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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Water vapor release from biofuel combustion” *by* R. S. Parmar et al.

R. S. Parmar et al.

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We thank the reviewer for his extensive comments. We try to fit our responses into the given (italicized) text.

We do not claim that we are making a large new scientific contribution. If this work helps to clarify features, the better, especially, if our contribution triggers the release of unpublished work.

Part 1: page S1408, line 17 We take from the reviewer’s text on: For the freshly cut sagebrush twigs and foliage (in the above "own" example) the average fuel moisture of the whole fuel complex was estimated to be 50 +/- 5%. This is a value higher than the stated "moisture of extinction" of 15-40% or below 30% mentioned on page S1413, line 16, at which flaming combustion will not be sustained. We will need to come back to this in Part 3.

The worries of Part 1:

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1. The ignition method is not described. They could be boiling off fuel moisture or absorbed water on surfaces (while volatilizing very little biomass carbon) during ignition, but not including this in the analyzed part of the data. Indeed the figures tend to show a spike in H2O near the beginning of each fire. An intense ignition method could also make it possible to burn fuels that might not burn under many circumstances in nature (vide infra).

We agree that the ignition method was not described sufficiently. We have amended the text in this respect by stating that we use a butane torch, but do not use the emission data until we have a self-propagating fire. The new text:

Text change (page 4487, line 6) .. downwards. The fires were started by a butane torch. A switch in the exhaust stack above the funnel was used to vent the emissions at the beginning vertically out through the chimney and thus discarded. When combustion conditions stabilized and were self-propagating without further support, usually after a minute or so, the switch in the chimney was used to redirect the effluents through a steel pipe of ˜20 cm diameter and 500 cm length into the sampling container. Assuming ... And after page 4488, line 1. .. natural fires. We consider the reloading during the combustion process as being similar to the propagation of a flame front in a wild fire. This includes that radiating heat already initiates vaporization of volatile compounds, which may boost the fire in case of light hydrocarbons or attenuate it in case of water vapor by being a heat sink.

2. They reload fuel during the experiment, which could introduce a heat sink. In nature, the fuel is preheated and to some extent dried by an advancing fire before ignition.

Indeed, we reload fuel during the combustion session intentionally and thereby in fact we introduce a heat sink as in nature, preheating and to some extent drying the fuel. We want to imitate a progressing fire-line. The changed text is already shown above.

3. The most important issue is that the data sets shown in their figures tend to end with the carbon emissions still at their peak values. This suggests that the data is not

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fire-integrated as the authors claim. The significance is that data is collected while fuel moisture is being boiled off, but not until all the carbon production is complete. This does not necessarily invalidate the data for all purposes, but would cause an overestimate of the bulk fuel moisture.

There may be a misunderstanding about the integration. Knowing about the pitfalls of artificial ignition and missing emissions from low smoldering, our aim is to cut a time sequence out of a biomass fire, which we try to mimick as naturally as possible. So integration as mentioned for Table 1 means for the time of the measurements, not for the full fire for a total carbon balance. Reloading keeps the fire going, thus fuel moisture will be boiled off. But this fuel moisture is part of the fuel. At the same time as the added fuel is in flaming combustion, fuels from the previous loads are in the smoldering stage, thus providing a full combustion cycle.

changed text (page 4489, line 1): The deltaH2O/(deltaCO+deltaCO2) ratios of all measurements of the combustion sessions are combined in Table 1 as averages in the first column.

Part 2: My main criticism of the paper, assuming that my concerns about the data can be addressed, is that it mostly states the obvious basic chemistry and does not yield new insight into the impact of the water in the plume. Thus, I make some suggestions for the authors to expand their analysis in that respect.

1. The authors have CO and CO2 data, so they could check for a correlation between FM (if their FM data is OK) and CO/CO2.

We have calculated the fuel moisture content and are aware that these data are derived values. Afterwards we now have no possibility to verify them independently. A correlation will not appear as different processes, e.g., the distillation of the fuel moisture and the formation of water due to combustion run side by side.

2. The authors have carbon-flux data and may be able to determine if FM affects the

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carbon-flux rate and thus the heat release rate. (The heat of combustion for various wildland fuels has been measured: Susott, R.A., Characterization of the thermal properties of forest fuels by combustible gas analysis, Forest Sci., 28, 404-420, 1982.)

