Please find below the reponses to the reviews, which include a description of the changes made to the manuscript.
Thanks

REVIEWER #1

This paper describes and evaluates the development and optimization of a global fire model. A key improvement compared to current approaches is a consistent approach to model multi-day fires. The paper also contributes to the understanding of fire drivers. Therefore this work can be considered important and is suitable for publication. The methods are sound and the paper is well written. The second part of the discussion give to much detail about future plans of the authors and reads more like a research proposal from then on. I suggest to remove the parts that simply describe future plans of the authors, while general possibilities of model applications may be mentioned. One rather weak point is that the authors suggest the model to be used for future projections. While the model is evaluated in space and for the interannual variability, the performance of the model on longer time scales especially with respect to the human influence is unclear. The parameters for human ignition and suppression are probably strongly constrained by the current spatial patterns, but may have a strong influence on simulations for the next hundred years.

We thank the reviewer for his feedbacks on the manuscript. We addressed concerns about the discussion beeing to focused on research plans (see below). We maintain that the model is useful to explore fire regimes under future environmental conditions. The human influence is a tricky aspect to model, especially the use of fire in agricultural and land management activities. In HESFIRE, it is dependent on countries' GDP (similar to other fire models), which clearly cannot capture the wide range of factors influencing fire use, as mentionned in the paper (Sect. 2.2.1.2). As the reviewer notes, the GDP influence is probably strongly constrained by current spatial patterns, and may not hold in the future. This "permanence" issue is common when modeling human activities which depend on rather unpredicable factors such as technological development and traditional practices. This is the case of deforestation fires in the tropics and preventive fires in sub-saharan Africa. There's not much ground to believe these practices will closely follow future GDP trends. GDP is certainly part of the equation, however (e.g. technological development for alternatives to fire use, fire suppression capabilities). Most importantly, fire projections using HESFIRE will be relevant despite this issue because they can include the resulting uncertainties, for example driving the model in a scenario with unchanged GDP (current practices assumed to continue in the future).

Title: why earth system? the model only interacts until the level of a vegetation model, no atmospheric or biogeophysical influences are discussed

We understand the reviewers concern but suggest we keep this nomenclature:
HESFIRE represents a number of interactions from human activities, ecosystems and the atmosphere, and although they are mostly one-way interactions, we feel it justifies the Human-Earth System (HES) label of the model. A fire impacts module with the implementation of HESFIRE in DGVM/ESM models is underway, and the name was chosen in anticipation of that too.
As for its use in the title ("an explicit fire model for projections in the coupled Human-Earth System"), it conveys the fact that the model can be used to explore fire regimes under contemporary drivers, including natural drivers (e.g. climate), anthropogenic drivers, and their interactions (e.g. climate change), thus the coupled Human-Earth System.

p. 10788, l. 5: what means normalized from 30-80%? are they normalized between 0 and 1 and below (above) the given thresholds the values are 0 (1)

Yes, this is how they are normalized. It is illustrated in figure 2. We added the normalization equation.

p. 10791, l. 25: As far as I am aware this is also a development of the optimization metric, other studies used least squares approaches. You might add a line to highlight this modification of the optimization metric and why you chose to define the metric by using classes not the actual values

We added a discussion of the optimization metric:
"The optimization metric was defined to minimize classification error across 7 classes of annual burned fraction (interval boundaries: 0, 1, 5, 10, 20, 35, 50+% of the grid-cell), and to maximize the correlation with observed interannual variability. Within each class, grid-cells are attributed continuous values based on linear interpolation: a grid-cell with 3% burned fraction is given the value of 2.5, being in the middle of the 2nd interval boundaries. This classification approach aims at capturing important changes that would have little weight on the optimization if using direct burned fraction value. The difference between 3% and 4% in fire-sensitive tropical forests is probably more relevant to capture than between 33 to 34% in fire-adapted grasslands of northern Australia."

p. 10793, l.1,2: is it reasonable to change the parameters to +50% and -50%? Another approach could be to increase the parameter value according to its variability, e.g. +/- its standard deviation/uncertainty derived by the optimization procedure

A number of studies have used fixed percentage changes, one parameter-change at a time, as we've done (Potter et al., 2001; White et al., 2000; Zaehle and Friend, 2010). There are a number of possible approaches, however (Saltelli et al., 2000), including the use of parameter probability density functions and runs with more than one parameter change to cover sensitivity to interactions between parameters (Quillet et al., 2013). We did run the sensitivity analysis with the standard deviation approach, as suggested, and the results are largely similar. The main change between both methods is in Africa, were some areas are now flagged as most sensitive to the anthropogenic instead of fragmention parameter (Figure 1). This is due to the standard deviation of the fragmentation parameters among the 20 optimization runs being relatively low (black horizontal line in figure 4 of the paper). We feel that this is not accurate, as the low standard deviation is actually due to the model being very sensitive to this parameter, thus finding similar values across optimization runs. Accordingly, we keep the first method in the paper, provide references to similar studies, and mention other approaches.

