Editor Initial Decision: Reconsider after major revisions *(14 Nov 2014) by Dr. Sönke Zaehle*
Comments to the Author:
Dear Authors,

many thanks for the revised version. Based on your responses to the reviewer comments and my own reading of the manuscript I see the capability of this manuscript to become publishable in Biogeosciences after you have taken on-board the below major opportunities to improve it.

Thanks for your feedback. We've revised the manuscript accordingly, as detailed in our response below. We highlighted the main changes in the manuscript in red.

Title: The title of a manuscript should confer with its content, not a potential future application. The model that you present is a typical offline model. It may be coupled to an HESM, but since it is currently not, the title of the manuscript is misleading. Please provide a title that corresponds to the manuscript.

We changed the title of the manuscript to:
HESFIRE: a global fire model to explore the role of anthropogenic and weather drivers.

Abstract:
Please limit the abstract to a concise description of your research. The first nine lines of it are pure introduction, without any direct link to your research. They may appear in the Introduction Section, but not here. Please shorten this motivation section of the abstract to one or two sentences.

Abstract P2 L3-4. "in boreal regions and xeric ecosystems, as well as fire size distribution" (otherwise the listing does not work out properly).

Abstract P2 L 4-7. The manuscript does not provide any evidence for the capacity of this model to make projections, and thus this expectation should not be raised in the abstract. The sensitivity of fire activity to what?

We did reduce the part you mention to 2 sentences. We also addressed your other comments, removing the part about projections, and do not mention anymore prospects of integration to DGVMs. We also give further details about the sensitivity analysis. The new abstract reads as follow:

"Vegetation fires are a major driver of ecosystem dynamics and greenhouse gas emissions. Anticipating potential changes in fire activity and their impacts relies first on a realistic model of fire activity (e.g. fire incidence and inter-annual variability) and second on a model accounting for fire impacts (e.g. mortality and emissions). In this paper, we focus on our understanding of fire activity and describe a new fire model, HESFIRE, which integrates the influence of weather, vegetation characteristics, and human activities on fires in a standalone framework. It was developed with a particular emphasis on allowing fires to spread over consecutive days given their major contribution to burned areas in many ecosystems. A subset of the model parameters was calibrated through an optimization procedure using observation data to enhance our knowledge of regional drivers of fire activity and improve the performance of the model on a global scale. Modeled fire activity showed reasonable agreement with observations of burned area, fire seasonality and inter-annual variability in many regions, including for spatial and temporal domains not included in the optimization procedure. Significant discrepancies are investigated, most notably regarding fires in boreal regions, in xeric ecosystems, as well as fire size distribution. The sensitivity of fire activity to model parameters is analyzed to explore the dominance of specific drivers across regions and ecosystems. The characteristics of HESFIRE and the outcome of its evaluation provide insights into the influence of anthropogenic activities, weather and their interactions on fire activity."

Keywords: This paper does not deal with the coupled human-Earth system (even if this is your ambition in the long-term). It simply describes a standalone fire model. Please remove "human-Earth system" from the key-word list.

We have removed human-Earth system, and added "anthropogenic activities" and "weather"

Introduction:

paragraphs 3 and 4. Given that in your paper you do not address future scenarios, this section could be shortened.

We have removed paragraph 3 altogether, and maintained paragraph 4 about other global fire models.

P 5 L 13: I do not see the need to include these references. Neither is this list a complete appreciation of the vegetation patterns simulated by current DGVMs (the papers are more than 10 years old!), nor do the quoted figures make any direct link of this vegetation distribution to fire. The sentence is ok without any other reference than the Scott one.

We removed the references to leave only the Scott one (P4, L15).

P5 L22: Reconsider the use of "fire ecology" (here and hereafter), as in your case this only seems to refer to the length of fires. Your model does not account for any e.g. biodiversity or stand density or size related aspect of fire.

We've revised the use of "fire ecology". We've removed it when referring to the specific case of fire duration. We maintained it when referring to fire ecology in general.
The sentence you refer to now reads as follow: "Our analysis has three main objectives: 1) explicit representation of fire ignition, spread, and termination, without exogenous constrain on fire duration; 2)…" (P5, L1)

Methods:

P7 L5: Remove text in brackets. Please also note that the address you supplied is incorrect. The code is not available when following this link.