This would require more precise temperature measurements, especially in the light of the rate of biomass combusted. We assume that for the present data set with amounts of 10 to 30 g per minute the heat release values would be to noisy, as often were the ambient temperatures as well.

3. The effect of a change in water concentration on the first hour or so of chemistry in a smoke plume could be investigated with a box model.

4. The effect that FM might have on the formation of superfog could possibly be further investigated. Superfog is likely simpler to investigate than pyrocumulus clouds as it is a ground-level phenomenon. (and text to follow)

5. The potential impact of FM on pyrocumulus formation could be treated more rigorously. (and text to follow)

We believe that these suggestions would go beyond the intention of this paper. The intention is to show that moisture content from fresh biomass contributes distinctly to the water vapor in biomass burning emissions. The suggestions by the reviewer certainly should be followed up.

Part 3: Other specific comments in order of their location in this draft. Title/Short Title/throughout text: The term "biomass" is preferred to "biofuel" since the latter usually denotes biomass that is used as a household energy source.

We agree with the reviewer. As the word biofuel only refers to biomass burned for energy production. It would not include wildfires. But we really want to speak about any kind of biomass fire. So we will change the text accordingly.

Abstract Line 9 and throughout the text and tables: Normally 15-40% fuel moisture (on a dry weight basis) is considered the "moisture of ex-

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tinction" above which dead fuels will not sustain flaming combustion (http://www.wrh.noaa.gov/sew/fire/olm/fuelmoisture.htm. The extinction values are usually below 30 % (http://www.fs.fed.us/land/wfas/firepot/ fpipap.htm). However, in the authors work 33% was the lowest fuel moisture obtained. This causes me to suspect there is an error in their FM determination (most likely the fact that the fires were not "completed" as explained above). We find that fuels stored indoors typically have fuel moistures ranging from 5-10%. The author's speculate that the traditional method of estimating biomass fuel moisture (drying until constant weight) could cause large overestimates due to internal water that can only escape when the fuel structure is destroyed. This is not apriori impossible and it is required by the laws of thermodynamics that heating/drying biomass until weight is constant leaves some water on/in the biomass. However, experiments done here find good agreement between fire-integrated smoke water and weight-based FM determinations. Also, work done here drying fuels shows that the constant final weight persists for weeks using ovens, microwaves, grinding, etc. Thus, I suspect that the weight-based determinations may be accurate to within a few percent. Therefore, a suggestion to the contrary should ideally be supported by straightforward, simultaneous measurements made by both techniques. Finally, it is of course easy to burn fuels in the lab that are beyond the moisture of extinction if you use an aggressive ignition technique that dries out a large enough portion of the fuel.

In the first part the reviewer describes own experiments in which it seems that the "moisture of extinction" is exceeded. Here it may be useful to come back to the general approach, or intent, of the experiments described here. A possibility is to start a combustion session only if the fuel is directly ignitable and define it as completed on extinction. This is different from cutting a time slot from a combustion session to imitate a fire in its natural environment. We have chosen an approach to imitate a fire line. In this case a pre-drying of fuel may happen as a process running in parallel to the combustion.

We did not suggest large overestimates, we said that underestimates may occur (page

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4490, line 18-19).

The ignition technique has been described. Once the fuel is burning, for the emission measurements it is a self propagating fire.

Page (P) 4485, Line (L) 11-12 Here the author's simply state that dead fuels normally have a moisture up to 10% as if there is no controversy about the weight-based methos.

We make use of the literature, in this case of Chuvieco et al., 2004.

Page (P) 4486, Line (L) 3 The H:C/2 fixes the H2O/CO2 ratio from complete combustion so no calculation is needed. Also either here, or somewhere before here, the authors need to mention the concept of excess concentrations above background and define the "del" notation, which is introduced later in this paper without explanation. Then the word "excess" or the symbol for excess (capital greek letter delta) should go before the word "water" and also probably before both H2O and CO2. That is because the background water and carbon need to be subtracted to analyze the smoke data.

On page 4486, line 3 we gave a brief explanation what Ward (2001) had described when using the Byram (1959) reference. It has been better detailed by the reviewer in his part 1.

We agree that we should have introduced the 'del' notation. However, we introduce this on page 4488, line 8.

new text: ... were burned. We make use of the (delta)- notation to indicate that we subtract the respective ambient values from the measured ones in order to deal with the newly added chemical species alone.

