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Interactive comment on "Biomass burning fuel consumption rates: a field measurement database" *by* T. T. van Leeuwen et al.

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This can be a useful data base for the scientific community with a bit more work. The authors could highlight in the abstract, or elsewhere sooner in paper, that this an updateable data base that resides on the internet.

A few methodological notes.

In much of the refereed literature "fuel loading" is considered equivalent to "total biomass." In US land management agencies, and some refereed literature, "fuel" has a very different operational definition meaning the biomass expected to experience significant consumption under the current weather and fuel moisture conditions. It's not uncommon then to calculate fuel loading (FL) as e.g. "biomass less than 2.5 cm in

diameter and less than one meter above ground." The authors allude to possibly using the more restrictive operational definition on page 4 line 14. It's important to distinguish because if one applies a combustion completeness (CC) calculated with respect to a restrictive pre-fire fuel loading to total biomass, the overall biomass burned or "fuel consumption" (FC) can be too high. The authors should ensure they do not fall in that trap.

Also, the temperate forest and chaparral ecosystem-average FC values seem too high and some effort should be made to distinguish wild and prescribed fire FC at least for the temperate forest ecosystem as explained in more detail below. Section 3.7 and Table 5 of this open-access paper provides some prescribed fire FL and FC measurements the authors may want to include: http://www.atmos-chem-phys.net/13/89/2013/acp-13-89-2013.html

The writing needs to have a sharper, higher-level scientific focus. The statement that readers must use "extreme caution with average values" doesn't meet the normal scientific criteria for expressing the situation nor does omitting the uncertainties. The way to explain it scientifically is that FC is naturally variable and hard to measure and there are few measurements for some ecosystems. Thus confidence in the average value is low and the coefficient of variation is large. It's important therefore to include uncertainties for each value in the text and let the user assess the implications for their application. In general, high uncertainty alone does not justify implementing a non-average value, but using non average values could be justified if they were produced by a validated model that explains the observed variability in field measurements. If the authors believe such a model exists they should promote it clearly. At present, a comparison is presented towards end of paper, but no conclusion is presented after the comparison. Using a non-average value, but within the uncertainty, could also be of interest (or convenient) if it systematically improves representation of e.g. downwind concentrations. In this latter case, it would ideally be made clear by the user if altering the FC is the only reasonable solution or if a change in other uncertain parameters (e.g. burned area) cannot be ruled out.

Also, the word "rate" is used erroneously through-out the paper since "rate" implies an amount per time rather than an amount per area.

I believe the authors intent is to offer this a useful database and not a comprehensive treatise on uncertainties in calculations of biomass burned at various scales, but they could provide a slightly broader summary of uncertainty at the top of page 5 by including or recognizing some of the following points: A fire that is missed by FRP may be seen as burn scar, this is a possibility, but not a given because many short-lived fires also have small burn scars. In general, detection of fires as heat, fire emissions, and burn scars is far from complete. Challenges for bottom-up or top-down approaches are clouds, the cloud mask, and orbital gaps. Added challenges for bottom up approaches include fires that are too small, canopy obscuration, sites that green up before next look, and detected fires assumed to be in wrong ecotype or uncertainty in FC in general. Additional weaknesses of top-down include uncertainty in injection altitude, meteorology, secondary chemistry, poor spatial and temporal resolution, and the unknown contribution of other sources. All approaches are highly uncertain, but work should continue on all because biomass burning is a very important source.

The need to assign ecosystems properly to use this data suggests a possible additional short section would be useful with recommendations on vegetation maps/layers or at least citations to commonly used options and/or any review articles on the topic.

Specific comments:

P3 (or 8117), L3: first use of "rate" which I suggest to eliminate

P4, L8: particles also

P4, L13: change "can be obtained directly" to "may be estimated" since there are options and it's not an exact measurement.

P5, L1: here "rate" is OK since power has time in the denominator.

P5, L10 "emissions" to "consumption"

P5, L15: append "which is updated on-line"

P5, L17: add "also" after the first "is" since FC is fundamentally the difference between pre and post fire biomass loading. Assuming that FL X CC is as useful is strictly true if FL and CC don't depend on each other.