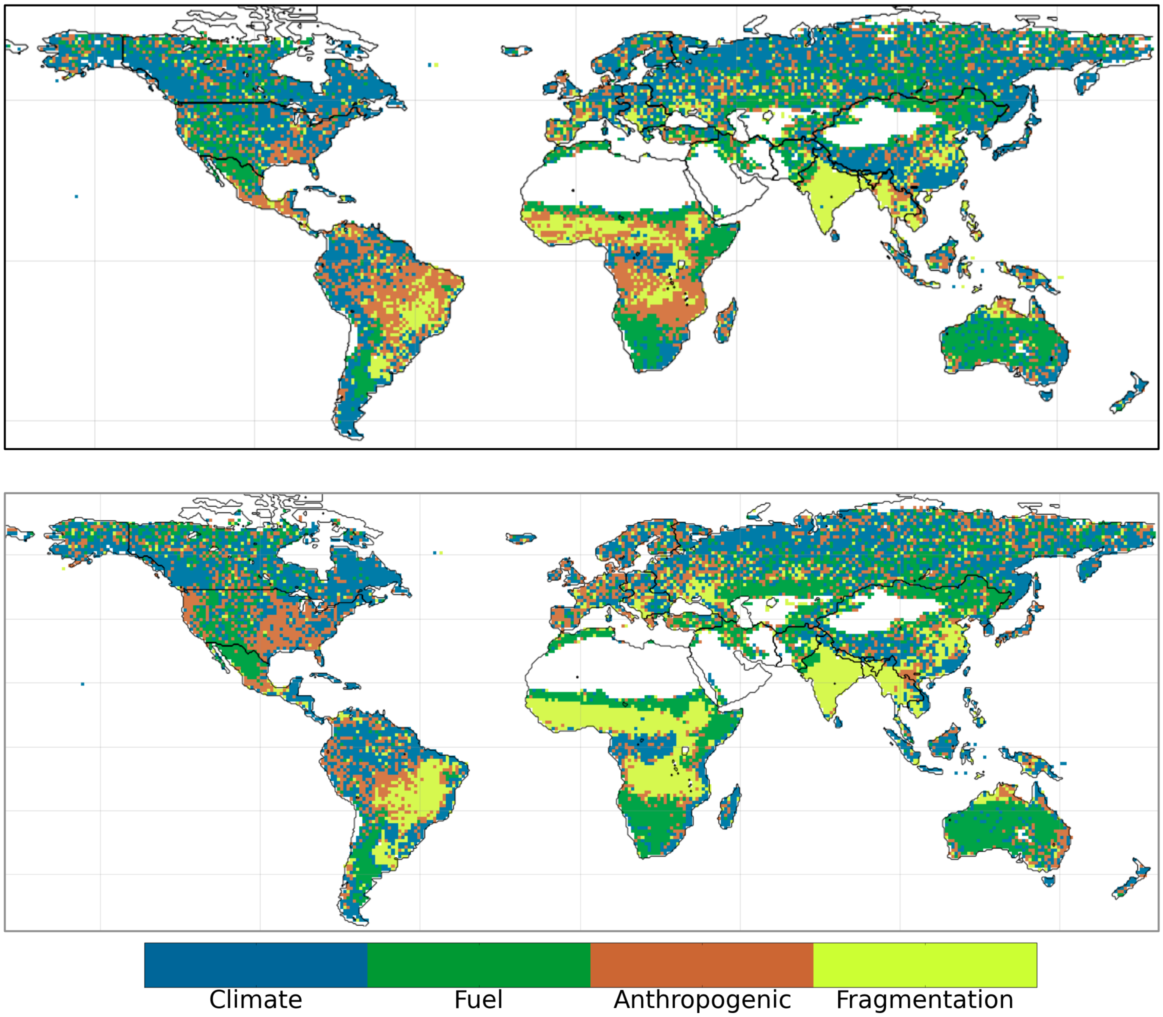

Figure 1. Comparison of the sensitivity map with parameter variation based on +/- std deviation (top) and on +/-50% as applied in the paper (bottom).

p.10792, l. 7-10: please be more precise on the criteria of the gridpoints used in the optimization, a matrix table could be useful here to show the different combinations of criteria represented in the choice of datapoints used in the optimization. Which biomes, which land use densities... were represented ?