"The link is now functional, and we removed the text in brackets. "HESFIRE is coded in Python 2.7 and is available at https://github.com/HESFIRE/HESFIRE1. The optimization procedure is included in the code." (P6, L10)

P8 L7: Add location of the fraction of no-fire-land here. A reader should be able to understand the model without having to go back and forth.

We now provide a more detailed description of what areas cannot sustain a fire in the text: "Areas contributing to fragmentation include croplands, urban areas, water bodies, deserts, as well as areas burned within the last 8 months, the latter to avoid repeated burns within the same fire season." (P7, L14)

P8 L9: Irrelevant in the Methods section. Remove this sentence.

Removed.

Eq 3: Something is wrong here: I've got a hard time understanding why you have an integral in this equation. The term in the integral (according to my understanding) is not a function of LU, but a scalar. There is a second error in this equation, because according to eq 1, LUign should be the result, not the input to this equation. And then, what is Humign? I appreciate that this is a heuristic function, but it's probably worth while plotting this function as supplementary figure, such that one can understand what's going on.

Indeed the function was erroneously reported, apologies for that.

1- First, the name LU_{ign} was assigned to 2 different variables: the number of ignition per km^2 of landuse (remains labeled as LU_{ign} now); and the total number of anthropogenic ignitions in a given grid-cell/day, which was named LU_{ign} in Eq.1 and Humign in Eq.3, and is now labeled as $ANTHROP_{ign}$.
2- In the integral, LU was not supposed to be the actual land use area of the grid-cell, but rather a variable from zero to the actual land use. It is now corrected.

We now provide a plot as a function of landuse and LU_{exp} for a given GDP and LU_{ign} (Figure S1).

