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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Emission factors for PM_{10} and PAHs from illegal burning of different types of municipal waste in households" *by* András Hoffer et al.

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The authors are thankful to the referees for their detailed reviews and helpful comments. Our responses are summarized below.

Response to Anonymous Referee #1

General Comments.

1. Abstract: About 75% of the abstract is background (lines 15-23); the presentation of this work would be more impactful if more of the results were included in the abstract. I recommend more inclusion of the PAH and PM10 emission factors.

We agree with the reviewer that emission factors should be included in the abstract; upon revision, we will add the following paragraph to the abstract:

It was found that the PM10 emission factors from the combustion of wood-based waste samples were about twice that of firewood, whereas EFs in the range of 11–82 mg g−1 were obtained for different types of plastic waste. The latter were also found to emit exceptionally high amounts of PAHs, by a factor of 50–750 more than upon the combustion of dry firewood under the same conditions. Since the more toxic 4–6 ring PAHs were predominant in the particulate emission from plastic waste burning, BaP equivalent toxicity was up to 4100 times higher than that from wood combustion.

2. 57 and results: At first this statement seemed overly broad to me, but upon my own search of the literature I was unable to find many articles looking at emission factors for specific plastic wastes. I still believe the manuscript would be improved if the authors provided an additional discussion of the emissions of PM and PAH from waste mixtures. Or even emissions of other hazardous components from these wastes. These might provide additional perspective into the relative importance of the emissions of the tested materials. In the results section, discussion of the EFs you observe relative to other wastes would also be interesting. Specifically, for the PAH results. Are there any EFs which exist for PAHs for municipal/household waste mixtures? Could we say anything about which materials might be the most important contributors to total PAH or toxicity?

Besides PAHs various gaseous and particle-phase compounds are emitted into the atmosphere from the combustion of municipal waste. Even if their determination is outside the scope of the manuscript, these species will be mentioned in the revised manuscript with references. The observed EFs relative to other wastes as well as the relative toxicity of the PAHs will also be discussed in the results section.

3. How the blanks/standards are treated is very important for accessing the accuracy of results in filter-based studies. I think this manuscript would benefit from additional

detail regarding the methods. I've listed a few specific comments around this issue below. 137: What type of blanks are these? Are they just conditioned and unused filters? Or, are they a type of background measurement during times when the coal was being burned?

The blanks represent background measurements during which only charcoal was burned. The sampling times of the blanks were comparable with those of the samples. For each waste type one blank sample was collected with combined air supply settings.

152: Is the PAH analytical method based on one found in literature or one that the authors developed? If it is from the literature, please include a citation. If it was developed by the authors, please include additional method validation data in the SI. 164: What about recovery from the filter? There is a relatively high potential for matrix effects in complex samples like those from combustion. The best way to account for these and get at the "true" recovery would be to spike a subset of filters (with particulate samples on them) with known concentrations of the target PAHs and examine the recovery including both filter extraction and any matrix effects. As performed, the QA/QC approach cannot account for these. If you are unable to perform these tests now, please provide a discussion of the actual recoveries that other studies have observed for similar filter extraction techniques and how these recoveries may impact your conclusions.

The PAH analytical method is the adaptation of the standard method EN ISO 17993:2003. Concerning recovery the sample preparation can be divided into 2 parts:

1. Solvent extraction of PAHs from the aerosol particles collected on the filter. This step can be characterized by the extraction efficiency. 2. Once PAHs are dissolved in the extract purification and concentration of the extract will further influence the recovery of the target compounds.

