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Interactive comment on "Impact of human population density on fire frequency at the global scale" *by* W. Knorr et al.

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Authors' response in normal text, referees' comments in *italics*.

[. . .] but given their results one could argue that population density and fire frequency are actually only very weakly related on a global scale.

Population density increased over 600% from 1800 to 2005 while according to their results fire frequency decreased by about 15%. That indicates very low sensitivity given that population increases were substantial in high fire zones.

Figure 6 shows that a substantial impact of population density on fire frequency occurs when population density increases for values between about 10 and 100 inhabitants per km^2, otherwise the impact is indeed only week. Therefore, the rather small impact

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of the substantial population increase since 1800 is not surprising given that even today only comparatively few regions of the globe have population densities above 10 to 100 inhabitants/km^2 and only those will have experienced large changes in fire frequency.

Indeed, our result is qualitatively the same as the one from the regional paleo-study of Guyette et al. (2002), as it is extensively discussed in the manuscript. In fact, we do not state that the impact is strong or weak, only that it is predominantly negative, and substantiate the claim by the fact that parameter e in all results is less than 0 by several standard deviations of its estimated uncertainty (and therefore significantly different from 0).

In addition to this low sensitivity, human land use often leads to smaller fires which are more difficult to detect by the burned area datasets the authors use. This is especially the case in agricultural regions (which are masked out) but also in areas undergoing deforestation many fires remain undetected by the burned area algorithms. The global importance of these 'small fires' is not well known but estimated to be about 35% with substantial uncertainty (see DOI: 10.1029/2012JG002128). Given the new paradigm supported by the authors that more humans means fewer and smaller fires one can argue that these small fires gained importance over time, offsetting part of the 15% decline predicted by the model. This would mean global fire activity is even less sensitive to population density.

In other words, the most interesting conclusions for the community (more people equals fewer fires) are not supported quantitatively. I would appreciate if the authors use the discussion phase to clarify this. As mentioned by the authors, very low population density yields higher fire frequency than no population at all so the above could be the result of offsets between areas moving from being uninhabited to having low population (increasing fire frequency) and areas with low population density becoming more inhabited (decreasing fire frequency). Or are there other reasons?

We thank the reviewer for pointing out this issue, which is indeed an important one, and which we had not fully appreciated when preparing the present version of the manuscript. Fortunately, we have been able to obtain the global climatology (2001-2010) of burned area based on GFED3 and additional inclusion of the estimated effect fires not detected by GFED3 from Guido v. d. Werf and Jim Randerson (authors of DOI: 10.1029/2012JG002128). We have divided this data set by the 2001-2010 burned area climatology from GFED3 and used the ratio as a correction factor for each annual fire frequency derived previously from GFED3 (using years 2000-2010). If the GFED3 climatology was 0, we used the small-fires climatology for every fire year repeatedly (4.5% of grid cells). This corrected data set was then used for an additional set of optimisations for all population densities, and up to 100, 10 , 1 and 0.1 inhabitants per km^2. All five optimisations were successful and resulted in a highly significant negative dependence of fire frequency on population density, even for the case with up to 0.1 inhabitants per km^2.

We have included these additional simulations in the manuscript (Section 2.2 last paragraph, Section 2.3 1st and 3rd paragraph, end of Section 3.1, Section 3.4 end of first paragraph, Table 1) and added a discussion of the possible effect of smaller fires on the results (and the issues raised by the reviewer) to Section 4 after the current fifth paragraph. The optimised parameters are shown in a new Table A3, the global optimised and observed fire frequencies for GFED-SF in a new Figure C1 (with the old Fig. C1 renamed D1), and the optimised fire frequency up to 0.1 inh./km^2 added to Figure D1 (formerly C1).

Randerson, J., Y. Chen, G. R. van der Werf, B. M. Rogers, and D. C. Morton (2012), Global burned area and biomass burning emissions from small fires, J. Geophys. Res., 117, G04012, DOI: 10.1029/2012JG002128.

