**Geoscientific
Model Development
Discussions**

Interactive comment on "SPITFIRE-2: an improved fire module for Dynamic Global Vegetation Models" *by* M. Pfeiffer and J. O. Kaplan

M. Pfeiffer and J. O. Kaplan

mirjam.pfeiffer@epfl.ch

Received and published: 18 January 2013

Answers to comments of Dr. Kirsten Thonicke:

We thank Dr. Thonicke for her helpful comments, which improved our manuscript. Detailed responses to individual comments are provided below.

The following statement on page 2354 "At the beginning of the current study, we attempted to use the equations and guidance provided in the model description of SPITFIRE (Thonicke et al., 2010) plus additional information from the authors (A. Spessa, personal communication, 2011) to implement SPITFIRE into our own version

of LPJ with the aim of simulating the dynamics of natural and human-caused fire over the preindustrial Holocene. However, given the information we had, we were not able to reproduce the model results presented in Thonicke et al. (2010)." describes a very sensitive interaction among scientists: The provision of already tested and published model code by the developers of SPITFIRE to other scientific colleagues for collaboration on similar topics. Those who receive the code have the advantage of not having to develop their own process-based model and can jointly follow-up ideas that cannot all be followed up by the original developers.

The unspecific description of our experiences with the Thonicke et al. (2010) paper is not relevant to the current manuscript and we removed the quoted sentences above. We apologize for any remarks that may have been taken personally.

This happens usually in exchange of ideas and shared co-authorships of both, original developers and new users of the model, with both sides expecting to profit from such synergy effects in joint publications. "Plus additional information from the authors (A.Spessa, per. comm. 2011)" is wrong, because only one author of SPITFIRE was contacted to discuss ideas, so the text should read "A.Spessa was contacted", but the material showing the mismatch to published model results was never send to all co-authors to give them the chance to comment prior publication. The authors must mark throughout the text where ideas that were discussed with A.Spessa were used to develop a specific function in the fire model further or developing a new fire function and if there are several he must be offered co-authorship to the paper. Other co-authors of SPITFIRE, with whom model results were quickly discussed, should be mentioned in the acknowledgements. This is the minimum of assuring good scientific practice and acknowledging the intellectual property rights.

We now include Allan Spessa as a co-author on our revised manuscript.

There could be various reasons why the results of the Thonicke et al publication could not be reproduced, first of all with changes in the LPJ model and/or SPITFIRE code, and second due to the input data used. The only way to find this out is through a thorough benchmarking test of the LPJ model versions to test if the variables which form the input to the embedded fire model are the same as for the version used in the Thonicke et al. 2010 publication.

Again, we removed the offending text from the manuscript.

Using the same model acronym does not mean that the content is still the same. However, it is essential in understanding the changes in model results among different code versions and must therefore be presented; otherwise it is a statement without providing the evidence.

While we wanted to acknowledge the major contribution that the original SPITFIRE was to our own work, we now changed the name of the new model, realizing that we do not own the SPITFIRE name.

The remaining part of section 2, which describes the improvements to the SPITFIRE model, is hastily written and mixes the sequence of processes ignition, spread and effect of fire and are thus not easily to follow.

We made an effort to improve the clarity of section 2, and in response to the reviewers' specific comments added, e.g., more justification for our choice of equations and parameters.

C1225

Questions regarding the quantification of human-caused ignitions or role of average fuel characteristics influencing fire spread could have easily being discussed with the original developers of SPITFIRE.

Acknowledging that we did discuss these issues with Allan Spessa is one of the reasons why he is now included as a coauthor on the paper.

Line 12-21 p.2357 describes why the influence of fuel size distribution was revised. This needs to be put in context of the scales at which a DGVM is operating. The LPJ DGVM is a so-called point model, which simulates processes for a certain grid point assuming that everything within the area that this grid point represents is homogenous. It is therefore not spatially explicitly defining where within a grid cell PFTs are growing, so assumptions about spatially explicit fuel arrangements are simply not possible in this type of a DGVM.

