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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate and demographic impacts on wildfire air pollution hazards during the 21st century" *by* Wolfgang Knorr et al.

Anonymous Referee #1

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Review:

Climate and demographic impacts on wildfire air pollution hazards during the 21st century

The manuscript analyses simulations presented in Knorr et al. 2006abc with a focus on PM2.5 emissions during the 21st century in relation to WHO air quality guidelines. The topic is interesting and suitable for publications in ACP. As a reader I do have however problems to figure out what I learned from this manuscript especially regarding the distinction in climate and demographic impacts. The manuscript is very lengthy and not well structured. As the simulations have been analysed in great detail in previous publications, I suggest to significantly shorten some of the analysis and parts of the

discussion and refer more to the previous publications.

Major points:

In general throughout the manuscript the distinction between climate and demographic impacts on wildfire air pollution hazards during the 21st century is hard to follow (see various comments below). The climate and demographic impacts on fire activity during the 21st century have been discussed in detail in earlier publications (Knorr et al.,2016ab) based on the same simulations but focusing on fire carbon emissions and burned area. Even though this paper focuses on PM2.5 emissions I'd assume that the major conclusions would be the same and suggest to focus this paper solely on the changes in wildfire air pollution hazards during the 21st century in relationship to changes in anthropogenic emissions.

Abstract: Line 32: The importance of wildfire emissions are in the abstract mainly discussed relative to the changes in anthropogenic emissions, but not in the light of changing fire emissions. E.g. wildfire emissions decline drastically in the future for Sub-Saharan Africa according to your simulations. This should be discussed more carefully in the abstract. Line 37: will this not suffice because wildfire emissions are high anyway, because they become enhanced in the future, or they are not strongly enough reduced? Please, be more precise.

Method: LPJ-GUESS-SPITFIRE results as published in Knorr et al., 2016ab are used to scale GFED4s observation based PM2.5 emissions into the future. Two scalars are introduced a population and a climate, vegetation scalar. The simulations introduced in Knorr et al., 2016b includes a simulation in which only the climate/vegetation effect is accounted for. Couldn't this simulation be used to distinguish between the population and climate impact. The population scalar is defined for each grid box, while the climate, vegetation scalar is averaged per country/region. How will this impact the results? Also Knorr et al., 2016c did not use a climate and population scalar. Does this lead to different results for Europe?

3.2. Simulated changes in emissions In discussing the trend in simulated changes in PM2.5 emissions it is for various regions differentiated whether the trend is dominated by climate or population changes (e.g. line 28-21, 326, 339, 317,353, 443,472,674). Given the results discussed in the manuscript I do not see how the overall simulated trend can be disentangled in climate and population driven. While the method section introduces a population and vegetation/climate scalar, this is not further used or shown in the result section.

Line 305: isn't it sufficient to discuss the changes in scaled GFEDv4 emissions? The changes from the simulations with LPJ-GUESS-SPITFIRE have been discussed in previous publications (Knorr et al., 2016ab). I suggest to shorten this section and refer more to your analysis made in previous publications.

Line 330: Similar to what?

Line363: The changes in scaled GFED emissions should be discussed in a new section.

Line370: country/region

The results in many cases are discussed along with changes in population density of the different SSP scenarios. Here a figure illustrating the changes in the single SSP scenarios considered in e.g. the Appendix would be helpful for the reader. Figure A3 partly shows this, but is not referenced in section 3.2.

3.3. Predicted changes in emissions by population density I do have problems to follow the arguments in this section. Line397:. Why is it important that the decline is absent in RCP85/SSP5 and why does an increase in population density within a given category lead to more fire suppression? What is the relation to Figure 1 and A2?

Line 399: why does woody encroachment get important here. It is not discussed anywhere else.

Line 409: From the figure I'd think that High-Income Europe shows the same pattern.

Changes in population impact the changes in emissions by population density because: (1) wildfire activity changes with changing population density and (2) regional changes in population distribution. On top changes in climate will change the regional distribution of fire prone areas. Is there a way from your simulations to disentangle these factors more quantitatively than discussing Figure 7?