P5, L20: I believe it is fire-integrated FRE (energy) divided by fire-integrated burned area that might give FC under ideal conditions. Getting FC from FRP would be like trying to measure how far a car drove by measuring its speed at one point.

P5, L23-24: I would just say that fine fuels usually have a higher CC than coarse fuels since there a general inverse relationship between FL and CC has not been demonstrated (at least not in this paper, e.g. more grass is not known to make CC decrease?).

P5, L24-25: In the absence of disturbances total forest biomass tends to increase at a well behaved rate, but depending on how FL is defined it can change with the weather. The authors should choose one definition of FL and use throughout – or clarify that this problem adds uncertainty.

P6, L9: Akagi et al listed 47 FC measurements for nine fuel types to provide examples, this paper is a first attempt at a comprehensive tabulation of refereed measurements.

P7, L11: "After the burn" implies a prescribed fire or slowly moving wildfire and comparisons in and out of fire perimeter are also done post fire.

P8, L5: is Mg ha-1 actually better? If using metric tons they are sometimes spelled "tonnes" to avoid confusion with British "ton" – either way it should be plural!

P8, L16&17: Reminder, improper uses of the word "rate"

P9, L3: using "dry savanna" before defining, fix suggested next comment

P9, L5-7: suggest moving these two sentences after the Gill and Lana reference on previous page.

P9, L4-5: Note I backed up. For grass production to limit area burned maybe it needs to be explained that fuel density can affect how well a fire propagates for a given wind-speed?

P9, L12-13: the lack of grasses that "restrict" nitrification causing moisture-independent low biomass in Australia. Can this be restated so it is more obvious what is meant?

P9, L14: Miombo and Cerrado and "Monsoon" Forest are also commonly called "tropical dry forest," maybe more often than a savanna? This is an important "gray area" that could be pointed out. In Akagi et al 2011 they adopted a percent tree cover value as an unambiguous threshold. Here the authors appear to have adopted yet another term that is seen sometimes: "wooded savanna."

P9, L20: I never heard of "dense woodland" meaning "tropical dry forest" or "open forest" or "wooded savanna."

P9, L24: Very important to add the variability here and throughout! I suggest to append standard deviation (or range in the case of only two values) to each average value given as a matter of habit.

P9, L28&29: not sure regional differences are "substantial" especially compared to uncertainties or natural variation and maybe also add "nominally" before "higher."

P10, L4: the "differences" are not statistically significant. "Conclusive findings" is a different concept.

P10, L14: "surface area to volume"

P10, L23: This or in discussion may be a good place to point out that the analysis of CC data by Akagi et al 2011 (Sect 2.4) suggests that CC increases over the course of the dry season as large diameter fuels dry out. This idea is consistent with a seasonal

decrease in MCE proposed by Eck et al.:

T. F. Eck, B. N. Holben, J. S. Reid, M. M. Mukelabai, S. J. Piketh, O. Torres, H. T. Jethva, E. J. Hyer, D. E. Ward, O. Dubovik, A. Sinyuk, J. S. Schafer, D. M. Giles, M. Sorokin, A. Smirnov and I. Slutsker, A seasonal trend of single scattering albedo in southern African biomass-burning particles: Implications for satellite products and estimates of emissions for the world's largest biomass-burning source, Journal of Geophysical Research: Atmospheres, Volume 118, Issue 12, 27 June 2013, Pages: 6414–6432, DOI: 10.1002/jgrd.50500

P10, L24: I think the more precise terminology is tropical "evergreen" forest? A sentence fragment or some idea on how common droughts are would be helpful since the Amazon has had quite a few droughts in the last few years.

P11, L7: "tons" to "t" or "Mg." I think you need to better differentiate at the outset between 1) deforestation fires, where as much biomass as possible is cut and piled and the desire is to remove the biomass as completely as possible, often in a series of burns and 2) mostly accidental or escaped fires in selectively logged forests where conversion to agriculture is not a goal. Then discuss the factors affecting these two fire types separately.