While LPJ does not track the spatial position of different PFTs within a gridcell explicitly, a fundamental assumption in the model is that no PFT is assumed to overlap in space with another, e.g., when calculating sunlight interception, each PFT receives direct beam radiation as if it was growing out in the open. Importantly with respect to fire, this assumption of non-colocation of PFTs implies that herbaceous vegetation is separated in space from woody plants. We therefore believe that we are justified in calculating fire rates of spread for herbaceous fuels separately from woody fuels when the mix of PFTs on the gridcell as defined on the basis of FPC reaches a certain mixing ratio.

In section 3.1.3 of the manuscript (see also supplementary figure S1), we show how we estimate the spatial continuity of natural vs. agricultural land on a landscape by means of a Monte Carlo simulation of a randomly distributed mixture of both types on

C1226

a gridcell. This also applies for fuel continuity. If, e.g., grass PFTs occupy more than 60% of the grid cell, the rate of spread should always be determined by the grass fuels as the possibility of arranging the remaining 40% of woody fuels in any way so that the grass area will not be contiguous any more becomes impossible. Below 60% cover of herbaceous fuels, the average contiguous area size rapidly decreases, at least as long as areas occupied by grass or trees are assumed to be distributed more or less randomly, and the influence of woody fuels on the overall rate of spread becomes more dominant. This is expressed in form of the weighting in Eq. (27). We now explain this in more detail in the text (section 3.2.4).

Secondly fine fuels influence fire ignitions; however a flaming front moving through a heterogeneous fuel bed consumes 1 to 100-hr fuel. So, in SPITFIRE conditions of an average fire are simulated integrating over various phases of burning that changes in reality with weather conditions and changes in the fuel bed as processes are calculated at the daily time step and not for individual phases of the combustion process.

We have not changed the fact that 1 to 100-hr fuels are consumed as a fire moves over the fuel bed. Also, we are still using SPITFIRE's rate of spread equations that include changes in fuel moisture or fuel quantity, and we are also still doing this on a daily time step. We calculate fire rate of spread as an average process, not with respect to individual phases of the combustion process. The major difference to the original SPITFIRE is in how we perform the weighting for averaging rate of spread by making calculations separately for the herbaceous and the woody fuel components for the reasons given above, and then directly weighting the two resulting rates of spreads, instead of calculating one rate of spread averaged over all fuels.

The changes in section 3.1.1 are redundant as fires ignited in a particular day and the

respective area burnt are not counted if there is not enough dead fuel provided, the fire spread is very low resulting in low surface fire intensity below the critical threshold (see Thonicke et al. 2010).

As described in our response to Reviewer 1, the thresholds we implement are designed to save calculation time. As biomass input and FPC are updated annually in LPJ, but the fire routine is called daily and estimation of Isurface requires a number of different equations to calculate, we save considerable computing time by using the thresholds for total fuel mass and continuity we established. This reasoning is similar to the methodology used by Prentice et al. (2011).

If the overall model were eventually revised to move most of the annual processes to a daily or monthly timestep as we recommend for future development, it would be necessary to remove or adjust the thresholds we specify. We clarify these points in section 3.1.1 of the manuscript.

The approach presented here regarding imposing a lumping of lightning-caused ignitions should also be discussed with respect to the modifications published in Prentice et al. 2011.

Prentice et al. (2011) apply a conversion factor of 0.030 to transform lightning flashes into observed flashes that reach the ground and have sufficient energy to start a fire. They further account for the fact that the ignition probability depends on the overall wetness of the month by introducing a factor P+ to reduce the effectiveness of ignition events in wet months. We account for the influence of wetness by including the FDI in the calculation of the ignition probability on a given day (see Eq. 4). This avoids introducing yet another parameter, which, in the case of the study presented by Prentice et al. (2011), had to be determined based on a single transect across

the Sahara, although it was intended to be used globally. An additional disadvantage of the scheme presented by Prentice et al. (2011) is the fact that ignitions are still deterministically scaled to lightning. This is particularly problematic in environments where lightning is rare, such as in the subarctic. We argue that in reality the nature of lightning ignitions is not purely deterministic or based solely on wetness and lightning energy, but includes an additional stochastic component which allows to explain why in certain cases one single lightning strike can be sufficient to cause a fire, whereas in other cases many lightning strikes within one thunderstorm do not cause a single fire. To take this stochastic component of lightning into account, we compare the ignition probability term to a pseudo-random number between zero and one and allow either one ignition (if the probability term is greater or equal to the random number) or zero ignitions on the given day. By allowing either zero or one ignition per grid cell and day, we also avoid the concept of fractional lightning ignitions, arguing that lightning ignitions are discrete rather than continuous events. We include this discussion in section 3.1.2 of the revised manuscript.

