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## ***Interactive comment on “What could have caused pre-industrial biomass burning emissions to exceed current rates?” by G. R. van der Werf et al.***

**G. R. van der Werf et al.**

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*Please see our response to the editor for a general overview of changes made.*

Anonymous Referee 3

Specific comments:

Page 3161, line 23: "...especially in the tropics, humans start most fires." This assumes that lightning has a reduced role in ignition for tropical environments, but based on NASA OTD/LIS data, the highest global density of lightning occurrence can be found over tropical regions. Considering most tropical lightning arrives with wet convection, the argument for human versus lightning ignition in the tropics will require further evaluation. The statement that most fires are started by humans is an assumption that has not been confirmed.

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*We agree with the reviewer that the statement that most fires are ignited by humans has never been systematically confirmed on a global scale. Please keep in mind this was a section in the introduction where we explained why previous studies had used a scaling approach based on population density to estimate historic fire emissions. We have modified that section so that it reads: "A scaling approach using population density makes intuitive sense because, especially in the tropics, most fires are thought to be human ignited although this has never been systematically assessed. However, it fails to recognize that the link between humans and fire activity is complex, that increased population density does not necessarily translate into more fire activity, and that fires are not solely caused by humans."*

*In addition, in the introduction we note that "Second, humans often increase the number of ignitions compared to those that occur naturally through lightning strikes, both in temperate and tropical regions (Mollicone et al., 2006; Nepstad et al., 1999). "*

Page 3161 line 26-27: Drivers of the 15th century downturn in biomass burning was recently revisited by Power et al. 2012 (the Holocene) and provides arguments for climate-driven changes in biomass burning, and does not support the conclusion that demographic collapse in the America's explains the decrease in fire activity. Also, this study provides new interpretations using charcoal-based fire reconstructions from southern South America that could inform some of the results in this manuscript.

*We tried to be balanced in the introduction, not favouring the climate or anthropogenic hypothesis as long as the recent research is in conflict and the impact of the LIA on tropical climate is controversial. The Power et al. (2012) paper itself states that "at a regional–continental scale, 'Little Ice Age' climate change was likely more important than indigenous population collapse in driving this decline." and we had originally "The charcoal record shows a decreasing trend over the first millennium, possibly as a result of global cooling (Marlon et al., 2008). In addition, the low values during the 16th and 17th century coincide with the little ice age, also hinting at the importance of climate in driving global fire rates. The downturn in the 15th century has also been linked to the arrival of Europeans in the tropics, especially in South America (Ferretti et al., 2005).*



"

*To address the reviewer's comment but maintain a balanced introduction we added a reference to Power et al. (2012) to give the climate hypothesis extra weight.*

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Page 3165, line 18: The" TM5 chemical transport model was driven by ECMWF meteorological fields. " the acronym ECMWF is not explained in the text, though I assume it refers to the European Centre for Medium-Range Forecasts.

*Clarified, thanks*

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Page 3165, line 26: By using the average CO mole fraction at the South Pole from the years 2002-2007, a period of large inter-annual variations (e.g. breakup of Larsen B ice shelf in 2002) assumes that average conditions contribute disproportionately to most fire emissions, whereas research in Amazonia suggest that anomalous conditions (drought linked to strong El Nino Southern Oscillation episodes) contribute disproportionately to total area burned and subsequent emissions (e.g. Nepstad et al. 2004 Global Change Biology). Recognizing that inter-annual variability contributed to less than 3% (shown in Table 1) in the sensitivity analysis, changes in large-scale circulation were likely significant factors in long-term emissions (but difficult to quantify as mentioned on Page 3179:line 1)

*Agreed, and we have further substantiated this uncertainty and that related to changes in OH in section 4.4 and mention more clearly throughout the paper that our results are based on present-day atmospheric conditions:*

*Abstract: "Based on estimates of contemporary landscape fire emissions and the TM5 chemical transport model driven by present-day atmospheric transport and OH concentrations, we found that CO mixing ratios at SPO are more sensitive to emissions from South America and Australia than from Africa, and are relatively insensitive to emissions from the Northern hemisphere. "*

*introduction: "We focused on the 1400AD – present time period which overlaps with the CO record from Wang et al. (2010). To some degree, our results are applicable to earlier time periods as well, although we do not account for changes over time in*

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atmospheric transport and chemistry that could affect our conclusions.",  
Methods section: "TM5 was driven by present-day climatic conditions and OH concentrations"

Most importantly, we substantially expanded the uncertainty section: "Another source of uncertainty stems from changes in atmospheric composition; CO is mainly removed from reaction with the hydroxyl radical (OH). If the OH concentration was reduced in pre-industrial times, lower emissions might suffice to explain the increase of CO at SPO. However, simulations of the global average pre-industrial OH-budget point to a relatively constant OH instead (Lelieveld et al., 2002). This is because in the pre-industrial an expected increase of OH due to lower CH<sub>4</sub> concentrations was partly balanced by less recycling of OH due to lower NO<sub>x</sub> levels. On a regional scale, however, OH levels may have been different by up to 25% because the CH<sub>4</sub> levels changed globally but the NO<sub>x</sub> levels changed mostly over the land surface. These non-linear interactions can be further explored using full chemistry-transport models (e.g., Stevenson et al, 2006), but are at least an order of magnitude smaller than the mismatch between current and past SPO CO concentrations as found in Wang et al. (2010)."