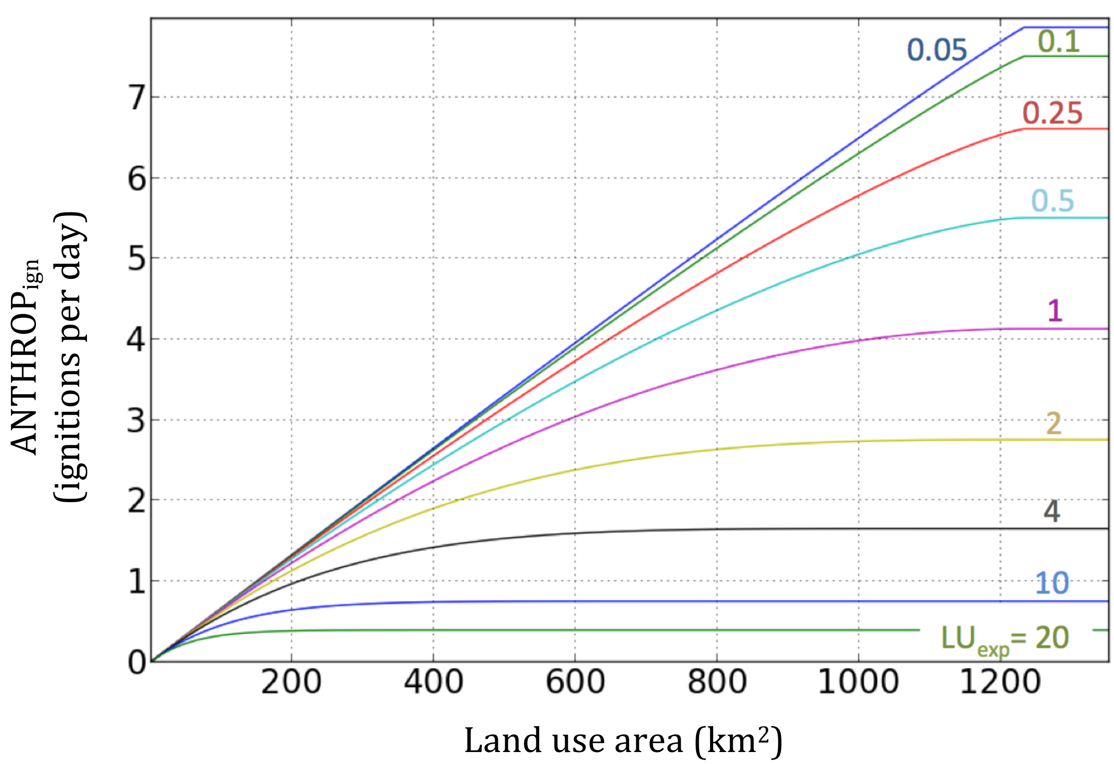

Figure S1. Number of anthropogenic ignitions ($ANTHROP_{ign}$) as a function of land use and LU_{exp}, for a 1-degree grid-cell at the equator (12300km^2). The initial number of ignitions per km^2 (LU_{ign}) and GDP were chosen equal to 0.01 and 20000$ per capita, respectively. They act as simple scalers of the whole function and do not affect its shape (see Eq. 3 and Eq.4). Note that as detailed in the manuscript, anthropogenic ignitions level off at 1230km^2 (land use fraction of 0.1), or earlier with high LU_{exp} values.

Eq 4 Max_rate is undefined.
Max_{rate} is now defined. "Where Max_{rate} is the maximum fire spread rate, constrained by observations…" (P10, L1)

P10 L 5-8 Figure S1 does not allow to constrain any of these parameters.

Indeed it doesn't and we only refer to Table 1 now (P10, L5). Figure S1 just illustrated the type of data mining we used to constrain parameters. It is specific to the precip/fuel relationship. It was more complex in the case of soil moisture, temperature, because these are bi-daily drivers, while precipitation for fuel is monthly. They thus involved mostly the model performance trials method, referred in table 1, while monthly scatter plots as the one shown in Figure S1 were used for a rough initial guess along with values used in other fire models.

P 11 L 10: why is the termination stochastic, if the rules laid out in line 6ff appear to be deterministic?

The factors leading to termination are computed as probabilities. These probabilities vary from 0 to 1, for landscape fragmentation, lack of fuel, and suppression, as described in Eq. 12, 13 and 14. Weather termination probablity is either 0 or 1 (Eq. 11). When combined, these probabilities lead to an overall probability of termination, itself from 0 to 1 (Eq. 10) It is thus not deterministic, fires have xx% chance of extinction, which is applied as a Bernoulli trial to know which of the ongoing fires in the considered grid-cell are actually terminating.

Eq 9: Please provide an equation for Weather_termp
We now provide the equation (Eq. 11)

Eq 10. I share reviewer #2 concern regarding the derivation of this equation. In particular, I cannot follow why fire should respond to rainfall 15 months prior to today - that seems a fairly large memory effect, which according to your response is not (as such) included in any of the cited studies. Please clarify in the manuscript why these values were chosen.

We ended up with -3 to -15 months after a first cut based on the literature (the papers we cite use different memories, from -12 up to -24 months), and its adjustment based on model performance with different values. We modified the explanation in the text, and hopefully it is now clearer (P11, L13):

"The averaging window was determined based on values from the literature (Greenville et al., 2009; Van der Werf et al., 2008; Van Wilgen et al., 2004), which consider a 12- to 24-months window, and adjusted through model performance assessment with different values.

The normalization range was determined based on simple data analysis and parameter value trials (see Table 1 and Figure S1 in supplementary material)."

P12 L5: The replacement of Fuel_exp through information from litter and other carbon pools is non-trivial. Either explain in detail how you would do this, or remove here and outline in the discussion.

We removed that sentence, which is addressed in discussion (Sect. 4.2.).

P12 L9f: How does the model allow for fuel build up? I thought fuel was not explicitly represented?

That wasn't clearly presented indeed. We intended to mean the actual fuel build-up that occurs in the field, not in HESFIRE. Especially in some xeric ecosystems which burn very frequently, and can burn again the following year after grassy fuel builds-up during the wet season.

We removed the reference to fuel build-up (P12, L5): "Areas that cannot sustain natural vegetation fires include croplands, urban areas, water bodies, deserts, and areas than burned up to 8 months prior to the day being considered, thus avoiding repeated burns within the same fire season, but allowing fires in the following fire season if enough precipitation occurs (e.g. in sub-Saharan Africa)."

P13 L 14: What do you need Eq 3 and Section 2.2.1.2 for if LUign is calibrated (and used in Eq 1)?

