



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

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Answers to comments of Reviewer 1

We thank the reviewer for spending time on this extensive and detailed review. We appreciate the effort and inputs that helped us to improve on the discussion version of our manuscript.

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 they were not able to reproduce the results presented in Thonicke et al. (2010) applying the

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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 its pros and cons and allows for improvements. 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.

As also requested by the other reviewers, we now present a comparison of the model results with the observed burned area products from GFED and Randerson et al. (2012) in section 4.5. We point out that the vast majority of the Earth’s surface is influenced by human activities at present. As it was never our intention to develop a scheme for modeling fire at present day, we make no attempt to simulate contemporary anthropogenic fire ignitions and active, industrial fire suppression. Instead, in our comparison with observations we model naturally-caused fire only, and concentrate on explaining the residual between observations and model in the context of anthropogenic activities. In those parts of the world with minimal human presence, we are able to identify limitations to our model and suggest areas for future improvement and model development.

We still believe that using Alaska as the region in which to make a comprehensive evaluation of the model results is valid. We chose Alaska because it is one of the few places of the world where anthropogenic impact is minimal and where high-quality long-term model driver data are available. While about 75% of the observed global burned area occurs on land that is substantially affected by human presence, Alaska is characterized by large, intact ecosystems that are largely free of roads, settlements, agriculture, etc. that influence fire ignitions and behavior. Furthermore, in Alaska more than 50 years of high-quality observations of burned area exist, and 25 years

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of ground-based lightning strike data are freely available. In contrast, GFED covers a 15-year period and is subject to a high degree of uncertainty when compared to other datasets, and the LIS/OTD lightning flash climatology for the extratropics is based on only four years of observations. Finally, Alaska fire statistics differentiate between fires of anthropogenic and natural origin, further facilitating our model-data intercomparison. This situation of minimal human interference with fire and availability of high-quality data both for model input and for evaluation are unmatched in the rest of the world. For further discussion on these points, please see our new text in sections 4.5 and 5 of the manuscript.

New features, such as anthropogenic ignitions, are introduced in the model description, but were not applied in the study presented.

The purpose of this manuscript, submitted for publication in a journal intended for model descriptions, was to present a complete description of our current model code. We have clarified this point in the introduction section 1 of the text. A separate publication on the role of humans and fire with focus on the past is in preparation and will highlight the model's performance with respect to the preindustrial human relationship with fire. We believe that publishing a complete description of the model in a single article will facilitate future publications and allow easier access to our work among the wider community.

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.

Thank you.

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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.

As described above, we now include in section 4.5 a global comparison of our model simulation with observed burned area datasets. We comment on the differences between model and observations in light of limitations to our model and suggest important areas for future development.

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.

We significantly shortened and streamlined the introduction section 1, and have tried overall to make the manuscript more concise. We changed the name of the model, and apologize for any confusion caused through our original choice of model name, which we chose to acknowledge that most of the concepts, equations, and parameters used in our model come from SPITFIRE. We further added Allan Spessa, one of the SPITFIRE developers, to the author list in acknowledgement of his contributions to our revised model.

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Improvements/modifications:

New thresholds of FPC > 50% and fuel load < 1000 g C 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?

The main reason for implementing these thresholds, which is analogous to the approach taken by Prentice et al. (2011), was to save computational time. We now clarify this in section 3.1.1 of the manuscript text.

The estimation of fire intensity (I_{surface}) requires a series of calculations from ignitions through fuel moisture to rate of spread. As changes in the litter pools and FPC are updated annually, but the fire routine is called daily, low fuel loading or discontinuous FPC at the beginning of a calendar year will always lead to a low I_{surface} , regardless of changes in FDI, ignitions, or other state variables. Therefore, in a region with a long fire season the savings in calculation time may be considerable. The thresholds for calculation of fire behavior were established on the basis of gridcell-level testing that showed the strong correlation between low fire intensity ($I_{\text{surface}} < 50 \text{ kW m}^{-1}$) and low fuel loads and/or discontinuous vegetation cover.

In a future version of the model, if allocation and other processes currently modeled annually are moved to a daily timestep as we recommend in our manuscript, it would be necessary to adjust or remove these thresholds.

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.

In places where the climatological mean number of lightning strikes is low, e.g., in

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the high latitudes, not accounting for interannual variability in lightning occurrence can mean that lightning-caused fires are never simulated. This result would not be consistent with field observations that show both considerable interannual variability in lightning strikes, and the occurrence of infrequent but large lightning-caused fires. We clarified this reasoning in section 3.1.2 of the manuscript and through an additional figure (Fig. 2) that shows the observed interannual variability in lightning in Alaska based on the ALDS ground-based lightning strike dataset. Our method for imposing interannual variability in lightning occurrence does approximate the climatological mean as can be seen in Fig. S7.

How does the interannual variability look like? Is for example the variability spanning two orders of magnitude as observed for Alaska reproduced?

As noted above, we prepared an additional supplementary figure (Fig. S7) that compares our new CAPE-based method of imposing interannual variability on the LIS/OTD climatology with the interannual variability in lightning strikes observed in the ground-based ALDS dataset. Scaling the LIS/OTD with CAPE-anomalies helps to introduce more realistic interannual variability in the lightning data we are using to drive our fire model. The variability of ground-based observations is still higher than the derived variability, although on a decadal average both are similar. In some regions where LIS/OTD observes no lightning flashes, but lightning is observed in the ALDS data, even our imposition of variability cannot rectify the situation. This is a clear limitation of the currently available global datasets on lightning occurrence.

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.

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It would have been possible for us to present three fire model scenarios for Alaska: 1) with the original SPITFIRE parameterization of lightning ignitions, 2) with lightning ignitions using our CAPE-based interannual variability, and 3) using observed lightning strike information from ALDS. In the interest of conciseness of the paper, we chose to show only the ALDS-driven experiments for comparisons with AFS dataset of observed burned area. We chose this in order to eliminate as much uncertainty as possible that is external to the model formulation itself when comparing the model results with observations. The effects of using the original SPITFIRE lightning ignition scheme can be seen in Thonicke et al. (2010), where essentially no burned area is simulated in Alaska. Our CAPE-scaling method is applied in the global simulations we present in the new section comparing our model results to datasets of burned area. The agreement between model and observations using LIS/OTD and our imposed interannual variability is not as good as when we use ALDS driver data, but this may be expected because, as mentioned above, some gridcells where lightning-caused fire is observed, e.g., in Alaska, have no observation of lightning flashes in the LIS/OTD climatology.

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 affects the results.

The nature of lightning is stochastic. A single lightning strike can be enough to start a large fire if it hits the right place at the right conditions. An example is the Anaktuvuk River fire in Alaska where a single lightning strike resulted in a fire that burned for more than three months and consumed 1039 km² of tundra (Jones et al. 2009). Conversely, thousands of lightning strikes within a single thunderstorm may hit the ground without causing a single fire, depending on what is hit exactly, e.g., bare ground, the rocky

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top of a mountain or cliff, a tree with dry leaves, a dead coniferous tree, a tree with young fresh leaves, an area with dry senescent grass, an area with young fresh grass, etc. We cannot represent such sub-grid heterogeneity in terrain and vegetation in our model. All we have is the number of lightning strikes per grid cell on a given day, and what this will cause exactly on a sub-grid level depends to a certain degree to a hit-or-miss chance factor, modulated by the probability of a successful ignition, itself in part a function of weather and antecedent conditions. Hence, we compare a pseudo-random number with the calculated ignition probability that depends on environmental factors such as vegetation type, fuel dryness (represented by FDI), and fuel availability depending on how much fuel has already been removed by previous fires. This parameterization is intended to mimic the characteristics of real lightning, which sometimes may cause a fire even though conditions were not favorable, e.g. by starting a fire that smolders for days or weeks before spreading, or conversely does not cause a fire although environmental conditions were close to perfect for an ignition. We clarify these points in section 3.1.2 of the text.