In any case, I would be careful with extrapolating the results found by the authors back in time given that fires are influenced by multiple other factors besides population density. For example, Fig. 7 looks totally different from the pattern derived from charcoal data referenced in the paper (DOI:10.1038/Ngeo313) yet there is no discussion about

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the causes of this discrepancy. I would either refrain from simple extrapolation (and focus on population density as one of the factors driving fire frequency) or keep the extrapolation in there but make it very clear that this is solely a response due to changing population and only valid of all other factors would be constant which we know they are not. This option does require a much larger chunk of text explaining differences between the simple extrapolation and results from other studies including ice cores and charcoal data.

This final comment of Referee 1 refers to the general technique of space-for-time substitution that was used here. The current version of the manuscript states in Section 2.4, 2nd paragraph:

“With this model application, we substitute results derived from the spatial dependence of fire frequency on population density by a temporal dependence. Also, we specifically consider the impact of population density alone, without the possible influence of a changing climate. The purpose is to test whether current patterns of population density and frequency of wildfires are consistent with the hypothesis put forward by Marlon et al. (2008).”

To make this even clearer, we have added a statement that this includes the assumption that all other factors would be constant after the first sentence. The same caveat has been added to Section 3.5 where the stated purpose is (beginning of 1st paragraph) “to test whether our results are consistent with the view that increasing human population since the 19th century has caused a decline in fire frequency”, thus further qualifying the meaning of the word “consistent”. We now make it explicit that for the purpose of consistency testing, the way population density is related to human impact on fire is assumed to be constant through time. Further we would like to point out that we use the word “sensitivity analysis” in Section 4 (p. 15756, line 10), and further below the current text already contains a rather strong qualifier:

“We must note, however, that this kind of space-for-time substitution has its limitations.

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Low population density in some areas today may be the result of land abandonment and the fire regime in such cases not comparable to the pre-industrial situation preceding population expansion. Also, phases of intensive land clearing tend to occur in tropical forests today, but in temperate forests during the previous centuries 20 (Mouillot and Field, 2005)."

However, we still believe that such a comparison is useful given the high degree of uncertainty surrounding historical fire record. This notion is further buttressed by an additional discussion of further derivations of past fire activity other than the study by Marlon et al., as suggested by the other reviewer, added to Section 4 (see response to Referee 2).

Interactive comment on Biogeosciences Discuss., 10, 15735, 2013.

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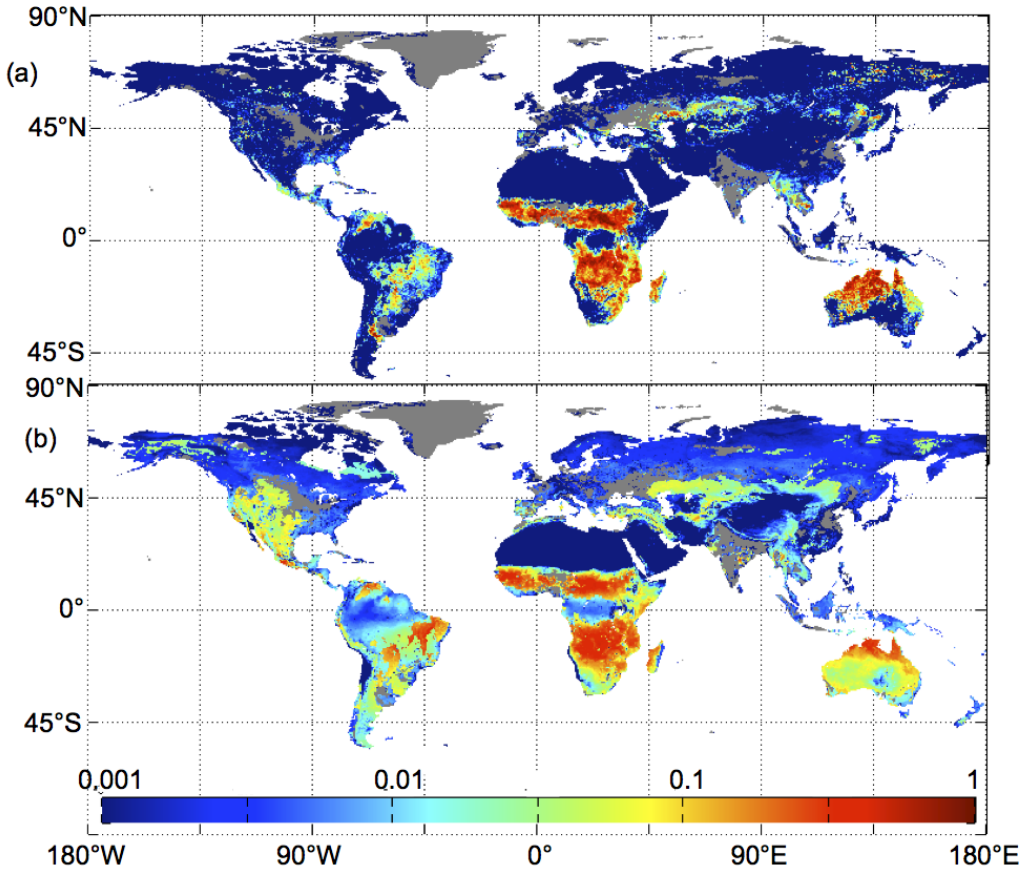