The authors put a lot of effort into developing functions quantifying human-caused ignitions in pre-historic times (equ. 5-11). Each equation is accompanied by a large number of assumptions that archeologists and other paleoclimatic scientists will perhaps never be able to prove or falsify. Does the presented approach allow for new insights in science? I doubt this.

With respect to the framework we outline for simulating anthropogenic fire in prehistoric and preindustrial time, the equations we developed represent one way of quantifying the history of human-fire interactions. For a variety of reasons, including nature conservation and protection, sustainability research, and quantifying the present state of the terrestrial biosphere, it is important to understand the long-term evolution of large-scale vegetation patterns and the role that anthropogenic activities, including fire, played in

shaping the biosphere over at least the Holocene (see, e.g., Bond et al.(2005); Bond and Keeley (2005); Bowman et al. (2009)).

The historical and archaeological evidence we have for the evolving relationship between humans and fire is mostly qualitative and only very rarely quantitative. Directly using qualitative information to drive a numerical model is not possible and so, in this manuscript we outline a mathematical representation that is empirically designed to capture observed and inferred human-fire relationships. Though we have not attempted to perform a thorough evaluation of our preindustrial human burning formulation in this paper, for completeness of the model description in a journal intended for model descriptions we felt it was important to include all of the equations included in our current code. This will facilitate disclosure in future papers where we demonstrate and test our preindustrial human fire parameterization. As GMD allows versioning of papers, any future updates to our preindustrial human burning equations, e.g., improved in light of new archaeological or paleoecological evidence, can be incorporated into the comprehensive model description paper.

Even with current knowledge and people's education, intentional fires, meant to burn wood debris from deforestation, went out of control in Indonesia during the 1997/98 ENSO event and emitted a huge amount of carbon (Page et al. 2002), and remain to be a problem in modern civilization as can be seen in, e.g., Spanish fire statistics. The authors explain the classification of pre-historic and preindustrial human-caused ignitions, but how this logic applies to today's conditions, for which the model is evaluated, is missing.

The model presented by us is specifically designed for paleo-applications prior to the industrial era. The model is clearly not suitable to represent present-day human-fire interactions, and we clarified this point in our revised manuscript (abstract, introduction, general discussion). For the model comparison with observations, including the

new section we added to describe global comparisons with GFED and the Randerson et al. (2012) datasets, we omit anthropogenic ignitions and industrial suppression and focus on the residuals between model and observations as a way of understanding the human imprint on global fire at present-day.

It is not explained for which land-cover type the respective equations are applied and how this specification improves reproducing observed fire regimes.

All equations presented in section 3.1 on anthropogenic burning are with respect to the non-agricultural land use tile, except for the 20% cropland burning prescribed in addition to the burning on "natural" (i.e., potential natural vegetation simulated by LPJ) land. We clarified this point in the text (section 3.1.4).

Can the new estimate of fuel bulk density for tundra ecosystems be validated in any way? Or is this a purely calibration factor to match observed area burned? This should be stated clearly and references be provided.

We empirically estimated fuel bulk density for tundra ecosystems and tropical grass-land ecosystems based on reported aboveground biomass values and estimated average vegetation height. We clarified this point and added references in section 3.2.2 of the revised manuscript text.

The crown-fire approach must be related to existing modeling approaches discussing the state-of-the-art and therefore providing evidence for the type of equations and their parameterization. Why is the biomass affected by a crown-fire added to the respective dead fuel classes and which proportion is added to the combusted fuel? How is the latter calculated?

Realizing that a proper treatment of crown fires requires more in-depth research, we removed this parameterization from the model code and from the manuscript text. Removing crown fire did not substantially change our model results, as the conditions under which our formulation simulated crown fires was exceedingly rare, and therefore unrealistic in any case.

The statement "Therefore, areas with high relief energy statistically should have smaller average fire sizes compared to areas that are completely flat." on page 2376 must be backed with data as fires accelerate their rate of spread and fuel consumption during the fire when spreading up-hill. How can this be distinguished at a 0.5 degree non-spatially explicit grid cell?

