

Interactive comment on “Climate, CO₂, and demographic impacts on global wildfire emissions” by W. Knorr et al.

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Response to comments by anonymous reviewer 2. Reviewer comments in italics.

1) The study of Knorr et al. explores a models sensitivity in fire emissions to climate CO₂ and population density for the 20th and 21th century. The study is based on a large number of simulations which differ in their input datasets. This and the differentiation between differnt degrees of urban population represents a substantial novel aspect.

The manuscript is well-structured with an appriate number and high quality of figures. My major concern is the design of the factorial experiment. Stein and Alpert describe a method for factor separation in numerical simulations. They show that the synergies

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between different factors can be rather large and are important to consider, when quantifying the effect of a variable. The effects of different variables are not simply additive as assumed in eq. 4. The authors here not only negelect the effect of synergies but map them into the effect of fertilization (eq. 6). This procedure can bias the derived effect of CO₂. The way the method is presented here is quite complicated. Following more closely the proposed method by Stein and Alpert (1993) could simplify the presentation and analysis and most importantly remove possible biases. See for instance Calvo and Prentice (2015) for a similar application of the method.

Reply: We thank the reviewer for pointing out the method by Stein and Alpert. After careful consideration, we believe that an analogous setup of our study using the said methodology could be approximated by the following combination of time windows from existing transient simulations:

Climate	CO ₂	population	time window available from existing simulations?
0	0	0	yes
1	0*	0*	no
0	1*	0*	no
0	0	1*	yes
1	1	0*	no
0	1*	1*	yes
1	1	1	yes
2	1*	1*	yes
1	2*	1	no
1	1	2*	no
2	2	1*	yes
1	2*	2*	no
2	2	2	yes

with the following definitions:

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- 0 1901-1930, time-varying input
- 1 1971-2000, time-varying input
- 2 2071-2100, time-varying input
- 0* 1900, fixed input
- 1* 2000, fixed input
- 2* 2100, fixed input

Of the required simulations, the following already exist (number of simulations for different SSP population scenarios in brackets):

- variable climate until 2100; CO₂, population fixed at 2000 values (1x)
- variable climate CO₂ until 2100; population fixed at 2000 values (1x)
- variable climate, CO₂ and population until 2100 (5x);

and the following would still be required:

- variable climate until 2000; CO₂, population fixed at 1900 values (1x)
- variable climate until 1930; CO₂ at 2000 and population at 1900 values (1x)
- variable climate, CO₂ until 2000; population fixed at 1900 values (1x)
- variable climate, population until 2100; CO₂ fixed at 2000 values (5x)
- variable climate, CO₂ until 2000; population fixed at 2100 values (5x)
- variable climate until 2100; CO₂, population fixed at 2100 values (5x).

This setup would therefore require 18 simulations to be added to the existing 7, per Earth System Model (ESM) and Representative Concentration Pathway (RCP) scenario. Since there are 8 ESMs and 2 RCP scenarios, 288 simulations would have to

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be performed in addition to the existing 112. Since all require a 1000-year spin-up, the fact that some do not need to be to run until 2100 creates only insubstantial savings in computer time. But apart from a 250% increase in the computational effort, there is one more fundamental problem here that leads us to believe that employing the method by Stein and Alpert needs some substantial additional thought before it can be implemented in the present context:

The problem is that the method lends itself most naturally to time slice experiments, such as those presented by Martin Calvo and Prentice (2015). But all simulations listed above are transient simulations that have not been run to steady state and therefore depend on initial conditions. They are therefore not directly comparable (cf. input variables listed above with and without *). And because of the transient setup, our sensitivities are derived from temporal changes between two periods (Equ. 4), and not from the difference between time-slice experiments each run to steady state.

In fact, a better representation of the setup in our study than Equ. 4 would be the following: we run a simulation with only climate varying and ask what the change in emission is between two time periods each (1901-1930 vs. 1971-2000 and 1971-2000 vs. 2071-2100). Then we add varying CO₂ and ask the question how much the change in emissions between the two time periods is affected in this particular simulation. This change is attributed to the combination of climate and CO₂. Finally, we also include varying population density and attribute the temporal change to the combined climate, CO₂ and population effect. Then we decompose the effects by subtracting the combined climate and CO₂ from the full effect, and the climate from the combined climate and CO₂ effect. Essentially we are asking the question: what is the additional change in emissions between two set time periods when making an additional input variable time variant instead of constant, starting from climate only via climate and CO₂ to the full effect. What we do not consider is a simulation where climate and population are variable, but CO₂ is fixed. The reason for this omission is that we simply do not consider this setup of interest and that we are not aware of any model study that has used

it.