The recovery of the second part can be determined by dissolving standard solution of PAHs in the extraction solvent (n-hexane in this case) and performing the extract purification and concentration steps. This has been performed and the recovery of the individual PAHs from the extract was determined accordingly. The recovery of the first part i.e. extraction efficiency can be determined by two approaches:

a) First the amount of PAHs is determined in a subset of a sample then another subset of the same sample is spiked with known amount of PAHs and then analysed again. From the difference of the two results the extraction efficiency can be determined. However, this approach involves the assumption that PAHs originally present in the sample and those added to the sample can be extracted with the same extraction efficiency which cannot be guaranteed.

b) The extraction efficiency can be determined without need for the above assumption by performing consecutive extraction and quantifying PAHs in the extracts as long as they can be detected. By increasing the solvent volume the number of consecutive extraction steps necessary to achieve nearly quantitative extraction can be decreased. We applied this approach and analysed PAHs in two consecutive extracts. In the second extract the amount of PAHs was only 1–3% of that present in the first extract. These results evidenced that under the experimental conditions applied (solvent to filter ratio, time of extraction) 97–99% of PAHs were extracted in the first step. Therefore, the extraction was not continued and the sum of the amount of PAHs in the two extract was considered as 100%. Because of the high efficiency of the first extraction step the samples were extracted only once and the results were corrected with the extraction efficiency (97%–99%). The revised manuscript will be complemented with a more detailed description of the sample preparation.

4. The EF data that the authors use is drawn from a diluted gas stream. This methodology makes sense given the high particle concentrations associated with combustion. That said, the authors should add additional discussion of how such a sampling method may affect their results. How do the authors think the cooling associated with dilution may affect the particulate mass and composition of the emitted particles? Is it reasonable to expect that this rate and magnitude of rapid cooling would also happen in the

atmosphere? This process will affect gas to particle partitioning; do the authors believe this is important for the levels of PAHs or PM they observed?

The physical processes took place during the applied sampling procedure were very similar to those occurring after the emission of smoke from the chimney into open air as in both cases the smoke cools and dilutes rapidly. The ambient temperature affects the gas-to-particle partitioning of the compounds especially those of the semi-volatile ones. During the sampling the ambient temperature varied between 16.1 °C and 26.0 °C, on average it was 20.3 °C, which is higher than typical ambient temperatures during the heating season. It implies that some semi-volatile species might be lost to the gas phase upon sampling, thus measured EFs for PM10 and total PAHs are likely lowerbound estimates for real-life conditions. It should be added that the standard procedure of the gravimetric PM10 measurements include the conditioning of the exposed filters for 48 hours at 20°C (and RH=50%) which might also result in some loss of semi-volatile compounds. Concerning PAHs, at the sampling temperatures some of the more volatile 2- and 3-ring compounds might have been lost to the gas phase as compared to the case at ambient temperatures in winter, and were likely to some extent underrepresented in the filter samples. These potential losses, however, do not influence the conclusions regarding PAH relative toxicity due to the very low toxicity of the 2-3-ring compounds.

5. The Authors introduce abbreviations in section 2.1, but do not consistently use them throughout the text. For clarity, I recommend using them any time material is referenced after the abbreviation is introduced.

The abbreviations will be updated in revised manuscript.

6. The discussion of PAH's in the manuscript is limited to bulk EFs and estimated toxicity. One interesting finding that could be extracted from these data is the EFs of individual PAHs for specific substances. Although a complete discussion of these data may be beyond the author's scope, at least a cursory discussion of any patterns

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observed in PAH concentrations would be very interesting. For example, are there any specific PAHs that could be used as a tentative tracer of the burning of a specific waste? I also recommend adding a table of the emission factors of each PAH for each material, either in the supplement or main text. Such a table would add value to this text for the readers. This would also increase the usability of the data to those who are interested in applying it in future emissions studies.

A table (S2) listing the EFs for each PAH and waste type will be added to revised the manuscript with a brief discussion of the results.

7. Results Section: I recommend adding SD values to any reported EF throughout the text. The only reason not to add these would be if there are too few measurements or that conditions were so different between tests that the experiments are essentially measuring different endpoints. If the latter, then these differences should be discussed in detail. Additionally, there are many points in the results the authors state that their results are similar to those in literature when at first look the values are quite different (see specific comments below). This manuscript could be improved by further delving into the sources of these differences.