This question is related to the error we made in writing about LUign and what is now ANTHROPign, sorry about that.

P14 L4: The text says that RH is scaled between 30 and 80%, but Figure 2 suggests that this was scaled between 30 and 70%. Please correct and outline the implications of this error.

Apologies again about this error. Figure 2 was made as a general illustrative figure of the single-parameter functions (exponent parameter) for a presentation at AGU. At the time, we were using a range of 30 to 70% for RH. The version of HESFIRE described in the paper is based on the 30-80% range, as also illustrated in Figure 5e. We updated Figure 2.

P14 L 13: Certainly there would also be convergence problems if reality would be strongly non-monotonic and you try to fit a monotonic function to it?

Problems with convergence would indeed occur if reality was non-monotonic (e.g. fire risk decreasing with RH up to say 60%, then increasing from 60 to 80%). It seems pretty unlikely and we removed the reference to non-monotonic functions. The convergence issue we anticipate with twice as many parameters is due to the exponential growth of the parameter-space the algorithm has to explore. Also, more parameters means more local minima in the optimization index. Adding a 10th parameter to the optimization increases the number of iterations to convergence by a factor roughly 1.5-2. We have not experimented adding even more parameters.

We slightly rephrased that section: "We acknowledge that complex fire driving relationships (e.g. sigmoid) cannot be accounted for here. Exploring such aspects would require 2 or more parameters per driver, which would lead to computational speed and convergence problems during optimization." (P14, L7)

P 15 L 10: Is it relevant or not? relevant according to which objective?
We edited that sentence to make it clearer: "In the context of ecosystem sustainability and fire impacts in general, a difference between 3% and 4% in fire-sensitive tropical forests is more relevant to capture than between 33 and 34% in fire-adapted grasslands of northern Australia." (p15, L7).

P15 L 19: I don't follow this argument. The model does not know about continents, but about temperature, precipitation etc. If you wanted to demonstrate that the model works outside the fitted driver data combinations, then you would need to demonstrate that in the space of the data that you provided the model with. How can you exclude that the drivers of fire in South America are indeed not captured by your selection of points?

The point we wanted to make with that argument wasn't clearly explained. The objective of leaving out an entire continent is to get insights on the performance of the model in a region were fire activity is dependent on specific conditions which are 1) not represented in the optimization subset and/or 2) not represented in the input variables. We left out more than half of the GFED temporal extent from the optimization procedure for similar reasons, e.g. to explore model performances in years with different weather conditions.

The first aspect – conditions not represented in the optimization subset – is somewhat limited, as you point out, because general ecosystem/climate/landuse conditions across South America are found elsewhere in the world (e.g. humid tropical forests, landuse/forest frontier in South East Asia). However, HESFIRE estimates fire activity based not on monthly or annual climate gradients, but on bi-daily data, which are inevitably different from one region to another, and from one year to another (e.g. specific drought events, the dynamics of soil moisture, RH, temperature and lightning strikes through the dry season, etc). GDP is also different, Brazil's GDP is higher than Indonesia or tropical countries of Africa.

The second aspect – conditions not represented in the input data – provides some insights into the suitability of the model structure and input drivers to estimate fire activity. For example, South America has specific agricultural activities, fire practices, landscape fragmentation patterns, which cannot be entirely accounted for by just fractional landuse and GDP. Yet the fire drivers we use lead to relatively good agreement between observation-derived and modeled fire activity.

We believe that analyzing how the model represents spatial patterns in South America, as well as some unique fire events in years not used for optimization (e.g. El Nino) is thus relevant to its evaluation. It would have been very revealing if modeled fire activity in South America was largely over/under-estimated, and we think that if only for that reason, it was important, or at least justified, to setup the optimization procedure as we did.

We rephrased that sentence: "No grid-cells were selected from South America, in order to test the model's ability to reproduce fire patterns under combinations of drivers it might not have encountered during optimization (e.g. Brazil's GDP is higher than other tropical countries in Africa and South East Asia), and under specific conditions that cannot be fully depicted by the model drivers (e.g. fire practices)." (P15, L18)

P16 L2-6: Unless you provide your insight from these different configurations etc. in the discussion, this section is irrelevant and can be removed.

We removed that part.

