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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Near-field emission profiling of Rainforest and Cerrado fires in Brazil during SAMBBA 2012" *by* Amy K. Hodgson et al.

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Review of acp-2016-1019

By Bob Yokelson

Several savanna fires and one forest fire were sampled in the near-field with a heavily instrumented aircraft in Brazil to measure emission factors (EF). These are the first EF measurements in Brazil since 2004 and 1996. The earlier studies sampled more fires, but there is way too little sampling of these extremely important, but variable sources. Further, this study used some instruments not previously deployed on fires in Brazil. The data should be published without a doubt. Since fires are variable, any EFs measured will fall on a Bell curve. Thus, the expectation is not that this data must agree with previous averages, but that it can be combined with previous data to

nudge an evolving global average. At the same time, the authors find no evidence that erroneously low EF in previous work caused the ubiquitous low a-priori emissions in regional to global models.

There is one serious oversight that needs to be fixed. Ferek et al., (1998) reported EF as gC/KgC not g of species /Kg fuel. Assuming the fuel is about 50% C, their EF for BC need to be divided by two to compare to the work here. The EF for CO_2 should be divided by two and then multiplied by (44/12) to convert to g CO_2/ kg fuel, etc. The authors should recalculate all the Ferek et al numbers correctly and then update their comparisons, which come much closer in many cases if this step is taken.

Minor terminology point: "rainforest" is kind of colloquial. I suggest evergreen tropical forest throughout as distinct from seasonally dry tropical forest or just tropical forest if they are not sure which type. It is important to distinguish between understory and deforestation fires, which they do.

One topic not mentioned that might make a good addition to the paper if possible. Were any aerosol optical properties measured? If so, did they scale with EC/OC or MCE as in Pokhrel et al., (2016)?

Some specific comments in order of appearance or occurrence to reviewer.

P1, L12: I suggest defining organic aerosol as "(OA)". OA is the primary measurement, but OC is reported by dividing OA by 1.6. Reporting OC facilitates comparison with more historical data, but it might be worth reporting OA too?

P1, L15: "fuel content" to "fuel mixture" maybe?

P1, L17: Perhaps change "scaling" to "scaling up" to set the context for why the possibility of low EF was interesting.

P1, L18: Maybe simplify the end of this sentence as e.g. ". . . . one potential cause of low a-priori emissions in modeling studies."

P2, L12: delete “to”

P2, L15: There is a far more complex mix of burning in Brazil than just two fire types as discussed at length elsewhere (section 2.3, Yokelson et al., 2007). Referees complained about the length of that section, but maybe a broader summary is in order here. Something like ∼”a range of climate and fire types occurs in Brazil and fire-impacted ecosystems include pure grassland, a gradient of wooded savannah into dry (seasonal) tropical forest (aka Cerrado), and evergreen tropical forest (Ward et al., 1992). In forested areas understory fires can occur, but deforestation fires to establish pastures or croplands, along with pasture maintenance and agricultural residue fires are the most common types of burning on an evolving heterogeneous landscape.” You could cite our 2007 paper or any of the excellent papers cited there-in. Then mention (move here) the later statements (already with citations) about burning shifting from forests to savannas.

P2, L23: Actual landscape fires can rarely be characterized by a single stage, but instead there is a dynamic, variable mix of flaming and smoldering processes.

P2, L29-30: By sampling more we get a better idea of the mean and range. We can also learn about the factors driving the variability, which I think this study may do thru its analysis.

P3, L1: Insert “and other” before “regions” as the mix of AMS and SP2 been used in the US on chaparral fires (Akagi et al., 2012), prescribed forest fires (May et al., 2014), agricultural fires (Liu et al 2016), etc.

P3, L1-4: The historical context and the contrast with the current effort is a bit oversimplified. Previous work was not necessarily less sensitive or unable to probe individual plumes. Yokelson et al., (2007) flew Artaxo’s calibrated nephelometer to get real-time PM10 in numerous single plumes and computed PM10 EFs for Brazilian fires. Light scattering measurements are pretty sensitive. It could be that PM based on light scattering is not as accurate as an AMS, but evidently a nephelometer was used to scale

the AMS data in this work. In SCAR-B in Brazil and SAFARI 2000 in Africa, a very large bag sampler was used and instantaneously filled in numerous individual plumes at multiple ages per plume. This allowed sensitive filter sampling, though filter artifacts can occur (e.g. Ferek et al., 1998; Sinha et al., 2003). Also, real-time data was acquired with a suite of PM instruments in individual plumes in the Mexican tropics in Yokelson et al., (2007, 2009, 2011). I think Capes et al., (2008) deployed an AMS on North African fires. To my knowledge this was the first study to use both AMS and SP2 in individual smoke plumes in the tropics. However, the sentence implying this is particularly true for airborne work should be deleted unless the authors can cite a ground-based study of individual tropical BB plumes that used both SP2 and AMS. Finally, depending on performance, different approaches have a different set of strengths and weaknesses.

P4, L1-5: The authors estimated the mass of PM from scattering data to scale the AMS mass. They should report what mass scattering efficiency they used and an error estimate. Then a scaling factor of 2.69 +/- ? Further, if the SMPS could be used to measure the AMS collection efficiency, it seems like it could also be used for an independent estimate of the AMS scaling factor. On L5 add “forest fire” before “data”.