The SD values are calculated and will be shown with the measured EF values. The large SD values indicate the effects of the different combustion parameters.

Specific Comments

8. Introduction: As written the introduction is all one paragraph. I recommend separating this section into at least 3 paragraphs to help with readability.

In the revised manuscript the introduction will be split into 3 paragraphs.

47: Be sure to spell out abbreviations the first time they are introduced (OSB here) throughout the text.

The abbreviations will be updated in the revised manuscript.

49-51: This line repeats much of what is said in line 47.

The sentence will be deleted.

51-52: This statement should be supported by references since it is essential to the motivation for this work.

References will be added in the revised manuscript.

Burning any type of waste poses excessive risks to the health of people living in those areas since a plethora of toxic, carcinogenic or mutagenic compounds are emitted in immense quantities compared to the burning of authorised solid fuels such as dry fuel wood or high-quality coal (Lemieux et al., 2004, Estrellan and Lino 2010, Gullett et al., 2010, Wiedinmyer et al., 2014).

Estrellan, C. R., and Iino, F.: Toxic emissions from open burning, Chemosphere, 80, 193–207, 10.1016/j.chemosphere.2010.03.057, 2010.

Gullett, B. K., Wyrzykowska, B., Grandesso, E., Touati, A., Tabor, D. G., and Ochoa, G. S.: PCDD/F, PBDD/F, and PBDE Emissions from Open Burning of a Residential Waste Dump, Environmental Science & Technology, 44, 394–399, 10.1021/es902676w, 2010.

Lemieux, P. M., Lutes, C. C., and Santoianni, D. A.: Emissions of organic air toxics from open burning: a comprehensive review, Progress in Energy and Combustion Science, 30, 1–32, 10.1016/j.pecs.2003.08.001, 2004.

Wiedinmyer, C., Yokelson, R. J., and Gullett, B. K.: Global Emissions of Trace Gases, Particulate Matter, and Hazardous Air Pollutants from Open Burning of Domestic Waste, Environmental Science & Technology, 48, 9523–9530, 10.1021/es502250z, 2014.

53-55: Please clarify what specified means in this context. Do you mean that these emissions are not included in emissions inventories? If so, please provide examples and/or references.

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Yes, the emissions from waste burning are not included in many current inventories (Wiedinmyer et al., 2014).

55: Do these reports from NGOs provide any estimations of magnitude? If so, it would be interesting to see them reported here.

The reports from the NGOs focused on the frequency of the waste burning and/or the type of the burned waste, the magnitude of the waste burning is not known.

53-55: This line is slightly contradictory since it states that emissions are not regulated but are illegal. I would interpret “illegal” as a form of regulation. Do you mean there’s no enforcement?

The reviewer is right, the original sentence was misleading. In the revised manuscript it will be rephrased for the sake of clarity. Yes, in Hungary basically there is no enforcement against this illegal activity.

64,65: Add citation for PAH carcinogenic and mutagenic properties.

In the revised menuscript citations will be added for PAH carcinogenic and mutagenic properties.

68-69: Please clarify what you mean here by a single study. Do you mean a single study reporting EFs of PM2.5 and PAHs or a single study with plastic waste in an industrial boiler? What about emissions factors from bulk household waste combustion?

We meant that there is only one study dealing with the emission factor of particulate mass and that of the PAHs obtained for the co-combustion of specific wastes (PE and PET) mixed with wood. In the revised manuscript the text will be modified accordingly.

74-102: Some abbreviations are introduced twice here. I would remove the first instance (starting at line 74) or use the abbreviations after they are introduced.

The abbreviations will be revised and updated.

109: As written this sentence is slightly confusing. I believe the authors are trying to communicate that burn conditions were variable, but later they report relatively consistent burning temperatures.