P4486, L10-13 Too little detail, no specifics on why rejected or should not be neglected.

We did not want to go into too many details.

We change the text as follows: .. on theoretical grounds. They claim that sensible heat strongly dominates over latent heat, even for high fuel moistures, so that water vapor

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from the fire is not important for pyro-cumulus convection. In contrast, Clements et al. (2006) showed an experimental study from which he deduces a confirmation of Potter's (2005) argument that water vapor from a wildland or grass fire can significantly modify the dynamic environment of the lower atmosphere.

P 4486, L 13-14 There is no question here. Obviously if water is measured it tells you how much water is in smoke. I would just say you measured the water concentration in smoke.

The intention of this contribution is to point out that fuel moisture will be emitted with the combustion products.

P4486, L25 Should "container" be "chamber" for consistency?

That is correct. Thank you. The text change includes the response to reviewer 3.

new text(page 4486, line 24): It consists of a chamber for burning biomass (Lobert, 1989; Lobert et al., 1990 (Figure 1); Lobert et al., 1991 (Figure 36,1)) and a container for smoke dilution, mixing and aging, which are depicted in Figure x. The fires were sustained on a fuel bed housed in a facility open to ambient air (Fig. x, left). In the burning chamber (Fig. x, middle), an inverted stainless steel funnel funnel with a 1.2 m diameter opening was positioned 0.5 m above the fuel bed. The smoke was lifted up via this funnel into the steel sampling container (32 m3) (Fig. x, right) at a typical flow rate of about 63 dm3 s-1 (min-max: 53.3 - 68.3 dm3 s-1) provided by a fan at the end of the sampling line.

P4487, L4 Standard 20 foot container = ?; the units are not metric or volumetric.

The wording was chosen as being customary in every-day life. We will revise the text here to 'standard 20 foot shipping container'. The photos may help.

P4487, L5 Do the words "another one" indicate the fuel burning chamber? If so, maybe just say that? Maybe a diagram is needed as reviewer 3 mentions.

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The photos mentioned before may have clarified this in Figure x.

P4487, L7 How much moisture could have been lost from the fuel during the ignition process and how much could have been liberated from the walls early in each fire? For instance, Figure 1 shows a prominent water spike at the beginning. We never saw that. How was fuel ignited? We usually use a match (minimal influence), but did they perhaps use a propane torch (heat and carbon) and for how long? Or did they use an electrically heated igniter (heat only)?

The measurements used start after the fire is self-propagating. Then fresh fuel is added, which is basically a moving of fuel from the side into the combustion zone. So the ignition phase is discarded. We have no information on how much moisture could have been liberated from the walls.

P4487, L28-29 Was a door opened to reload fuel? How much fuel was reloaded? It seems like this would act as a heat sink and also cause a temporary dip in emissions production that I may see in the figures. Not sure this mimics real fires where heat radiated from the fire can pre-dry the fuel before the flames impinge on it.

The combustion facility is open to ambient, so there is no door opening which could boost or lessen fire intensity. Reloading, which basically is a moving of fresh fuel from the sides to the center of the combustion table where the fire is, in a way, is happening always, as the fuel never is combusted in an instant but is a process in time. We have difficulties in believing that pre-drying does not happen in nature.

P 4488, L 7 Here the excess notation is used without prior definition as noted earlier.

This is correct and already changed.

P4488, L11-27 These are somewhat non traditional lab fire trajectories compared e.g. to published work by us or Lobert et al. In light of my earlier comments about these fires, I think the authors may need to re-think their interpretation - at least regarding fuel moisture determination.

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The basis of our data evaluation is as detailed by the reviewer in his part 1. As mentioned above, we tried to emulate a continuous fire, with all phases present simultaneously.

P4489, L1-2 "combined" should be "presented"?

We present our data by combining them into a table.

P4489, L9-10A value larger than 0.75 (not 1.0) suggests a contribution of FM and is expected since a perfectly dry fuel is impossible.

We have stated earlier that 0.75 would be the threshold (page 4486, line 3, and do so again on page 4490, line 12). So if we have used the term unity here, it was to point out that fuel moisture distinctly contributes.

P4489, L15 The words "must be" should be changed to reflect that this is an estimate.

We do not believe that a change is necessary here. We give a statement, not an estimate.