The method of using a random does not allow using 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.

All of the random numbers used in the model, both in the weather generator that is part of the original SPITFIRE, and the random numbers that are drawn for comparison with the calculated ignition probability, are pseudorandom numbers. At the beginning of each model run, for each grid cell, a random seed specific to the gridcell's longitude and latitude coordinates is initialized. All consecutive random numbers drawn for a specific grid cell throughout a model run are a series of numbers developed from the initial random seed. What we are in fact using are pseudorandom sequences

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that exhibit statistical randomness while being generated by an entirely deterministic causal process. Therefore, each run using the same input files will generate exactly the same series of random numbers for a specific grid cell. We clarified this point in section 3.1.2 of the text.

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 it 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.

The task of separating human populations into different groups based on their subsistence lifestyle is beyond the scope of the current manuscript and is the subject of separate publications that are under preparation by our group. We include the formulation for anthropogenic burning for these different groups of people for completeness of the model description in a manuscript destined for a journal for model descriptions and to facilitate referencing of the complete model formulation in future publications that deal specifically with past patterns of anthropogenic burning. For the purposes of this manuscript we believe that it is sufficient to acknowledge the differences that these groups would have with respect to their relationship to fire, and to say that such a distinction would be necessary when modeling anthropogenic fire ignitions in preindustrial time.

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.

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Studies like Pechony and Shindell (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.

Active, i.e., industrial, fire suppression is very important in many parts of the world at present day and we agree that it cannot be ignored in applications of global fire models for the industrial era. For the prehistoric and preindustrial periods for which we have developed LPJ-LMfire, active fire suppression in the form that we know at present did not exist, as people lacked the technological equipment, and sometimes motivation, that is required to successfully fight and extinguish most wildfires (e.g., Williams (2002)). Therefore, to address our current research goals we have not attempted to implement a scheme for active fire suppression. Researchers with an interest in present-day burning are welcome to build on our model or on LPJ-SPITFIRE and add an implementation of active fire suppression or a differing implementation of anthropogenic ignitions that is suitable to address their research interests. We discuss these points in more detail in sections 4.5 and 5 of the revised manuscript.

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.

We agree that several of these statements are overly vague and have changed

C1255

our wording to be more specific about the impact of various changes we made. In retrospect, with all of the changes we implemented compared to the original SPITFIRE, it is difficult to disentangle precisely which change had what effect on the final model result. We believe that a systematic treatment of every parameter and formulation we changed compared to the original SPITFIRE would also make the manuscript prohibitively long and not contribute to the usefulness of the current paper. Nevertheless, we make specific comments in our general discussion section 5 on how the changes we implemented affect modeled fire compared to the original SPITFIRE. With respect to the introduction of the O-horizon and variable specification of live grass bulk density, in section 3.2.2 we now illustrate the effect of the changes by providing specific examples of how rate of spread and therefore total area burned is affected by each change.

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.

As described above, we now include a comparison of our model results with contemporary observed burned area datasets. We also provide additional figures showing seasonal fire occurrence and the simulated range of variability in global burned area (Figs. S5, S6). We find this additional analysis illustrative, although we specifically want to emphasize that, due to anthropogenic ignitions and active fire suppression not being implemented for present day conditions, we cannot directly evaluate our model results against observations, except in a few parts of the world where human impact

C1256

on fire is minimal. In the new section 4.5 we comment on the limitations of our current model and suggest areas for further improvement.

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 parameterization.

As explained above, the purpose of this paper is to present a single, comprehensive description of our model as it currently stands in a journal that is intended for technical model descriptions. We are working on separate publications demonstrating the application of the anthropogenic burning implementation for scenarios over the Late Glacial and Holocene. We believe that publication of all of the model's equations in one paper will facilitate referencing of the model in our future works, and promote easier dissemination of our methods to other researchers who may be interested in using our model.

In addition, and also mentioned several times in the manuscript, anthropogenic ignitions are essential for present day fire occurrence. Ignoring anthropogenic ignitions will not allow any evaluation with present day data or comparison to other fire models.

This is correct, and one of the reasons why we chose Alaska for model evaluation, as described above. Our goal in this paper was to describe a model we expressly designed for paleoenvironmental studies. As we note in our new section 4.5 on model comparison to global observations, evaluating our model against present-day observational data is problematic as people today have a completely different relationship to fire than they had during preindustrial time. We also note in sections 4.5 and 5 that

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future work could adapt our model with an existing or new scheme for anthropogenic fire ignitions and suppression to study present and future fire.

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).

We are aware of the fact that the observed biomass data that we use for comparison are the integrated result of environmental and anthropogenic factors that affect biomass, e.g., deforestation, anthropogenic land cover change and land use history, climate variability, and natural and anthropogenic fire. For the biomass comparison in the Amazon region, we 1) study a region where fire is very infrequent, e.g., as observed in the global burned area databases, and 2) we included present-day land use as well as human burning on the level that we defined for farmers. We clarified these points in section 3.2.2 of the text.

From the manuscript it becomes also not clear how land use change is treated.

For the present-day simulation results shown in the manuscript (Alaska results, biomass comparison results for the Amazon region, the new global comparison to GFED) we used the HYDE 3.1 anthropogenic land cover change scenario (Klein Goldewijk et al. 2010). We clarified this point in section 3.4 of the text.

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

C1258

We deleted this statement.

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.

We deleted this paragraph as part of a restructuring and streamlining of the introduction section of the manuscript.

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 (2005) or Pechony and Shindell (2010) do have the same potential.

We revised this statement as part of the restructuring of the introduction.

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).

In section 2 we now provide specific examples to illustrate what we meant regarding deficiencies in the original SPITFIRE scheme, and have removed these vague state-

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ments.

p. 2356/ line 24: "Fires ... 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?

The way the original SPITFIRE handles the effect of previous fires is to reduce available fuel load on average basis over the entire grid cell. Run on a half-degree grid, gridcells have a size varying between roughly 1000 and 3000 km² depending on the latitude. If one fire burns x% of the grid cell and consumes the fuel on the burned area, original SPITFIRE averages the fuel reduction resulting from these x% of burned area over the entire grid cell area. This may work to reduce the fire spread rate for consecutive fires, but is not representative of how fires actually work. Why should the fuel be reduced by a small amount for the entire grid cell when only part of the grid cell burned, and the rest of the grid cell's fuel remains unaffected? Our approach assumes that a fire burning x% of the grid cell will remove the fuel from x% of the grid cell based on the calculated fuel consumption, but the fuel on the remaining part of the grid cell will be unaffected. However, as more and more fires burn within the grid cell over the course of a fire year, the fuel within the grid cell will become increasingly fragmented. The likelihood that a lightning strike will hit a patch of ground that has not yet been burned decreases as well as the likelihood that a new fire will spread far before it runs into a patch that has already burned and been cleared off its fuel. As long as processes cannot be modeled on a subgrid level in a spatially explicit way they need to be taken into account indirectly, and we believe that simply reducing the average rate of spread by thinning out the fuel of the entire gridcell after a fire has affected part of the grid cell is not a very realistic approach to what is actually happening in cases

C1260

where more than one fire per grid cell occurs during a fire year. We clarify this line of argumentation in section 3 of the revised manuscript text.

p. 2359/line 4: "probably" can be removed

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 1000 g m⁻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?"

