# Author reply: Climate changes and wildfire emissions of atmospheric pollutants in Europe by W. Knorr et al.

*Referee comments in italics*

We would like to thank both referees for their thorough reading of the manuscript and for their very detailed, constructive and useful comments, which show their dedication to improving this manuscript.

**Response to comments by anonymous referee #1**

*1) The relatively new aspect is thereby the combined assessment of anthropogenic emissions and wildfire emissions and the assessment of air quality impacts. This should be reflected more in the title of the manuscripts.*

Reply: We had thought a lot about the title, which needs to describe a chain of events: climate change driving changes in wildfire occurrence driving changes in emissions. We suggest to change the title to "Air quality impacts of European wildfire emissions in a changing climate". We believe including the comparison with anthropogenic emissions in the title would make it too long.

*2) The manuscript reads in part a bit lengthy and could be shortened (e,g, the discussion on the pros and cons of different fire models). In parts I was confused whether model results or GFEDv4 is discussed.*

Reply: See reply to referee #2's comment 2), which contained detailed suggestions on this point. One aim of the manuscript is to provide a review of the status of fire scenario modelling in Europe, as such a review is not currently available in the literature. We chose to include this in a paper on future emissions rather than a separate review paper because we believe that the former sets the context for the latter. To help the reader we provide sub-headings of Section 1, so that parts of the introduction can be skipped.

We have clarified this by moving the last paragraph of Subsection 1.1 to the end of Section 1, before the first sub-heading.

*3) Fire model results are used to scale satellite based observed burned area (GFEDv4) into the future. The scaling is done on a country basis. Countries are not related to fire occurrence. Does averaging on a country basis impact your results? Also I was wondering whether SIMFIRE does actually produce fires in all regions of Europe, i.e. do you get a scaling factor for each country in Europe? Here it would also be helpful to show how SIMFIRE actually compares to GFEDv4 in Europe.*

Reply: A detailed comparison of SIMFIRE with GFED is provided by Wu et al. (2015). We have added the following sentence at the end of paragraph 1 of Section 2:

"A comparison of LPJ-GUESS-SIMFIRE burned area for Europe against observations is shown in Wu et al. (2015). Agreement was within 20-50% in most parts of Europe, including the Mediterranean, which is the largest fire-prone region on the continent." Fire occurrence is driven to a considerable degree by management practice (Moritz et al. 2014), as can be seen for example when comparing burned area in Finland with that in north-western Russia (Fig. 2 in Knorr et al. 2014). We therefore scale simulated emissions for every pixel in a given country by a uniform scalar.

The reviewer is right that in some cases, the model might not simulate any fire for a given countries. This is indeed the case for Moldova, which we have excluded from the analysis because the prediction did not yield valid results (see Table 3). We have added a statement to explain this in the new Section 2.4, first paragraph. "reading what?"

*4) What about future landuse change? Is this considered in SIMFIRE?*

Reply: SIMFIRE considers human impact through a statistical approach related to population density, which includes land use. Since the simulations are based on a model trained on recent data, we implicitly assume that the relationship between land use practice and population density is invariant over time. A statement has been added to clarify this to the first paragraph of Section 2:

" The effect of changing land use is considered implicitly by the use of population density (Knorr et al. 2016a, b)."

*5) Regarding the chemical species: Do you use the species provided by GFEDv4 and apply the emissions factors or Andreae and Merlet only to your model results, or are the emission factors applied to both? Is this consistent?*

Reply: There is indeed a slight inconsistency here, which however does not affect the results. GFED uses emission factors by Akagi et al. 2011, but SIMFIRE those by Andreae and Merlet, albeit with a recent update (Knorr et al. 2012). Since from SIMFIRE we only use the spatio-temporal changes and not the absolute emissions, the only case where this could affect emissions is when the biome category of a pixel changes over time. Since, however, all of Europe is assigned "extra-tropical forest" for all of the simulation period, this does not affect the results and therefore the emission factors by Andreae and Merlet (and differences with Akagi) are eliminated in the scaling. In order to increase clarity, and because this is mathematically correct, we remove mention of the Andreae et al emission factors and explain the general scaling approach in the first paragraph of Section 2.

*Minor comments*

*6) Line 155: were does the number two come from? Does this refer to Table1?*

Reply: We had discarded Scholze et al. (2006), because it does not specifically show any burned area, but of course simulation of carbon emissions also implies simulation of burned area (usually). We have therefore replaced the sentence in question by:

"Most of the early predictions of future fire activity did not simulate burned area, with the exception of Scholze et al. (2006), which however only reports probability of change. For example, the pioneering ..."