It is true that slopes may increase the rate of spread where fires are burning up-slope. However, every up-slope has a corresponding down-slope on the opposing side of the ridge. Fires that quickly burn up one slope will at a point reach the crest of the ridge or the summit of the mountain, and then need to burn down on the opposite slope. The down-slope will have a different exposure and therefore likely also differing conditions with respect to fuel moisture and vegetation type. For example, northern slopes are wetter than southern-exposed slopes. Moreover, in mountainous terrain the fuel continuity can be broken due to rock outcroppings and sparsely vegetated areas, or valley bottoms may have water courses which are wide enough to stop the expansion of a fire.

Please see also our response to a similar comment by Reviewer 1. No monolithic slope covers the spatial extent represented by a half-degree gridcell anywhere on earth. With gridcells of 1000-3000 km^2, considerable topographic heterogeneity exists in mountainous terrain. Experiments we performed during model development and

testing convinced us it was necessary to account for complex terrain in some way. A different parameterization would have to be found if running the model at higher spatial resolution, having the unfortunate effect of locking our current formulation into a degree of resolution dependency. To illustrate the effect of the slope factor we applied, we provide a supplementary figure and we clarified the text with further explanation of the role of slope on fires (section 3.2.3).

The 3 mm precipitation threshold is part of the original Nesterov index. The modification to account for multiple-day burning must be clearly written and presented in form of equations to improve the understanding of the modeling approach, and the relation to the other ignition and climatic fire risk calculation must be explained. In the original SPITFIRE, during dry spells several fires might be started in each day, resulting in the same area burnt, but perhaps overestimating numbers of fires. The feedback to the vegetation might be the same. Observed data should be presented showing that the multi-day burning approach improves simulated fire pattern.

In areas such as Alaska where lighting strikes are rare, the persistence of individual ignitions that may evolve into large fires over several days or months is essential. In the original SPITFIRE scheme, all fires are extinguished after a maximum burning period of 241 minutes. Tests we performed showed that in areas with little lightning, e.g., in the boreal and subarctic regions, not allowing for the potential of multiple day burning resulted in an underestimation of burned area when compared to observations. We clarify this rationale in our revised manuscript text and support it with additional references (section 3.2.1). See also our response to comments of Reviewer 1.

With dead fuel updated daily as a result of biomass burning, the correction for already burned fraction in a gridcell in Eq.. 32 becomes redundant. Again, dead fuel load influences fire spread and with large proportions being burned already, available dead

C1233

fuel is reduced so less ignitions should develop enough surface fire intensity, thus area burnt should be reduced as the model approach reacts to such changes.

As discussed above and in our responses to Reviewer 1, our method of accounting for burned area assumes that fire occurs in spatially discrete patches on the landscape. Because of the nonlinear nature of the rate of spread equations, removing combusted fuel on a gridcell average basis leads to unrealistic calculations of burned area as the fire season progresses. Distinguishing the parts of the gridcell that are already burned from the unburned fraction is possible because the model tracks number of fires at any given time and therefore daily burned area. We found that separating burned from unburned fractions was essential when we implemented our scheme for multi-day burning and the possibility for smoldering ignitions. We clarified these points in our revised text (section 3.2.4).

The new PFT parameterization presented in Table A1 must provide references that justify the modification of RCK as well as justification of why all PFTs have the same fuel bulk density, ignoring the respective literature reporting different values. In the original SPITFIRE model, PFT parameterization followed the logic that preference was given to observed values (scientific literature, observed plant traits) even though they would result in less perfect model quality. Reducing the PFT parameterization to calibration factors removes the opportunity to learn something new about the functioning of ecosystems. What is the effect of this new parameterization compared to the original values? This is important to show as only that way the effect of the new or modified functions can be discussed.

The current version of the model uses a constant RCK-value for all woody PFTs, and a p-value of 3. When we used the original RCK-values (close to 1 for all woody PFTs with the exception of PFT 2) an undesired result of our multiple-day burning scheme

C1234

was that much of the simulated global vegetation cover converted to grasslands in places with frequent fire occurrence. Observational data, e.g., the GLCF tree cover data, showed that many of these places clearly should have tree cover.