Temperature did change by changing the air supply of the stove, but it was largely independent from the type of waste combusted in the stove at any given air supply ratio.

140: Which parameters?

In the revised manuscript the sentence will be modified to ‘the RH and temperature data were recorded.’

186: For clarity, I recommend using the abbreviations you introduced in your methods throughout the results section. Also, include the EFs for RAG here since they have been included for the other materials.

In the revised manuscript the EF of RAG will be added to the text. The abbreviations will be revised and updated.

186-187: If they exist, also report the standard deviation of all measured EF values.

In the revised manuscript the SD values are reported.

190: Please explain how these values are similar. As written, it looks like the values reported here are 2x the values found in the literature.

The sentences will be modified: The only available EFs reported for PET co-combustion in a boiler are between 1.8 ± 0.3 and 5.8 ± 0.5 mg $g-1$ depending on the power output (Tomsej et al., 2018). The differences between our and the reported EF values can be explained by the vastly different burning conditions (e.g. different combustion units, and no fuel wood was co-fired in our experiments as against in those reported by Tomsej et al.). Here we note that if we estimate the EF of the PET from the data reported by Tomsej et al. (2018) by taking into account the mass ratio of the burned materials (wood to plastic ratio was 93:7) and assuming linear combination of the EFs (which might be loaded with large uncertainties), the EF derived for combustion of 'pure' PET turns out to be identical (11 mg $g-1$) to that obtained in the present study.

208: Capitalize RAG

It will be changed in the revised manuscript.

212: Your values do not seem similar to the literature values as you stated. Please comment on the differences.

Since the EFs depend on several parameters and refer to the specific experimental conditions, we believe that even a factor of 2 difference can be interpreted as a relatively good agreement. The relatively high SD values obtained in our study for a given plastic type indicate the effects of some key combustion parameters on the EFs. Nevertheless, the term 'similar' will be removed from the text and the comparison will be detailed more.

219-220: I think these are referring to the emissions relative to wood. Using the word "increased" here is confusing because it makes it sound like something is causing the change. I recommend modifying this sentence for clarity.

This sentence will be modified as requested.

227-232: add SDs to the reported PAH emission factors.

SDs will be added to PAH EFs.

246: This is a very interesting result, I recommend highlighting it in the abstract.

In the revised manuscript the relative toxicity of PAHs will be highlighted in the abstract.

Conclusion: Again, add SDs to all reported EFs.

SDs will be added to PAH EFs.

Figure 2: Clarify which axis is for which CO2 measure. Although this can be deduced by the reader, it should be explicitly stated. Additionally, it would be interesting the highlight the filtering times on this figure.

Figure 2 will be modified to clarify which axis is for which CO2 measurement.

Tables 1-3: It would not be too difficult, and would increase the clarity of the presentation, to combine all three of these tables into one. This would give the reader a single point where they can access all the results of the study as opposed to the current presentation where they need to jump between 3 different tables.

In the revised manuscript Tables 1-3 will be merged into a single Table for better readability.

Response to Anonymous Referee #2

Major comments:

In general, I find that there is insufficient context to the discussion. The manuscript still leaves open the question of importance, which I am sure any reader would ask. The only quantitative evidence I can find in the manuscript is a survey of Hungary, which found that 2-10% of participants are engaged in some sort of household waste combustion activity. So how much is there? All the results here are reported in emission factor ("emission" per "mass burned"), but there are virtually no data on "mass burned". Even an order of magnitude or a relative estimate would be useful. For example, for an average household who burn household waste, what is the ratio of mass of wood burned to mass of plastic burned?

The primary focus of the manuscript was to determine the emission factors of PM10 and PAHs from the burning of different household wastes in stoves. An assessment of the relative contribution of waste burning to air pollution in any region was outside the scope of the manuscript. The polls conducted in Hungary focused on the time frequency of waste burning as well as on the relative fraction of the population who admittedly burn wastes at their homes, but there was not any single question about the amount of waste burned. Since waste burning is an illegal activity but law enforcement is very rare, there are no estimates for the potential amount of waste burned in households in any regions. Using a tracer methodology, we plan to address this issue in another paper in the future.