P11, L20: This is a bit oversimplified: This paper: http://www.atmos-chem-phys.net/7/5175/2007/acp-7-5175-2007.html Sect 2.3.2 gives a more specific discussion of past work by Fearnside, Kauffman, Cochrane, Morton, etc. In general, forest slash that doesn't burn in a first fire may be subjected to additional fires during the same dry season. If conversion to pasture is the goal more residual biomass can be tolerated and it is mostly removed during pasture fires in subsequent years. If conversion to e.g. mechanized soybean production is the goal, the slash (or residual material) is often assembled in windrows (long piles) to enhance CC. Other times crop residue fires or deforestation fires accidentally escape and burn some nearby degraded forest.

P12. L3-4: The authors should use more consistent definitions of various ecosystems. Here tropical dry forests are mentioned in the tropical forest section and many people might include Miombo in that. One possibility is to harmonize with the emission factor compilation of Akagi et al 2011 in which 60% canopy cover was the delineation between wooded savanna and tropical dry forest. From page 5 of that paper: "Tropical dry forest is also called "seasonal" or "monsoon" forest. Tropical dry forests (TDF) differ from "woody" savanna regions in that TDF are characterized by a significant (>60%) canopy coverage or closed canopies (Mooney et al., 1995; Friedl et al., 2002). Savanna regions are qualitatively described as grassland with an "open" canopy of trees (if any)."

P12, L8: reminder "FC" ok by itself does not need "rate" to follow it

P12, L15: The observation of size or class dependent CC goes back to at least Ward et al 1992

P12, L16: "surface area"

P12, L22: I suggest that this section be divided into prescribed and wild fires (PF and WF). Otherwise people may apply FC values of 93 t/ha for PFs where the typical value is ∼5 t/ha: a huge overestimate for a fire type that applies to circa one million ha a year in US. To continue: the temperate forest FC totals and FC by class both seem way too high. E.g. 42 t/ha for duff as an average for temperate forest fires is already almost ten times typical total FC for prescribed fires which account for a large fraction of the burning. At the least, it may be that some attempt is needed to weight the "type averages" for WF and PF in this ecosystem by their relative occurrence. In addition, as a general consideration, the authors could consider weighting individual studies by the number of measurements in the study.

P13, L6: The Mexico study should be included in average and weighted by the relative number of measurements. FL and CC are usually secondary products from measuring FC anyway and the FL definition has not yet been clarified.

P13, L25: very little woody debris on sites subject to frequent PF.

P14, L4: Much of the Asian boreal forest is disturbed by illegal/legal logging in Siberia. Vandergert, P., and Newell, J.: Illegal logging in the Russian Far East and Siberia, Int. Forest. Rev., 5, 303–6, 2003.

P14, L10: Most of the FC in a crown fire can be duff.

P15. L5-8: just properly describe this method near the beginning of the paper, give it acronym and use acronym. The biomass in plots is oven dried and weighed both pre and post fire or at burned and adjacent unburned sites and FC is the difference.

P15, L12: The boreal forest FL average is lower than the temperate forest FL average, but is this only if the co-located boreal peat deposits are ignored? Currently the paper discusses boreal peat separately in Sect 2.9 and it would be useful to provide a little guidance on whether peatlands are a greater percentage of the boreal forest biome than the temperate forest biome and a few words of general guidance on how to couple the FC data for biomes that overlap geographically.

P16, L3-6: The direction a mountain slope faces is called "aspect" and aspect has long been known to correlate with ecosystem variability in the temperate zone as well. There should be plenty of references to that if a discussion of this is appropriate. The effect is only insignificant in the tropics where the sun angles are higher. Of course there are wet-side dry-side issues and altitude based variation in mountains world-wide, but not sure a discussion of "sub-grid" variability is appropriate.

P16, L10: "forest" to "deforestation" – it's helpful to distinguish between "deforestation" and "accidental" forest fires.

P16, L19-21 and L25-27: Re "Note that two studies represent shifting cultivation measurements and were not included in the biome average calculation." Why are they in the "pasture" table/section then? Aren't they part of some biome and should they be included in some category such as tropical forest?

P17, L5-7: The ignition pattern seems like an un-needed detail, especially since it is not given for other fires. More importantly probably, the fuel geometry varies globally from short-lived burning of loose residue in the field to long-lasting smoldering combustion of small hand-piles of residue, both hard to detect from space.

