Clim. Past Discuss., 8, C2659–C2667, 2012
www.clim-past-discuss.net/8/C2659/2012/

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.

G. R. van der Werf et al.

guido.vander.werf@vu.nl

Received and published: 4 December 2012

Please see our response to the editor for a general overview of changes made
J. O. Kaplan (Referee)
jed.kaplan@epfl.ch
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.
We fully agree with the reviewer that annual burning is possible in most of the savanna ecosystems, except for arid regions, something that was also brought forward by re-

viewer 1. In the conclusions we state for example that "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 in arid regions, not all savannas build up enough fuel each year to be able to burn annually." (final sentence was modified to be more precise). Thus, we do not state that savannas cannot burn every year, but instead say that not all savannas can burn each year.

We looked more into the literature and found support for several levels of importance of carry-over of fuels, and agree with the reviewer that our approach may be biased towards woody savannas. We tried to mimic GFED fuels as good as possible, which requires carry-over. However, GFED has been criticized for having too much fuel in savannas which may overlap with the reviewer's comment. We have therefore performed a sensitivity run. When we run without fuel carry-over we found the same maximum amount of CO at SPO that could be due to biomass burning, which was expected because carry-over is not important when fires burn annually. However, it did lower fire emissions when fire return times were longer, roughly to a third of original values for fire return times now found in savannas (new Table 4). This would deteriorate the agreement with GFED fire emissions to a large degree. What matters though is that this modification has no real impact on our main findings because it does not change the maximum amount of emissions possible when all savannas are burning annually. It does indicate that when fires are less frequent, emissions drop more substantially which basically indicates that landscape fires are an even more unlikely explanation for the factor 4 increase.

This sensitivity analysis is described in section 4.4 (uncertainties) and results are in a new Table (4): "We also tested the sensitivity of our approach to the amount of fuel that is carried over from one year to the other. In our approach and in GFED, not all biomass decomposes during the wet season so the amount of fuel increases over time (Figure 6). This may be more representative for woody savannas while in grasslands and open savannas most of the fuel decomposes (Cheney and Sullivan, 2008). In other

words, fuel may be more constant from year to year than our model with substantial fuel carry-over predicted. This did not influence our emission estimates for annual burning because in this case fuel build-up is irrelevant, but it did substantially lower emissions for those areas that burn infrequently to estimates well below those from GFED (Table 4). This analysis also showed that our results may overestimate the potential for savanna fires to account for the difference in SPO CO mixing ratios because emissions decreased compared to the standard run when fires do not burn annually."

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 be- ginning 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)?

We had not taken charcoal production into account in our estimates and appreciate the reviewer brings this up. Combining 10 Mt / year with the emission factor of 255+189=444 g CO / kg DM of Akagi et al. (2011) yields annual CO emissions of 44 Tg CO, or about 9% of the current global CO biomass burning budget (Table 1) and about 5% of the increase in CO emissions required to match the peak pre-industrial SPO CO concentration. We have modified the methods section and conclusions to acknowledge that we did not account for this source and that it may have contributed to the enhanced CO emissions.

Specifically, in the abstract we refer to "landscape biomass burning" instead of the more general "biomass burning" and in the introduction we state "In this paper we aimed to understand what these ice core measurements actually mean for the fire landscape, neglecting the potential of additional sources such as charcoal production to account for some of the discrepancies. ". In the methods we now state: "In our analysis we did not account for charcoal production and burning that may have been more important in the past than it is now. During the production and burning process it may release about 450 g CO per kg dry matter burned (Akagi et al., 2011), 8 times more than savanna fires do. Its magnitude is uncertain but likely to be in the order of 10% of total contemporary biomass burning CO emissions. This number was derived from combining CH4 estimates from Sapart et al. (2012) with emission factors from Akagi et al. (2011)." and in the conclusions: "Increased emissions in Southern hemisphere temperate forest, increased rates of charcoal production, and increased rates of deforestation and slash and burn agriculture in the tropical forests could have helped in explaining the excess biomass burning rates necessary to mimic pre-industrial CO SPO mixing ratios, but the

magnitude of these sources is much smaller than that derived from savanna fires, even when extreme scenarios of deforestation are envisioned. "

With regard to the last comment, we used the estimates of Yevich and Logan (reprocessed for the EDGAR database). We added the following statement to be more precise: "GFED does not include emissions from the burning of fuelwood; these were taken from the Emissions Database for Global Atmospheric Research (EDGAR) version 4.1 (http://edgar.jrc.ec.europa.eu/; Olivier et al., 2011) with the methodology partly based on the work of Yevich and Logan (2003). Besides fuelwood burning this source also includes smaller contributions from agricultural residues and dung burning. The burning of agricultural waste in the field is partly captured by GFED. "

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?

That is right, we have updated this

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.”

Please see the response to the first general comment where we addressed this issue

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

Please see the response to the first general comment where we addressed this issue

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.
Agreed, and changed to "If we take typical literature values for biomass stocks, combustion completeness, emission factors, and the efficiency of 4.5 ppb CO Pg-1 CO-1 (the mean of forest and non-forest in Table 1), then total CO mixing ratio at SPO would be elevated by 26$\pm$14 ppb (Table 3) if all burning from clearing of forests over the past three centuries happened in one year, or on average 0.09$\pm$0.05 ppb per year over the past three centuries if clearing rates were constant. 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."

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.
Changed to smallholders

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.
We agree with the reviewer, and have therefore not scaled the savanna fires (which occur mostly in the seasonal tropics the reviewer refers to) with population density as we cannot say a-priori what the relation is. Instead, we tested the sensitivity of savanna emissions to changes in fire frequency to study what the pre-industrial fire frequency should have been to match atmospheric CO ratios.

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.
Just like the first comment, we feel this comment is partly due to a misunderstanding. To clarify what we mean and to acknowledge that annual burning is not unrealistic at all in most savannas we have revised this paragraph so that the relevant part now reads: "This scenario is an upper bound and not very likely for at least two reasons. First, although most savannas can burn annually because they are productive enough to build up a continuous fuel bed each year, more arid regions do not support annual fires. As a case in point, the interior of Australia rarely burns but in 2011 it burned large areas for the first time in a decade due to excess rainfall in the wet season. Second, we assumed that the whole grid cell is available for burning while in reality landscape features such as rocky outcrops, escarpments, and river valleys will not burn."

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. *Please see comment above, and please also note that a large part of Australia falls in this category: "As a case in point, the interior of Australia rarely burns but in 2011 it burned large areas for the first time in a decade due to excess rainfall in the wet season. "*

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.
*We have redone all figures to minimize white space and saved them as pdf's to make zooming easier. The scatter plots have been replaced by contourplots to make this possible as the pdf files became too large (100.000 points). The size of the graphs has increased somewhat but doubling its size as suggested would make the world maps larger than is commonly done in the journal. We believe that the better zooming ability will satisfy those that do want to zoom in while keeping the maps the size of those used commonly in CotP.*âĂČ

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