

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

K. Thonicke

kirsten.thonicke@pik-potsdam.de

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The following statement on page 2354f “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

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to develop their own process-based model and can jointly follow-up ideas that cannot all be followed up by the original developers. 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 sent 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.

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. 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. 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. 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.

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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 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. 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. 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).

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

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. 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 pre-industrial human-caused ignitions, but how this logic applies to today's conditions, for

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

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.

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?

The statement "Therefore, areas with high relief energy statistically 10 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° non-spatially explicit grid cell?

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 spell 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. With dead fuel updated daily as a result of biomass burning, the correction for already burned fraction in a grid cell in equ. 32 becomes redundant. Again, dead fuel load influences fire spread and with large proportion being burned already, available dead fuel is reduced so less igni-

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tions should develop enough surface fire intensity, thus area burnt should be reduced as the model approach reacts to such changes.

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.

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.

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

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 realistically 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.

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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.

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.

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 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.

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.

So is this model modification well thought-through and name it SPITFIRE-2? Is it really

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a new model as stated several times? It remains to be shown. At least the title of the manuscript must be changed.

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