P17, L15: Excellent place to cite the classic work of Yevich and Logan!

P17, L17: Another good paper on fuel consumption in rice straw burning is Oanh et al., Characterization of particulate matter emission from open burning of rice straw, Atmos. Environ., 45, 493-502, 2011.

P17: L18-19: probably doesn't add much to give years of measurements in the text throughout.

P17, L20-22: 88% should be expressed as a fraction to be consistent. Also, isn't 0.88 CC too high for pre-harvest burning, which I understand is the most common type of burning at least globally? It would imply that a) the sugar cane field is almost 90% weeds since pre-harvest burning is to remove undesired plants prior to harvesting the cane, or b) the 0.88 is only for post-harvest burning. Re-examining the study of Lara et al, without providing methodology or references, they simply state that FC for Brazilian sugar cane fields was "about" 20 t/ha. It may be that more reliable info is now available.

P17, L22-23 and P18, L2: 0.88 is expressed as a fraction, but attributed to EPA source on P18, L2. Whereas earlier the same CC is attributed to both McCarty et al and French et al. It actually doesn't agree that "good" with 0.65 value given on P17, L27. In general it's better to avoid words like "good" and just give percent differences so the reader builds up a quantitative knowledge of well things agree. Also clarify sources if possible.

P18, L3: eliminate "wildly." This variability is exactly what you expect for growing different monocultures.

P18, L5-8: Is this a good guess or a documented fact with references? And not sure

the FC from the study of Lara et al bears inclusion.

P18, L24: The FC for chaparral of 31.5 t/ha based indirectly on two studies is higher than the total FL in 3 of 4 studies listed in Akagi et al., 2011 Table 2 and higher than the one study by Hardy et al that actually reports FC in the authors work. Having been to several chaparral fires where only the foliage burned and the charred woody biomass remained. I think this number may be too high, but suggest the authors attempt to consult with experts at CalFire or USFS. Alternately, the Cofer et al FC value may just be unreferenced, recycled "conventional wisdom" whereas the Hardy et al measurement is definitely from a detailed, dedicated FC study. If this is the case, the Hardy et al value may deserve much higher weighting.

P18, L23-24: Stick to fractions or percentages for CC. Also, the authors seem to be saying they took the Cofer et al FC and multiplied by (1/.78) to get derived Cofer et al FL and then averaged with Hardy et al FL to get ecosystem average FL. If so, be more explicit.

P18, L24-26: The last sentence on this page doesn't make any sense to me. Why would a young and old stand essentially reflect no growth and what is "of and the same counts of FC rates"

P19: L3-4: "Southeast Asia"

P19, L5: "but only the peat above the water table can burn."

P19, L7: nice pun

P19, L 10-11: What is meant by "(although more variable)"? Also, two more references with tropical peat carbon content, Christian et al., 2003 (JGR) and Stockwell et al 2014 (ACPD) bring total range of peat %C to 53.83 to 59.71.

P19, L15: It is widely reported that the reason to drain the peatlands was a failed attempt at conversion to rice production and commercial logging doesn't require draining swamps per se. However, some commercial logging also occurred after the fact. You might say “Commercial logging in drained peat swamps has increased their susceptibility to fire.”

P19, L18: “four studies provided FC measurements in tropical peatlands . . . ” (skip the years throughout).

P19, L19-22: I don’t recall seeing pre-fire measurements in most of these peatland studies. In some anyway, I think the FC was estimated simply from post-fire observations of burn depth with prefire conditions reconstructed from adjacent unburned areas.

P19, L23: “fire regime” refers to patterns of fire occurrence and not an ecosystem and is misused here and several other places. Suggest “tropical peatland had highest FC . . . including overstory”

P19, L25-27: Delete “was found to be representative” since there is only one data point! Evidently 314/0.27 was used to calculate 1056 t/ha as the ecosystem average FL? In general for the peatland biome you should make clear when you are considering the peat only and when you are considering the peat plus the rest of the biomass in the ecosystem and also that some peatland fires consume overstory forest fuels, but much of the overstory has already been removed in some peatlands.