I am also curious why plastic materials yield much higher PM and PAHs than wood materials. Is the composition of the material itself, or the mechanism of combustion? Both are organic polymers, but the size of the macromolecules may be different. Wood is generally made up of carbon, hydrogen and oxygen, where in plastics and rubber, nitrogen and chlorine are also present. The PM emissions seem to be somewhat linearly additive, as evidenced by the emission factors of RAG (a mixture of PET and wood) to be in between PET and wood.

We found higher PM10 and PAH emission factors for those plastic materials which contain styrene units in the polymer chain (PS and ABS, which are made up of carbon, hydrogen and oxygen). This result indicates that the chemical composition affect the magnitude of the PAH formation and the PM10 emission factors. Beside the chemical composition the burning parameters, i.e. the mechanism of combustion may also affect the emission factors and the formation rates of PAHs. Indeed the emission factor of the RAG is between those of its constituents, but this might not be universally valid.

This manuscript also makes no mention of all the other toxic compounds that are known to be emitted from plastic burning. Two of the most studied ones are dioxins and furans. There may also be a lot of metals involved, such as from tyre materials. That is an important body of knowledge that is highly relevant to this discussion.

Besides PAHs various gaseous and particle-phase compounds are emitted into the atmosphere from the combustion of municipal waste. Even if their determination is outside the scope of the manuscript, these species will be mentioned in the revised manuscript with references. (See also the response to Referee#1)

I would also note that the introduction, results and discussions are focused on the "illegal" burning of waste, but I am not sure why this aspect is important. What is it about "illegal" burning that needs special treatment? (It is even in the title.) To me, this is just combustion of different materials, and some materials yield much more PAHs and PM than others. Are "illegal" combustion conditions any different? If so, it might be useful to highlight. This issue is not just one of semantics – it is important for understanding the context of these results too. These emission factors may also apply to "legal" waste combustion, such as incineration facilities. For example, the high emission factors from plastics may provide additional incentives to separate them from other waste prior to incineration. Another potential area of concern is the increasing prevalence of wildland-urban interface fires. More manmade materials and household items would be burned in these fires, and these results have potential implications on the PM emissions and toxicity. My opinion is that the manuscript in its current form does not draw any connections to many other relevant areas.

In our opinion, illegal and legal burning of waste should be treated separately. The legal waste burning includes waste combustion in municipal solid waste incinerators and/or in power plants where the emission of the pollutants and the burning processes are strictly controlled and regulated. This means that the emission factors obtained in the present study cannot be applied for waste incineration plants, as for them much lower emission factors are expected due to the more efficient combustion conditions and the use of high-efficiency scrubbers, electrostatic precipitators, and filters. In contrast, illegal waste burnings are performed household stoves (or in open air) without any emission control to reduce the emissions. In this study we determined the EFs from waste burning in a commercial household stove under different and potentially relevant burning conditions. Burning of municipal waste in residential stoves is strictly prohibited in most countries that is why the word "illegal" is in the title of the manuscript.

There are also some major questions about the methods:

First is that based on the pictures in Figure 1, the materials appear to be prepared such that they are all roughly the same size. How does the material preparation affect the emissions? Would large pieces combust differently and emit different amounts of pollutants?

The size of the specimens of any given waste type was determined in preliminary test burns with the objective that similar filter loads be obtained in each burning test. We agree with the referee that large differences in the size of the specimens might affect the burning mechanism and thus the measured emission factors. In our case relatively good agreement was found with available EF data in the literature (in the case of wood and PET) implying that the method may be acceptable.

Second, how are the emissions normalized to calculate EFs? What is the mass for normalization? Is it the mass of materials put into the stove, or the mass loss from combustion?

