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).

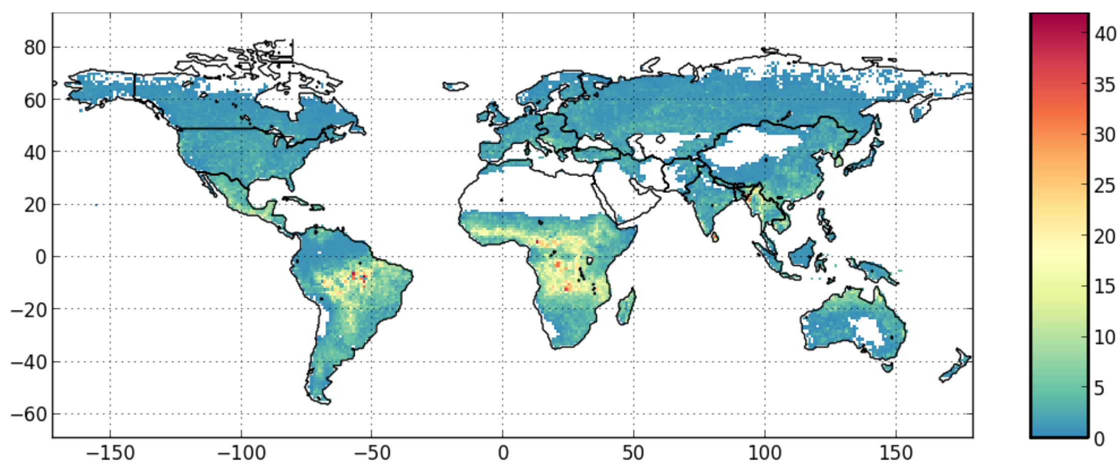


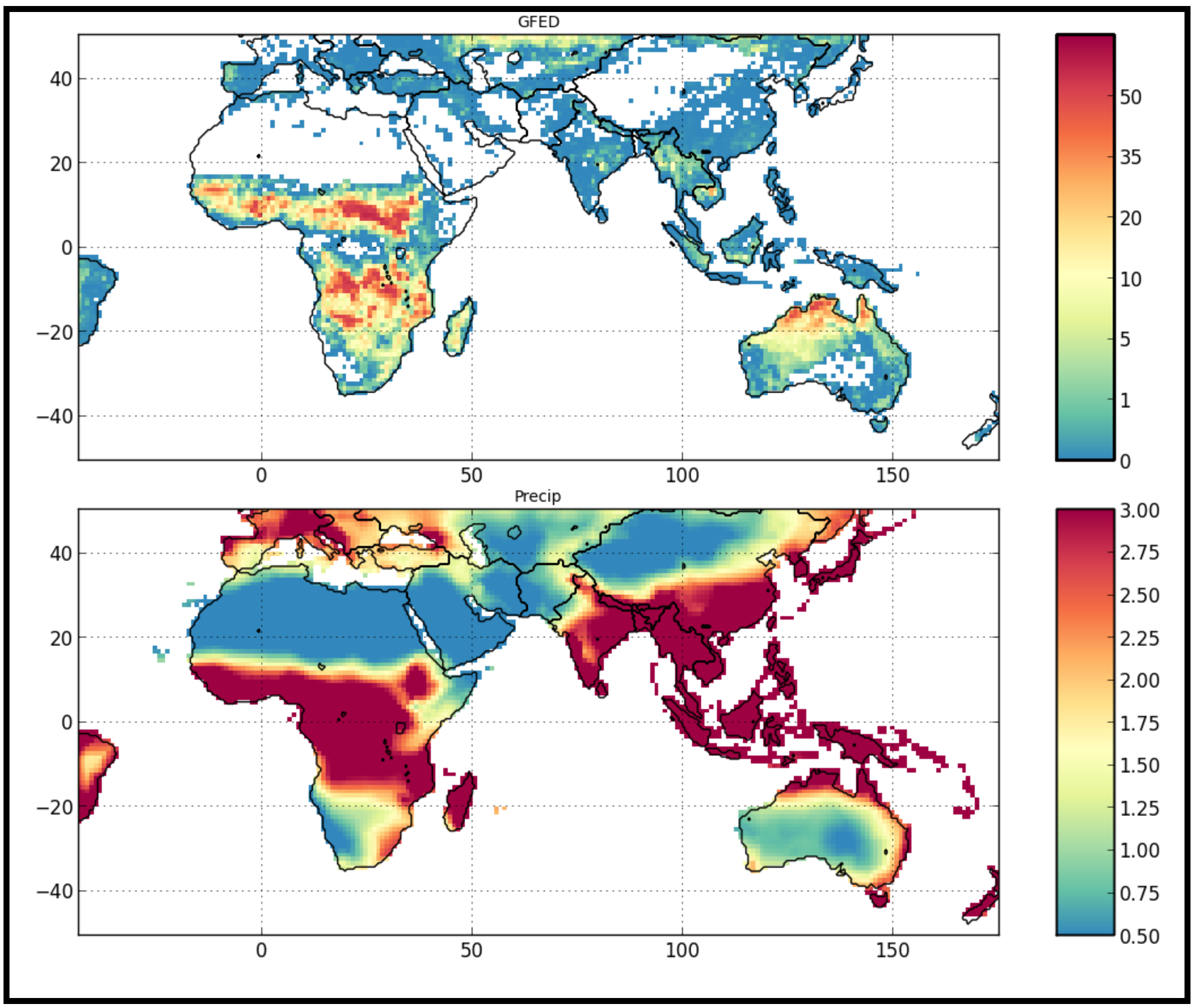
Figure 1. Maximum fire duration for each grid-cell, in days. Note that some grid-cells which do not have any fire in the paper do have fires in this figure: we re-ran the model and the stochastic modeling of ignition success means that successive runs are not identical, and fires may occur where they 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.