P20, KL13-14: In “susceptibility of peat fires to fire during different moisture conditions” delete “fires”?

P20, L16: how will paleoecological studies improve knowledge of FC?

P20, L18-19: This text doesn’t make sense as written: “the peat depth was sampled to determine the peat density” L19: is bulk density the same as density? Define “bulk density.”

P20, L21: As written this could imply that the two studies had the same average FC value to three significant figures. I think you mean the “average of the two studies.” This is a case where the standard deviation of the mean with one study at 42 and the

other at 43 very likely underestimates the real uncertainty in the biome average since site to site variability within the studies is much larger than that. Suggest using average uncertainty in this case.

P20, L22-25: Interesting, one might expect the permafrost to prevent deep burning and the hummocks to be better drained and more susceptible to fire?

P21, L5: delete “storage”

P21, L10-11: So is there evidence fires are increasing or not?

P21, L27: change “good” to “sufficient” or somehow indicate the problem is quantity and not quality.

P22, L8: Shouldn’t “fire occurrence” be “fuels”? In general, there is more to this than geographic coverage. More complex systems require a larger number of samples to have confidence in the mean and/or trends. The authors may want to consider whether these final sections really prove geographic trends or add new insights beyond what has already been presented and delete them if not.

P22, L18: change “in not now” to “is not now”?

P23, L3-5: in general CC can increase as the dry season is prolonged as argued elsewhere for savanna fires (Akagi et al., 2011).

P23, L13-14: The forestry literature has dozens of tropical forest biomass measurements for forests of specific ages. They tend to show a nice increasing trend. Here the authors note that “primary tropical evergreen forest, tropical evergreen second-growth forest, and tropical dry forest” have different FC values. I suggest that these categories (or numerical stand age if available) be indicated in the table for models with access to that sort of detailed vegetation information.

P23, L16-19: Re “Clearly, the definition of a certain biome is not always straightforward, and the regional discrepancies found within the different biomes should be taken into account when averaged values are interpreted and used by the modeling communities" So here the authors seem to claim that geographic differences in the measurements within the same nominal "biome" are statistically significant, but I don't think that has been proven?

P23, L22: delete "more" since todays models need values to use now.

P23, L20-26: These could be good ideas if they work, but then give some citations to some of these models and at least a summary of how well validated they are. Or a hint that such a discussion is in next section?

P24, L10: define "grid cell"

P24, L12: define "pixel"

P24, L13: define "fractionation" and explain how this calculation was done in clear terms

P24, L13-14: define "regions" and "time period" explain why and how seriously does this over/under estimate biome average and is it expected to be biased?

In general, it's a better test of the model to compare GFED values spatially and temporally as closely as possible to the published measurements, because the ability to accurately portray trends or geographic variability (or lack there-of) is the main justification for the extra complexity of using the model. It's not clear at the beginning of the discussion that this apparently is the objective as revealed finally at L17.

P24, L21: add "co-located" before "GFED3"

P24, L27-28: To be objective, another possibility that should be mentioned is that GFED underestimates the fire return interval.

P25, L3 "difficulty" to "uncertainty"

P25, L4-6: Improving models will not make the field measurements more representa-

tive. As far as improving the models, a simple statement that it will happen seems like unsupported, vague speculation. If some specific model advance is planned this could a good place to describe it in concrete terms. Otherwise change "will" to "may"

P25, L10: The statement about "repeated fires" doesn't make any sense yet. Do you mean you increased the fuel consumption for some burned areas to account for follow-on attempts within the same dry season to burn residual material that failed to burn in the first fire of that dry season? All ecosystems have repeated fires at some time scale – especially the savanna so this needs to be clarified. In general, the paper needs to be written so that people who did not do these calculations know exactly what you did.

P25, L18: Another reason to think about providing a column with rough or actual forest age and maybe even fitting a FC vs forest age relationship.

P25, L19-28: Wildfire fuel consumption is higher than prescribed fire fuel consumption according to conventional wisdom, common sense, and the data in Table 1 (I think, it would help to label each fire as PF or WF).

P25, L21: "focused" or "included only" or "9 out 10" please be specific.