As indicated in the manuscript, the gridpoints were selected manually, that is without any statistical method to go through the space of climate/anthropogenic/vegetation conditions. We implemented such a statistical approach but it came out as quite complex to go through the space of all variables, and causing additional issues more than anything. For example, some regions have biased input data (e.g. boreal), which erroneously influenced the optimization early on and were thus largely excluded in the final optimization grid-cell subset. Also, some environmental conditions did not influence the optimization and just a few of the corresponding grid-cells were selected. That includes desert grid-cells for example, which are below the precipitation proxy lower threshold, thus will not have any fires, whatever the optimized parameters. We thus manually selected grid-cells as optimization subsets (this was done roughly, without care for the exact location). Figure 2 shows a map of the subset used for the main optimization (without any grid-cell in South America). Note the sparse grid-cell density in boreal regions, in the Sahara, in the Himalayas, etc. This figure was added to the supplementary material.

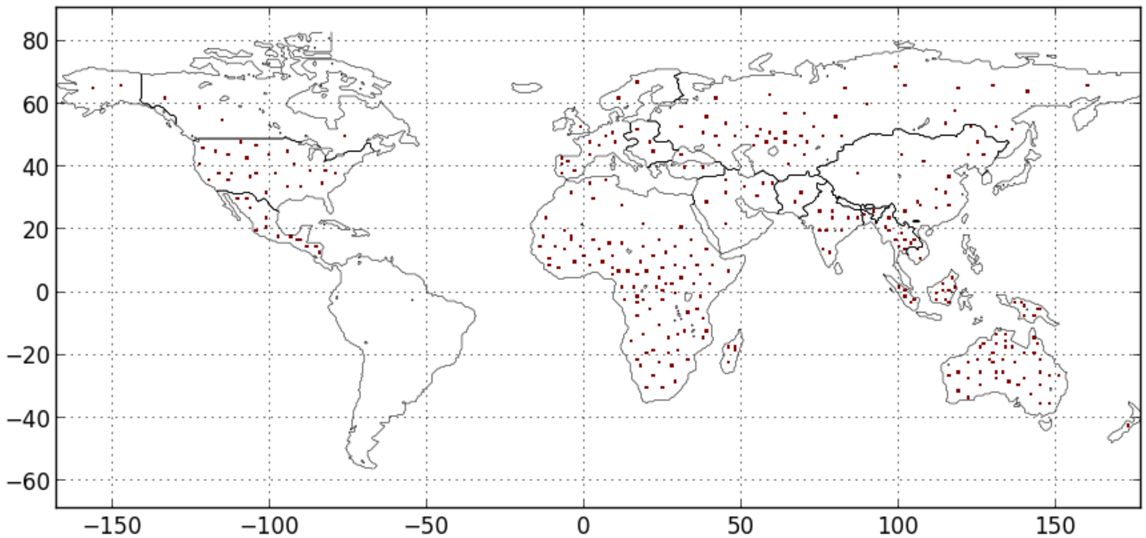

Figure 2. Grid-cell subset used for the main optimization run. Note that no grid-cell was selected over South America, and the selection was sparse over boreal regions (to avoid bias in model parameters due to biased input climate) and over arid regions were fire do not occur (e.g. deserts)

p10795, l. 11: ignition-saturated means to me that more ignitions don't lead to an increase in fire activity. I think here, it just means that more anthropogenic activity (land use) does not result in more ignitions. Moreover, do you really think that ignitions and suppression can be seperated well in your approach?

We indeed intended the meaning that more anthropogenic activities do not result in more ignitions beyond the landuse threshold. We now changed it in the manuscript: "Regarding anthropogenic sources, the optimization procedure suggests that the number of human ignitions saturates at a relatively low landuse fraction, with any additional land use beyond 2–3% of the grid-cell area having no contribution (Fig. 5a)."

Regarding the separation of human influence on ignition and suppression, we agree that it is not necessarily achieved well in the model. GDP and landuse influence both ignitions and suppression, and for GDP, the relationship to fires is negative in both cases. The parameterization can thus easily swap influences between these 2 pathways of GDP fire-driving. This is one of the reasons why we force the ignition-GDP and suppression-GDP parameters to have the same value.