As explained above, this threshold was introduced mainly to save computation time. A similar approach is used in Prentice et al. (2011). Simulated fires will not spread and are extinguished when surface fire intensity is low ($I_{\text{surface}} \leq 50 \text{ kW m}^{-1}$). Grid-point-level testing has shown a connection between low fuel loads/low vegetation cover and low fire intensity ($I_{\text{surface}} < 50 \text{ kW m}^{-1}$) that will result in fires that don't spread and die. As the fire module is run on a daily basis and the decision whether a fire will spread or die due to low surface fire intensity is made towards the end of the routine after the calculation of fuel characteristics and rate of spread, we made the decision to include the check on FPC and fuel load already at the very beginning of the routine, and quit when it is to be expected that the routine will quit later anyway due to low surface fire intensity. This helps to save computation time. The thresholds mostly take place in areas that naturally do not have much fire, such as desert areas (low vegetation cover, low total fuel load) and the cold high latitudes where vegetation comes to its climatic limits. We clarified these points in section 3.1.1 of the text.

C1261

p. 2361/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 anomaly 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.

Please see our response above in the section addressing general comments.

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

The close correlation between lightning and precipitation is well established (see, e.g., Jayaratne and Kuleshov (2006) and references therein; Michaelides et al. (2009) ; Katsanos et al. (2007)). Observations of lightning strikes under clear sky conditions have been reported, but these are exceedingly rare and are associated with thunderstorms and rain tens of km from the strike. We therefore feel it is justified to disaggregate monthly total lightning strikes to occur only on days with precipitation as defined by our weather generator.

On the other hand, the phenomenon of “dry lightning”, which has the meteorological definition of lightning occurring with less than 20 or 25.4 mm per day, is well docu-

C1262

mented (see, e.g., Rorig and Ferguson, (1999); Hall (2007)). Dry lightning occurs under specific mesoscale meteorological conditions, e.g., in the western United States in spring and summer. Our model approach allows for simulation of dry lightning events, because precipitation is not distributed evenly among all rain days in a month, but rather according to a gamma probability distribution following Geng et al. (1986). This determination of daily precipitation amount means that most days will have relatively low amounts of precipitation. As the monthly total number of lightning strikes is distributed across the wet days of the month, some lightning strikes will occur on days with very low amounts of precipitation (dry lightning). Thus, if antecedent weather conditions have sufficiently dried out the fuel and the total precipitation on any given day with lightning is low enough to not rehydrate the fuel on that day, lightning ignitions will occur.

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.

It is well established that different plant types are more susceptible to lightning ignition than others because of canopy architecture, phenology, typical leaf hydration levels, phenol content, etc. (see, e.g., Hall 2007). Because burned area in SPITFIRE is very sensitive to the number of ignitions, we noticed that treating all PFTs the same way with respect to ignition efficiency was problematic, especially when comparing the tropics, where lightning strikes are extremely frequent, to the extratropics where fewer strikes appear in some cases to cause equal or more amounts of fire. In developing

C1263

PFT-specific ignition efficiency parameters, we took a rather unsatisfactory top-down approach, where we attempted to match the performance of the model with field observations of fire behavior. This optimization of the parameters led to the large range of values selected. We realize this choice of parameters is a limitation of the current model, and highlight this as an important area for further model improvement in section 3.1.2 of the revised manuscript text.

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.

As described above and in the responses to the other reviewers, we do not believe that it is correct to average the effects of individual fires on remaining fuel across the entire gridcell on a daily basis. The SPITFIRE methodology has the advantage of being able to provide an explicit estimate of burned area, as mean fire size times number of active fires, on every fire day. With this information, we calculate the fraction of the gridcell burned, and remove the possibility that this fraction re-burns during the current calendar year. We believe that repeated burning of the same area within an individual

C1264

fire season is rather unrealistic because of the near total consumption of fine fuels during fire spread. Furthermore, lightning ignitions tend to be co-located in space, e.g., concentrated on high terrain (see e.g., Hall, 2007), meaning that with increasing area burned the probability for further fires should decrease. On the other hand, it is certainly a limitation to our current model to reset calculated burned area at the beginning of each calendar year. Fortunately, in much of the world the fire season does roughly correspond with the calendar year, though this is not true in the southern part of the southern hemisphere and in parts of Monsoon Asia. Future developments to the model should include a continuous tracking of burned area over time, which would necessarily involve changing some of the processes that are currently calculated annually in LPJ, e.g., allocation, turnover and mortality, to a daily or at least monthly timestep. We have clarified these assumptions and recommendations for future improvements in sections 3.1.2 and 5 of the text.

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.

While this comment is valid, the modifier on ignition efficiency we added saves calculation time and is further supported by numerous observations (see, e.g., Hall (2007); Kotroni and Lagouvardos (2008); Mazarakis et al. (2008); Uman (2010)). Thonicke et al (2010) define the FDI "in a narrow sense, as the probability that an ignition event will start a fire (regardless of how large the fire becomes once started)". FDI, i.e., the moisture status of the fuel, should have an influence on both the ignition efficiency and the rate of spread. Wet fuel will catch fire with more difficulty than dry fuel when exposed to an ignition source, and if it catches fire then the fire will spread

C1265

more slowly in wet fuel than in dry fuel.

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.

Please see our explanation given on this point in the “major comments” section above.

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?

As we explained in the discussion paper, the amount of land that can be burned by one person in a year depends on the length of the fire season, fuel characteristics, etc. In certain environments, e.g., in tropical savannas, a small group of people can burn thousands of km² in a matter of a few days (see, e.g., Eva et al. (1998)). We therefore do not believe that it is appropriate to assign burned area quota to individuals, which in turn precludes the possibility of directly linking anthropogenically burned area to human population density.

In this revised version of the manuscript we revised the way hunter-gatherer burning is estimated. Instead of using a fixed burning target, we now use a time-dependent function that accounts for the ability of humans to modify their environment using fire. As originally, we assume that foragers prefer to live in semi-open environments (references provided in the text) and that foragers will attempt to use fire to open any landscape that has a high degree of forest cover. The evolution of human burning is

C1266

however time dependent: fire ignitions by foragers are gradually reduced if either 1) the landscape achieves the desired degree of openness (about 50% tree cover) or 2) if burning of the forest has no effect on tree cover, a case in which people “give up”. We introduce our new formulation for forager burning and clarify the above discussion in section 3.1.3 of the revised manuscript text.

For the other two groups of people, farmers and pastoralists, we prescribe fixed burn targets that are a fraction of the non-agricultural area of the gridcell. These burn target fractions therefore represent a specific fire return interval. For example, a target of 20% of the gridcell per year implies that pastoralists will try to burn any given place within a gridcell every 5 years, and a target of 5% implies that statistically the same place will be burned every 20 years.

We estimated the 5-year-return interval for pastoral land use based on present-day recommendations for prescribed prairie and pasture burning (http://www.prairiesource.com/newsletters/92_spr01.htm, http://www.agric.wa.gov.au/objectw/imported_assets/content/lwe/regions/nrr/fire_management_guidelines_for_kimberley_pastoral_rangelands.pdf), with 5 years (20% area per year) being a more conservative estimate. 5% burned area per year for farmers (return time of 20 years) is our own estimate, based on the assumption that farmers will burn little on non-agricultural land, e.g., to clear new land or manage areas of natural land adjacent to their agricultural land and villages.