Fig. 1. C1: Observed (a) and modelled (b) mean fire frequency [1/yr] for GFED-SF burned-area data. Model results are with optimised parameters using GFED-SF observations.

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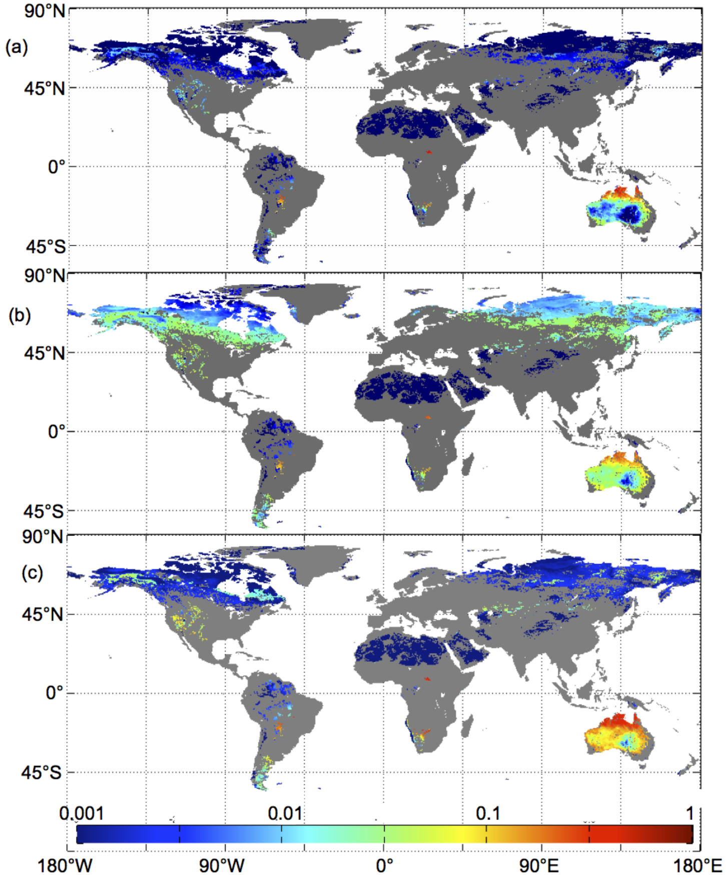

Fig. 2. D1: Modelled mean fire frequency with parameters optimised against (a) MODIS, (b) L3JRC and (b) GFED-SF observations for areas with up to 0.1 inhabitants/km2.