In the presentation of the fire suppression equation (Eq. 12), we added: "Note that GDP_{exp} is the same parameter as in Eq. 3 for human ignitions. GDP has a negative relationship on fires through both ignitions and suppression, leading to an underconstrained optimization if maintaining 2 separate parameters."

p10796, l. 13,14: probably due to the simple representation of fuel.

Indeed, the smoother-than-observed fire incidence patterns in southern-hemisphere Africa are probably due to vegetation classes and the fuel proxy. We now refer to the specific discussion section on this issue.

p10798, l. 17: do integrated assessment models also provide GDP? figure 1. is cut off

Integrated assessment models do provide GDP, at various spatial scale depending on the model (e.g. GCAM divides the world in 14 regions, and we would thus have to apply the same GDP changes to all countries within each region). See Van Vuuren et al. (2011) for the global GDP trajectories from the 4 Representative Concentration Pathways (RCPs) of IPCC AR5 (Figure 3).

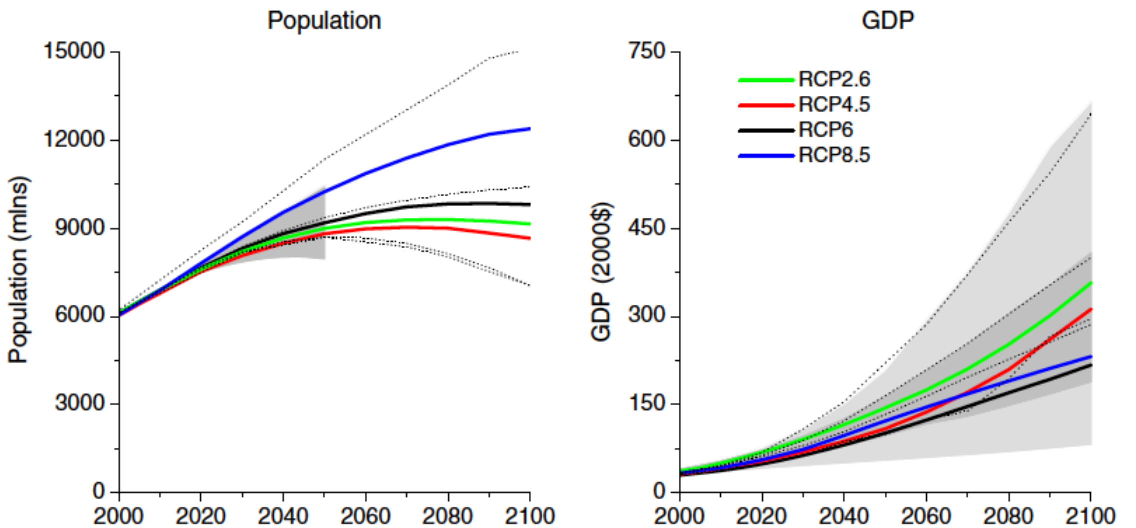

Figure 3. Population and GDP projections of the four scenarios underlying the RCPs. From Van Vuuren et al. (2011).

Figure 1 in the paper (model diagram) was voluntarily cut-off, as a way to show that the model goes on through bi-daily timesteps (Day 1, Night 1 , Day 2, etc), repeating the same computations.

p. 10801 l. 15 ff: In my opinion the description of your future plans should not be described here. The discussion should deal with the results presented here.
p.10801, l. 26/7: same
p. 10802, l. 6 ff: same
p. 10803, l. 12, whole paragraph: This whole paragraph sounds like a research pro-posal, I don't see the benefit of this discussion with respect to your results

We agree with the reviewer that the paper was too heavy on future plans. We also think that identifying major issues in the model, proposing strategies to address them, and discussing potential applications is relevant to a model-description paper. We have substantially revised the discussion section, trying to find an adequate middle ground. As part of that, we re-worded sentences which were focused on our future plans to suggest potential research areas to improve global fire models in general, and Sect. 4.2 has been reduced substantially. The whole section about the potential of regional versions of the model has been removed.

p. 10803 l. 2: I am surprised that here the interest in collaborations is expressed, I think that this is inappropriate here

The mention of potential collaborations was removed from the paper.

Figure 7: the IAV correlation is significant? The correlation is based on the annual Values ?

The correlation is based on the annual burned fraction. We now provide an indication of the significance of the correlation. Note that classic significance tests on goodness-of-fit were not applicable because of the small sample size (14 data points for each region) and non-normal distribution of the data (both in GFED and in the model). We now report the Spearman correlation (ranked correlation), which is not subject to a normal distribution assumption, and indicate its significance for $p<0.05$.

