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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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In this paper van der Werf and his co-authors address the interesting problem of the late Holocene record of biomass burning based on ice core methane and CO archives. Interpretation of the ice core record, additionally supplemented by limited global synthesis of charcoal records, presents a challenge because these records suggest that global biomass burning was greater in the past than at present, or at least that there was a millennial-scale minimum in fire in the 18th and 19th centuries. Using a combination of land surface modeling, fire emissions inventories and estimates and atmospheric transport modeling, the authors of this study explore various explanations for the late-Holocene trend in biomass burning. They conclude that the problem remains elusive and that their results cannot point to a definitive explanation. Overall this is a

good study that is clear in its methodology and thorough in its analysis. The manuscript is well written and the figures and tables are generally clear and well presented. However, some of the assumptions, particularly in the ecology of tropical seasonal ecosystems and the modeling of potential fire return intervals require more evidence to be properly substantiated. The authors also neglect discussion of a potentially important source of CO in preindustrial time from charcoal production and consumption. Otherwise, this manuscript makes a useful and important contribution to the field, in particular in the way it combines land surface with atmosphere modeling to model the entire cycle of trace gasses from production to emissions to deposition. With moderate revision I would be pleased to recommend this manuscript for publication in Climate of the Past.

General comments

There are two important issues in this study that require more analysis and substantiation: 1) the fuel dynamics of tropical and subtropical grasslands and savannas, and 2) the source of CO and methane from charcoal production and consumption.

With respect to fuel in tropical and subtropical ecosystems, I disagree with the assumption that tropical grasslands and savannas need several years to recover from fire to accumulate a fuel bed that is capable of sustaining continuous burning. This is a key assumption of the present manuscript and one of the reasons why the authors state that annual burning of tropical and subtropical grasslands and savannas would be unrealistic. Grass fuels in most seasonal tropical environments can easily accumulate a continuous fuel bed in one growing season, and, because decomposition rates in these ecosystems is typically very high, if there is no fire during the dry season the fuel is largely decomposed by microbes in the current year and little year-to-year fuel accumulates.

In this sense, the authors need to provide a more detailed reference for their statement on Page 3167, line 2-3 as it is central to their hypothesis regarding realistic fire return intervals in tropical seasonal environments. The reference by GD Cook (2003) provided on the following lines appears to refer more to woody litter dynamics than to grasses (Cook 2003, pgs. 48-49), and references a single experiment in Northern Australia that may or may not be representative of the seasonal tropics as a whole. Furthermore, Cook (2003) mainly refers to the accumulation of woody fuels affecting fireline intensity, and therefore potential tree kill, as opposed to fire rate of spread or total burned area. Cook (2003) makes a further reference to Cheney and Sullivan (1997), who on page 8 of the 2008 revised 2nd edition of their book describe in detail the annual cycle of tropical grass fuels. Using tropical Australia as a reference, Cheney and Sullivan describe most herbaceous fuels in the seasonal tropics as being essentially annual, and, as written above, largely decomposing in one year if not burned. Therefore, multi-year accumulation of fuels in tropical seasonal environments is more likely to affect fire intensity than fire spread, and would in fact not be necessary to explain the possibility for annual burning of tropical grasslands and savannas. In this sense, the hypothesis that tropical environments would need to burn unrealistically more frequently than observed today is in fact, not at all impossible. The authors should describe the fuel cycle of tropical grasses more accurately in their revised manuscript, and accept that annual burning could well be possible, particularly if ignitions during the dry season are caused by humans.

My second major comment is that it is not clear if the authors account for charcoal production and consumption in their calculation of CO and CH4 emissions. The emission factors for fuelwood consumption are not provided directly in this paper, and it is not easy to trace the values back to those originally used in GFED and see precisely what they include. The pyrolysis of wood for charcoal production, because it by necessity occurs in a low oxygen environment, releases substantially more volatile trace gases than the simple consumption of firewood for fuel. Charcoal was the preferred fuel in urban environments in the preindustrial world, and was the only fuel suitable for iron smelting before the use of coal became widespread in the 19th century. Charcoal production results in 2-5 times more CO emitted than other types of biomass burning, and

further releases 1.5-3 times more CO than other types of biomass fuels when combusted (Akagi et al., 2011). Therefore, across its lifecycle charcoal emits 3.5-8 times more CO than fires from natural fuels or from most other biofuels. Similar relatively high emissions factors for charcoal exist for methane.