*7) Line 238: Emission factors by Andrea and Merlet: Many studies use emission factors by Akagi et al. For completeness it would be nice to document the emissions factors applied in this study and compare them to the one given by Akagi et al.*

The emissions factors used do not influence the results, See reply under 5 for detailed information).

*8) Line 308: Please explain the different Pegasus scenarios used in the Table.*

Reply: These were explained in the footnotes of Table 2. We have added a reference to the table and moved the description to a separate column.

*9) Line 355: Knorr et al. ? – please complete.*

Reply: We meant to refer to Knorr et al. (in review), but this paper has now been accepted (Knorr et al. 2016b).

*10) Line 355: Figure1/Figure2. Are the wildfire emissions in Figure1 and Figure 2 from SIMFIRE or from GFEDv4? I thought the climatological mean refers to GFEDv4. In this case, however, I do not understand the discussion on SIMFIRE here.*

Reply: Correct, this is a discussion of GFEDv4.1s emissions, hence the average of 1997-2014. What was meant here was the peaked function describing average wildfire emissions against population density, where emissions first increase with population density despite of the result reported in Knorr et al. (2014) that burned area (driving emissions) almost always declines with increasing population density because the fire regime is ignition saturated (Guyette et al. 2002). This has to do with the fact that population density is also correlated with other factors driving burned area or emissions, e.g. plant productivity. A discussion of this is provided by Bistinas et al. (2014) and in Knorr et al. (2016b). We feel that a discussion of this and of ignition saturated fire regimes would be out of topic and we decided not to expand this here.

We have modified the text as follows (Section 3.1, first paragraph):

"The decline of total fire emissions towards dense population found in the GFED4.1s data (Figure 1) is consistent with the SIMFIRE model, which predicts generally declining burned area with increasing population density. By contrast, the declining emissions from a peak at intermediate values towards low population values at first sight seem to contradict the assumptions made in SIMFIRE, which assumes burned area being largest in these low population regions. In some cases, there might only be a very small increase in burned with increasing population density at very low population density (ca. 3 inhabitants / km$^2$, Guyette et al. 2002). However, co-variation of other environmental variables that drive fire occurrence with population density (Bistinas et al. 2014, Knorr et al. 2016b) explain why the more complex relationship seen in Figure 1 is consistent with the model formulation. Furthermore, areas with fewer than 3 inhabitants / km$^2$ (see Appendix, Figure A1) are all situated in boreal regions or northern highlands, with low fire occurrence (Giglio et al. 2013)."

*11) Line 359: Are the climatological means comparable for Portugal and Russia, or the single large wildfires events in these regions. Please clarify.*

Reply: Yes, this was not clear. It refers to the climatological average, but during the respective peak month of the fire season, which is August for both. The amount is about 0.1g/(m$^2$ month) for the region around Moscow, and about 0.4 for northern Portugal. This is remarkable, as the Russian value is likely dominated by a single event, whereas

Portugal experiences frequent fire events, albeit with 2003 and 2005 more than twice the average annual burned area of 1980 to 2012 (JRC 2013).

We have reformulated the text to:

" ..., we find August climatological CO emissions for the area near Moscow – where large, devastating wildfires occurred in July and August 2010 (Kaiser et al. 2010) – to be of comparable magnitude to the climatological emissions of northern Portugal, with its large and frequent wildfire events (JRC 2013) ."

*12) Line 372: I'm not sure I understand this. Fire emissions you have monthly, but anthropogenic emissions only annual? For the annual anthropogenic emissions the 'residential and commercial' sector is excluded when calculating the contribution of wildfire emissions in the peak burning season? Please clarify this.*

Reply: Yes, anthropogenic emissions were only available on an annual basis. Therefore, we employ a simplified model of the seasonal cycle of anthropogenic emissions, which assumes that emissions from room heating in the 'residential and commercial' sector (which concerns only small-scale commercial installation and could be heating of office blocks or schools for example, but also gas cooking stoves, which we neglect here) are zero during the fire season, while other emissions have no seasonal cycle. Therefore, the average monthly anthropogenic emissions during the fire season equal (annual emissions - emissions from residential and small-scale commercial combustion) / 12. This has been clarified (last paragraph of Section 2).

*13) Line 398: The paragraph on the relative importance of different regions for the total wildfire emissions in Europe would fit better into the previous section were the climatological mean is discussed and not so much in the 'predicted change' paragraph.*

Reply: Thank you for the suggestion. We have moved the paragraph in question to the end of Section 3.2.