The success that farmers and pastoralists have in reaching their desired burn target will depend on fire weather and the number of people available to start fires. At very low population densities and under very wet or cold conditions, the burn target may not be achieved. On the other hand, high population densities will provide a sufficient number of people to start fires and reach the burn target under nearly any weather conditions, e.g., as is observed in the traditional burning of wet moorlands of northwest Europe. The burn target itself is not linked to population density, but the fraction of non-agricultural land typically decreases with increasing population density, thus

C1267

limiting the amount of land available for burning.

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.

This scaling factor on active human agents of fire is most important when calculating ignitions among forager populations. In agricultural and pastoral groups, population density will nearly always be high enough to ensure that an overabundance of potential arsonists is available to try to reach our prescribed burned area targets. Among groups of people with a variety of subsistence lifestyles, cognitive, genetic, and economic factors mean that human social organization leads to hierarchies of group sizes. Numerous archaeological and ethnographic studies have demonstrated that these relationships are remarkably stable over time (see, e.g., Hamilton et al. (2007); Whiten and Erdal, (2012)). Marlowe (2005) suggests that the optimal size of a hunter-gatherer group is 30 persons. We assume that three members of this group, e.g., able bodied young males, will be responsible for fire management in the territory of the group, and allow for the possibility that the total number could be smaller at times, e.g., during colonization of new territory. We now clarify this reasoning in section 3.1.3 of the text.

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.

The risk factor (rf) should be used as a multiplication factor in equation 10 and 11 to cut down on human ignitions as fire danger increases; we corrected this omission in our revision. We chose a lognormal distribution with a maximum of 1 at an FDI of 0.25 that then quickly declines towards zero as FDI increases and makes it more and more unlikely that people will keep causing fires when it becomes too risky. A more simple

C1268

function might serve the same purpose, though lognormal relationships are common in nature. Moreover, this combines with the restriction that people will stop igniting fires on days when the average fire size increases to more than 100 ha. Using the risk factor below this absolute cutoff threshold is designed to capture the way in which (preindustrial) people will become more careful and conservative about starting new fires when they notice that fire danger is high.

p. 2367/line 7: Equation (9) is not clear to me. What is A_{cg} ? Do you really take the difference? And why is $burned_{bf}$ taken into account?

“ A_{cg} ” is a typo and should be A_{gc} , which is the grid cell area (Tab. 1). We corrected this in the manuscript. We are taking the difference between the annual burn target of a specific population group and the area that has already burned during the course of the current fire year in order to reduce the target as people are approaching it. Once the target has been reduced to zero, people will stop igniting fires. The reason we subtract the 20-year-average $burned_{bf}$ is to take into account the baseline burning that occurs that helps people to reach their target. For example, in a place that already has substantial amounts of natural fire, people will reduce their target accordingly so as to not overburn.

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 A_{gc} stand for. Does this equation allow accounting 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.

C1269

We fixed a typo in Eq. 12 in the revised version of the manuscript. A_{gc} is the size of the grid cell in ha (Tab. 1), and f_{nat} (Tab. 1) is the fraction of grid cell area that is covered with natural (i.e., non-agricultural) vegetation. The equation accounts for changing land use, as fragmentation is recalculated every year based on the land use dataset used to drive the model. The size of a natural patch is not limiting when land use is less than 40%. For agricultural land occupying between 40 and 60%, the fragmentation effect increases very quickly, reducing the average contiguous patch size for the natural land from 99.5% to 20.8% of the grid cell area. The probability that the size of a natural patch will become limiting to the average size of an individual fire therefore depends on the land use fraction of a given grid cell and the environmental conditions restricting the average size of an individual fire (rate of spread, fire duration). With respect to the question of why the degree of fragmentation limits average size and not the actual size, SPITFIRE calculates only the average size of individual fires (cf. Thonicke et al., 2010). We have added a figure showing the scatter plot and curve fitting parameters resulting from the Monte Carlo simulation as supplementary material (Fig. S1).

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.

In this version of the model we do not specify the timing of the burning of agricultural land. Depending on specific agricultural practices, crop residues may be burned in-situ on the fields, or collected and burned throughout the year, e.g., as a fuel source. Fields that are burned may be burned immediately after harvest, or shortly before planting, or both. In favorable environments where double or triple cropping was and is common, it is conceivable that the same fields are burned several times per year. Currently, cropland burning is evenly distributed on all days of the year that have no

C1270

snow cover and a temperature above 0 °C. We clarified this rationale and assumption in section 3.1.4 of the text. Future improvements to the model could attempt to resolve the temporal pattern of cropland burning by using a more sophisticated crop module for LPJ (e.g., that of Bondeau et al., (2007)).

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?

In the new supplementary figure S3 we present a global-scale before-and-after comparison with respect to the changes to decrease overall simulated biomass in LPJ (increase of the maximum possible crown area, establishment rate). This figure shows the changes in simulated biomass in a scenario completely without fire, and a scenario with natural fire and land use. The 5-15% reduction in aboveground biomass mentioned in the manuscript refers specifically to the area of the Amazon basin shown in Fig. 2, based on a simulation that includes natural burning caused by lightning, anthropogenic burning and human land use, to make the simulation result most comparable to the Saatchi et al. (2009) satellite observations. The modifications we made to LPJ to reduce aboveground biomass lead to a biomass reduction on a global scale, although reductions are small in areas with low biomass and are most pronounced (up to 15%) in areas with very high biomass such as the humid tropics. In order to judge definitively if there are places globally where the reduction would be too strong, we would need to compare our simulated biomass to observations of actual biomass. Outside of the tropics forest inventory data could be used to evaluate our revised formulation, but no single homogenized dataset exists. A systematic comparison to a variety of data sources would require detailed information on forest history and is beyond the scope of this already long manuscript. We acknowledge

C1271

the need for further benchmarking of LPJ as a worthwhile goal for future research. Overall, our qualitative assessment of our updated LPJ results against point observations of aboveground biomass is that the model still has a tendency to consistently overestimate biomass rather than underestimating it. We cover these points in section 4.1 of the revised manuscript text.

p. 2370/line 26: “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).

This contradiction is a result of unclear wording in our original manuscript, which we clarified in our revision. The simulation results of global burned area fraction in Thonicke et al. (2010, Fig. 3c) indeed show that the original SPITFIRE simulated very low amounts of burned area (less than 1%) in the southern boreal regions of Canada and Russia (note that all burning down to as low as 0.4% is in the lowest category in this figure, and that the color scale used is highly nonlinear). In this same figure, the northern boreal regions and subarctic appear to be completely within the zero burned-area category. The original version of SPITFIRE (referred to on p. 2377, l. 7) does not simulate fire in these regions that is consistent with observations.

The overburning in the boreal regions that we refer to on page 2370, l. 26 refers not to the original SPITFIRE, but to an intermediate, modified version of the model that, e.g., introduced multi-day burning. Somewhere in the process of making these modifications, once we managed to introduce fire into the boreal regions, this problem became apparent and needed to be addressed and solved, and, as we describe, we traced the problem back to the excessively deep fuel bed that accumulated in the model, which we reduced by introducing the new O-horizon. We clarified these points in the text (section 3.2.2).

C1272

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?

