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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

Anonymous Referee #1

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Review

Pfeiffer and Kaplan, SPITFIRE-2: an improved fire module for Dynamic Global Vegetation Models

The manuscript introduces a new version of the fire model SPITFIRE as part of the LPJ vegetation model. The authors state that this new fire model was needed as the they were not able to reproduce the results presented in Thonicke et al. (2010) applying the original LPJ/SPITFIRE model. They included various new processes and modifications into the model, which according to the abstract results in "significant improvements in simulated burned area over previous models."

As every model, also the SPITFIRE model has it pros and cons and allows for im-

provements. However, this manuscript does not convincingly demonstrate how the modifications introduced here lead to an improved model version. The evaluation for Alaska only is not sufficient for a global fire model, especially not as many other observational datasets for further evaluation are available. New features, such as anthropogenic ignitions, are introduced in the model description, but were not applied in the study presented. The topic of the manuscript is relevant and in general well suited for publication in GMD. The approaches for anthropogenic ignitions, multiple day burning and crown fires are interesting. However, the manuscript will need major revision before publication, with the current presentation of results it is not possible to assess whether the model actually performs well or not. In general, the manuscript is in part very lengthy written and could be significantly shortened.

In the following I will list the major concerns with the manuscript:

title: I was puzzled to find a SPITFIRE-2 model description manuscript with not one of the developers of the SPITFIRE model included in the author list. Every model will over time undergo modifications and new release versions are published indicating a new development cycle. The right to publish new release cycles should stay, however, with the developers of the original model. The authors might want to consider a different naming for the model.

Improvements/modifications:

New thresholds of FPC > 50% and fuel load < 1000 gC m-2 for fire occurrence are introduced. How important are these thresholds and aren't they already implicitly accounted for in the threshold for the fire intensity?

lightning interannual variability is scaled with cape anomalies. The procedure is not entirely clear from the text and the scaling to the max and min observed CAPE is not correct, as the mean lightning frequency cannot be conserved with the described method. How does the interannual variability look like? Is for example the variability spanning two orders of magnitude as observed for Alaska reproduced? In the end the

CAPE scaled lightning rates are not used in the Alaska case study that is used for evaluation. Thus, the improvement is not evident from the presented results.

The efficiency of a lightning strike to cause an ignition is strongly downscaled (0.04 to 0.5) depending on the vegetation type, previous burning and fuel moisture status. In the end the downscaling factor is translated into a random number between 0 and 1. It is not clear why such a procedure has been chosen and how this effect the results. The method of using a random does not allow to use the model to reproduce simulations with identical results, which should be generally avoided. If such a random procedure is included in the model, the authors should demonstrate how this influences the stability of the results, e.g. by including a model ensemble.

Anthropogenic Ignitions: SPITFIRE accounts for anthropogenic ignition solely as a function of population density. This is certainly not sufficient to reproduce the complex fire-human-vegetation interaction. Here the authors distinguish between different groups (hunter-gatherers, farmers and pastoralist) which will greatly help to improve the representation of anthropogenic ignition especially during times when the relation of these groups changed. However, from the text is becomes not clear how one can divide population into these different groups and how well the relationship between fire occurrence and these individual groups is known. Active fire suppression is nowadays in many parts of the world significantly altering anthropogenic and also natural fire regimes and should be accounted for as well. Studies like Pechony et al. (2010) introduced simple relationships between population density and fire suppression and demonstrated that active fire suppression has to be accounted for in fire models to reproduce present day fire occurrence.

Fuel characteristics: various changes were introduced in LPJ to improve the fire simulations. This included an improved aboveground biomass representation, a new carbon pool in LPJ, a simple permafrost-moisture link and new fuel bulk densities for grasses. Here the authors should be more specific how these single changes improved the results. It is several times very vaguely stated that before these changes “spread rates

were unrealistically high”, “unrealistic accumulation of surface fuel occurred”, “larger amounts of fires than expected were noticed in certain parts of the world”, etc. . To make a convincing point that these changes in the fuel characteristic improved the model at least one before/after improvement plot must be shown.

Evaluation only for Alaska is not sufficient. More data on burned area for different regions is available, e.g. based on satellite observations. As a global model SPITFIRE – 2 requires global evaluation. A case study for Alaska is not sufficient as fire regimes in different regions function completely differently. In addition to burned area distribution, the interannual variability and the seasonality of fire occurrence are key variables of a global fire model that have to be confronted with observations on a global scale.