The EFs were calculated using the weight of the waste specimens put into the stove. Here we note that the ash content of the plastics reported by Zevenhoven et al. (1997) (LDPE, HDPE, PP, PVC, PS) is below 3%.

Zevenhoven, R., Karlsson, M., Hupa, M., and Frankenhaeuser, M.: Combustion and gasification properties of plastics particles, Journal of the Air & Waste Management Association, 47, 861-870, 10.1080/10473289.1997.10464461, 1997.

Third, what is done about positive and negative artifacts for sampling semivolailte organic compounds and PAHs on quartz filters?

The temperature of sampling does influence the partitioning of PAHs and other semi-volatile compounds between gas and particulate phase as a consequence of the temperature dependency of the vapour pressure of these compounds. See also our answer to Referee #1. We have not applied any method (e.g. dual filter trains, denuders etc.) to correct for potential sampling artifacts. Although this may add to some uncertainties to our results, especially with regard to the most volatile 2- and 3-ring PAHs, the main conclusions of our manuscript remain valid and robust.

How are the experiments repeated? I see there are uncertainty bars (and S.D.) reported in the figures. Are they replicate samples, or samples from different burns?

Each type of waste was burned in separate test burns under different combustion conditions (see Figure S1 in the Supplement). SD values were calculated from the different test burns for each waste type.

Other comments

Title: the choice of the word “illegal” is questionable here. At first I thought this was a field study, sampling actual illegal combustion in households. Rather this work is done in a highly controlled environment. Therefore I find the title misleading and inaccurate.

See also our corresponding answer above. The reviewer is partly right, we are not after illegal waste combustion in settlements. However, we did mimic illegal solid waste combustion in households in controlled laboratory experiments, therefore we believe that the use of the word “illegal” in the title can be justified. An additional argument is that by using our results the magnitude of the problem of illegal waste burning in settlements can be assessed knowing the amount of waste burned under such conditions.

Abstract: The abstract should mention more of the results and significance of this work. In this work, the major finding is that EF of PM10 and PAHs for plastics are much higher than expected, and toxicity equivalent factor is much higher. There are also some unsubstantiated claims about waste burning (“unprecedented health hazards”) that should be moderated.

In the revised manuscript more results will be added to the abstract (see the answer to Referee#1). We agree with the referee that the word “unprecedented” is unsubstantiated. It will be replaced with “significant”.

Abstract: the synonym EF is defined here but not used again in the abstract.

In the revised manuscript it will be corrected.

Introduction: There are instances where the background discussions reflect a Euro-centric bias and invoke negative stereotypes. For example, the first sentence mentions “In developing countries, more than three billion people use solid fuels. . .”. The fact that developing countries are burning solid fuels is irrelevant to this work and mentioning “developing countries” is unnecessary, since this work seems to focus on illegal waste burning in European countries. Futhermore in a later paragraph, “. . .the illegal burning of municipal solid waste in households in several countries of the world, even in Europe”. The way this sentence is constructed reflects an assumption that illegal activities are not expected in Europe and perpetuates a stereotype. I urge the authors to use more inclusive language and consider more diverse perspectives in their writing.

We agree with the referee and will rephrase the objected section accordingly.

Page 2, lines 58-59: these papers should be cited, since they represent the state of knowledge on this topic.

The following references will be added to the revised manuscript.

Gu, Z. P., Feng, J. L., Han, W. L., Wu, M. H., Fu, J. M., and Sheng, G. Y.: Characteristics of organic matter in PM2.5 from an e-waste dismantling area in Taizhou, China, Chemosphere, 80, 800−806, 10.1016/j.chemosphere.2010.04.078, 2010.

Kumar, S., Aggarwal, S. G., Gupta, P. K., and Kawamura, K.: Investigation of the tracers for plastic-enriched waste burning aerosols, Atmospheric Environment, 108, 49−58, 10.1016/j.atmosenv.2015.02.066, 2015.