The fast soil organic matter pool has a nominal turnover time of 20 years at a temperature of 10 °C. The nominal turnover time of the O-horizon is closer to the litter pools that it replaced. We clarified this point in section 3.2.2 of the text.

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 1 and 12 kg/m³. 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/m³. This should be mentioned here.

The GDD20 we used is part of the original LPJ DGVM (Sitch et al. 2003) and is defined as the 20-year running mean of the annual sum of degree-days on a 5 °C base. In the tropics, the annual GDD sum can be as high as 10000, whereas in high latitudes values are typically 1000 or less, which leads to the range of densities that we wanted to approximate with this equation. Equation 13 only applies to the bulk density of grasses, and is constrained to not exceed 12 kg m⁻³; we apply no such constraint for woody vegetation. Abundant field evidence demonstrates that tropical grasses are typically tall, whereas herbaceous tundra is short and often grows in dense tussocks (see e.g., Breckle (2002); Gibson (2009)). We developed this relationship because we wanted to avoid introducing new plant functional types (although that could have been a potentially more valid approach) and we use the 20-year running mean GDD because grass morphology (therefore bulk density) should not be influenced by interannual variability in climate, as individual species tend to display a relatively stable growth form over time. We clarify these points in section 3.2.2 of the text.

C1273

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?

The original equation by Mell et al. (2012) does not take fuel bulk density into account. We introduced gs as a multiplication factor to establish a dependency on fuel bulk density. Compared to the original SPITFIRE rate of spread equation the equation based on Mell et al. is simpler in so far as it depends on less parameters. For a given fuel bulk density, the new equation only depends on wind speed and the ratio of relative fuel moisture to its moisture of extinction (rm), whereas the original rate of spread equation in addition requires information on the surface-to-volume-ratio and the total dead fuel mass. Given a fuel mass of 1 kg m^{-2} and a surface-to-volume-ratio of $66 \text{ cm}^2 \text{ cm}^{-3}$, at a fuel bulk density of 2 kg m^{-3} , the new equation produces consistently lower estimates for the rate of spread in grass compared to the old equation, except for completely dry fuels ($rm=0$) and low wind speeds of less than 50 m min^{-1} . For example, for an rm of 0.1 and a wind speed of 60 m min^{-1} , the new equation results in a ROS of 13 m min^{-1} , whereas the old equation would produce a ROS of 30 m min^{-1} using the parameters given above. We clarify these points in section 3.2.2 of the text.

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

After further reflection and model testing, we decided that the crown fire scheme we originally presented was unsatisfactory, e.g., simulated crown fires never occurred in many places where they are observed, and so requires further development. In the interest of making the paper shorter, we removed the section on crown fires from our model and from the manuscript, and highlight this as an important topic for future research in section 5 of the revised manuscript.

C1274

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.

See previous comment.

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 actually favor high fire spread rates. With the median slope angle between 2 and 17.2, 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.

It is true that individual slopes may favor higher fire spread rates. However, every up-slope has a corresponding down-slope on the other side of the ridge. Fires reaching the crest of a ridge can be impeded or slowed down by the fact that from this point onward they will burn down the opposite side of the slope, together with the fact that the downslope will have a different exposure and therefore water status (fuel moisture, vegetation type) than the upslope, that the fuel continuity may be broken at the top of the slope due to rock outcroppings, or that valley bottoms may have streams wide enough to hinder the further expansion of fires.

LPJ-LMfire is designed to be used at 0.5 degree spatial resolution. At this scale, no individual gridcell of $1500\text{-}3000 \text{ km}^2$ represents a single slope. Rather, each gridcell contains a variety of slopes, with different aspects and drainage densities. The slope factor we introduce, now shown in a global map in supplementary Figure S2, reduces mean fire size in mountainous areas with complex terrain, where we would expect topographic heterogeneity to be most important. We clarify these points in section

C1275

3.2.3 of the text.

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?

In the original SPITFIRE all fires were extinguished after a maximum of 241 minutes of spread. With the implementation of our new scheme for multi-day burning and smoldering ignitions, we needed to define the criteria under which a fire-stopping or fire season ending event would occur. Field observations have shown that the amount of precipitation required to slow or stop wildfires differs depending on the type of fuel that is burning (Latham and Rothermel (1993); Hall (2007); Hadlow (2009)). We revised this imprecise wording in section 3.2.1 of our revised manuscript.

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.

The behavior of active fires typically has a distinct diurnal cycle, with the most active period of burning occurring during a few hours from midday into the late afternoon (Pyne et al. 1996). During the night and early morning, when wind speeds are typically low and humidity increases, fires may smolder in place without spreading, only to resume spreading the following afternoon. We took the 241-minute limit to fire duration in the original SPITFIRE to reflect this variable fire behavior over the course of a day, effectively by calculating rate of spread for a few hours based on mean daily wind speeds. We acknowledge that this is a great simplification to our model (as it was to the original SPITFIRE), but one has to remember that we have tried, similarly to the original SPITFIRE, to implement a fast, simplified fire behavior model that can be applied globally on centennial to millennial time scales. Simulating the diurnal course

C1276

of fire behavior would require calculating rate of spread on an hourly or even shorter timestep, thus requiring additional input data and complexity, similarly to what might be used in an operational fire forecasting model.

By carrying over active ignitions from one day to the next, we separate the concept of an active ignition from a spreading fire. We believe that this formulation is valid given the purposes for which we developed the model, and has the further advantage of being able to simulate large amounts of burned area in places where ignitions are rare, e.g., as is observed in the boreal regions and subarctic. We clarified these points in section 3.2.1 of the text.

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

Thank you for pointing this out, we have corrected the mistake in this equation in our revised manuscript. The total number of fires is used to calculate the total area burned by multiplying the number of fires with the average size of an individual fire.

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?

The merging of fires is realized in equation 32 by reducing the fires on the current day by the product of burned area fraction and sum of fires from the previous day and the current day. We clarified this in section 3.2.1 of the text.

p. 2378/line 14: “too many trees being killed”, please show or give numbers on the

C1277

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.

This qualitative assessment was based on a comparison of simulated total tree cover to observations of global tree cover such as the GLCF tree cover product (<http://glcf.umd.edu/data/treecover>). Essentially, in the model before we corrected it, all of the woody vegetation in a landscape that we expected to be forested was being killed by fire. We therefore arrived at the conclusion that we were “killing too many trees”. We clarified this text in section 3.3 of the manuscript.

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

We specified a constant p-value of 3 for all tree PFTs. This value differs from the original SPITFIRE only for PFT 3 (temperate needleleaf evergreen), which had the value 3.75. We clarified this point in section 3.3 of the text.

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.

The standard version of LPJ has the limitation that it takes several model years (3-10, depending on climate) for the simulated aboveground biomass of the herbaceous PFTs to reach equilibrium. This is not realistic based on field observations, where most grasses can grow from bare ground to a closed canopy and complete their life cycle over a period of months (Gibson, 2009). In a situation where herbaceous biomass is burned and removed from the aboveground live biomass, realistic annual maximum

C1278

amounts of biomass will therefore never be reached. This limitation feeds-back not only to the rate of spread calculations, but also to the general biogeochemical cycling simulated by LPJ. Our adaptation of simply not removing the herbaceous aboveground biomass was also used in Kaplan et al (2011, 2012) and leads to more realistic model results. Ultimately this limitation to the model, which was present in the original LPJ-DGVM, should be addressed by moving to a daily timestep for grass allocation, e.g., as has been done for crops in LPJ-ML (Bondeau et al., 2007). We clarified these points in section 3.3 of the text.

