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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.

Anonymous Referee #1

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Review of the manuscript entitled "What could have caused pre-industrial biomass burning emissions to exceed current rates¿'; presented by Guido van der Werf et al. The authors present an interesting study, were they analyzed potential contributions of pyrogenic emissions to the CO concentrations stored in ice cores in the South Pole. It relates to findings from Wang et al (Science 2010, ACP 2012), who reconstructed CO concentrations from ice cores and who found that the pre-industrial pyrogenic emissions must have been 4 times higher to leave such a signal in the ice core record. This is the starting point of the current study by van der Werf and colleagues, where they apply the GFED pyrogenic emission model to separate out the different contributions of biomass burning sources and use these emissions as an input in the TM5 atmospheric chemical transport model to compute the resulting CO concentration at the South Pole. This is an interesting research question and important to advance the discussion about

the role of pre-industrial biomass burning. However, I have some major points regarding the modelling aspects which I urge the authors to address completely to improve and correct the presentation of the findings in this manuscript. These are listed in the following:

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.

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.

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.

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 m^2 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?

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 fire return times are shortened, when does fuel start to limit fire emissions?

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.

Minor points:

1. Figure 7 should state the time period for which these data were compiled.

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.

3. Is the knowledge still state-of-the-art, that OH budget can still be regarded as constant compared to pre-industrial conditions?

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