P25, L23: what do you mean by "ground fuels" litter plus duff, duff plus roots, dead and downed wood included? Define terms near beginning of paper and then use as consistently as possible.

P25, L25: prescribed fires tend to burn less fuels and the studies that do not include canopy fuels were probably for prescribed fires. While it is easy to imagine the CASA model generating grass and litter and then GFED using a CC assumption to burn some of that grass and litter, I have no clue how FC is calculated in GFED for a complex forest environment and a paragraph summarizing that would be useful. Without that, this section and important comparisons will be enigmatic.

P26, L11: 1.6 t/ha (also in Table 3) seems like it has to be a misprint as that number is not physically realistic. If not, how can GFED be more than 50 times lower than the measurement average?

P26, L12, It may not be that all the measurement locations were “wrong,” but that the overall sample is skewed. It may also be the mix of fire types that might be non-representative. Or the model could be wrong. Change “indicates that the” to “suggests that the mix of” and add “and fire types” before “shown.” It’s nice to consider all the data, but a review article may justify having to reject some data.

P26, L13: “counts” to “holds”

P26, L14: The authors may find that the USDA Cropland database is helpful to fine-tune their comparisons by crop type: http://www.nass.usda.gov/research/Cropland/SARS1a.htm

P26, L17: “measurement” (no “s”).

P26, L20: change first “on” to “of” and delete “studies on”

P26, L21: Many FL measurements exist also for different aged tropical forests in neotropics.

P26, L22: make it clear if the spreadsheet at the link includes the values in the paper and additional values not in the paper both. Instead of saying “it may change the average” say how it does change the average if included, but also why that was not considered appropriate for the paper.

P27, L1-29: Few things could be improved. First, the FRP/FC relationship is given to three significant figures with no uncertainty three times, which is unrealistic. 0.316 +/- 0.05 seems more reasonable. Plus that’s only when there is no obscuration at all. FRP is at best sensitive to the momentary rate of fuel consumption, but not the total FC for the whole fire. FRP could be indirectly related to FC if all of some fire product was detected and that products emission factor was known and highly constrained. But emission factors are variable. And when viewing from space in practice, if a cloud/cloudmask covers the smoke, but not the hotspot, the emission/FRP is es-

sentially zero. When the cloud/cloudmask covers the hotspot, but not the smoke, the emission/FRP is infinite. Thus, the relationship is likely to be fairly uncertain. FRP has to be integrated over the life of the fire to get FRE to estimate FC more directly. Geostationary data (with fifteen minute time resolution) would be better than MODIS for this, but many tropical fires are small and only live 15-30 minutes. In general observed, emitted energy is going to be less than actual energy, but there may be an over-/under-correction to produce final estimate. The second paragraph says that FC measurements by FRP are “anecdotal” but the third paragraph gives a FC from FRP with no uncertainty attached and seems to indicate that the approach works almost perfectly. Maybe what is missing is whether the “FRP-based” calculation of FC was tuned to match available measurements or if there was fortuitous cancellation of errors, etc. Also be clear if it “worked” at an ideal point or on a broad landscape scale.

P28, L5-6: Most of the burning in Brazilian Amazon is pasture fires or crop residue fires so 250 t/ha is really high unless the study site was small enough to only include slashed and burned tropical forest.

P30, L1: “reasonable” to “reasonably” and add “co-located” before “measured” Somewhere in conclusions the fact that measured/GFED3 FC for temperate forest is 93/1.6 unless this is rectified during the revisions.

Table 2b: “logs” versus “large woody debris” same thing or different?

Table 2c: the FL of the litter alone is greater than the total FL in Table 5 of Yokelson et al 2013

As a former wildland firefighter, prescribed fire lighter, etc I think 60% CC for duff and 96% CC for dead downed logs is only applicable to extreme fire conditions. These fuels quite often experience only surface charring. I would say more typical is 10% CC for each of these fuel components during wildfire season.

Fig. 2: Use “Wooded Savanna” instead of “Woodland” which is easier to confuse with

forest?

Fig 6: make clear all US (McCarty) except Lara is Sugarcane Brazil.

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