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REVIEWER #2

LePage et al. (2014) have developed a new processed-based global fire model HESFIRE (Human–Earth System FIRE) that aims to provide a tool for investigating what drives current fire activity in terms of climate, ecosystems and anthropogenic activities, and to predict future

changes in fire activity. They used an optimization method to derive best fit parameter values on based on the Metropolis Algorithm and GFED3 burnt area data.
The work demonstrates considerable progress in advancing the field of global-regional fire modelling, especially in the attention given to simulating human-caused ignitions, fire suppression and the effects of land fragmentation and land use on fire. As such, I recommend the paper be accepted for publication after the following comments are taken into account

We thank the reviewer for the helpful comments. Please find our responses below.

1- Please present a more full discussion of the reasons why the model over/underestimates burnt area. You should discuss the effectiveness of the 'stop/start' rules for fire spread in the model. Eqn 8 implies that if $Fuel_{termp}$, $Frag_{termp}$, $Supp_{termp}$ *and* $Weather_{termp}$ *are all zero (as would be the case during the dry season in remote savanna ecosystems under your model assumptions), then fires will continue. This is obviously not the case, and hence, you should more carefully discuss the impact of the following on active fires and fire spread: soil moisture (which you assumed to reflect fuel moisture), topography (refer Pfeiffer et al 2013), fire suppression, and model resolution (which is relatively coarse, 1deg). You attribute the underestimation of fires in Indonesia and the Boreal zone to the relatively coarse resolution of the NCEP climate dataset as model input (2.5 degs), but they are presented in different sections. Clearly, weather operates at much finer resolutions in determining how fast fires spread, and for how long they spread, and thus, final burnt area. In summary, I would like see a paragraph in the discussion that draws together these disparate points regarding the impact of resolution of climate input data and model resolution on fire simulation (and how this may lead to over/underestimation) because it is important for future modelling efforts*

We tried to provide a clearer discussion of the over/underestimation patterns of the model outputs. We feel that we already provided a rather detailed analysis of the main reasons behind regional model discrepancies. For example, we identify data input biases for under-estimation in boreal regions, the weaknesses of the precipitation proxy to capture fuel availability in semi-arid areas, leading to under/overestimation depending on the region, as well as the role of anthropogenic practices and fragmentation/topography factors in some mis-representations. Others were not discussed, such as the use of soil moisture to reflect fuel moisture dynamics. We now mention this aspect in the model description (Eq 4.) and in the discussion section about the model implementation within a dynamic vegetation model with process-based estimates of fuel moisture. As for model resolution, it is unclear to us whether this is a major issue. 1-degree is not that coarse for a global fire model and captures substantial spatial patterns in the fire drivers. Going to higher resolutions on a global scale and with bi-daily timesteps would also be challenged by computational limitations, data input availability and the need to consider fire spreading through neighboring grid-cells.

If possible, we would like to maintain the performance discussion as was submitted, i.e. with 4 sections focusing on what we think are the main issues.

Regarding the start/stop rules, there is virtually no region where all terms of equation 8 would be zero for extended periods of time. Nighttime temperature, humidity and soil moisture would have to be >30degrees, <30% and <0.2, respectively. Additionally, the precipitation proxy has to be >3mm/day over the 12months period applied in the model for $Fuel_{termp}$ to be zero, which further reduces the potential area. Finally, $Frag_{termp}$ being zero implies no landuse, no water bodies, no rocks, and no burned area over the last 8 months in the grid-cell. Recent burned area contributes to the fragmentation index, and thus increases the probability of termination in the case of extended droughts on high fuel load and remote landscapes. We are confident that these rules do not lead to unrealistically long-duration fires: with the final optimized parameters of HESFIRE, the longest fires are 30-40 days, and confined to a few grid-cells in Africa and South America (see figure below, which we added in supplementary material).

