 'obtained closer to 500 nm' is unclear, presumably this refers to illumination wavelength? Would be best to make these MSE values more directly comparable if at all possible.

Authors: We changed "Either value of the MSE is close to MSEs obtained closer to 500 nm" to "Either value of the MSE is close to MSEs obtained at illumination wavelengths in the range 532 - 550 nm."

On the comparison: We have data at 405 and 870 nm and most of the literature MSE values are at wavelengths such as 532 or 550 nm, which is much closer to 405 nm than 870 nm. We have seen (and discuss) SSA measurements for peat aerosol at 781 nm, which is close to 870 nm and, having some idea that it was reasonable, we estimated an SSA at wavelengths between our data. We have not seen MSE measurements for peat aerosol at 405, 781 or 870 nm and are not confident about how to interpolate or otherwise estimate a value closer to 540 $\pm$ 10 nm based on our data.

R3.9: P15, L16-18 – This sentence is very hard to read/understand. Overestimated by what, relative to what?

Authors: There is a tendency to time field campaigns to capture peak behavior, which makes sense, but that is not the only valid time to sample. We changed: "should not be overestimated" to "is difficult to estimate"

R3.10: P15, L33 – Not sure what is meant by 'significant areas' or 'deep burn depth'. Please clarify.

Authors: Upon reflection, this sentence just repeated the point of the previous sentence, which was that our measured burn depths are too high for some burned areas. We've deleted the sentence.

R3.11: P15, L35 – What is distinctly wrong about 'fire products used in top-down approaches'? This sentence is unclear.

Authors: We explained and modified the text as described in response to Referee #2. Briefly, in top-down (inverse modeling) approaches, fires and burned area can be uncertain due to factors such as uncertainty in air mass factors, emission factors, smoke injection altitude, meteorology, and the evolution of species used as constraints.

The new text on P16 L10-17 now reads: "On the other hand, burned area is likely underestimated in inventories since they rely on remote sensing data that misses some of the hotspots and burned area used in bottom-up estimates, as well as some of the

fire products (e.g. CO, aerosol) used in top-down approaches. The information gap is caused by high regional cloud cover; orbital gaps; rapid growth of new vegetation, which is strongly associated with shallow burn depth (Cypert, 1961; Kotze, 2013); and other factors (Lu and Sokolik, 2013; Reddington et al., 2016; Reid et al., 2013). Thus, overestimating burn depth and underestimating burned area tend to cancel when coupling these terms to estimate fuel consumption."

R3.12: P16, L1 – Following from previous confusing sentences, not sure what is meant by 'tend to cancel'.

Authors: In addition to the above new text, please see the detailed response to Referee #2.

R3.13: P19, L4-7 – This is a bit of a non-sequitur as you are talking about exposure and health impacts, and then shift to an EF comparison concerning lab/field measurements which doesn't really so much apply to these very 'high level' estimates. If anything a consistency in air toxic/PM ratio could be highlighted, as this is what you're using to estimate air toxic exposure concentrations.

Authors: Six of the air toxics were only measured in the lab study so we needed to show the relevance of the lab data if we intended to use it, especially for the two more potent pollutants measured in both settings. The Referee points out correctly that some sort of transition was needed and we have added this at P19, L4: "Two of these key species were measured in both the field and the lab and we compare the results."

The companion paper on PM will be based on a much larger, and thus more representative, set of filter data and provide a better comparison to CO and other air toxics.