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Climate of the Past Discussions

Interactive comment on "What could have caused pre-industrial biomass burning emissions to exceed current rates?" *by* 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 1
Major points:
1. On page 3164, lines 4-5 the authors state that they have focused on the 1400AD-present period which is the time period covered by Wang et al. 2010 and that the results would be applicable to even earlier times. This statement is misleading, in these lines as well as at every other place throughout the manuscript, as the modelling setting is not covering the climate conditions of the historic time period. The TM5 model is driven by ECMWF climate data covering 2002-2007 and using contemporary CO+OH chemical reaction rates (see methods section). The authors must state clearly that

they did not test historic climate conditions and the resulting atmospheric chemical reactions under the conditions since 1400; nor that any transient simulations covering several centuries were conducted. What the current study presents is a scaling of human-caused biomass burning sources to pre-industrial conditions using historic data on human population density. It must be clearly stated at the end of the introduction that these different pyrogenic emissions were used in the TM5 chemical transport model under contemporary climate and atmospheric conditions in time slices of just 7-years simulation. I do not see automatically that these settings of simulation experiments could be applied to even earlier historic times. I ask the authors to remove this statement as it is, in my opinion, overselling the ability of the current modelling setting.
Response: we agree with the referee and have now further emphasized that our simulation is based on present-day atmosphere. This is done throughout the manuscript:
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 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-

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industrial an expected increase of OH due to lower CH4 concentrations was partly balanced by less recycling of OH due to lower NOx levels. On a regional scale, however, OH levels may have been different by up to 25% because the CH4 levels changed globally but the NOx 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)."

2. The methods section describes the GFED and TM5 model setting, fire emission setting and scaling of savannah fires, including a new scaling method of dead fuel in GFED. This section must have another subsection describing the input data for each experiment to clearly describe the difference between the three parts of the analysis and simulation experiments. The information on the input data is spread across the sections and makes it difficult to understand.

We do realize that our methodology is complex because it consists of several different modeling frameworks with each its own set of input parameters. We had made a subsection for each of the 3 steps and have now substantially expanded section 2.1 where we detail about the different parts of the analysis to better state that these are relatively independent. We now mention for each step what the main input datasets are in section 2.1 so that it reads:

"Our analysis consisted of three parts. First, we built a fire emissions climatology based on 14 years of recent fire activity from the Global Fire Emissions Database (GFED3; Giglio et al., 2010; van der Werf et al., 2010). These emissions were partitioned into five fire sources: deforestation, forest, non-forest, tropical peat, and fuelwood burning. This analysis was for a large part driven by satellite data of the land surface, with burned area and vegetation productivity as main input datasets. We then input the emissions climatology into the TM5 chemical transport model (Krol et al., 2005) to estimate the contribution of these five fire emission sources to CO mixing ratios at SPO. TM5 was driven by present-day OH concentrations and climate. In the second part of our analysis, we estimated historical fire emissions since 1400AD assuming

that the human-driven fire sources (deforestation, tropical peat, and fuelwood) were population-density dependent. This step was driven mainly by the GFED fire emissions estimates partitioned into different sources and reconstructions of population densities. And finally, we built a simple model that could mimic GFED emissions estimates for savanna areas to estimate the sensitivity of savanna fire emissions to changes in fire return time (i.e., the time between consecutive fires for a given area). The main input datasets for this step were temperature, precipitation, and satellite-derived burned area as used in GFED. These three steps are described below in more detail."

3. I appreciate that the authors clearly stated that past changes in atmospheric circulation relative to today is not included in the analysis. I think this should be extended to further deficiencies of the experimental setting, that the study assumes no changes in vegetation composition (forest to non-forest or vice versa as a result of long-term climate variability) or its productivity since 1400, that the sources of pyrogenic emission did not change, e.g. peatland accumulation and peatland burning is constant, and that the influence of climate on pyrogenic sources as well as atmospheric oxidization is not considered.

We agree with the reviewer but feel these are of second order. For example, it is likely that savanna productivity has changed in the past centuries due to changes in climate, but the impact of these changes on emissions are at least an order of magnitude smaller than the changes we had to make to the burn frequency (going from burning every 32 years to burning every year or every other year). For example, Figure 2 indicates that expected changes in climate would have impacted NPP by a few percent. In addition, we did change the rate of peatland burning in our population-density scaling approach. However, we do agree with the reviewer and added the following sentences to section 2.3: "In this scaling approach we do not account for changes in emissions due to other impacts. For example, changing climate and vegetation composition have influenced fire emissions over time, but likely to a smaller degree than the changes invoked by humans."

