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Interactive comment

Interactive comment on "Emission of trace gases and aerosols from biomass burning – An updated assessment" *by* Meinrat O. Andreae

Anonymous Referee #1

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This paper summarizes decades of fire emission factor measurements into a table with recommended emission factors for dozens of species. This is a worthy addition to the literature following earlier meta studies by the lead author and by Akagi, Yokelson, and co-authors. A quick look at the reference list also shows that this a testimony to the enormous contribution of dr. Andreae to this research field. I would recommend publication of the paper, but it would benefit from a longer discussion about differences with other meta analyses and recommendations for future research. In addition, I feel the uncertainty in global emission estimates is artificially amplified, please see below

1-21: Fires are obviously a source of CO2, but it would be good to add a statement on whether fires are a net source of CO2 to avoid confusion

2-10: The Johnston et al paper estimated 339,000 annual premature deaths, the num-

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Discussion paper

ber mentioned here is an interquartile range.

2-22: Please specify the units, C or DM? Also, a range of estimates is not necessarily the same as uncertainty, please see final point below

2-34: To some degree this differentiation was also done by Akagi et al. (2011), would be good to credit them

3-19: I applaud using top-down constraint, but it also makes for blurring the distinction between bottom-up and top-down measurements. For example, it is widely accepted that there is about a factor 3 difference in AOD calculations based on bottom-up and top-down methods (e.g., Kaiser et al., 2012), merging both approaches may hide this issue and thus requires a bit more information on whether and when merging these estimates is appropriate. Also, the author talks about 'appropriate correction methods' but this is not further specified as far as I can see.

One of my main questions is to what degree the approach of this paper ("Ideally, these measurements had been made within minutes after the smoke was released from the fires") differs from that of Akagi ("smoke that has cooled to ambient temperature, but not yet undergone significant photochemical processing"). What does that mean for the number of studies included, what does it mean for the average values, to what degree do ground-based studies (which in general include the smoldering phase) differ from airborne studies which may be biased towards flaming combustion with more pyroconvection, etc? The latter is mentioned in the text (e.g. 6-22) but in the end all measurements are averaged. In general, the modellers which will ultimately use this dataset need to know whether and why these numbers are different from the numbers being used so far. This is a key question but not addressed at all and a table that addresses these differences and potential causes for the most frequently used species would be welcomed

8-26: This is a somewhat surprising statement to me. Differences in bottom-up and top-down results can originate from uncertainty in many parameters, emission factors being one of them. The standard deviation of CO in boreal and temperate forests is relatively speaking not that much larger than in savannas which to me is not surprising given the large variability in moisture regimes and tree species and density in those forests. I feel it would be more useful to analyze whether there is a difference in ground and airborne studies to say something about RSC.

My other main point of criticism relates to Table 2 and the statement in the conclusions that the uncertainty in biomass burning emissions nowadays is as large as in the 2000s. Table 2 shows various estimates and the large range stems for a substantial part from outliers such as FLAMBE which predict 10 times higher emissions in tropical forests compared to savannas, totally different from for example GFED4 and GFAS1.2 (derived from GFED3). I understand that it is beyond the scope of this paper to assess which one is right but this deserves some explanation, for example using previously mentioned top-down estimates based on CO. Simply combining the 8750 Tg DM in tropical forests from FLAMBE and the CO emission factor (105 g CO per kg DM) indicates CO emissions from this biome alone of 900 Tg CO per year, something not corroborated by inverse estimates and also at odds with the best estimates of deforestation (e.g., Houghton and Nassikas, 2017, https://doi.org/10.1002/2016GB005546).

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