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Discussions

Interactive comment on "Climate, CO_2, and demographic impacts on global wildfire emissions" *by* W. Knorr et al.

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Point-by-point response to comments by anonymous reviewer 1. Reviewer comments in italics.

1) I suggest the title be changed to "Climate CO2 and human population impacts on global wildfire emissions". Demography can be either vegetation or human, and this wasn't entirely clear given the model being used and that the abstract/introduction discusses 'woody thickening' also as a demographic process.

Reply: We accept that "demographic" is also used in the context of vegetation dynamics – especially for the readership of Biogeoscience – and are grateful for the clarification.

2) Please provide a more mechanistic description of why LPJ-GUESS woody en-

croachment results in reduced emissions under the CO_2 scenario. Is it because of a decrease in fine fuels (i.e., grass), or is it because of the fire resistance of the woody PFT being greater than grasses? It is not clear in the manuscript which process in LPJ-GUESS actually causes the decrease in emissions associated with woody thickening. From an LPJ-GUESS perspective, what exactly is 'woody thickening' ? Is it an increase in the number of individuals per cohort, i.e., stand density, or an increase in stand biomass, e.g., greater productivity?

Reply: Woody thickening in the context of this study is an increase in the cover fraction of woody vs. herbaceous plant functional types (PFTs). Cover fraction is measured as the fractional leaf area index of each PFT at full leaf development. This measure is closely related to the ratio of the number of woody vs. herbaceous cover on a grid cell, i.e. the total over all patches and cohorts. Because woody plants are on average of higher biomass, it also means an increase in biomass per grid cell, but this is only a consequence of the change in community composition.

As to the first question of how woody thickening leads to decreases in emissions: There are two effects, one on fire spread and one on fuel load. When the fraction of shrubs increases, the area belonging to the biome "shrubland" increases relative to the area of the biome "savannah and grassland". $a(B)$ (Equ. 1 or 3) is an empirical parameter representing fire spread and is 0.889 for savannah and grassland, but 0.470 for shrubland, so that for a given climate, leaf area index and population density, an increase in the fraction of shrubland immediately leads to a decrease in burned area. The second effect results from the fact that in LPJ-GUESS, 100% of live and dead leaves burn in a fire for grasses. In the case of woody vegetation, 100% of dead leaves but only between 46 and 59% of live leaves (depending on mortality), 20% of dead wood and no live wood burn in a fire (Knorr et al. 2012).

The first effect can be seen by a decreasing effect of CO_2 on fire emissions via burned area (Figures 4 and 5 in the manuscript), because increased atmospheric CO_2 favours shrubs over grasses in most regions except the coldest and most arid. The second effect is demonstrated by Figure 1: Woody thickening also leads to a decline in the fraction of NPP that is eventually emitted in fires, which is highest in savannahs and grasslands in general, but markedly declines in Africa and North Australia, where the highest emissions per area occur.

3) Much is made of the Bistinas 2014 and Knorr 2014 paper on whether increases in ignitions cause increases in fire frequency. However, this seems at odds with how population density (a proxy for ignitions) is linearly related to burned area. Given that the demography plays such an important role in this paper, can you try clarify these statements in the context of how the model is developed.

Reply: Our argument is that in most of the world today, the fire regime is ignition saturated, decoupling burned area from the number of ignitions. The argument rests no so much on Knorr et al. (2014) and Bistinas et al (2014) as it does on the regional study by Guyette et al. (2002) who demonstrated this effect very clearly in historical data from North America. We believe that the findings by Guyette et al. provide a compelling explanation of why several studies have found an overall negative impact of population density on burned area, not only the ones cited for the global scale, but also Archibald et al. (2008, 2010) and Lehsten et al. (2010) for Africa.

4) Lightning is also an important cause of fire. Romps et al. (2015 Science) suggest lightning strikes in North America will increase by 50% this century. Some discussion of lightning ignitions, and lightning strike changes with climate seems warranted.

Reply: Thank you for pointing out this recent publication which we have not been aware of. We expect that in the few areas where the fire regime is still ignition limited (parts of the boreal zone and Australia), an increase in lightning strikes could potentially lead to an increase in emissions (see previous comment on ignition saturated fire regimes). However, the sensitivity experiment where we assumed an ignition limited fire regime below 0.1 inhabitants per km^2 resulted only in very small changes in simulated emissions. Even though this simulation did not account for possible changes in lightning

ignitions (but it implicitly assumes a certain constant background ignition in the absence of humans), the small impact it had on simulated emissions makes us believe that the effect of lightning on fire emissions will remain small.

5) Minor comments - P15015 Line 25: I think you mean < 3 mm (not > 3 mm) when describing the Nesterov Index

Reply: Thank you for spotting the error.

6) - Check EMS mix up with ESM throughout manuscript.

Reply: Thank you again.

7) - Can you describe how the patches are selected to be burned once the grid cell burned area is estimated.

Reply: This is a good question, as there are indeed two modes of operation possible with LPJ-GUESS. We have used the stochastic-mortality mode, which means that for woody vegetation each individual in a patch is selected as either killed or surviving with a probability equal to one minus the PFTs fire-related mortality (see Table 1 in Knorr et al. 2012). For grass PFTs, the number of individuals in a patch is not defined (Smith et al. 2001). We do not select whole patches to be killed at random, which is motivated by the large size of a grid cell compared to the burned area of the average fire. This would be different if a simulation was carried out at the single-ecosystem scale, or the number of patches simulated was several orders of magnitude higher.

8) - The relationship between demography and burned area is optimized for present-day observations. However, we know that the fire suppression and management policies change regularly. Additional discussion on the role of management, particularly on whether new policies that might allow large fires to burn, thus changing the burned area/demography relationship, would be useful.

Reply: This is indeed a useful suggestion for the conclusions and outlook, for which we are grateful. While we believe that the main reason why humans suppress fires has to do with the erection of barriers to fire spread rather than direct fire prevention or suppression, we can certainly see a role for changes in fire policy which studies as the present could inform.

9) - Change “Climate effects” -> climate affects.

Reply: Thank you. Found in Line 20, page 15019, to be corrected.

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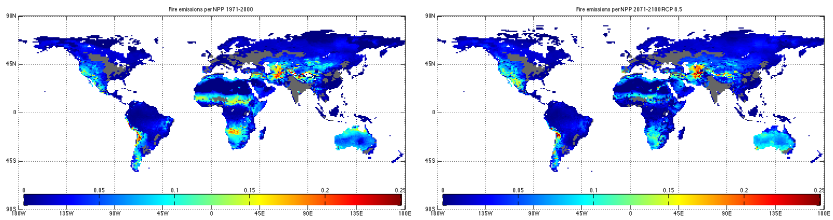

Figure 1: Fire emissions as fraction of NPP. Left: 1971-2000 ensemble average. Right: 2071-2100 RCP8.5 ensemble average.

Fig. 1.