We agree that the way Equ. 4-7 are presented could easily be misinterpreted to represent single-factor sensitivity experiments that are assumed to add up the full change. This is not the case, because for one we are dealing with a combination of factorial experiments and time change, and also because we do not perturb the system one by one but sequentially. Nevertheless, we have chosen to fix CO₂ and population in the middle of the transient period in order to minimise the size of the difference between the fixed and the variable input.

In order to make this clearer, we can change Equ. 4-7 to the following:

$$E_{T2} = E_{T1} + \Delta E, \quad (1)$$

$$E_{T2}^{p2} = E_{T1}^{p2} + \Delta E^{p2}, \quad (2)$$

$$E_{T2}^{cp2} = E_{T1}^{cp2} + \Delta E^{cp2} \quad (3)$$

with

$$\Delta E = \Delta E_{clim} + \Delta E_{CO2} + \Delta E_{pop}, \quad (4)$$

$$\Delta E^{p2} = \Delta E_{clim}^{p2} + \Delta E_{CO2}^{p2}, \quad (5)$$

$$\Delta E^{cp2} = \Delta E_{clim}^{cp2}. \quad (6)$$

A further note on the method by Stein and Alpert: In our opinion, their method, just as the sequential-perturbation / time change method used here, simply asks a concrete question based on finite differences: What if input A changes by a given amount, what if input B changes, and what if both change? (And analogous for more than two separately varied input fields.) The interaction term is then simply the change in output when both input variables change minus the sum of the changes when only one input is modified. (Or equivalent for more input variables). Even though the equations presented by Stein and Alpert resemble those of a Taylor expansion (and each term

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can in fact be expressed as one sub-set of the Taylor series), it is important to note that their exchange term is not the same as the two-dimensional cross term a_{xy} of the Taylor series:

$$f(x + \Delta x, y + \Delta y) = f(x, y) + a_x \Delta x + a_y \Delta y + a_{xx} \Delta x^2 + a_{yy} \Delta y^2 + a_{xy} \Delta x \Delta y + \dots \quad (7)$$

and for that reason we disagree with the notion that their terms are necessarily easier to interpret than the ones we use. As Stein and Alpert note in their article, a difference field derived from output between two experiments that vary in one input field can be difficult to interpret when the underlying model is non-linear. However, we believe that their method does not solve this issue, but only offers one possible gradual improvement. (Another possibility would for example be to derive additional Taylor expansion coefficients based on finite difference approximations, such as a_{xx} , a_{yy} and a_{xy} .) Given finite resources, we believe that investing them into a larger ESM ensemble has been the better choice than employing the method by Stein and Alpert. Doing this in conjunction with transient simulations could be the subject of a later study, but would probably involve the output from only one or two climate models.

2) Another assumption that has the potential to change the explored trends is the crop mask. Regions with high changes in crop cover often show strong changes in fire, these regions are masked in the present study although at least on the regional scale these gridcells might strongly contribute to the trends. See for instance Andela et al. (2014) for the importance of cropland cover increase on trends in Africa. Also a number of studies on cropland abandonment exist.

Reply: We have deliberately chosen to keep the crop mask constant because we wanted to avoid accounting for the direct effect of wildland being replaced by cropland. This would have either required the inclusion of a model for agricultural burning, which we do not have, or would constitute a trivial effect when — as here — we only consider wildfires, because they cannot occur on croplands solely by definition. However, we believe that the main effect of cropland expansion is not limited to suppressing

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wildfires on croplands, but also due to the infrastructure they tend to be accompanied by, which creates numerous barriers to fire spread. This is discussed extensively and demonstrated on the basis of historical tree ring data by Guyette et al. (2002), and also noted by Andela and van der Werf (2014; 3rd line on page 793):

“This rapid initial decline [of burned area as a function of cropland extent] suggests that agricultural activity does not affect burned area only in the land that is actually converted, but also in the remaining savannas. [...], large uncontrolled fires are less likely to occur owing to changes in fire management and lower fuel continuity due to, for example, more developed road networks (Shaffer 2010).”

