

Response to Comment by Nic Surawski

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

Firstly, I would like to concur with Dr Ichoku that the current contribution represents a timely addition to the biomass burning literature in light of recent contributions by Akagi et al. (2011) and Yokelson et al. (2013) both of which were published in Atmospheric Chemistry and Physics. My main criticism with the current contribution involves the activity data that is used to report the final emissions factor in the supplied tables. The authors report the emissions factor per unit of dry fuel consumed (i.e. g/kg dry fuel consumed)...

Actually, all emission factors in this paper are in units of g/kg of dry mass burned. This is stated in the footnote to Table 1. It is now also stated explicitly in section 2.3.

... whereas I believe that for certain combustion scenarios, especially for charcoal making, it would be more worthwhile to report them as a percentage of total burnt carbon. A fairly recent paper by Surawski et al. (<https://www.nature.com/articles/ncomms11536>) demonstrates the biases that are likely to ensue from neglecting the change in carbon concentration associated with the combustion process. Given the importance of charcoal making in certain parts of the world e.g. Africa, this may be worth revisiting.

The problem here is that while it might be desirable to do so, the information needed is generally not available. In most studies, the amount of carbon or fuel burned is inferred from the mass balance of measured atmospheric carbon species, while assuming a fuel carbon content. Converting to carbon burnt would require an additional assumption about the yield of char. Furthermore, most users of the EF data apply them to activity estimates in units of dry fuel burnt or consumed per unit time. Providing the EFs in units of carbon burnt would require me to make additional assumptions, and subsequently the users to reverse these assumptions or make additional ones to calculate emissions. For a more detailed discussion, please refer to the review of this paper by Yokelson (<https://doi.org/10.5194/acp-2019-303-RC3>).

Apart from referring the authors to my own paper, I find it strange that emissions factors are reported the way they are by Andreae et al. (2019) given that the alternative approach we cite was indeed developed by the Max Planck Institute for Chemistry in the late 1980s/early 1990s (sensu Jürgen Lobert). The benefits of reporting biomass burning emission factor as a percentage of burnt carbon appear clear to my group (and others) so was hoping to see this approach reflected in a revised manuscript.

Lobert's work was based on laboratory measurements, where he had the benefit of being able to make a complete budget of fuel carbon, char residue, and emissions to the air. This information is not available in most field studies.