P4, L15: It’s not super clear how coincidence was solved. Evidently they did not account for small rBC particles below the SP2 size cutoff as is sometimes done, but the figures seem to indicate this correction is not needed in this case.

P5, L15-20: Perhaps report both OA and OC and use OA along with the other species to estimate and compare fine PM?

P5, L23: Technically “hydrocarbons” should be non-methane organic gases (NMOGs) since O-containing VOC dominate. Likewise on P6, L17

P6, L2-3 don’t need, smoke dilution rates slow down exponentially with time and can be even faster on ground-based measurements closer to the source.

P6, L22: A “few” percent is probably more accurate than 1-2% if particulate carbon,

which was measured is not included in total C.

P6, L26 “molecular” should be “atomic in the case of carbon (an error I have made many times).

P7, L15: This is a nice description of the fire. Hotspots are shown in the figure on the day of sampling. It is stated there were no hotspots the previous day and the fire may have started the same day it was sampled. The fire seems a bit large to emerge in one morning and the authors could check a few of the previous days for hotspots since, due to cloud cover or orbital gaps any particular day may have no hotspot data. Brazilian fires can be anthropogenic and off-road due to the presence of indigenous peoples. We likely sampled some of these in an indigenous preserve along the Xingu River.

L26: 30 ppm is really fresh! That’s a good sign that the EF are not distorted by mixing.

P8, L24: The high r-squared likely indicates that the plume was well-mixed and the fire burned a homogeneous fuel bed as opposed to a “common source.” (relevant also to P10, L8)

P8, L25-26: Perhaps the authors are trying to say that there is more variability when sampling a group of fires than a single fire, but the group of fires has a fairly high r-squared nonetheless.

P9. L4-5: Also a general comment. The data for the Rondonia fire is in good agreement for PM and CH_4 with the data for tropical dry forest fires in Mexico (Table 3 in Yokelson et al., (2011)).

Yokelson, R. J., Burling, I. R., Urbanski, S. P., Atlas, E. L., Adachi, K., Buseck, P. R., Wiedinmyer, C., Akagi, S. K., Toohey, D. W., and Wold, C. E.: Trace gas and particle emissions from open biomass burning in Mexico, Atmos. Chem. Phys., 11, 6787-6808, doi:10.5194/acp-11-6787-2011, 2011.

P10, L2: It’s worth clarifying if they are showing the size of the BC core or the core plus coating, and also confirming that coated BC particles were included in the computation

of BC mass.

P10, L15: I would delete “more than” and possibly add “approximately” in light of the uncertainties.

P10, L25 – P11, L6: Several things can be quickly fixed in here and throughout. The Ferek et al values that are compared to need to be converted to g species per kg of fuel. After doing that, the Akagi et al review paper lists these values for Ferek et al as examples: Savanna (OC 2.94, BC 0.35 (note good agreement with Sinha et al., (2003) for African savannas)) and Tropical Forest (OC 4.34, BC 0.46). So for instance, on P11, L3, the Ferek et al value is 0.35 and not 0.7 for EFBC. The authors should go thru all the text, tables, and also figures if applicable, and update the values quoted for Ferek et al and the comparisons as well. Further, most of the “BC” measurements they compare to are actually “EC” measurements that can be impacted by charring of OC. (The PAX measurement in Stockwell et al are an exception.) This should probably be mentioned here (along with later mention) and might inform the comparison.

P11, L2: The Stockwell measurements were by in-situ photoacoustic spectroscopy. This may be a good place to mention EC artifacts and the possibility of lower EFBC because of an unusually smoldery fire. Nonetheless, the data can be factored into the global average nudging it down.

P12, L22: change “to” to “as”

P13, L9: change “is” to “was”

P13, L26: Re “the instrument” – was the SAMBBA SP2 or the May et al SP2 or both SP2’s calibrated with urban-BC relevant material? Suggest clarifying by changing to “our instrument” “both instruments”, etc as appropriate.

P13, L27-28: Informational only, we have just completed such a comparison in FIREX

P14, L19: If there is no clearing then a fire is not a deforestation fire, but understory fires can be indigenous/anthropogenic to promote favored tree species, for hunting, or

to improve access.

P14, L20: “numerous” may be a bit overstated for what looks like about ten hotspots in 3 groups. Maybe “several” or “a group of” is better.

P14, L24: “illustrate” to “confirm” since the differences in initial emissions is a fairly well-known topic.

Fig. 1. Nice fire pics, perhaps a larger version in supplement would be worthwhile?

Fig. 4. It seems odd that the values are larger for the Tocantins fires given the lower emissions??

Table 1: header, reference 9 is Akagi et al., (2012). “laboratory” entry could be “lab/field” since the cottonwood log was in lab, but the Zambian log was in the field.

Table 2: There should not be any missing (“-“) values since all three gases were measured in all cited studies. For the Brazil smoldering logs (ref 2) the value for CH4/CO (X1000) is 143 not 14.3. This value for ref 7 may also be a factor of ten low?

Table 3: Third and thirteenth entries down for $EFCO_2$ (ref 3) looks suspicious.