Production and consumption of charcoal has been important in Eurasia since the beginning of the Iron Age at about 1000 BC. In Africa iron smelting was widespread south of the Equator by AD 500. On the other hand, iron smelting was probably unknown in pre-colonial South America and Australia, but total global iron production in the preindustrial world was probably on the order of 0.1 Mt annually, resulting in a typical charcoal demand exceeding 10 Mt p.a. (Sapart et al., 2012). It would be very interesting to see a more thorough analysis of the potential role of preindustrial "industrial" biofuel use, including charcoal, on CO concentrations, and at very least acknowledge charcoal as a potentially important source. The authors should also generally be more precise about what they mean by fuelwood, for example does this also include combustion of biofuels more generally as in Yevich and Logan (2003), or is it strictly firewood. If the latter, than how do the authors account for burning of agricultural wastes, which was probably even more important worldwide in preindustrial times than at present, and typically emits more CO than burning in natural ecosystems (Akagi et al., 2011)?

Specific comments

Page 3166, line 9: The citation should be to "Klein Goldewijk, 2001". Also, how did you get population data for the period before AD 1700, as the reference cited only covers the 1700-2000 time period? Perhaps you used and meant to reference the newer study: Klein Goldewijk et al., The Holocene (20), 2010?

Page 3168, lines 10-12: Following my comments above, this assumption is unrealistic. Quoting Cheney and Sullivan (2008, pg. 8) writing on tropical grasses "...grass cover commonly grows to 3 m, and sometimes as high as 4 m during the wet season and collapse with the last rains to form a uniform fuel bed around 0.5-1.0 m high. If not burnt, the annual grasses decompose almost completely during the next wet season and only a thin layer of organic material remains on the soil surface at the start of the following dry season."

Page 3171, lines 9-10: Likewise, update this statement on time for fuel buildup.

Page 3173, lines 5-15: The time course of global anthropogenic deforestation in the preindustrial Holocene is controversial. Several recent studies have shown a very different spatial and temporal pattern compared to Ramankutty and Foley (1999), including Klein Goldewijk et al. (2010), Kaplan et al. (2011, 2012) and Pongratz et al. (2008). In particular, Kaplan et al. (2009, 2011, 2012) suggest that peak deforestation in Eurasia and Africa probably mostly occurred before AD 1700, and even recovered somewhat during the 18th, 19th, and 20th, centuries. This forest recovery has implications for the potential reduction in biomass burning emissions inferred after the 14th century. In contrast, most deforestation in the Americas took place after AD 1700. The authors should acknowledge the complex history of global deforestation in preindustrial time.

Page 3174, line 20: The word "peasants" is not used appropriately. "Smallholders" or "shifting cultivators" would be a more precise and less politically derogatory term.

Page 3176, line 25: Looking at the GFED burned area product for the past decade, some of the areas of the world with the most amount of fire have the lowest population densities; this is, for example, especially true in northern Australia, where a number of sources and official statistics, list human ignitions as the major cause of fire at present. Indeed, many Australian regions where human caused fire is common have no permanent population at all, in particular areas inhabited by aboriginals practicing traditional lifestyles. Thus, at least in the seasonal tropics, the link between population density and fire frequency is tenuous at best. In the past, e.g., in pre-Columbian South America, and pre-colonial Africa, burning by nomadic hunter-gatherers might have been much more frequent than at present. It is important here to make the distinction between

population density and human lifestyles. Farmers and herders have a very different relationship to the landscape and to fire than foragers do, and therefore changes in human subsistence strategy as opposed to changes in total population could be the most important factor influencing anthropogenic fire regimes.

Page 3177, lines 7-12: As noted in my general comments above, annual burning of tropical seasonal ecosystems is not at all "unrealistic" either from a perspective of the availability of a continuous fuelbed or from human motivation to burn. This paragraph should be revised.

Page 3180, lines 5-6: As written above, I disagree with the statement that savannas do not build up enough fuel to burn annually. Simple modeling with a Dynamic Global Vegetation Model could be used to test this assertion. Only in the very driest environments, in deserts, could this maybe be true, but these areas occupy a relatively small part of the Southern Hemisphere, e.g., in the Kalahari and in southeastern Patagonia.

Figures 3,5,8,11: All of these figures that present world maps (or part of the world), are printed too small to be useful. The size of each map figure should be roughly doubled to make the information on the maps legible. The maps should also be provided as native PDFs (as opposed to jpeg or other image formats) so as to allow zooming in to focus on individual continents, and avoid criticism that the authors might be trying to hide something in the details of the model results. As these maps are currently produced, they look blurry under magnification.

References (that were not cited in the original paper)

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