P 16 L 9: give years

We now specify the timeseries, 1997-2010. (P16, L5)

P 16 L10: I assume you refer to area burnt, not the size of a fire?

We do in fact refer to the distribution of individual fire sizes within a region. In each grid-cell, the model tracks individual fires as they spread, until termination. We thus have an estimate of the size of every fire that is modeled, which is evaluated with the MODIS data. (P16, L6)

P 16 L 13: Why do you not take account of the information from the optimisation procedure and use the standard deviation of these parameters? This would be a much better way to not only talk about model sensitivity, but model uncertainty. A missed opportunity.

We gave some thoughts to that option, and performed the sensitivity analysis using the standard deviation instead of a 50% change (see reply to reviewer #1).

We maintained the original method (+/-50% change), because we feel it makes more sense. Applying the standard deviation leads to what we considered as unwanted bias in the inferred sensitivity. For example, the model is pretty sensitive to the fragmentation index in many regions. For that same reason, it is well constrained: its value doesn't vary much among the 20 optimization runs. Applying the standard deviation to infer sensitivity thus erroneously suggests that the model isn't very sensitive to that parameter.

We feel that the sensitivity map and parameter ranges in Table 1 and Figure 5 provide insights on both sensitivity to parameters and on parameter uncertainty.

P26 L 18-P27 L 6: Given that this is outlook and not discussion of your research per se, please shorten. I also don't see the need for this to have a separate sub-heading.

We've removed future plans altogether, instead mentioning potential applications and development prospects in conclusion. The conclusion now reads as follow:

"This analysis highlights the strengths of the HESFIRE model as well as its limitations and opportunities to address them. The representation of multi-day fires opens the perspective to explore regional sensitivities of fire duration to climate change (e.g. longer droughts). The calibration of the anthropogenic ignition function - suggesting a very rapid saturation of ignitions with land use density – can be applied to gridded land use scenarios to explore potential implications of terrestrial policies for fire activity. Ultimately, exploring interactions between fires, the terrestrial biosphere and the atmosphere relies on frameworks of the coupled Human-Earth System. The data-assimilation methods applied here to infer fire-driver parameters may provide additional guidance for the parameterization of such complex models. The integration of HESFIRE into a dynamic global vegetation model (DGVM) could also bring insights on the contribution of fire-driving assumptions, observation data and DGVM-derived vegetation/fuel characteristics on model performances." (P26, L14)

P28 L 16: Whether or not this piece of software is a relevant tool should be judged about by the people who will now use it - I do not think that it is an appropriate statement to make here. Please rephrase. Also "certain aspects" is very weak - please be more precise.

This sentence was removed as part of the edits made in reply to the previous comment.

Figure 1: To avoid confusion with the cut on the right-hand side of the figure, remove all boxes that are not fully displayed.

We removed partially displayed boxes.

Reviewer #1 responses

#1: Please clarify precisely, how you have addressed this comment (I cannot identify this from e.g. "see below" - where precisely? Your response seems to suggest that there are too many factors, which are relevant, that the model does not take account of. Possibly these factors can be included, but you do not seem to provide any suggestion as to how you would actually do this consistently. So, with this lacking strategy in mind, how can you then conclude that the model's predictions will be relevant also in the future (other than simple sensitivity tests that the available models are equally capable of)?

Apologies for not being more specific, the "see below" was about the discussion being too focused on research plans ("We addressed concerns about the discussion beeing to focused on research plans (see below)." We addressed this concern later in our reply (in response to the comment "p. 10803, l. 12, whole paragraph: This whole paragraph sounds like a research proposal, I don't see the benefit of this discussion with respect to your results").

As for the representation of the human influence, we do indeed discuss the limitation of landuse and GDP as proxies, both in our reply to reviewer #1 and in the text (Paragraph [14]): e.g. "There is not much ground to believe fire practices will closely follow future GDP and land use trends, but they are part of the equation". We highlight ongoing research in that area "Research towards a better representation of broad classes of fire practices is ongoing (Li et al., 2013), and, as mentionned in other studies, fire driver analysis on longer time periods (e.g. with historical reconstruction, (Mouillot and Field, 2005)) would provide further guidance."

