

## ***Interactive comment on “The biomass burning contribution to climate-carbon cycle feedback” by Sandy P. Harrison et al.***

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### Response to Referee #1

1. We focused on global relationships (a) because the methane and methane-isotope data, which we use to support our assumption that the charcoal records provide a global signal of biomass burning, are by their nature globally integrated and (b) because quantifying the strength of a global feedback requires global data; climate feedbacks cannot be assessed on a regional basis. However, we agree that it is interesting to test whether there are regional differences in the nature of the response of fire to specific drivers. There have been multiple papers examining the controls on fire as recorded by charcoal data, both at a global scale and at regional scales, only some of

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which are cited in this paper (e.g. Marlon et al., 2008; Harrison et al., 2010; Daniau et al., 2012). We think that it would be useful to cite some more of these papers (e.g. Power et al., 2008; Marlon et al., 2009; Mooney et al., 2011; Marlon et al., 2013; Marlon et al., 2016) and to expand our discussion of the charcoal/temperature relationship as discussed in these papers, and particularly focusing on the results from Daniau et al. (2012) and Marlon et al. (2013). Daniau et al. (2012) showed similar positive relationships between temperature and fire, and unimodal relationships with moisture (precipitation minus evaporation), at a global scale in both charcoal data covering the last 21,000 years and satellite (GFED3.1) data. Marlon et al. (2013) showed positive relationships between temperature and charcoal on centennial to millennial timescales over the Holocene (past ca 12,000 years) for data-rich regions, including Europe, North America and eastern Asia. The strength of this relationship varies, from an  $r^2$  of 0.85 in North America to 0.33 in eastern Asia, showing that other factors also play a role, but nevertheless the nature of the relationship itself (higher temperatures, more fire) does not change from region to region.

We also agree that it would be useful, in the context of the present paper, to include some additional information about regional relationships between temperature and fire. Many recent papers have already described spatial and temporal patterns and correlates of biomass burning based on GFED products. Given the fact that the global relationships in the satellite-era are statistically weak (even at a global scale) because of the short length of the record and the strong anthropogenic impact on recent global emissions, we have focused these analyses on the charcoal record. Because the data coverage is uneven across continents we have confined the analysis to broad latitudinal bands, i.e. N and S tropics and extratropics. This analysis strongly supports our treatment of biomass burning variability as a function of temperature. We propose adding it to Section 3.4.

The key point here is that, as we discuss in the paper, fire initiation and spread are controlled by multiple variables including ignitions, temperature and moisture-related

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climate variables, vegetation properties, and anthropogenic influences on landscape fragmentation and land management. Analyses that have separated the independent role of each of these variables (e.g. Krawchuk et al., 2009; Bistinas et al., 2014) show that the apparent effect of any one variable in a specific region or at a specific time is not the same as the fundamental effect. For example, the fundamental impact of climate on the fire regime can be overwhelmed by changes in vegetation properties or by human activities, but nevertheless, increased temperature will always lead to an increase in burning (all other things being equal). At the global scale, the impact of temperature is paramount because changes in temperature influence other aspects of the climate system: e.g. the equator-to-pole temperature gradient controls atmospheric circulation patterns and wind strengths, and there is a strong dependence of rainfall patterns on global temperature changes. Thus, for a global feedback analysis, there are excellent reasons to quantify fire feedbacks in terms of global temperatures, as has also been done for other feedbacks (e.g. Arneeth et al., 2010).

2. We discuss (in paragraph 2 of the Discussion) the reasons why the feedback strength might be different in the pre-industrial and recent periods, specifically because of the impact of human activities on deforestation, land-use, landscape fragmentation and fire suppression. We will add a further comment on this in the abstract, to the effect that although the feedback estimates from palaeo and satellite-era data are in agreement, this is likely fortuitous because of the pervasive influence of human activities on fire regimes during recent decades.

3. Thank you for drawing our attention to the Petrenko et al. (2017) article, which shows considerably lower methane emissions (15.4 Tg CH<sub>4</sub> a<sup>-1</sup>) based on the ice core record of the Younger Dryas. This is considerably lower than the figure given by Schwietzke et al. (2016). However, both papers make the assumption that the geologic flux is constant. We do not use any estimate of this flux in our calculations; it is simply assumed to be constant. Thus, the quantitative disagreement between Petrenko et al. and Schwietzke et al. is immaterial to our argument. However, we will add the

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reference to the text because the difference in the estimate of this assumed constant flux may be of interest to readers.

4. We agree completely, and indeed this is what we say (page 1 lines 17-18, page 8, lines 3-8, page 10, lines 19-25) in the manuscript: the modern signal is dominated by fires associated with deforestation and peatlands, and although both of these vary with climate they are primarily an anthropogenic signal.

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