Using parameters obtained from observed plant traits is a good strategy, given that the simulated model trees are well able to represent actual plant individuals. However, given the unrealistic allometry simulated by LPJ's average individual, direct representation of the characteristics of individual trees is impossible. Applying parameters that are derived from "real-world" observed individuals to trees as they are represented in the model with "unrealistic" size and shape results in an unrealistic outcome for fire-induced mortality. An obvious way of rectifying this situation would be to make the representation of individual trees more realistic, so that real-world parameters are more likely to produce realistic outcome, e.g., by using a cohort-based model such as LPJ-GUESS. We clarified the abovementioned points in our revised text (section 3.3).

Interesting to note, that the cambial damage as a cause of post-fire mortality was removed from the model. The authors have decided to take out one of the most important causes for post-fire mortality.

This was a temporary fix during model development. The problem has been solved and cambial kill is included in the current working version of the model using the equations from the original LPJ-SPITFIRE. See section 3.3 of the revised manuscript text.

Lines 4-14, p. 2379 are purely hypothetical and no evidence in terms of data or literature is provided. This discussion does not relate to fire mortality and should be simply removed.

Please see also our response to the comments of Reviewer 1. There is a fundamental issue with LPJ's simplistic treatment of herbaceous vegetation that we needed to change when simulating fires. In reality most herbaceous vegetation grows quickly enough to complete their lifecycle within one growing season (see, e.g., Cheney and Sullivan (2008); Gibson (2009)), while in LPJ it takes 3-10 years (depending on soils and climate) for herbaceous PFTs to reach equilibrium levels of aboveground biomass. In areas with frequent fire, the original LPJ parameterization led to very low values of growing season biomass compared to observations, which affected fuel loads, rate of spread and burned area calculations. The approach of leaving herbaceous aboveground biomass in place is a simplification, but was also used in Kaplan et al. (2011, 2012) when dealing with crop harvest. The desirable alternative approach would be to move processes currently calculated annually by LPJ (allocation, establishment, mortality, etc.) to a daily or monthly timestep. Such an exercise was beyond the scope of the current manuscript but we recommend it as an important goal for future research. We clarify this discussion in section 3.3 of the revised manuscript text.

The discussion on the dynamics of herbaceous fuels relates both to mortality and fuel load. We believe it is in the correct place in the manuscript, but also mention the effect of our changes to the model when discussing fuel load.

The authors evaluate the updated fire model against fire statistics from Alaska. But if the new approach is robust to reproduce different types of human-caused fires, the model must be evaluated against data from human-dominated fire regimes as well, and if the model is supposed to be applied to reproduce past human-caused fire regimes it must show this evaluation as well. I understand that this is a big task for this manuscript and it would be more realistical to remove the sections talking about pre-industrial human-caused fires as this is not the topic of this manuscript. An evaluation against burned area and fire emission from the different fire types as quantified by GFED-3 is essential if the authors want to prove that the updated fire

model is robust.

As highlighted above, we now include a comparison of the fire model results with observational datasets of global burned area. As also mentioned above, we prefer to include the description of our preindustrial anthropogenic burning scheme for completeness of the record in a journal that is intended for the publication of model descriptions. A separate publication showing results from anthropogenic burning for the past using the new scheme is in preparation.

Evaluation of Alaskan fires: line 10-13, p. 2392: This statement applies to all other DGVMs of the LPJ type, this nothing specifically new that has now arisen due to the updates of this model and must be modified accordingly. Fig. 7 misses to show lightning strikes at least according to the figure caption.

This statement was not meant to be exclusively true for only one model, but rather to be an advantage of models in general: They allow to study theoretical questions that cannot be directly observed in the real-world. Of course this is true for all other DGVMs as well, and never was intended to be understood exclusively. The phrasing has been revised to make that clear. The omission of lightning strikes in Fig. 7 is the point of the figure and serves to explain why it is hard to get any simulated fire for ecoregion BTA: The potential time window during which burning would be possible (absence of snow) is already limited to 4.5 months in the summer. During that time period it rains frequently, so getting fires started and then keep them going without them being extinguished again quickly due to the rain is challenging. Nevertheless, there would be a short period in July during which it is sufficiently dry (FDI increases to 0.9). But the key point is that there is no lightning at all during this year, so the lack of lightning is clearly the limiting factor for getting fire into that ecoregion. Other years have some lightning, but then often the timing of the lightning strikes is not right and

C1237

rain prevents large fires. Given an ignition limitation plus rather wet conditions makes getting fires right in ecoregion BTA a hit-or-miss problem.