3.4. Future patterns of pollutant exposure

Line444: Many area in Africa? Line446: I can not identify an additional zone further south in Figure 9. Line448: how do you distinguish between climate and demographic control. Line452: It is impossible to identify any change in Portugal from Figure9. Figure 9: To include a category exceeded under current conditions would be helpful.

The analysis discussed in the paragraph line 456 and following is the most relevant in the manuscript. I'd suggest to include these results in the abstract.

4. Discussion

Line 512: The discussion of simulation results for Western US seems a bit out of context. I suggest to remove it.

Line 527: "broadly reconfirms" this is misleading. All studies (Knorr 2016abc) and this one are based on the same simulations.

Line 534: wildfire risk to humans – how do you define this? Line 537: climate and vegetation changes – you did not discuss vegetation changes Line 539: the approaches – which approaches? Line 552: Both studies? Which studies? Does Giglio et al. use country information? Also as for as I understand does this study only scale the climate/vegetation part by country/region. The population scaling is done on a gridbox level. Line559: Do not understand why this explains that deforestation and peat fires are excluded.

Line 579: in accordance with the results of Andela and van der Werf (201X). The increase that was discussed by Andela and van der Werf is caused by the analysis of

a timeperiod with an incomplete El Nino cycle it should be not interpreted as a trend.

Line 580:"are broadly representative" you compared with one study in Africa and one in the western US. This is not enough to come to any conclusion on the representativeness of the model as a whole. Given that the results of the model have been used in previous studies I'd expect that the evaluation of the model has been discussed in more detail in these studies.

Line 597: "This is opposite to what we find .." Is this opposite for all regions or only sub-Saharan Africa. Please be more precise.

Line 652: " and climate change will lead to new areas" the impact of climate will be the same for MFR and CLE.

Line 646: The discussion of the emission inventories by Kaiser and van der Werf seems out of context.

Line 655: changes?

Minor comments:

Line 51: ". . . due to climate change efforts to reduce anthropogenic emissions . . .. "I do not understand the connection.

Line 53: Reference Flanning et al., 2012 is missing.

Line56: please specify the regions these studies are for.

Line 132: climate change → emission

Line 171: the reference to figure A1 is not clear to me. Figure A1 would be helpful in the result section.

Line 265: Given that the manuscript is already quite long. I'd suggest to leave out the NOx and CO analysis. As the authors state, anthropogenic sources are dominant.

Line 287: proper ?

Line 296: Threshold of 1 inh./km2 to distinguish between anthropogenic and wildfire dominance. I can not identify this in Figure 3. Is this for all regions?

Line 305: I do not get how this differs. When the trend of LPJ-GUESS-SPITFIRE is used to scale GFEDv4. The relative temporal changes should be the same.

Line316: Demographic trends are by far the dominant driver, while the differences in climate scenarios are minor. I do not see how you draw this conclusion from the presented results. Your have two simulations with different climate projections, but no simulation without the climate or population effect. That the differences between RCP45 and RCP85 are only minor, does not imply that the climate impact is not important.

Line 326: .. fire activity driven by population trends. Similar to comment line 316.

Line 334: .. climate is the dominant driver. Similar to comment line 316.

Line 363: The comparison of LPJ-GUESS-SIMFIRE and GFEDv4s does not fit so well in the section 'emission changes' as it discusses only the present day emissions. Maybe this could go into the method section?

Line 442: For which timeperiod?

Line 444: .. but do not exceed them in 2090 ?

Line 464: scenario → threshold

Line481: not sure I understand what you mean. Area that are exceeding verus areas that will fall below the threshold.

Line 515: Doer and Santin does not discuss the future.

5 Summary and Conclusions

I'd suggest the remove the first paragraph on future fire emission for Western US. This is not the focus of the study.

Typos: Line 340: of? Line 376: Africa, South Africa Line 377: shows Line 960: and

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