

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.

Anonymous Referee #3

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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 Niño event. These very challenging measurements were collected with mobile sampling set up incorporating FTIR to measure a range of gas phase species, several photoacoustic extinctions to measure aerosol light absorption and scattering at two wavelengths and filter and canister samplers to collect 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.

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As evidenced by the startlingly high PM concentrations ($>3000 \text{ ug/m}^3$) 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.

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).

Minor Points:

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

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deviation determined? And why is 10% of PM concentration used?

P12, L3-4 – this is imprecise, BC does not absorb light ‘proportional to frequency’.

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

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.

P13, L13-14 – Awkward sentence, what would such characteristics be?

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.

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.

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

P15, L33 – Not sure what is meant by ‘significant areas’ or ‘deep burn depth’. Please clarify.

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P15, L35 – What is distinctly wrong about ‘fire products used in top-down approaches’? This sentence is unclear.

P16, L1 – Following from previous confusing sentences, not sure what is meant by ‘tend to cancel’.

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.

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-411, 2016.](#)

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