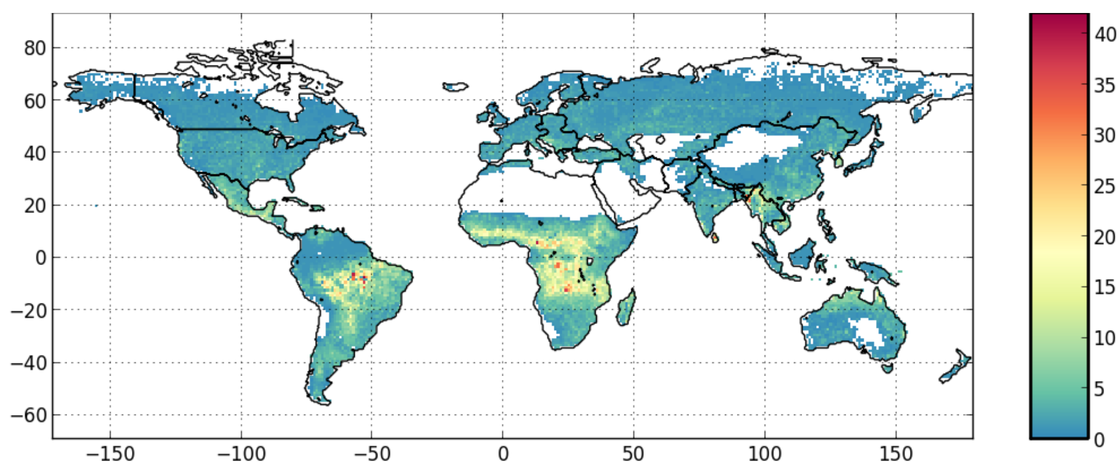


Figure 4. Maximum fire duration for each grid-cell, in days. Note that some grid-cells which do not have any fire in the paper do have fires in this figure: we re-ran the model and the stochastic modeling of ignition success means that successive runs are not identical, and fires may occur where they did not before. Note also that long-duration fires that do occur in boreal regions (up to 50+ days, Sedano and Randerson, 2014) are not captured by the model, in line with the climate data bias and other limitations mentioned in the paper.

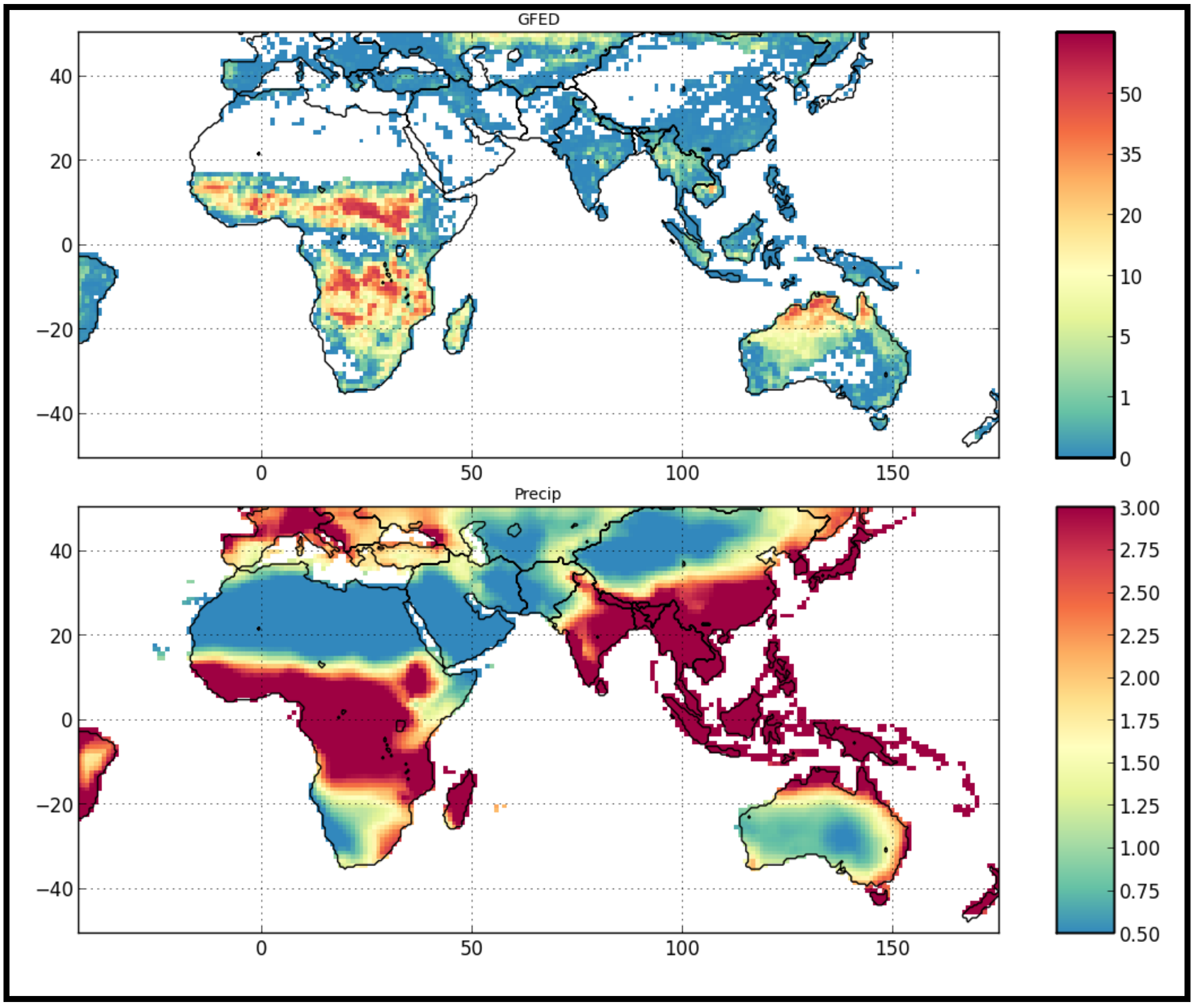
2- Please justify Eqn 9. Why do you take the average precipitation from - 15 to - 3 months ? Why do you normalize between 0.5 and 3mm per day ? There are numerous field studies that demonstrate a relationship between rainfall and fuel load, especially for grass - dominated savanna ecosystems. However, which (if any) report these sorts of values ?