We maintain that the model projections will be relevant, especially for simple sensitivity analysis. Other models can indeed do sensitivity analysis, but having multiple models doing so is essential, as illustrated by the knowledge achieved in the many model-intercomparison studies performed in Earth Sciences. Each model also has specific features that can be taken advantage of. For example, HESFIRE may have better capabilities than other models to look at the sensitivity of tropical fire activity to the length of climate anomalies (e.g. droughts), given it models fires over consecutive days.

#2: I agree with the reviewers comment. The title of a manuscript should confer with its content, not a potential future application. There is no such thing as a one-way interaction (contradictio in adjecto). If a affects b, but b does not affect a, then this typically called effect. The model that you present is a typical offline model. It may be coupled to an ESM, but since it is currently not, the title of the manuscript is misleading.

Please revise the manuscript wrt to this. Please also propose an alternative title, which reflects the content of this manuscript, such that the readers get an appropriate impression of the manuscripts topic (the development and evaluation of an offline fire model).

We have changed the title to:
HESFIRE: a global offline fire model to explore the role of anthropogenic and weather drivers on fire activity.

Through our edits in reply to reviewers and to your comments, we think we have now addressed issues regarding Human-Earth system interactions.

#6 I suggest to plot the precipitation-temperature space as an additional supplementary figure, to demonstrate that you've captured the dominant climate regions. I am confused by the omission of South America - given that your objective is to develop an Earth system model - why omitting an entire continent?

We now add the precipitation-temperature space covered by the grid-cell subset used for the main optimization run (Figure S4). The omission of South America is aimed at contributing to the model evaluation, as detailed in our reply to this specific comment.

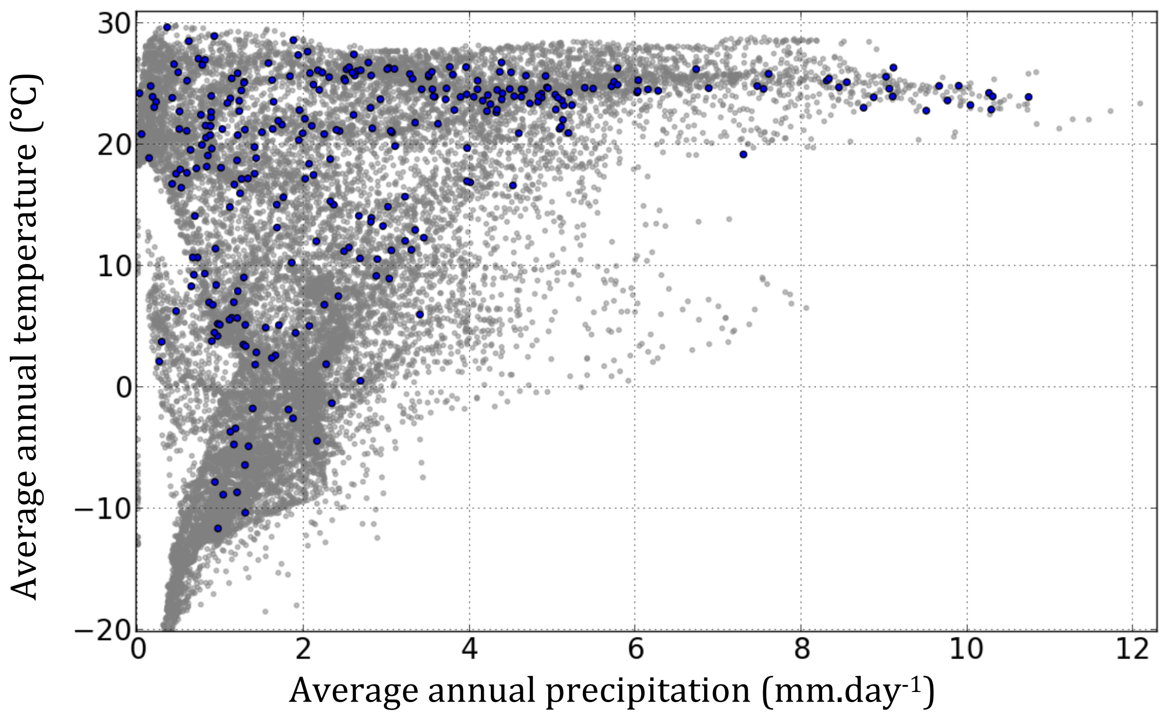

Figure S4. Scatter plot of average annual precipitation and temperature for all terrestrial 1-degree grid-cells (grey) and for the subset used for the main optimization. Note that the area not covered by the subset (0 to 10°C and 4 to 8mm per day) are confined to coastal areas of western Canada. This region was screened out of the subset given the climate input bias mentionned in the text.