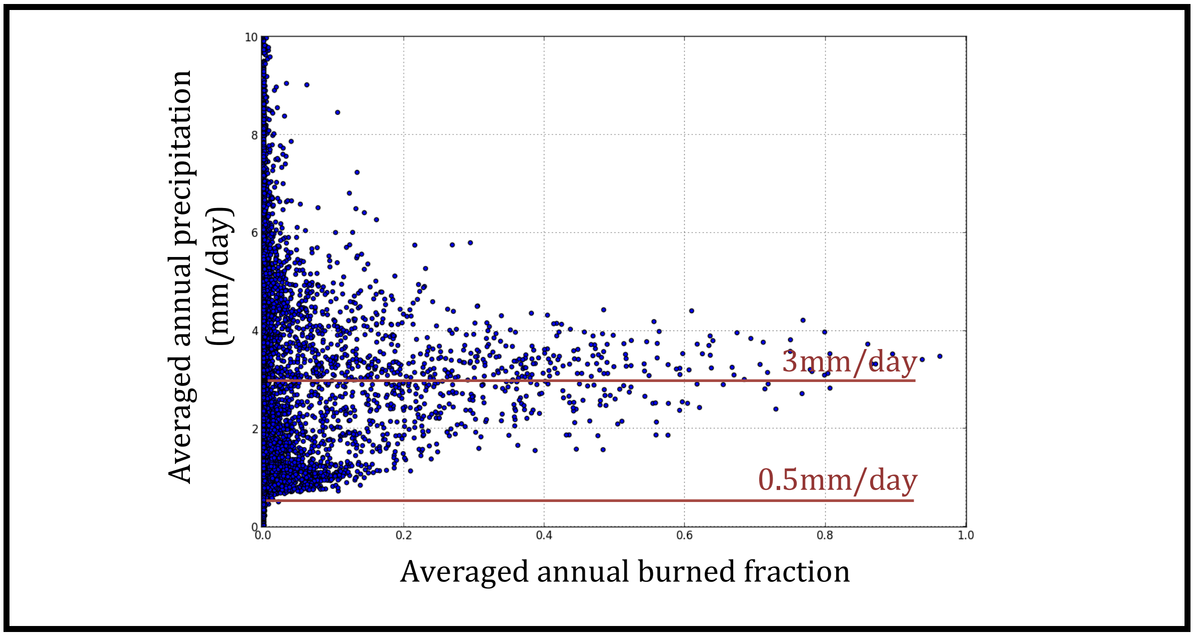


Figure 2. Top: GFED averaged annual burned fraction. Middle: GPCP averaged annual precipitation within the 0.5-3mm/day range. Bottom: scatter plot showing the distribution of GFED burned fraction versus GPCP precipitation.

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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