There are indeed a number of studies looking at the relationship between precipitation, fuel and fires. The approach in HESFIRE and associated parameters are derived from these studies, with some adjustments based on data analysis and model performances. Van Wilgen et al. (2004) use average precipitation over 2 years, applied in Archibald et al. (2009) for Southern Africa from July 2001 to June 2003, with a fire season typically from May to

September. Greenville et al. (2009) use several precipitation indices for arid grasslands of central Australia, including average precipitation over 1 year and over 2 years. In a study covering the tropics and sub-tropics (van der Werf et al., 2008), precipitation over a 13-month period preceding the peak fire month was used.

In HESFIRE, we use a 12-month period, ending 3 months before the day being considered. We tried several parameterizations and this was the one leading to the best fit in semi-arid regions, albeit with little sensitivity of the model performances to the duration and position of the averaging window within the values reported above. We now refer to these studies in the paper.

For the normalization range (0.5-3mm/day), we explored the data (figure 2), and also performed model optimizations with different values to select the best range. Figure 2 is now included in supplementary material.



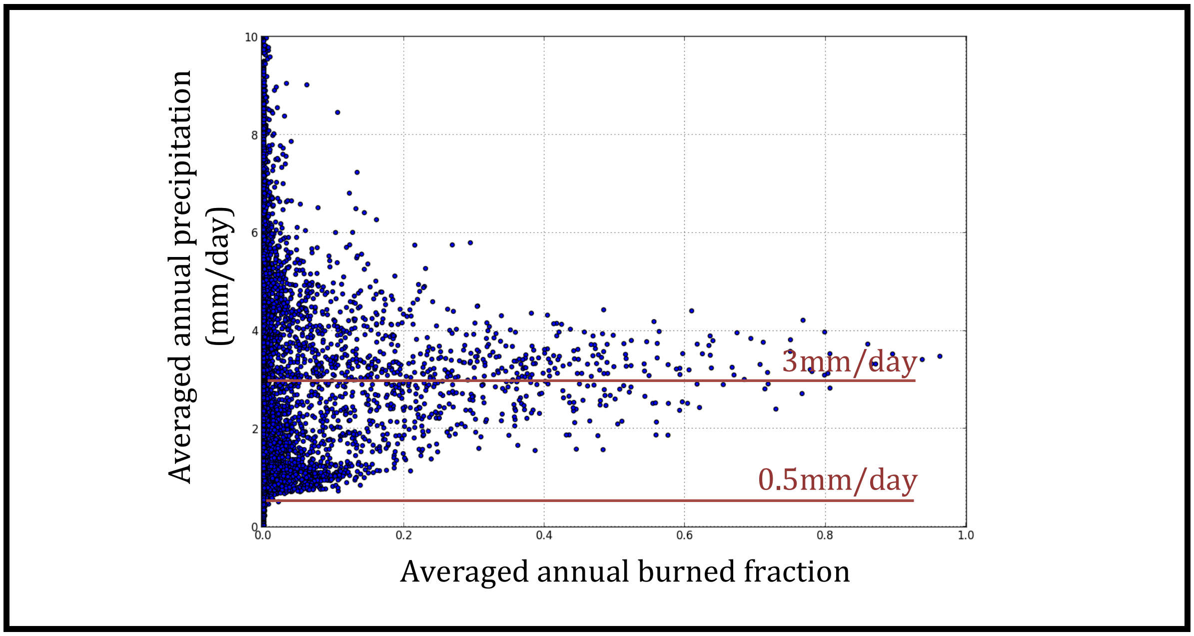


Figure 5. Top: GFED averaged annual burned fraction. Middle: GPCP averaged annual precipitation within the 0.5-3mm/day range. Bottom: scatter plot showing the distribution of GFED burned fraction versus GPCP precipitation.