*14) Line 424: Do these numbers refer to Table 3? Please, check.*

Reply: The ensemble maximum (last column in Table 3) states +211% for Greece by 2090, +301% for Italy, and +143% for Portugal. This was probably an oversight, as the line below Portugal states +303% for Romania, an increase from a much lower base, though. We have corrected and clarified this:

"... indicate that Portugal cold more than double, Greece triple and Italy quadruple its wildfire emissions ... (Table 3)."

*15) Line 449: Please rephrase this sentence.*

Reply: Thank you, done:

"Monthly wildfire CO and PM2.5 emission rates during the peak fire season, however, may come close to those from anthropogenic sources for regions with population densities between 3 and 100 inhabitants / km$^2$ (Figure 4)."

*16) Line 458: Why doesn't the change in population contribute to the change in wildfire emissions?*

Reply: Revised to:

" The climate and $CO_2$ effect, and in some areas population decline, lead to higher wildfire emissions compared to present day."

*17) Line 466: How is this consistent with Figure 4?*

Reply: Thank you for spotting this. The temporal change is consistent with Figure 3b. This has been corrected:

"For RCP8.5, there is also a marked emission increase by 2090, consistent with Figure 3b, which occurs across the entire range of population densities."

*18) Line 473: Please rephrase. The paragraph could be moved to the discussion/conclusion.*

Reply: We have moved the paragraph to the beginning of Section 3.4.

*19) Line 506: A mfr for air pollutants does not necessarily relate to less climate change.*

Reply: The sentence refers to the scenario MFR-KZN-450, which includes a 450ppm climate target (hence "stringent climate policy") in addition to MFR. See also reply to comment 8).

*20) Line 546: boundary layer height*

Reply: Thank you, corrected.

*21) Line 561: reported*

Reply: Corrected.

*22) Line 559: I do not understand how derive 1.6 mug/m3.*

Reply: 80% of the long-term average equilibrium concentration of 2 mug/m3, because 2012 had 80% of the long-term average burned area. This has been clarified.

*23) Line 564: why do you consider a level of 3 mug/m3 and not 10 as the WHO does?*

Reply: Because an additional contribution of 3mug/m3 from wildfires could bring the total concentration, including that caused by anthropogenic sources, over the WHO threshold. Added

"... , as it could bring the total concentration above the WHO target."

*24) Line 574: This discussion might be better placed in the conclusion section.*

Reply: Good suggestion. We have moved this so that it appears as the last bullet point of Section 4.

**Response to comments by anonymous reviewer #2**

*1) Page 7, Line 160. "but also no change in fuel load". Incorrect statement. The Pechony and Shindell (2010) fire model does have a dependence on fuel load. I believe it is through sensitivity to changing LAI, but you may need to check the exact formulation with the developers.*

Reply: Correct. Pechony and Shindell (2010) refer to Pechony and Shindell (2009) for methods. According to Equ. (3) therein, flammability (und thus number of fires) is influenced by vegetation density. However, the sentence in question states something else: that one would have to assume constant fuel load and average fire size to use projected numbers of fires as a proxy for future emissions. For clarity, it has been modified to:

"Number of fires, however, is not a suitable indicator of fire emissions, unless one would assume not only constant emission factors and combustion completeness, but also no change in fuel load and average size of fire."

*2) The Methods section needs to be re-written/re-organized/untangled with sub- sections that describe which modeling exercise refers to which specific project goal. Many different datasets are introduced and it is hard to keep a track. At present, the reader is essentially left to work out which experiments and datasets are used for which task. For example, the anthropogenic and fire emissions comparison aspect involves the GFED inventory for present day, which is confusing because the study is initially presented as a dynamic fire prediction project.*

Reply: The dynamic aspect of the study lies in the prediction of biomass combusted, not in the prediction of per-species emission, as in Knorr et al. (2016a). We believe that his has contributed greatly to the confusion and have therefore clarified this in the first paragraph of Section 2, and have removed mention of the SIMFIRE emission factors altogether (see detailed reply to comment 5 by reviewer #1).

In addition, we have re-structured Section 2, introducing sub-sections: 2.1) Simulations, 2.2) Model input data, 2.3) Data for current wildfire and anthropogenic emissions, and 2.4) Method of analysis.

*3) On extension of this point (2), how does the present day dynamic fire prediction scheme compare with GFED inventory? I suspect these results are in one of the Knorr et al. papers but it is not clearly explained where and what is the status of the validation.*

Reply: see reply to comment 3 of referee #1.