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.

In the current version of the model, consistent with the original LPJ, biomass pools are updated annually along the other annual processes in LPJ (carbon allocation, plant allometry and growth, leaf and fine root turnover, mortality and establishment). As discussed above, ultimately it could be desirable to calculate many of these processes on a daily timestep, e.g., in the context of grass dynamics, but that is beyond the scope of the current model description.

The fundamental issue here with respect to fire is the issue of spatial averaging on a gridcell of 1500-3000 km². As discussed above, we believe that, because of the nonlinear nature of the rate of spread equations, our scheme tracking changes in burned fraction over time is more realistic than reducing the total aboveground biomass pool on a gridcell average. Removing biomass on a daily, spatial average basis results in unrealistic rates of spread and fire size as the fire season progresses, particularly when multi-day fires are implemented as we have done it here. Ultimately, it could be desirable to account for subgrid-scale disturbances with cohorts or replicate

C1279

patches much in the way this is treated in LPJ-GUESS (Smith et al., (2001)), although this would present a new problem of tracking inter-patch fires in space. We clarify these points in section 5 of the manuscript.

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.

We moved this text out of the results section into a separate subsection at the end of section 3.

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 cannot be explained by natural ignitions, as the authors state in an earlier paragraph.

As discussed above, we include the section on anthropogenic ignitions for completeness of the model description, for transparency, and to facilitate the usefulness of this paper as a comprehensive model description, in a journal specifically intended for model description papers. Additional manuscripts describing the application and evaluation of the anthropogenic burning scheme are in preparation. We did include anthropogenic burning from farmers in the biomass simulation shown in Fig. 2 for the Amazon area.

p. 2380/line 24: please add the respective model values for the different ecosystems,

C1280

otherwise these numbers do not contribute to the evaluation.

The values for aboveground biomass provided here are rough guidelines of what may be expected for the different ecosystems. Naturally, these are averaged estimates and significant variation around these mean values can be observed due to varying site conditions, e.g., as a result of varying soil properties and local climate. The way in which these guideline values compare to simulated biomass under natural conditions, i.e., no land use and with lightning-caused fires, are provided in Fig. 10a.

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 it 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 patterns you identified from literature? Please focus on the evaluation of your model.

As for the litter and soil organic matter pools in the original LPJ, the turnover time quoted here refers to the nominal turnover time of the pool at standard temperature and moisture conditions. The actual decomposition rate of the O-horizon is temperature-dependent and handled in the same way as the decomposition of the other soil pools (fast soil pool, slow soil pool) Our approach to modeling an organic soil horizon may be simplistic compared to site-scale complexity of soil dynamics, but this then also holds true for the generally simplistic way of modeling other soil pools in LPJ which are essentially handled in the same way. The establishment of the O-horizon was essential for realistic simulation of fire rate of spread in boreal and subarctic ecosystems that otherwise accumulated unrealistically deep, lightly packed fuel beds. In section 4.2 we provide observations of O-horizon properties to demonstrate that our modeling approach is reasonable.

C1281

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

Removed.

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?

We were specifically interested in higher latitudes, because this is where we had the problem of excessively deep fuel beds, and because low latitude terrestrial ecosystems typically have small amounts of stored litter due to fast turnover at high temperatures. According to FAO nomenclature, soil orders with an O-horizon (e.g., spodosols, histosols), are characteristic of the high latitudes. As can be seen in Fig. 3., relatively little carbon is simulated in the O-horizon at low latitudes.

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

As noted above, we added a global-scale comparison to observational datasets of global burned area in section 4.5 of the manuscript.

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?

The O-horizon does not increase the interannual variability of the fuel load. Rather,

C1282

it systematically decreases the depth (and increases the density) of the fuel bed in places with slow decomposition, as described above. The effects of using a pseudorandom number sequence are also discussed above in response to the general comments. In short, all runs for the same geographic region with the same input data are 100% reproducible.

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.

We added a series of new supplementary figures (Fig S7) comparing the ALDS lightning with our CAPE scaling approach to the LIS/OTD climatology to demonstrate the effectiveness of our approach. It is not perfect, but in areas with low amounts of lightning on average, our method does generate a realistic amount of interannual variability.

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

In the supplementary figures S5 and S6 we show the variance and seasonality of simulated fire. In Alaska, where the fire season is short, our model simulations show good correspondence with the observed seasonality in fire. In other parts of the world, anthropogenic activities affect the seasonal pattern of fire to varying degrees, and as described above, capturing these effects was beyond the scope of the current study. We clarified these points in section 3.1.4 of the text.

p. 2386/line 17: Why do you describe these single fire events? Can your model

C1283

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.

This paragraph has been shortened and clarified. The point was to highlight that exceptional fire years within the Arctic tundra are known to occur, and that we are able to simulate occasional years with large amounts of fire, and fires that persist for several months. We clarified this text in the revised manuscript (section 4.3.1).

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.

As described above, we now include a global comparison of modeled burned area with observational datasets. We also include supplementary figures S5, S6 showing the modeled seasonality and interannual variability of fire. We discuss the discrepancy between modeled and observed biomass in our response to the following point.

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.

In some regions, LPJ-LMfire simulates biomass greater than observations where fires are rare, e.g., in the deserts of central Australia and the southwestern USA, and in the humid areas of the central Amazon. In other regions, we compare simulated biomass with field inventory observations that are specifically measuring biomass in

C1284

undisturbed settings. We clarified these points in section 4.4 of the text.

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

We rephrased this sentence to explain the advantages of our updated model, highlighting, e.g., the boreal regions and subarctic where SPITFIRE did not simulate burned area that was consistent with observations.

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?

In the current version of the model, we reincorporated the cambial kill equations and parameters used in the original SPITFIRE. To accomplish this, we adapted the height-class structure used in the original SPITFIRE model (not published in Thonicke et al., 2010), applied observed bark thickness-stem diameter relationships, and tested the cambial kill parameters. This procedure is described in section 3.3 of the revised manuscript.

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

As described above, we keep the anthropogenic ignitions discussion for completeness of the model description. Nevertheless, we removed this paragraph as we agree the discussion is not necessary.

C1285

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.

Our conclusion on this point serves to remind the reader that we outlined a strategy for modeling anthropogenic ignitions and that it is one of the novel aspects of the current manuscript.

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

In order to streamline the text and make it easier to read, and following general practice for this type of paper, we provide all of the variable definitions and units in Table 1 and Table A2.

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

These are now defined.

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

Thank you for pointing this out. We moved this equation to the main paper in the section on fuel moisture and rate of spread.

C1286

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

Eqn A44 was incorrect, and should not have included the slope factor. We corrected it and leave this original SPITFIRE equation in the appendix.

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

The total fuel consumed ($FC(class)$) is calculated in $g\ m^{-2}$. To update the fuel pools, eventually it is necessary to multiply the consumption per m^2 with the total area that burned.

Figure captions for supplementary material

Fig. 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.

Fig. 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.

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Fig. S3:

This figure illustrates the effect on global biomass caused by the changes to maximum crown area and maximum establishment rate in LPJ. Panels a) to c): Scenario completely excluding fire, to illustrate how the underlying basis biomass for fires changes. Panel a): Old LPJ parameterization, with a maximum crown area constraint of 15 m² and a maximum establishment rate of 0.12 individuals m⁻². Panel b): New parameterization with a maximum crown area constraint of 30 m² and a maximum establishment rate of 0.15. Panel c) Difference in biomass between b) and a): a reduction in living biomass can be observed globally, but total values of reduction are highest in the equatorial tropics where total biomass is highest. Panels d) to f) show global biomass for a simulation run including anthropogenic land use based on HYDE land use and lightning-caused burning on non-agricultural land, for the old parameterization of maximum crown area and maximum establishment rate in panel d) and the new parameterization in panel e), and the difference between e) and d) shown in panel f).