#8 Help the editor and identify the sections you change - makes it easier for the editor to believe that what you state has entered the revisions.

Refering to:
Reviewer #1: p10796, l. 13,14: probably due to the simple representation of fuel.
Reply: 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.

Sorry about that. We had edited the sentence the reviewer was referring to, adding a reference to the section discussing fuel proxy limitations: "In Africa and Australia, HESFIRE generally captures high fire incidence in grassland areas, although modeled spatial patterns in Africa are more uniform than observations (probably due to the simple representation of fuel, see sect. 4.1.2)." This is now section 4.2.

#9 Please provide this information in the revised manuscript (the fact, not the figure). See comment on Figure 1 below.

Regarding the fact that GDP is part of the Representative Concentration Pathways provided by Integrated Assessment Models.
We added: "Second, it includes climatic, anthropogenic and vegetation drivers, and the input

variables were chosen so as to enable projections under altered conditions (GDP and landuse are reported in future projections from integrated assessment models (Van Vuuren et al., 2011)." (P22, L12)

Reviewer #2 responses:

#2 Maybe 1° is not coarse for a fire model, but certainly relative to the size of the average fire. Regarding the model-data discrepancy, I would expect that the linear averaging over climate conditions, which have non-linear consequences for the process you model will certainly have implications for its performance. Given that, as you rightly state, you will not be able to increase the resolution in an Earth system modelling context to avoid scale issues, identifying and discussing principle limitation of the model performance against Earth observations is important.

We added a sentence about the resolution aspect in the manuscript, in the discussion section about the limitation of the precipitation-fuel proxy.
"Finally, semi-arid regions generally feature strong precipitation gradients which influence the spatial distribution of vegetation and fuel load, and are not captured accurately by the raw input data (2.5 degree) or through their interpolation to 1-degree." (P24, L19)

The issue is also mentionned elsewhere in the manuscript about moisture dynamics in drained peatlands and degraded forests of South East Asia: "Fires preferentially occur on areas with degraded forests and drained peatlands in Indonesia (Page et al., 2002; Van der Werf et al., 2008), which moisture dynamics is not captured in a 2.5-degree resolution dataset." (P20, L12)

We feel a detailed discussion of the actual scale at which processes occur, their representation at coarser scale in models, and the issue of observation data resolution is not essential to the paper, as this is a problem shared by all Earth sciences models.

#3 Even is this term is renamed, it is still important to justify the shape of the equation (13). If there is no or limited evidence, how can you justify an equation with 4 unknown parameters and an undefined function. Please provide a derivation of this equation and an explanation for the wind function.

Equation 13 (now Eq. 15) was defined by default, as a simple combination of the variables which have an obvious impact on fire intensity, thus its suppressibility. There hasn't been enough research on the subject to justify any specific shape. The function assumes that when conditions are little prone to fires (e.g. RH close to 80%), suppressibility is maximum and the probability of fire suppression depends entirely on the GDP and landuse factors (Eq. 14). When conditions are very fire prone (RH<=30% and strong winds and high temperatures), then suppressibility is minimum and the probability of fire suppression tends towards zero, whatever the GDP and landuse. It seems to us it is better to have that function rather than leaving out the influence of fire conditions on fire suppression, which translates to fixing these 4 unknown parameters to zero.

In the text, after Eq. 15, we added: "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." (P13, L7)

We edited the description of the wind function: "Fires are modeled with an eliptical shape, with higher winds leading to higher fire spread rate and to more elongated fires. The influence of wind, *G(W),* is computed following the method adapted from Arora and Boer (2005) described in Li et al. (2012), as a function of the length-to-breadth (*LB*) and head-to-back (*HB*) ratios of the elliptical fire, both of which depend on wind speed (*w*)." (P10, L6)