3- Please justify Eqn 12. Why is fire intensity written this way? What is the literature evidence for the form of this equation? I cannot see how the units for fire intensity (kW per m) can be derived from your equation. How do your values for fire intensity compare against observations ?

Equation 12 defines a "fire intensity" index based on weather and fuel conditions, we agree it isn't necessarily a good name. It was designed to implement a feedback on the ease of fire suppression. It doesn't estimate the actual fire intensity in kW/m. Consequently, we renamed that index to "fire suppressibility". The variables used in that index clearly play a role in fire suppressibility: the more fuel and the drier/windier the conditions, the more difficult a fire is to suppress. However there is little guidance on these relationships, especially for fire suppressibility, but also for fire intensity. Previous studies are limited and have mostly focused on process-based modeling (Rothermel and Forest, 1972; Thonicke et al., 2010). Our approach is thus a simple combination of the fuel and weather variables that have an impact on fire suppression, until more research is done on the subject.

4- Please discuss the accuracy of the MODIS BA product used for model calibration and benchmarking in your study. Previous work has shown that the MODIS burnt area product tends to underestimate fire activity because a 16 day cloud free mosaic is necessary to map the burnt areas (Roy et al. 2008)

The GFED3 database is used to optimize the model, and both the GFED3 and MODIS MCD45A1 are used for the evaluation, for fire incidence/IAV/seasonality and fire size, respectively. Satellite-derived fire observations feature substantial uncertainties, as illustrated by comparisons of different fire datasets (e.g. Giglio et al., 2010). Grid-cell uncertainties in GFED3 is estimated around 10-20% in figure 8 of that paper, however it may actually be larger given the average burned fraction from GFED2 to GFED3 changed by more than 25% in 8 out of 14 global regions. From GFED3 (our study) to GFED4 (newest version), the magnitude of these changes is somewhat lower but still quite large (Giglio et al., 2013). These uncertainties were not acknowledged in the paper and are important to keep in mind for parameterization and model evaluation. We now refer to them in the method/data section:

"The Global Fire Emission Database (GFED, version 3, van der Werf et al., 2010) was used in the optimization procedure as well as to evaluate the representation of fire incidence, seasonality and interannual variability in HESFIRE. The regional distribution of fire was evaluated with observations from the MODIS *MCD45 burned area product* (Roy et al., 2008). *Note that both of these products feature substantial uncertainties (Giglio et al., 2010, 2013; Roy et al., 2008). In the case of burned area from GFED version 3, we consider uncertainties to be roughly 25-50% based on these papers and their comparison between GFED versions 2, 3 and 4."*

5- Please provide much greater justification for the various parameter values you use throughout the manuscript, where possible by reference to previous published studies. For example, why is GDP per capita set at 60k USD? Countries like Canada, Australia, and USA are close to the upper limit of GDP you use, and yet the incidence of human ignitions is relatively high in all three countries but only in particular regions where land use density is high. Does your model take this account? Please explain. Why do human ignitions saturate once 10% of the landscape is saturated? Include an extra column in Table 1 giving the source(s) of each parameter value and ranges used

Similar to the precipitation fuel proxy issue (question 2), we acknowledge that we didn't provide enough details on some of the parameters of the model. As suggested, we added a column in Table 1 with the source of each parameter value, be it literature, data mining, comparison of model performances with alternative parameters, or full optimization procedure.

Regarding GDP, only one country is beyond 60000$/capita, Qatar, and consequently doesn't have human ignitions. Australia, USA and other wealthy countries have some human ignitions, but not that many since they are relatively close to 60000$. It is difficult to assess whether this is realistic without quantitative number to compare to, but the model performs quite well in some areas of these regions with substantial fire incidence (e.g. Australia). Human ignitions saturate at 10% of land use because higher thresholds led to very high values of the optimized parameter LU_{exp}, suggesting the rapid saturation of human ignitions with land use density. These aspects are discussed in the description of Equation 3.

6- I have spotted the following errors in the references. Please correct these and ensure that the reference list matches those in the text

Thanks, we have corrected these errors.

7- Please make sure that all variable names are used consistently through the text. Why do you use variables with subscripts and sometimes not e.g. NATign, FRAGexp? The manuscript would benefit greatly with an extra table describing what each variable name denotes

We revised all variable names to make them consistent. We have also added in table 1 the meaning of each variable name.

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