Fig. S4:

Panel a): Simulated maximum crown area for a world without fire after implementation of a maximum crown area threshold of 30 m² instead of 15 m². Panel b): Simulated maximum crown area for a simulation run with lightning-caused fire. All places with maximum crown area between 15 m² and 30 m² are areas where the increase of maximum crown area contributes to the reduction of live biomass by decreasing individual density compared to the old parameterization.

Fig. S5:

Panel a) Average annual burned area fraction for a simulation run without agricultural land use, and lightning-caused fires over 25 years. Panel b) Variance in annual burned

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area fraction in reference to panel a). Panel c) Average annual burned area fraction for a simulation run with lightning-caused fires, but fires being excluded from agricultural land. Panel d) Variance in annual burned area fraction in reference to panel c).

Fig. S6:

Figure illustrating the seasonality of fire under natural conditions (no land use, lightning ignitions). The top four panels show the average burned area fraction per season over 25 years. The two bottom panels identify simulated peak fire month based on burned area fraction and a seasonal summary highlighting which season has the highest simulated burned area fraction at a given location.

Fig. S7:

Statistical comparison between ALDS lightning observations and LIS/OTD-derived, CAPE-scaled lightning for the time period 2001-2010. While average annual lightning strikes between ALDS (panel a)) and LIS/OTD-derived data (panel s)) are comparable, the variance between years is higher for the ALDS data (Panel b)) than for the LIS/OTD CAPE-scaled data, indicating that even with the scaling to CAPE anomalies the total range of interannual variability in lightning is still underestimated. Using LIS/OTD-data for Alaska is in general problematic as there are overall only four years of data available. Panels d) and e) compare the minimum lightning strike density for each grid cell between ALDS data and LIS/OTD-derived data, and panels f) and g) the maximum lightning strike density. It becomes obvious that the underestimate in interannual variability for the LIS/OTD-derived data is both due to an underestimate of maximum lightning strike density as well a tendency to overestimate minimum lightning strike density.

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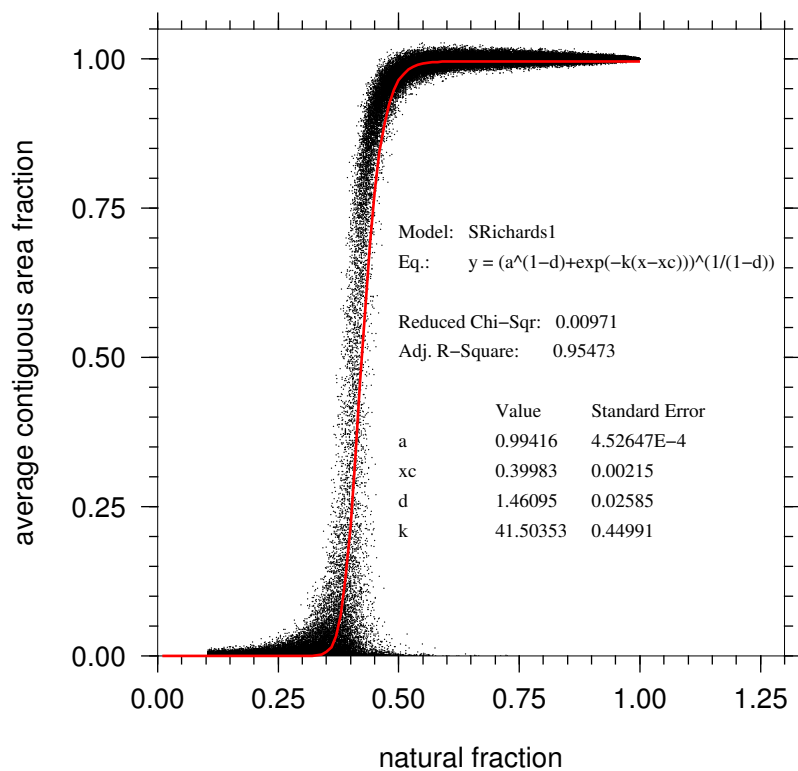


Fig. 1. Supplementary figure S1

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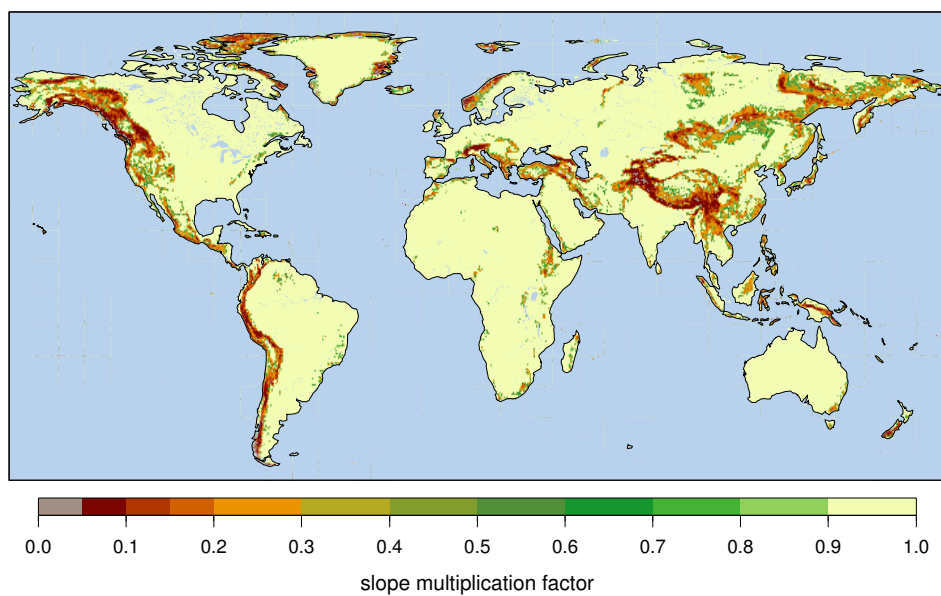