Regarding the discussion of Alaskan fires, the statement on p. 2396, line 15 "where the original SPITFIRE did not simulate any fire" is not correct. Fig.3 in Thonicke et al. 2010 shows low fire risk for Alaska with numbers of fires between 0.0 and <0.004 number of fires per sqkm and year, resulting in fractional area burnt of between 0.004 and 0.01, the evaluation figure 7 in Thonicke et al. 2010 is also showing pixels, where LPJ-SPITFIRE simulates fire. This is perhaps an underestimation of observed area burnt but it is not correct to say no fires were simulated.

We revised our text to be correct with respect to these comments and Thonicke et al. (2010).

The statement "Likewise, the calculation of fuel wetness as a mass balance function of drying and wetting rather than relying on a yes/no decision depending on an arbitrarily chosen precipitation threshold of 3mm as originally proposed by Thonicke et al. (2010) makes SPITFIRE-2 more successful at realistically simulating fire behavior." on page 2396, l. 19-23, is not correct. As stated above, the precipitation threshold is nothing that was invented for the original SPITFIRE model, but is part of the original Nesterov index. If this is what makes the big difference in model results, then all the other new functions are not so important, aren't they? The authors must be careful with such statements in the discussion section; none of the figures have shown a comparison between the original SPITFIRE model and the updated model version shown here.

The Nesterov Index has been established for fire modeling and prediction as a simple fire-danger rating system. It offers the advantage of being fairly easy to calculate as

C1238

it only depends on a fixed precipitation threshold and temperature, both parameters that can be measured quickly and without complicated technical equipment. However, the existence of a long-established index does not automatically imply that there could be no other way of assessing fire danger that is a bit more sophisticated and gradual instead of being just a simple yes/no decision. Studies in heterogeneous landscapes have shown that fine fuel moisture depends strongly on the type of vegetation in the overstorey (e.g., Uhl and Kauffman (1990)). The mass balance function for drying and wetting that we introduced is also not a new approach either, and is essentially based on the way in which soil moisture is calculated in LPJ using a water bucket approach, in this case tracking changes to a schematic fuel wetness pool. Similarly to the way in which a certain amount of precipitation would affect soil water saturation, fuel wetness depends on the history during previous days and weeks. We argue that 3 mm of precipitation will have a different effect on fuel moisture if the fuel was completely dry at the time when rain came, or still fairly wet from rain events during previous days. We have now revised the paragraph in section 3.2.3 and explain our approach in more detail.

I disagree with the statement that equ. 32 already implies effects of fragmentation. The authors need to clearly describe how crop, grassland and forest are handled within a grid cell and how the updated fire model is applied to each type. Accounting for permanent agriculture is already a 1st-order fragmentation factor, but this is not adequately described.

First, the parentheses in Eq. 32 are set wrong, as Reviewer 1 correctly points out. We apologize for that and have corrected it in this revised version of our manuscript. Second, the purpose of Eq. 32 is not to mimic fragmentation due to different types of land use, but to account for fires that stop burning due to merging into another fire or a place that burned previously and therefore lacks fuel to sustain a second

C1239

fire. The number of fires burning on a given day within one grid cell will be reduced with respect to the number of fires that burned on the previous day depending on how much area of that grid cell has already been burned up to that day by subtracting the burned area fraction times the sum of fires from the previous day and the current day. With increasing area burned, the number of fires that will "survive" without becoming extinguished decreases. Regarding the second point on land use fragmentation, so far we only distinguish between "natural land" and "agricultural land", with agricultural land being used for growing crops, and natural land being the fraction of the grid cell that is covered with the potential natural vegetation cover simulated by LPJ. As explained in Kaplan et al. (2011, 2012), grid cells are subdivided into tiles when agricultural usage is included into simulations. The fraction of the grid cell that each tile occupies is based on the input land use file (e.g., HYDE land use for present day, or past land use scenarios such as HYDE or Kaplan et al. (2011). The way in which fragmentation of the natural tile due to the presence of the agricultural tile is handled is explained in detail at the end of section 3.1.3 (Eq. 12).