While the treatment of anthropogenic ignitions are introduced in the model description in great detail and are emphasized in abstract and conclusion, they have not been used in the simulations presented in the manuscript. As such it is impossible to judge the performance of the anthropogenic ignition parametrization. In addition, and also mentioned several times in the manuscript, anthropogenic ignitions are essential for present day fire occurrence. Ignoring anthropogenic ignitions will now allow any evaluation with present day data or comparison to other fire models. Also the comparison with observed biomass data is invalid in this case as in many regions fire has an important control on aboveground biomass (as also shown in this manuscript). From the manuscript it becomes also not clear how land use change is treated.

Specific major comments:

p. 2353/line 3: That some fire models do not report trace gas and aerosol emissions can not be listed as a shortcoming of the model. These can be purely diagnostically derived from the reported carbon emissions.

p. 2353/line 11: The study by Kloster et al. (2010) is not cited correctly. The study did not use fire count data for evaluation, nor were the lightning ignitions constant. The model used was not CTEM but CLM.

p. 2354/line 23: “Thus, SPITFIRE represents the most comprehensive fire model for DGVMs currently available, and the only one that is potentially able to both represent human-vegetation-fire dynamics . . . “ This is not true, other fire models by Arora and Boer (2004) or Pechony and Shindell (2010) do have the same potential.

p. 2356/line 17: “Rationale for Improving SPITFIRE” The authors state that the “implementations of the equations from Thonicke et al. (2010) led to a model that (1) burned too much in some parts of the world and not enough in others” this statement is too vague. In order to be able to judge the improvements described in this manuscripts the authors need to show the results of the original version and more global results of the improved version, e.g. the global distribution of burned area is not shown. (see also general comments).

p. 2356/ line 24: “Fire . . . also feedback on themselves, as fire takes away fuel for consecutive fires . . .. This feedback effect of fire on itself is not represented in the published documentation of SPITFIRE” Isn’t that implicitly accounted for in SPITFIRE, in which the rate of spread is a function of available fuel load, i.e. when a fire consumes fuel subsequent fires will have a reduced fire spread rate?

p. 2369/line 4: “probably” can be removed

p. 2360/line 10: “We do not allow fires if the total vegetation foliar projected cover (FPC) of a given grid cell is less than 50%, or if the total amount of fuel . . ... is less than 1000gm-2. These are two rather stringent thresholds that are introduced into SPITFIRE, which is actually setup to account for those limiting factors dynamically by explicitly simulating fire intensity. Do these thresholds often take affect?

p. 2363/line 9: I do not understand the equation (1). The monthly lightning is calculated from the climatological mean modified by CAPE anomalies. The CAPE anomalies are according to the text scaled in a way that the max CAPE anomalie is +1 and the minimum is -1. According to equation (1) in case of max CAPE the lightning is 10 times the climatological mean; in case of min CAPE the lightning is 0.01 of the climatological

mean one.

What is the reasoning to scale between 0.01 and 10? the relationship between CAPE and lightning is highly non-linear. Do monthly mean CAPE values still reflect monthly mean lightning? The scaling of the CAPE anomalies to max/min values will not preserve the mean value of the observed lightning unless it is normally distributed. Therefore the lightning rate that enters your fire calculation does not reflect anymore the climatological mean of observed values.

p. 2361/line 12: Monthly mean lightning is disaggregated to daily values using precip data. Are precip and lightning that closely correlated?

p. 2362/ line 5: Similar to SPITFIRE the authors introduce a factor that downscales total flashes to flashes that are efficient enough to produce a fire. This efficiency factor ranges according to Table 1 between 0.05 and 0.40 depending on the vegetation type. SPITFIRE uses a factor of 0.04 independent of vegetation type. Why is there such a difference? Is this based on observations? An order of magnitude difference in the ignition efficiency between a “tropical broad leaf evergreen” and a “temperate broadleaf summer green” seems rather high.