Fig. 2. Supplementary figure S2

C1295

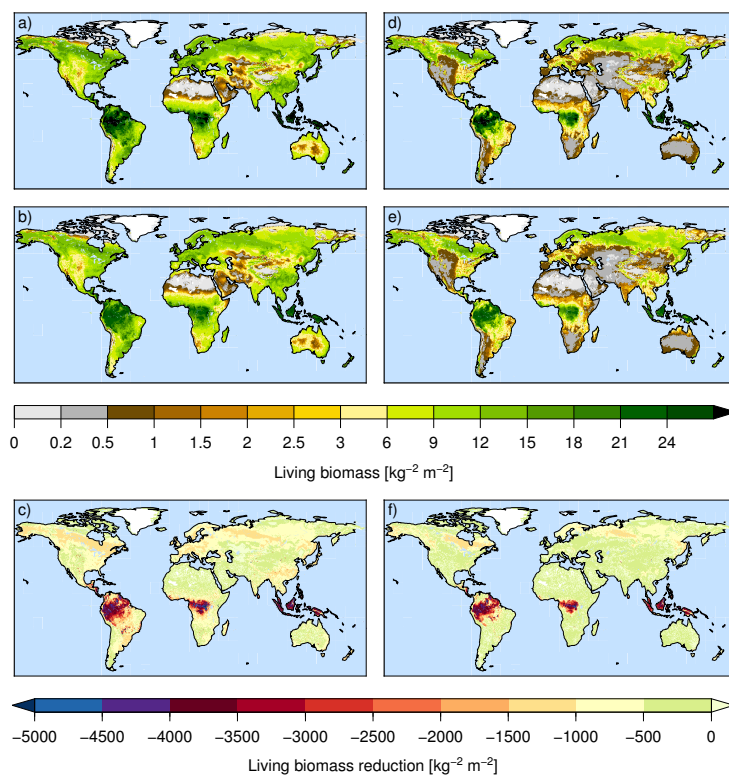


Fig. 3. Supplementary figure S3

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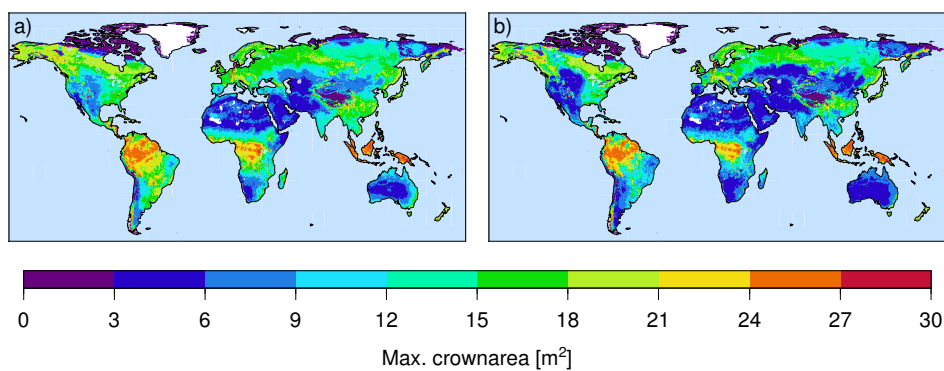


Fig. 4. Supplementary figure S4

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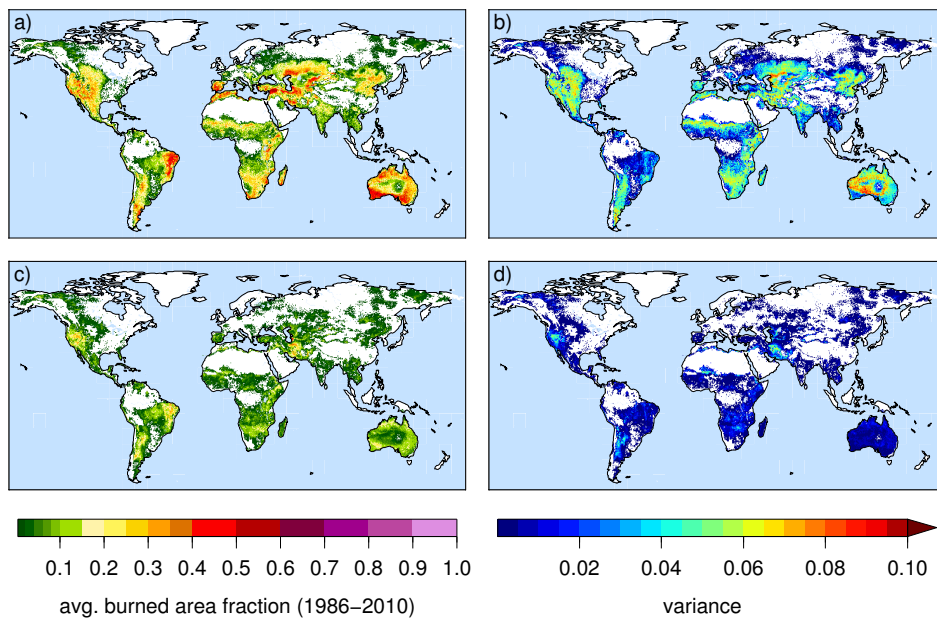


Fig. 5. Supplementary figure S5

C1298

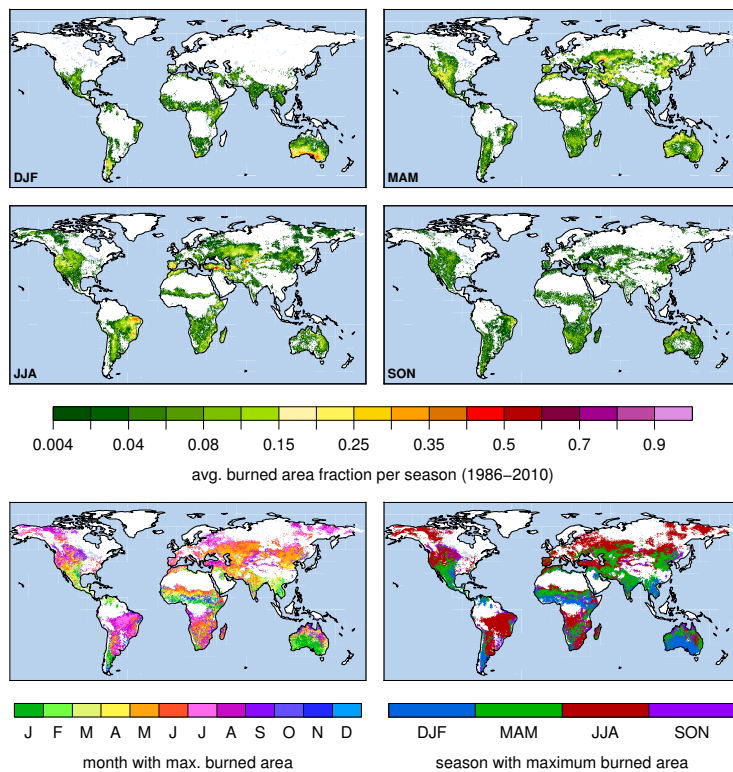


Fig. 6. Supplementary figure S6

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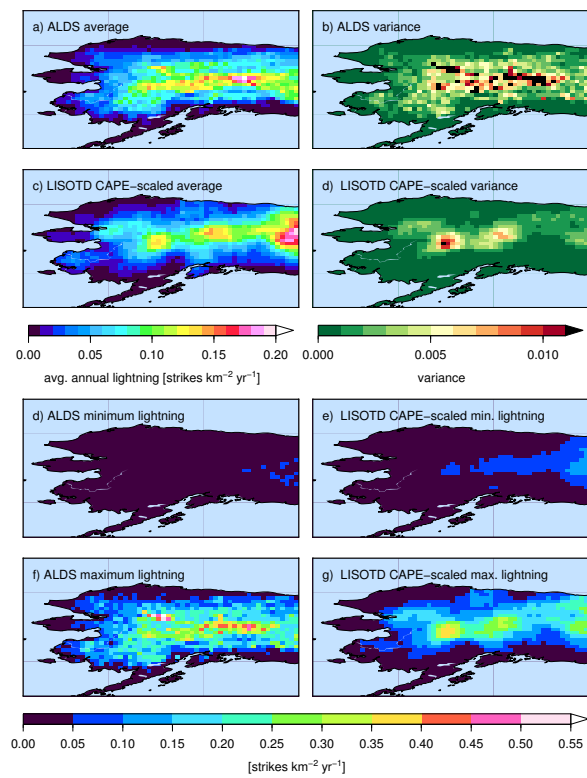


Fig. 7. Supplementary figure S7