Page 3166, lines 4-5: It is unclear why the authors assumed there is no seasonal cycle in fuelwood emissions. In higher latitudes, one would expect a stronger seasonal signal from fuelwood combustion during winter months. A brief clarification would be helpful for why the seasonal fuelwood was assumed constant.

*The vast majority of fuelwood combustion occurs in developing tropical countries where fuelwood is the main source for cooking which can be assumed constant over the year. When used for heating purposes, the reviewer rightfully points out that we should include a seasonal cycle but this is most likely constrained to higher latitude regions in the northern hemisphere and thus not of importance for this study. We have clarified this in section 2.2: 'The seasonal cycle in fire emissions was based on GFED, while we assumed that fuelwood emissions had no seasonal cycle because the majority of fuelwood combustion is for cooking purposes which is not thought to vary much over the course of a year.'*

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Pages 3167-3168: The model developed to identify savanna emissions, driven in part by Mean Annual Precipitation (Huffman et al. 2009) and MAT (Hansen et al. 1999), suggests that the wet season increases grasses, which then become susceptible to burning in the dry season. The use of MAP (versus seasonal or monthly precipitation) may obscure the fact that some savanna regions can have two wet seasons and two dry seasons, resulting in greater variability in savanna fuel production and fire return times in response to inter-annual changes in moisture. How would an alternative approach (using seasonal moisture availability versus MAP) influence NPP and emission scenarios for savanna systems?

*We have investigated the satellite fire record, and except for agricultural burning in the deltas of Afghanistan and northern India, and some grid cells in Sudan we did not find evidence for areas that burn twice a year. The areas that the reviewer refers to are probably located outside the tropics, as the vast majority of the savannas in the tropics and subtropics have one wet and one dry season due to the seasonal movement of the ITCZ. As an alternative approach, we now use the NPP from CASA which is based on satellite data. This is described in section 4.4 and the new Table 4.*

Page 3169, line 23: Typo, remove the second use of the word “ratio” (high SPO mixing ratios ratio to emissons. . . )

*Removed, thanks*

Page 3174, lines 17-23: Research on fire in recently cleared tropical forests suggests that fire frequency and total area burned tends to increase significantly after the first time a forest patch has been cleared and burned by “fire creep”(e.g. Cochrane and Barber 2009 Global Change Biology). Emissions from slash and burn agriculture may accelerate over time based on the interpretations of Cochrane et al. and may contribute a larger proportion of CO than assumed by scaling the CO emissions with rural population data. A sentence or two inserted could address this uncertainty around the time-transgressive nature of fire emissions once a tropical forest has been initially cleared and burned.

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*We appreciate the comment, but think this effect is time independent. In other words, if it is important now, then it was also important in the past which would justify the simple scaling approach. In addition, recent studies have questioned this effect in seasonal forests in the Amazon where most deforestation has occurred over the past decades (Balch, J.K., D.C. Nepstad, P.M. Brando, L.M. Curran, O. Portela, O. Carvalho Jr., and P. Lefebvre. 2008. "A negative fire feedback in a transitional forest of southeastern Amazonia." *Global Change Biology* 14:2276-2287.).*

*To address this we have included the following statement in section 2.3: "Clearly, tropical deforestation has happened in the centuries before and our approach cannot properly account for the dynamic deforestation process, for example related to feedbacks between fire and deforestation (Cochrane and Barber, 2009; Balch et al., 2008). "*

Page 3174, line 28: "there is clear evidence of elevated use of fire in earlier time periods" This statement should be qualified with a specific example or citation, because not all researchers working in Amazonia would agree that there is clear evidence of elevated use of fire. For example, a recent Science paper by C. McMichael et al. (2012) has received much attention (+ and -) because of the controversy around this issue.

*In hindsight we do not understand why we had mentioned "elevated" because we meant to show that there is evidence for the use of fire, but not that it is elevated compared to modern rates. We have deleted this and now have: "More importantly, there is evidence of use of fire in earlier time periods here."*

Page 3175, line 1: The body of evidence that suggests a long history of human-induced disturbance in soils and lakes from the Amazon is relatively small (I would delete "substantial" from the sentence) and the sedimentary charcoal records from soils are more problematic in terms of chronologies and interpretation than those from lake sediments (hence the controversy around the McMichael et al. 2012 paper in Science).

*We have modified the sentence so that it reads: "Several studies using charcoal records from soils or lakes in the Amazon find a long history of human-induced disturbance in the region (Bush et al., 2007; Cordeiro et al., 2008; Sanford et al., 1985;*

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Page 3176, line 20-23: A synthesis of all evidence of fire activity in Australia has recently been published by Mooney et al (2011 Quaternary Science Reviews), suggesting that the role of humans in biomass burning in Australia is minimal, at least until the last 200 years.

*We appreciate the link to this paper and have inserted the following sentence: "Charcoal data indicate that in most of Australia, fire activity has increased since 1400AD with a peak around 1900 (Mooney et al., 2011)." and also updated the discussion in section 4.3: "On-going work using the charcoal database shows that several crucial regions including South America and Australia display an increase over the past centuries (Marlon et al., submitted; Power et al., 2012; Mooney et al., 2011)."*

Table/figure comments:

Tables- fine

Figure 1 (page 3193): Typo in the figure legend, remove the word "data". "The CO data ice core data. . . "

*done*

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Interactive comment on Clim. Past Discuss., 8, 3159, 2012.

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