P4490, L3 Yes live fuels have higher moisture. The authors should quote some work on the relative consumption of live and dead fuels to strengthen their case.

We have reported in the introduction about fuel moisture contents. The intention of this contribution is to point out, that fuel moisture will be emitted with the combustion products. Our results can not be used to interpret the relative consumption of live and dead fuels.

P4490, L5 The authors seem to misunderstand the nature of sugar cane fires. The farmers do not burn the live sugar cane plants, which would be a disaster for them. The fires are set to remove accumulated dead biomass, weeds, and animal and insect pests before cutting the live cane. After exposure to the high temperatures of the fire, the live plants deteriorate rapidly and must be pressed at the mill within 24 hours. For this reason, the burns are usually scheduled in early morning to allow time for subse-

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quent harvest and loading/transport during daylight. Additional reasons to burn in the morning are that the lower wind speeds reduce both the chances of the fire escaping the desired perimeter and the smoke impacts on local populations. It follows that burning in the morning with lower wind speeds and a lower boundary layer increases the chances of the plume penetrating to the free troposphere and forming a pyrocumulus cloud. Finally, the ambient humidity is typically higher in AM.

We do not believe that a discussion is necessary here. We had the experience of sugar cane burns in mind, which are cited in Andreae et al. (1996). Here we know that the debris of harvesting of the day, i.e., leaves etc., was burnt the same afternoon. We are aware of 'household fires'to remove accumulated dead biomass, weeds, and animal and insect pests before harvesting. Sugar cane was mentioned here much more to make readers aware of the wide range of potential fuel moisture contents.

P4490, L15 A little confusing because all combustion would disrupt the structure of biomass.

We suggest that only after the structure of the biomass is disrupted water from the vacuoles can be set free completely.

P4490, L17-19 It seems like some simple experiments to test this should be feasible to add to this work?

We have stated in our text (page 4486, line 15) that we report on a re-analyzed data set.

P4491, L14-17 The author's fuel moisture values may not be accurate as mentioned above. In any case, the value of 40% should be investigated by using a range of live and dead moistures and some estimate of the relative proportion of these fuel types that burn. E.G. If the dead fuels were at 10% and the live fuels at 200% and a crown fire consumed about equal amounts of each then Trentmann's estimate of 40% might be low. But if the live fuels were at 80% then it would be very close. So at this point,

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the authors should probe this question with some real literature values rather than just speculating.

The point of this brief contribution is to show that fuel moisture data can be obtained with an independent method. This independent method has it's own merits, as stated in part 1 of the review, and own constraints, certainly. It may help, however, to improve the current understanding how much water vapor may be present in a freshly developing plume. A listing of some real literature values would be appreciated.

P4491, L21-23 We (and probably others) have massive amounts of smoke water data, but as mentioned above, the interpretation is not simple. Long before the plume has evolved to the point where a cloud might form it has diluted enough so that the water enhancements in the boundary layer plume are small compared to the water variability in the background air.

We would welcome any publication of such data. They may help constrain input data for modeling approaches. There is no doubt on water vapor enhancement by advection in a rising plume. But this does not contradict the usefulness of starting with water vapor input data that are as correct as possible.

Table 1 It is interesting that the fuel with the highest FM "pine with green" also had one of the lowest fuel moistures in another run. Also the "dry run" of "pine with green" had a higher minimum H2O/C ratio than the "wet" run of the same fuel.

We have reported our data without filtering. The variability shown was intended to give the reader an opportunity to assess the variability we found.

Summary. The authors need to rigorously determine which results are valid and then add some detailed, new scientific analysis to make this paper useful. Stated another way, a few key issues in point ... response format are:

1. Smoke has water from evaporated fuel moisture in it ... that's obvious. 2. The smoke water content was measured ... good, but more needs to be said about how

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representative the fuels and measurements were. 3. The smoke water could have importance ... yes, but shed some new, quantitative light on this to make the paper a valuable addition to the literature.

We agree that smoke from biomass combustion may have water from evaporated fuel moisture in it. Unfortunately, data virtually never have been published. The fuels spruce, pine and oak are typical for Europe and certainly elsewhere in the northern hemisphere. They may be consumed in confined as well as in wild fires. The same will be true for musasa and savanna grass for the southern hemisphere.

(Figure X is with the editor and can be requested, of course).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4483, 2008.