Response to Comment by James Roberts

I thank James Roberts for his positive and interesting comments. My responses are detailed below. (Roberts's comments in italics).

The Author is to be commended for taking on the task of assembling, digesting, and tabulating reported biomass burning emission factors (EFs). However, the author's large-scale dismissal of laboratory studies that present EFs seems limiting and unjustified. The reasoning of the author seems to be based mostly on one completely unrealistic study, but many other studies have shown how to harmonize the lab studies with field studies including Yokelson et al., (2013), Stockwell et al., (2015), and Selimovic et al., (2018). While some of these studies have been used, the methodology there-in could have been applied to many other recent state-of-the-art studies. Instead the fuel-specific info for a wealth of important compounds not measured in the field is lumped into a large "lab average category."

The decision to rely (with few exceptions) on field studies was made after careful consideration and comparison of lots of field and lab data. The "unrealistic" study that Roberts refers to is but one extreme example. There are just so many variables that are important in field fires, which cannot be reproduced in the lab, e.g., fuel moisture, air flow, spread rate, etc., but which influence emissions. In addition to the papers mentioned by Roberts, the studies by Christian et al. (2003) and Burling et al. (2011) analyze the differences between field and lab measurements. Including lab studies would have required me to make many, more or less arbitrary, decisions about which lab studies are "realistic" and which not, and what kind of corrections to apply to other scientists' data. The required correction factors are often large (see Yokelson et al., 2013) and for most studies, the information required to make such corrections would not have been available.

Over reliance on field data mean that fast chemistry may not be captured properly, e.g. HONO, and perhaps other fast actors such as 2,3-butanedione, especially for large fires that could not be sampled close-up.

Indeed, this is a shortcoming, which is severe for some fast reacting species. For other species, which are very prone to surface uptake, e.g., HCl, open-path lab studies may be the only way to obtain realistic emission measurements (Burling et al., 2011). One can hope that improved measurement techniques may provide in the future the possibility to measure these substances close to sources in the field. However, in this author's judgement, the alternative of making more or less arbitrary decisions about which lab data to blend in for which species, and how to assign them to the combustion categories used in the paper, would not provide enough benefits to justify the loss of a clear criterion for data selection. To give readers the option to use lab data for specific purposes, I have included all lab studies that I am aware of in the Supplement. This will help them find the original papers and evaluate them before using these values for their specific purposes.

In addition, this arbitrary decision has resulted in the Author missing (omitting) several new developments in BB emissions measurements.

I object to the use of "arbitrary" here. The decision to base this study on field studies and to include lab measurements only in the form of a general column in Table 1, but with full documentation in the supplement, was taken after careful study of the literature and deliberation of the alternatives. I am very familiar with the studies mentioned by Roberts, and will discuss them below.

The work of Sekimoto et al. 2018 that found that most of the variability in VOC emissions was explained by just two factors, related to low and high temperature pyrolysis, and that are valid for a variety of fuels, was not mentioned.

This study is based on lab measurements and does not report explicit emissions data, and is thus not included in the data base for Table 1. However, I agree with Roberts that it provides a novel and interesting approach to generalizing emissions data and have now included a sentence referencing it in Section 3.1: "An interesting and novel approach to generalizing VOC emissions is provided by Sekimoto et al. (2018), who showed that most of the variability in VOC emissions measured in a lab study using a wide variety of fuels was explained by just two factors, related to low and high temperature pyrolysis."

No mention was made of the importance of isocyanic acid (HNCO), an N-compound of emerging health interest (Roberts et al., 2011) and which Koss et al., (2018) have shown to often be more abundant than HCN in laboratory fires. These new features/results do not yet have field measurements of EFs to back them up, but soon will.

Regrettably, future measurement cannot be included. I am aware of the potential importance of HNCO, but have not found enough field-relevant data to justify inclusion at this time. But, as stated in the conclusions, I am continuously updating the data base and Table 1, and would be happy to include this species once enough data are provided. I encourage Roberts and other interested readers to supply me with relevant data on this and other substances in the future.

Strangely, 25+ year old laboratory results from the Mainz group were included in several cases in the main table (Table 1), while important new results were overly consolidated and relegated to the SI spreadsheet (e.g. the I/SVOC work of Hatch et al., 2015), or not listed at all (HNCO; Hatch et al., 2017; 2018).

I don't quite understand this comment. For one, Table 1 does not contain primary data from specific studies, but only aggregated data. Then, the fact that data are 25+ years old does not qualify them for exclusion from the data base.

The Hatch et al. (2015) paper is an excellent study, but based on lab data only, and was therefore included in the lab studies section of the supplement. The Hatch et al. (2018) paper is a methods intercomparison and synthesis of data that had already been published in previous studies, therefore it had not been included. I made efforts not to include the same data published twice or more in the database used for the specific combustion types in Table 1. However, I have now included it in the lab studies section. The Hatch et al. (2018) study on I/SVOC emissions, while very interesting, did not contain species that I had considered for inclusion in Table 1. In view of the potential importance of these compounds, I have added a sentence and reference in the Conclusions: “Another example are the emissions of semivolatile and intermediate-volatile compounds (I/SVOCs), which are important in the context of organic aerosol from biomass burning, but for which at this time only laboratory measurements are available (Hatch et al., 2018).”