As far as the effect of land abandonment is concerned (not only cropland abandonment), the study by Moreira et al. (2011) points out that at least in southern Europe, this phenomenon has generally increased flammability of the landscape. This is the reverse of the effect simulated for Africa, but plays a role in Europe where the SSP3 scenario predicts a substantial population decline. These effects are included through the empirically derived shape of the burned-area vs. population density relationship (Equ. 1).

3) *The effect of CO₂ fertilization has been addressed by some previous studies that should be mentioned and discussed (for instance Calvo and Prentice, 2015, Lasslop and Kloster, 2015, Kelley and Harrison, 2014).*

Reply: Thank you for pointing out these studies.

4) *4) Technical Comments p.1502, l. 10: also here synergies are neglected, moreover, can only fuel change or could it also be the fuel combustion completeness.*

Reply: See the reply to this reviewer's Comment 1 on the merits of the Stein and Alpert method. In this case, we express the change in emissions due to burned area change in a first-order forward computation using the previous time slice's ratio between emissions and burned area and the change in burned area attributed to CO₂ change derived

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from a sequential / transitional simulation setup. Synergy terms are not neglected, but do not appear in this configuration. However, we agree that it is important to point out that we summarize all of the remaining emission change due to CO₂ to the fuel effect, including effects that differ from the first-order forward calculation. This calculation therefore only serves as a first approximation for separating these factors.

As for the question of fuel load vs. combustion completeness, the reviewer rightly points at an inconsistency in the formulation: emissions change due to either changes in burned area, or changes in the amount of fuel combusted per area – see for example Figure 6, where this quantity is called "combustible fuel load". By contrast, the text referred to mentions emission changes via changes in "fuel load".

5) *p.1502, l. 17: in order "to"*

Reply: Thank you.

6) *p. 15029, l. 15: there are also some studies indicating an increase at least at the end of the 20th century that are worth mentioning, for instance Mouillot and Field (2005).*

Reply: A comprehensive discussion of the issue of historical changes of fire emissions can be found in van der Werf et al. (2013), which mentions the study by Mouillot and Field (2005) – a historical reconstruction – but also discusses various arguments based on ice core proxies.

7) *p. 15030 l.23: a large part of savannas are used as pasture and fire is used as a tool to avoid woody encroachment. pasture areas may most likely be maintained as grasslands and therefore woody encroachment could actually lead to an increased use of fire to maintain the pastures. Is your model applicable to such systems over such time scales where strong changes in human land use can be expected? As far as I understand the only way you consider land use is by masking cropland areas?*

Reply: SIMFIRE has been trained on actual observations of burned area that reflect the effect of land use. These effects are subsumed under the effect of population

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density. The model by Bistinas et al. (2014) also considers cropland and grazing land as statistical contributing factors for predicting burned area. However, we do not believe that existing scenarios of these factors in conjunction with population density are of sufficient quality yet to be included in these simulations.

Since our simulations are based on a model trained on recent data, we implicitly assume that management practices do not adapt themselves to woody encroachment. However, Wigley et al. (2010) report a strong and rather universal increase in woody vegetation cover based on aerial photography from South Africa between the 1930s and the early 2000s. The increase also happens in lands managed for grazing, despite of the well-known efforts of herders to decrease shrub cover. Furthermore, grazing might even lead to decreased shrub cover, contrary to the efforts of herders, because of over-grazing (Bond and Midgley 2012). There is also evidence for woody thickening over large scales in Australia based on a 27-year long satellite record (Donohue et al. 2009). These observations support our modeling results and we therefore believe that the effects of management tend to be insufficient to counteract a general trend towards woody thickening and that the assumption of temporally invariant management is therefore a reasonable approximation.

p. 15032, l. 25: fire frequency in terms of burned area?

Reply: Yes. Sometimes "fire frequency" is indeed used as number of fires per time interval.

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