Simoneit, B. R. T., Medeiros, P. M., and Didyk, B. M.: Combustion products of plastics as indicators for refuse burning in the atmosphere, Environmental Science & Technology, 39, 6961−6970, 10.1021/es050767x, 2005.

Page 2, lines 65: again, references for the toxicity of these compounds should be cited.

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References will be added to the text.

Page 3, line 109: what does high/low air supply ratio mean? This is qualitative and not really explained.

The stove used was a commercially available model which allowed setting the air supply entering the combustion chamber of the stove through an adjustable slit. There were two endpoints of this slit, thus either larger (high air supply ratio) or smaller volume of fresh air (low air supply ratio) was allowed into the combustion zone.

Page 3, line 119: how much of the CO2 comes from the background smouldering of coal?

As Figure 2 illustrates the CO2 concentration of the background case (between peaks when no waste specimen was being burned) is a few per cent (between about 4% and 5%).

Page 5, line 190: what does significantly higher nominal power mean? Why is that relevant to emission factor? It might be more direct to say how the combustion conditions differ. Also, I believe “though” should be “through”, but I am not sure.

Indeed, it is difference between the combustion conditions that matters. The sentence will be modified accordingly.

Page 6, line 196: tyres contain a lot of metals and their emissions are potentially toxic too. This paper examines PM and PAHs, but metals and organic compounds could be redox active and cause oxidative stress. This may be a good topic for discussion in the conclusions section.

We agree with the referee, but given that the topic was outside the scope of our study and no relevant measurements were carried out, we did not discuss it in the conclusion section.

Page 6, Line 200: ABS is very interesting. Since acetonitrile is involved, there may potentially be nitrogen-containing compounds in the PM as well. Again, there is lack of discussion of why the PM emission factors (and PAH toxicity equivalents) are so different, and no attempts to examine the link between PM/PAH and the fuel itself.

The referee indeed raised very challenging questions. As this is a pioneering study and rather limited in its extent (limited number of test burns, burning conditions, etc.), it is not possible to answer to any of these questions in a responsible way from the results. Further dedicated studies are clearly warranted to give scientifically sound explanations. We just hypothesize that the propagation of flame and heat inside the plastic sample specimens might be responsible for the observed high EFs.

Page 6, Line 208: lower case “rag” was used here, but upper case “RAG” was used earlier.

It will be corrected.

Page 6, line 232: What does “reactive share of PAHs” mean? It is not clear what reactivity the authors are referring to.

It is a typo “Reactive share” should be “relative share”.

Page 7, lines 236-261: I wonder if the difference has to do with volatility. What do the volatility distribution of PAH look like between the samples? Is it because in general plastics produce “heavier” PAHs, and the other samples generate “lighter” PAHs? (I understand the toxicity discussion and it is very important, but I am looking for some more information that would tell us how these PAHs are generated.)

This issue is closely connected to the question raised above (Page 6, Line 200). See our answers there. Combustion conditions inside plastic specimens should be unique and seem to produce markedly different volatility distributions of PAHs. But further studies are clearly warranted.

Conclusions: I don’t really see the point of repeating all the numbers if they are already shown in the table and in the figures.

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In the conclusion just a few key (pooled) numbers are emphasized.

Figure 2: what does O2 rise and CO2 drop at some times? Is this when the combustion chamber is open to place the sample?

The peak in the CO2 concentration represents the combustion of a small specimen of a given waste. During the sampling several waste specimens were combusted successively. Indeed, the concentration of O2 increased when a specimen was placed into the combustion chamber.

I don't really see why it is necessary to have both a table and a figure showing essentially the same data. (Table 1 and Figure 3, Table 2 and Figure 4)

The figures illustrate and compare the magnitude of the relative (to wood) emission factors of the different waste types, whereas the tables list absolute emission factors (per unit mass burned) which may be directly used in compiling emission inventories and air quality modeling.

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