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4. In section 2.4 a simple model to derive litter or dead fuel production from NPP and taking temperature, precipitation and Q10 into account. However, it must be stated that litter production as a result of changes in vegetation composition (i.e. mortality in forest due to other reasons than fire) are not accounted for. But I do not understand, why NPP is no longer used as simulated by CASA, but modelled as a function of MAP and MAT? Please improve the description of this approach and explain why you had to change the NPP approach in GFED. Furthermore, I disagree with the authors that the discrepancy between modelled NPP and CASA NPP is low. A difference of 250 gC per m2 and year is substantial, especially in the southern Hemisphere and the tropical regions, which are important for the correct simulation of Southern Hemisphere pyrogenic sources. To illustrate the impact of this discrepancy, the authors should present a calculation with original CASA NPP and discuss this as a source of error and quantify the deviation in atmospheric CO. Why are these results not compared against data from other DGVMs, which use a different approach to simulate NPP and other carbon fluxes

In section 2.4 we have clarified our approach and we now use the CASA NPP values in a sensitivity run discussed in the uncertainties section. To clarify why we did not use CASA NPP for the main analysis we have inserted the following sentences:

"The reason to model NPP instead of taking the values derived from satellite-based vegetation indices as done in CASA was to minimize the effect of humans on NPP; in many regions humans have modified the landscape, lowering grid-cell average NPP."

and "As an alternative, we also performed a run driven by CASA NPP, this is described in section 4.4."

4.4: "To test how sensitive our results were to the new NPP fields we replaced those with the CASA NPP values (Table 4). Lower NPP values translated to a reduction of up to 7 percent in emissions, indicating that it would be even more difficult for landscape fires to explain the discrepancy. "

5. What is important to report in the sensitivity of fire emissions is, whether the model captures the feedback between vegetation productivity, i.e. NPP and litter production (thus fuel availability), and fire. How is the NPP and fuel production affected, when the

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fire return times are shortened, when does fuel start to limit fire emissions?
The reviewer rightfully points towards the fact that at very low fire return times, fuel loads may be insufficient to support the spread of fires in arid regions. When we wrote the paper we tried to find out what the minimum amount of fuel was to sustain burning, but the literature is vague about this, probably due to the fact that not only carbon density but also vegetation structure is important. It is clear however that in low productivity regions this is an issue and we had used this fact to dismiss the possibility of annual burning on a pan-tropical scale for all savannas, for example in the conclusions (bullet 1): "However, to fully explain the difference in CO mixing ratios all of the Southern hemisphere non-forest land had to burn annually or bi-annually during the highest fire episodes. This is not likely if only for the reason that dry savannas do not build up enough fuel each year to be able to burn annually."
Our response to the reviewer is thus somewhat similar to question 3 and 4: we have built a simple model to capture the first order effects. The issues the reviewers brings up are real but probably all second-order and require a full-blown DGVM to address properly, something that is both beyond the scope of the paper and this response. By better acknowledging these issues we hope to have addressed this satisfactory.

6. Please add a discussion of the influence of climate changes during the past millennium to the uncertainty section 4.4. Past reconstructions of Mann et al. and other climatologists should certainly be discussed here and the influence of lower temperatures on fire occurrence and completeness of production being discussed as well as on atmospheric chemistry, even though it could not be considered in the modelling setting.
Please see our response to 3. The temperature reconstructions for the tropics are not as well established as those for higher latitudes as far as we know, and Figure 2 shows that while NPP is a function of temperature, the rate of change in the tropics is about 50 g C per degree Celsius. So again, we feel this is a second order effect but have acknowledged the omission now more clearly in the text in 2.3: "In this scaling approach we do not account for changes in emissions due to other impacts. For example, changing climate and vegetation composition have influenced fire emissions over time, but

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likely to a smaller degree than the changes invoked by humans."

Minor points:
1. Figure 7 should state the time period for which these data were compiled.
done

2. Please check if more recent values are available regarding carbon emissions from land use change. The numbers presented in Ramankutty and Foley 1999 publication might have been updated already.
We checked the most recent data from Houghton et al (http://cdiac.ornl.gov/trends/landuse/houghton/houghton.html). These are emissions and not area though. Cumulative emissions (excluding regrowth) in temperate regions over 1850-2005 were about 70 Pg C. The number we used from Ramankutty and Foley was 1E13 m2 of cropland. With our assumed biomass loads of 21.4 kg DM / m2 and dry matter carbon content of 45% we estimate 96 Pg C. The difference can be partly explained by the omission of pre-1850 clearings in Houghton's number. More importantly, we show that the land use change fire sources are by far too small to account for the large discrepancy we were after, even if we would consider a large error on the Ramankutty and Foley estimates
Based on an overlapping comment by reviewer 2 we have expanded the section: "Even though clearing rates were not constant and the history of deforestation is complex (Klein Goldewijk et al., 2010; Kaplan et al., 2009) temperate forest clearing can only explain a tiny fraction of the discrepancy."

3. Is the knowledge still state-of-the-art, that OH budget can still be regarded as constant compared to pre-industrial conditions?
We have looked more into the literature and we think it is indeed constant, but only on a global average scale. We have substantially expanded the discussion of this uncertainty: "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

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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 CH4 concentrations was partly balanced by less recycling of OH due to lower NOx levels. On a regional scale, however, OH levels may have been different by up to 25% because the CH4 levels changed globally but the NOx 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)."

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