p. 2362/line 11: the efficiency of lightning is further reduced if the grid cell has been previously burned. Again, I do not understand why this has to be explicitly accounted for. That fire spread is reduced when the fuel load is lowered (for example caused by previous fires) is accounted for in the rate of spread calculation in SPITFIRE and has not to be explicitly introduced. According to equation (2) the lightning efficiency (and as such the resulting burned area) is reduced by 0.037 in case the grid cell has burned 50% in the previous days of the years. This is will strongly suppress repeated burning, which is, however frequently observed in savanna regions. In addition, the beginning of the year is not an appropriate time boundary for defining previous burning. In this case, the beginning of the fire season should be used. Moreover if the previously burned area is explicitly excluded, the fuel load needs to be computed for the not burned area only (excluded the low fuel load of the already burned areas), otherwise the reduction in fuel load is accounted for twice.

p. 2362/line 15: the lightning efficiency is then further reduced by the fire danger index (FDI) to account for the fact the lightning strikes will result in ignition depending on the fuel moisture status. Again, that moist fuel will not lead to large fire spread is accounted for in the fire spread calculation and does not need to be explicitly introduced in the lightning efficiency.

p. 2362/line 16: Why the efficiency term is compared to a random number between 0 and 1 is not clear to me (see also major comments). This has to be further explained.

p. 2363 / Anthropogenic ignitions: The authors distinguish between anthropogenic ignitions caused by hunter-gatherers, pastoralists, and farmers. Whereby each individual of these groups has a limit to which extend a grid box will be burned. Is a limit linked to the grid box size actually meaningful? Wouldn't this be rather an absolute number, e.g every person can burn x ha. The chosen 50, 20 and 5% are these values based on observations?

p. 2365/line 25: "we allow every 10th person present in a grid box to ignite fire purposely" This number will also be likely variable over time.

p. 2366/line 6: Equation (5) looks rather complex. How was this derived? I wasn't able to find where rf was further used in the calculations.

p. 2367/line 7: Equation (9) is not clear to me. What is A_cg? Do you really take the difference? And why is burnedbf taken into account?

p. 2367/line 45: In order to estimate the effect of increasing land use intensity Monte Carlo simulations were performed. From the resulting equation (12) it is not clear what fnat and Agc stand for. Does this equation allow to account for changing land use over time? Is the size of a natural patch often limiting the average size of an individual fire? And why is it limiting the average size and not the actual size? Is the fit you use to

derive equation (12) actually a good one? A graph on the results of the Monte Carlo simulation would be helpful here for the reader.

p. 2368/ line 14: "Burning of cropland" How do you account for the seasonality of cropland burning. In terms of biomass emitted the seasonality (after/before harvest) becomes important.

p. 2370/line 15: Figure 2 does not show a comparison to the original LPJ version, which, however, would be needed here. The 5 to 15% reduction is this globally or only in the Amazon Basin? Is the improvement compared to the data global or are there also areas, where the reduction is too strong?

p. 2370/line 24: "overburning in the boreal regions was frequently observed" this contradicts other statements that SPITFIRE simulated too low burned area in boreal regions (p. 2377,l.7, p. 2383, l. 16).

p. 2371/line 9: how different is the turnover time of 2 years for the O-horizon from the turnover time of the fast pool?

p. 2371/Equation(13): GDD must be the total number of growing degree days within 20 years and not the average number. Otherwise the equation does not lead to densities ranging between $\sim$1 and $\sim$12 kg/m*3. In general the equation needs further explanation. Why did the authors choose such a relationship. Also when the fuel bulk density is applied it is reduced to a maximum of 12 kg/m*3. This should be mentioned here.

p. 2872/Equation (15): where does gs originate from. I couldn't find this in Mell et al. (2012). How different is the grass ROS from the original SPITFIRE one?

p. 2376/line 4: The rate of spread for a crown fire is not defined.

p. 2377/line 5: "In case of a crown fire . . ..and their biomass will be transferred to the corresponding litter pools" Crown fires do also emit directly into the atmosphere.

p. 2376/line 20: Equation (30) introduces a reduction factor that accounts for the terrain effect. It assumes that with higher median slope angle the fire size is reduced. On what do you base this assumption on? In some regions high slope angles actual favour high fire spread rates. With the median slope angle between 2 and 17.2 and the factor ranges between 0.67 and 0.04. So even with a moderate slope the average size of an individual fire will be significantly reduced. A map showing the distribution of slf globally, would be helpful to judge how strongly the terrain effect will impact the simulated burned area.

p. 2377/line 16: “fires were extinguished too easily” can you show some results on this, give some number and a reference on how many fires actually survive in reality?