So is this model modification well thought-through and name it SPITFIRE-2? Is it really a new model as stated several times? It remains to be shown. At least the title of the manuscript must be changed.

The model description we present in this manuscript is not the original SPITFIRE after the modifications and changes that we made, but we believe that it is also not correct to call it a completely new model, because we did not develop a new model from scratch but essentially changed a few representations and parameterizations of SPITFIRE. As for the name of the model, we have no problem changing it to something different, and have done this in our revised manuscript. As we pointed out in our original introduction, our intention was to pay credit to the hard work of SPITFIRE's authors by not pretending we invented something completely new of our own, when in

C1240

fact we modified what others had provided through their own efforts. But apparently this can also be seen differently – as a violation of proprietary intellectual rights – a thought that did not occur to us. We wanted to avoid giving our model a completely different name and then being accused of inadequately acknowledging the model's lineage. We apologize for any misunderstanding we may have caused.

Supplementary Figures:

S1: Scatterplot of Monte Carlo simulation results on a 100 x 100 grid. For each fractional combination of natural land vs. agricultural land on a step size of 0.01, pixels on the 100 x 100 grid were randomly assigned to be either natural land or cropland, and the average contiguous area fraction of natural patches was calculated based on an 8-cell neighborhood, for 1000 repetitions at each land use fraction level.

S2: Spatial distribution of the slope factor (slf) derived from Eq. (30) in the Discussion paper. Constraining effects of terrain size on the average size of fires are estimated by using slf as a multiplication factor on the default average fire size calculated by the fire model.

References

Bond, W. J. and J. E. Keeley. 2005. Fire as a global 'herbivore': the ecology and evolution of flammable ecosystems. Trends in Ecology and Evolution 20:387-394.

Bond, W. J., F. I. Woodward, and G. F. Midgley. 2005. The global distribution of ecosystems in a world without fire. New Phytologist 165.

Bowman, D. M. J. S., J. K. Balch, P. Artaxo, W. J. Bond, J. M. Carlson, M. A. Cochrane, C. M. D'Antonio, R. S. DeFries, J. C. Doyle, S. P. Harrison, F. H. Johnston, J. E. Keeley,

C1241

M. A. Krawchuck, C. A. Kull, J. B. Marston, M. A. Moritz, I. C. Prentice, C. I. Roos, A. C. Scott, T. W. Swetnam, G. R. van der Werf, and S. J. Pyne. 2009. Fire in the Earth System. Science 324:481-485.

Cheney, P. and A. Sullivan. 2008. Grassfires: Fuel, Weather and Fire Behavior. CSIRO Publishing.

Gibson, D. J. 2009. Grasses and grassland ecology. Oxford University Press, Oxford, UK.

Kaplan, J. O., K. M. Krumhardt, E. C. Ellis, W. F. Ruddiman, C. Lemmen, and K. Klein Goldewijk. 2011. Holocene carbon emissions as a result of anthropogenic land cover change. The Holocene 21(5):775-791.

Kaplan, J. O., K. M. Krumhardt, and N. E. Zimmermann. 2012 The effects of land use and climate change on the carbon cycle of Europe over the past 500 years. Global Change Biology 18:902-914.

Page, S. E., F. Siegert, J. O. Rieley, H.-D. V. Boehm, A. Jaya, and S. Limin. 2002. The amount of carbon released from peat and forest fires in Indonesia during 1997. Nature 420:61-65.

Prentice, I. C., D. I. Kelley, P. N. Foster, P. Friedlingstein, S. P. Harrison, and P. J. Bartlein. 2011. Modeling fire and the terrestrial carbon balance. Global Biogeochemical Cycles 25:GB3005.

Randerson, J. T., Y. Chen, G. R. van der Werf, B. M. Rogers, and D. C. Morton. 2012. Global burned area and biomass burning emissions from small fires. Journal of Geophysical Research 117:G04012, 04023 pp.