*4) How was the CMIP5 data downscaled to 1x1 deg for the fire-vegetation model?*

Reply: This was done as described in used the same data as Ahlström et al. (2012), which is explained in Knorr et al. (2016a), from where we use the dynamic emissions simulations. We have added this information to the present manuscript (end of first paragraph of new Section 2.2).

*5) To the conclusion "The evidence for changes in fire regimes in Europe for the past several decades is not clear enough to attribute any changes to climatic drivers", what statistically robust physical climate changes have occurred in Europe over the period? What has happened to temperature and precipitation, and extreme meteorological events? For example, if not much actual climate change has occurred (yet), then it's obvious that there wouldn't be any climate-driven changes in fire regimes (yet).*

Reply: The region 10°W to 40°E and 30 to 75°N ("Northern Europe" north of 48°N and "Mediterranean Basin" south of 48°N, Harris et al. 2014) has seen an upward temperature trend of 0.1°C/decade for 1901-2009 that is significant at the 95% level for both regions separately, which is also clearly visible in the annual data. There is also a significant upward precipitation trend for Northern Europe of 0.9 (mm/year) / decade. The downward trend for Mediterranean Basin is not significant for CRU, but significant for GPCC. A sentence has been added to the beginning of Section 1.2 to describe this:

"Since the beginning of the 20th century, climate in Europe as been warming by 0.1°C per decade, a trend that is significant at the 95% level. A the same time, there has been a significant increase of annual precipitation by around 0.9 mm per decade in northern Europe, and a decline by between 0.3 and 0.5 mm per decade for southern Europe and Mediterranean Basin, where the higher estimate is also significant (Harris et al. 2014)."

In addition, a discussion of results from a recent publication (Turco et al. 2016) has been added to the last paragraph of Section 1.2:

"High-quality quantitative data on fire occurrence Europe-wide, recompiled in the European Forest Fire Information System (EFFIS), is only available starting from the 1980´s. This is unfortunately just after the previously described drastic increase in fire occurrence for various regions over the Mediterranean basis. Data by EFFIS show a general decreasing trend in burnt area (1985-2011) over the European part of the Mediterranean basin (Spain, France, Italy and Greece), except Portugal where no trend was observed (Turco et al., 2016). However, just as for Greece and a region in Spain, data for Italy show an upward trend during the 1970s. It is hypothesised that the decreasing trend in burned area over the last decades is due to an increased effort in fire management and prevention after the big fires of the 1970´s and 80´s (Turco et al., 2016). "

*6) Page 22, Line 525. "Likewise, the uncertainty in the published range of even the present anthropogenic emissions is of similar relative magnitude". Is this true? Based on this and other studies, seems that uncertainty in wildfire emission estimates must be larger than for anthropogenic sources?*

Reply: Probably yes. However, 2010 total anthropogenic CO emissions range from 15 to 27 Tg/yr for Western and from 6 to 12 Tg/yr for Central Europe (Granier et al. 2011), so uncertainties are of comparable magnitude, even though probably smaller. The statement has been amended accordingly.

*7) What about surface ozone impacts, which depend on the wildfire-anthropogenic emissions interactions?*

Reply: We have added a paragraph to the end of Section 3.4:

"We also estimate that for Europe, ozone ($O_3$) produced from wildfires emissions, a secondary air pollutant (Miranda et al. 2008, Jaffe and Widger 2012), are and will remain below levels that make them relevant for air quality targets. Using a ratio of 3:1 for CO to $O_3$ production for temperate North America, CO emissions for Portugal from Figure 2 and a similar residence time than for PM2.5 (Jaffe and Widger 2012), we estimate a wildfire contribution to the $O_3$ average concentration for Portugal in August of 0.4 μg / m$^3$, one fifth of the corresponding value for PM2.5, while the WHO 8-hour limit of 100 µg / m$^3$ is four times higher than the 24-hour WHO limit for PM2.5 (25 µg / m$^3$). "

*8) Page 15, Line 355. Missing reference year. Page 18, Line 439 delete "more". Page 20, Line 473. delete "with". Page 21, Line 493. "implemented". Page 21, Line 514. delete "wildfires".*

Reply: These have been corrected.

**Other changes to the text**

Correction of Lasslop et al. (2015) reference.

Update of Knorr et al. (2015, in review) to Knorr et al. (2016a).

Added references to Akagi et al. (2012), Ahlström et al. 2012), Jaffe and Wigder (2012).

## References

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