p-2377:/line 24: the maximum daily fire duration is still limited to 240 min (see eq. A42). This is inconsistent with the multiple day burning.

p. 2378/Equation (32): The equation is wrong; the parenthesis have to be set differently. Please check. How does the total number of fires enter the calculation of the fire occurrence?

p.2378/line 9: the merging of fires is not described in any of the equations presented, how do you account for this process within the model?

p. 2378/line 14: “too many trees being killed”, please show or give numbers on the improvement (how many trees were killed before and now?), are there observations on how many trees are killed due to fire, please give a reference or explain on which basis you assessed the improvement.

p.2378/line 25: p-values are not given in table A1.

p. 2379/line 9: This feeds back to the rate of spread equation and might be a reason why you needed to include the already burned area explicitly in your calculation of the number of fires.

p. 2379/line 10: “at the end of the year” This sounds as if you update your carbon pools only once a year? Updating the carbon pools every day should strongly improve the

ability of the model to get the fuel limitation right and you might be able to remove the modifications where you include the already burned fraction.

p. 2370/line 14: please include a section on the datasets you use for evaluation and also move the description of the model runs here. It is confusing for the reader to have it as part of the results section.

p.2380/line 12: the anthropogenic ignitions are not used for this study? In this case, they should not be presented in the model description. It is a core part of the model presented here and results should be shown and discussed. Also, not using anthropogenic ignitions limits the comparison with present day observations. This is also the case, for remote locations like Alaska in which 20% of the burned area can not be explained by natural ignitions, as the authors state in an earlier paragraph.

p. 2380/line 24: please add the respective model values for the different ecosystems, otherwise these numbers do not contribute to the evaluation.

p. 2381/line 23: The o-horizon in your model is treated overly simplistic with e.g. a constant turnover time of 2 years. Is is actually expected that such a simple treatment will match observations? I also do not see the relevance of this comparison for this study

p. 2381/line 24: Does your model reproduce these patters you identified from literature? Please focus on the evaluation of your model.

p- 2382/line 6: remove the first two sentences (repetition).

p. 2382/line 16: how do you know that it is a good approximation? You only give numbers for high latitudes. Did you find any studies on the global patterns?

p. 2383/line 4: evaluating a global model only for Alaska is not sufficient.

p.2383/line 25: how did the other modifications influence the interannual variability, including the O-horizon might also increase the interannual variability of the fuel load.

The random number generator you included might also cause an additional variability, how robust are your results when repeating the simulations with different random numbers?

p. 2384/line 2: When you use ALDS lightning data for your case study you do not show how well your new CAPE modified climatological mean observed lightning parametrization works. In this case you could actually compare your new lightning approach with observations.

p.2386/line 4: What about the intraannual variability? How does the simulated seasonality compare to the observations??

p. 2386/line 17: Why do you describe these single fire events? Can your model reproduce them too? Do the fires in the model continue for 3 months? Please compare these observations to your model results or remove the paragraph.

p. 2394/line 19: Instead of the results of global fire under natural conditions an evaluation of global fire patterns (burned area, seasonality, interannual variability. . .) would be needed. The impact of fire on carbon pools has been shown before by other studies (Bond et al. , 2005, Scheiter and Higgins, 2009). It is not clear how the results relate to your study.

p. 2395/line 6: Excluding the anthropogenic ignition does not allow to compare the modeled biomass values to observations, as anthropogenic ignitions can increase fires (and therefore reduce the biomass) in regions where fires do not occur naturally.

p.2396/line 4: you did not show that your results have really improved compared to SPITFIRE.

p.2398/line 3: I don't understand the connection between the average individual and the tendency of LPJ to simulate tall trees with thin bark. the relation between tree height and bark thickness is not based on observations?

p. 2398/line27: the whole paragraph can be removed as no results are presented on

human ignitions.

p. 2399//line 21: the anthropogenic ignitions are not described clearly and no results or evaluation on the performance is shown, therefore no conclusions can be made based on this paper.

p. 2400/line 6: please include the definitions including units for the variables in the text.

p.2400/line 16: please define the fuel classes

eq. 36: not part of SPITFIRE, please include in the main paper

eq. A44 is not the same as in Thonicke et al. (2010), please include in the main paper

eq. A55: burned fraction needs to be included at some point to get the total fuel consumed.

Interactive comment on Geosci. Model Dev. Discuss., 5, 2347, 2012.