Thonicke, K., A. Spessa, I. C. Prentice, S. P. Harrison, L. Dong, and C. Carmona-Moreno. 2010. The influence of vegetation, fire spread and fire behaviour on biomass burning and trace gas emissions: results from a process-based model. Biogeosciences 7:1991-2011.
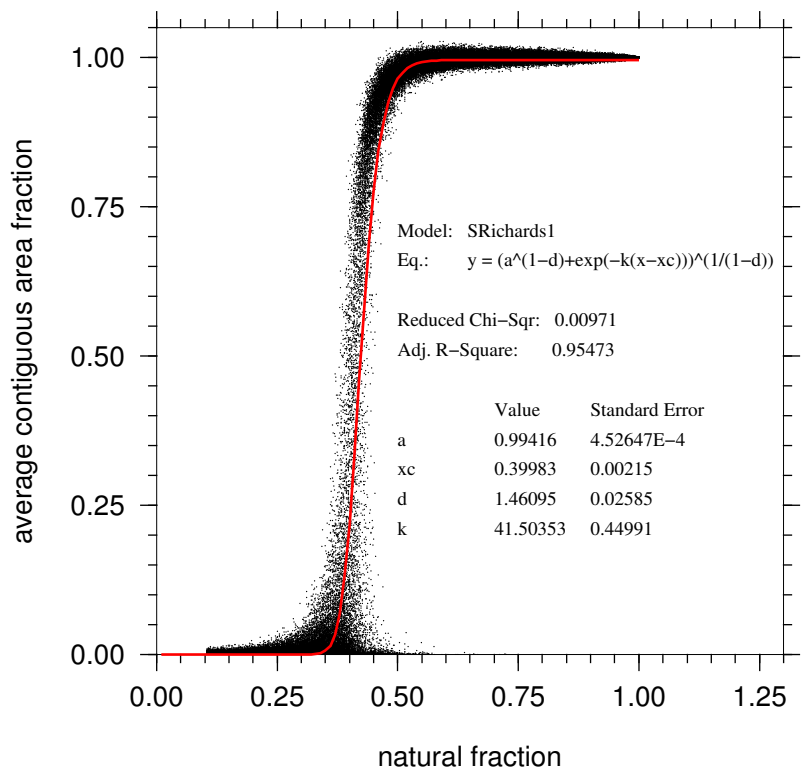
C1242

C1243

Model: SRichards1
Eq.: $y = (a^{(1-d)}+\exp(-k(x-xc)))^{(1/(1-d))}$

Reduced Chi−Sqr: 0.00971

Adj. R−Square: 0.95473

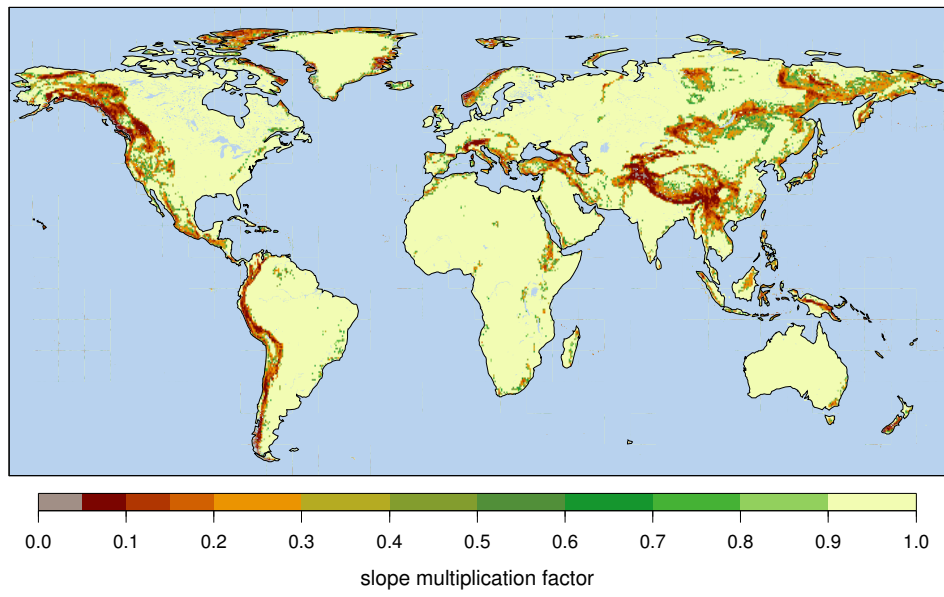|	Value	Standard Error
a	0.99416	4.52647E−4
xc	0.39983	0.00215
d	1.46095	0.02585
k	41.50353	0.44991

Fig. 1. Supplementary figure S1

Fig. 2. Supplementary figure S2

C1245